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State Child Tax Credits and Parental Labor Supply: Evidence from Staggered Adoption

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State Child Tax Credits and Parental Labor Supply: Evidence from Staggered Adoption*

Abstract

States increasingly supplement the federal Child Tax Credit (CTC) with credits that vary in generosity, refundability, and earnings requirements. We examine changes in parental employment, labor force participation, and work hours following state CTC adoption using the Current Population Survey Annual Social and Economic Supplement (CPS-ASEC), 2004–2026. Our preferred stacked difference-in-differences design includes 12 adopting states with complete event-time support and compares them with never-treated states and states that adopted after our evaluation window. Averaging over the first three post-adoption survey years, the estimated effect is 0.05 percentage points (SE 1.03) for employment and −0.46 points (0.82) for labor force participation; estimates for work hours are also close to zero. Across secondary specifications and CPS Basic Monthly analyses, extensive-margin estimates were similarly small. Larger simulated benefits are associated with fewer hours among employed parents, but the relationship is also evident before adoption and may partly reflect how benefits depend on earnings. Overall, the estimates provide little indication that state CTC adoption has substantially reduced parental employment, although meaningful effects in either direction cannot be ruled out under moderate departures from parallel trends.

JEL classification

H24, I38, J22, H75

Keywords

Child Tax Credit, labor supply, employment, state policy, difference-in-differences, tax credits

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1 Introduction

The Child Tax Credit (CTC) is one of the main ways the U.S. provides financial support to families with children. Before 2021, the federal CTC was only partially refundable, and families needed earnings to receive its refundable portion. The temporary federal expansion in 2021 increased the maximum benefit, extended the full credit to families with little or no earnings, and provided part of the benefit through advance monthly payments (Collyer et al. 2023). The expansion contributed to a historically large decline in child poverty (Parolin et al. 2022), while renewing a longstanding policy debate over whether making child benefits more generous and less conditional on work reduces parental employment (Ananat et al. 2024; Corinth et al. 2021). Only six states had adopted their own CTCs before 2021, but more states have enacted or expanded their CTCs after the federal expansion expired. These state policies differ considerably in maximum benefits, child age restrictions, refundability, earnings requirements, and income phase-outs (Parolin 2025; Vinh et al. 2025).

The effect of a CTC on labor supply is theoretically ambiguous. A credit that provides income independent of work can generate an income effect that reduces labor supply, while a benefit that phases out with earnings can increase effective marginal tax rates and further discourage work (Goldin, Maag and Michelmore 2022). Conversely, credits that phase in with earnings can encourage employment, as the Earned Income Tax Credit has done (Eissa and Hoynes 2006; Eissa and Liebman 1996). State CTCs also may relax liquidity constraints that impede employment—for example, by helping families pay for child care, transportation, and other expenses that make working possible (Goldin, Maag and Michelmore 2022; Morrissey 2017). The net effect therefore depends on program design, and parents may respond differently depending on their economic circumstances and whether they can adjust employment, hours, or both (Eissa and Hoynes 2006; Saez 2002). The considerable heterogeneity across state CTCs means that the implications of a child tax credit for labor supply are not obvious a priori.

Most of the recent empirical evidence comes from the 2021 federal expansion. Pac and

Berger (2024) found at most a small decline in caregiver employment using difference-in-differences comparisons with childless adults. Ananat et al. (2024) also reported inconsistently signed and statistically insignificant effects on parental employment, whereas Han, Meyer and Sullivan (2022) found somewhat larger employment reductions. These estimates generally suggest smaller responses than some *ex ante* simulations predicted (e.g., Corinth et al. 2021). Evidence from state policy is more limited. Using administrative data and quasi-random eligibility changes in California's Young Child Tax Credit, Goldin et al. (2024) found very small maternal labor force exits following the removal of an earnings requirement. Unrath et al. (2026) drew on recent reforms to calibrate simulations of a hypothetical unconditional state credit and predicted modest employment reductions. It remains unclear how parental labor supply has changed following the adoption of the varied CTC policies that states have actually implemented.

State CTCs offer an opportunity to examine labor supply responses outside of the unusual economic and institutional circumstances of 2021. Unlike the temporary federal expansion, the state CTCs generally establish recurring changes to tax transfer schedules, and their different designs provide a broader view of how child benefits operate. This variation also complicates estimation. States adopted credits at different times, have often revised them after enactment, and may experience different treatment effects. Conventional two-way fixed effects (TWFE) estimators can be biased under staggered adoption when effects vary over time (de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Roth et al. 2023; Wang, Hamad and White 2024). Comparing generosity across states also requires translating their statutory rules into benefit measures that can be applied consistently to survey respondents.

We study state CTC adoption using Current Population Survey (CPS) Annual Social and Economic Supplement (ASEC) data from 2004–2026. For each state, we identify the first tax year in which its credit is available and assign event year 0 to the following ASEC survey year. Our primary approach is a stacked difference-in-differences design that compares adopting

states with never-treated states and states that adopted after the relevant evaluation window, applying the corrective weights proposed by Wing, Freedman and Hollingsworth (2024). We estimate, first, effects based on a binary indicator of whether a state adopted a CTC and, second, continuous-treatment models based on simulated CTC benefit amounts. We also use conventional TWFE models and CPS Basic Monthly data, along with placebo, triple-differences, and other sensitivity analyses.

We make two contributions to the evidence on child benefits and labor supply. First, we estimate responses to CTCs as enacted across states, rather than extrapolating from the temporary 2021 federal expansion or a single state reform. We code 16 state policies adopted during tax years 2004–2024, and observe outcomes through the 2025 tax year. Twelve adopting states have complete support for the primary balanced event window (-10 through $+2$); additional cohorts enter an unbalanced-support sensitivity analysis. Second, we examine both employment and labor force participation and changes in hours worked, with complementary analyses relating these outcomes to simulated credit generosity. We assess the credibility of the identification assumptions using event-study diagnostics and sensitivity analyses that allow for departures from parallel trends. Across these analyses, estimates provide little indication that state CTC adoption has reduced parental employment.

2 Methods

2.1 Data and analytic samples

We use the CPS-ASEC from 2004 through 2026 as the primary data source. The CPS is a serial cross-sectional survey administered by the U.S. Census Bureau for the Bureau of Labor Statistics, and the ASEC provides detailed information on household composition, income, tax filing, and labor market outcomes (Flood et al. 2023). The start year for the analysis reflects the availability of IPUMS CPS tax unit identifiers needed to link adults and dependent children consistently over the study period.

The main ASEC analytic population consists of adults ages 18-64 in tax units with at least one child age 0-17. We exclude respondents coded as serving in the Armed Forces and observations for which tax filing status is missing (n=12,228); filing status is required to assign CTC eligibility and benefits. The analytic file contains both adults with children and childless adults, so that the latter can be used in placebo and triple-differences analyses. For the primary employment and labor force participation outcomes, the TWFE analysis contains 1,005,413 person-year observations. The stacked data contain more observations because the same control observation can appear in multiple cohort-specific sub-experiments; the primary stacked employment and labor force participation models contain 2,366,798 stacked person-year observations. The primary stacked models contain 1,685,365 observations for usual weekly hours at current jobs and 1,910,956 for usual weekly hours in the prior calendar year. Work-hours samples are smaller because the primary intensive-margin analyses are restricted to respondents reporting positive hours.

We use the CPS Basic Monthly files from 2004 through 2025 for secondary analyses. These data offer substantially more observations and higher-frequency outcomes but less detailed income and tax information. The full Basic Monthly analytic sample contains 7,661,939 person-month observations after sample restrictions and successful assignment of a simulated CTC benefit for the continuous-treatment analyses. We also analyze the subset of Basic Monthly respondents who can be linked to detailed ASEC information for the same year (Flood et al. 2023; Parolin et al. 2022).

2.2 State CTC policy coding

We assembled a state-by-tax-year policy file describing state CTC eligibility, refundability, benefit formulas, earnings requirements, and phase-in and phase-out schedules. Coding was based on enacted legislation, state tax instructions and revenue department guidance, and secondary policy compilations (National Conference of State Legislatures 2026; Vinh et al. 2025). Appendix Table A1 summarizes the policy rules used in the simulation for tax year

2025 (the policy snapshot illustrated in the supplement) and identifies the first effective tax year used in the analytic data. The coded policies range from small nonrefundable credits to refundable credits worth several thousand dollars for some families. By 2025, 12 of the 16 coded state CTCs were refundable.

For each state, we distinguish the enactment year, the first tax year in which families could claim the credit, the corresponding ASEC survey year, and subsequent major expansions. Several states changed their credits substantially after adoption, making this distinction important. Maryland is a modeling exception: although a narrowly targeted disability-based credit existed beginning in tax year 2020, we code 2023 as the effective treatment year because the earlier provision applied to a very small group that cannot be identified reliably in the CPS.

The policy file includes 16 states with a qualifying CTC during the study period. Because the District of Columbia's and Georgia's CTCs begin after the tax years observed in the data, both are treated as unexposed throughout the current analyses. Simulated benefits represent statutory policy rules and not observed payments. For several nonrefundable credits, the simulation reports the allowable statutory amount without capping it at state income tax liability; Appendix Table A1 identifies these cases.

2.3 Treatment measures

Our primary treatment is an indicator for adoption of a state CTC, regardless of refundability. We focus on refundable credits in sensitivity analyses (Appendix Tables A5 and A6). For ASEC analyses, a tax-year policy is first coded as treated in the following year's ASEC, because it reports income and tax information for the preceding calendar year. For example, Colorado's tax-year 2022 CTC first enters treatment in the 2023 ASEC. Current-week employment and labor force participation are therefore measured during the 2023 survey period, when families would generally be filing for the 2022 credit, whereas prior-calendar-year hours align directly with labor supply during 2022. For Basic Monthly analyses, treatment

is assigned at the calendar-year level using the corresponding start year for the state policy. Because treatment is defined by policy exposure rather than individual receipt, the binary coefficients are intent-to-treat estimates of policy effects for the age-defined parent samples, not treatment-on-the-treated effects among actual recipients. Some parents in the treated samples may be ineligible based on income or other policy rules.

We also construct continuous measures of CTC generosity. We calculate a simulated benefit for each respondent by applying the statutory state formula to CPS measures of income, filing status, and the number and ages of qualifying children. The resulting measure captures potential statutory benefits rather than actual receipt, which is not observed in the CPS and may itself depend on employment decisions. The primary stacked continuous-treatment model uses the benefit implied by each state's first-year CTC rules, expressed per $100, and holds that policy rule fixed across event time. The secondary TWFE continuous-treatment model instead uses the current simulated benefit under the rules in effect in each state-year, where the treatment equals zero in untreated state-years. The stacked estimate therefore reflects generosity under the original policy, whereas the TWFE measure also captures subsequent changes in state credit rules.

The continuous measure should not be interpreted as an exogenous cash transfer. Simulated benefits depend partly on observed income and family structure, actual take-up is unobserved, and earnings may themselves affect benefits through phase-ins and phase-outs. Similar approaches have been used to approximate tax credit eligibility and benefit amounts when administrative receipt data are unavailable (Collin et al. 2021). We therefore interpret these coefficients as complementary dose-response evidence within the statutory policy design rather than as structural labor supply effects. Prior tax credit studies have used a similar approach to reduce endogeneity concerns by constructing simulated benefits from policy parameters and fixed or lagged earnings distributions (Bastian and Michelmore 2018; Hamad and Rehkopf 2016; Hoynes and Patel 2018).

For Basic Monthly records, detailed tax-unit income is unavailable. We therefore assign

simulated benefits from ASEC donor cells defined by calendar year, state, family income category, number and ages of children, marital status, and number of adults, using progressively less detailed cells when an exact match is unavailable. The main monthly continuous analysis assigns the mean simulated ASEC benefit within the matched cell. A sensitivity analysis assigns the maximum benefit.

2.4 Outcomes and covariates

Our primary outcome is employment, defined as being employed during the CPS reference week, generally the calendar week containing the 12$^{\text{th}}$ day of the interview month (February to April), and expressed as a percentage-point outcome in the regressions. The secondary extensive-margin outcome is labor force participation. We examine two measures of the intensive margin: usual weekly hours at current jobs and usual weekly hours in the prior calendar year. The primary hours models are restricted to respondents with positive reported hours; a sensitivity analysis retains non-workers and sets their hours to zero so that the outcome reflects total labor supplied rather than hours conditional on working.

Pre-specified individual covariates are age, sex, race/ethnicity, educational attainment, marital status, number of working-age adults in the tax unit, and number of children. State-level time-varying covariates are the state minimum wage, state Earned Income Tax Credit rate, and paid family leave policy status (University of Kentucky Center for Poverty Research 2026; Wielk 2026). For ASEC observations in survey year t, state-level covariates refer to calendar year $t-1$, consistent with the tax year defining CTC exposure; hence these covariates precede the survey-week employment outcomes. The CPS Basic Monthly models use state covariates indexed to the observation's calendar year. A sensitivity specification also adjusts for the state unemployment rate and per-capita income (Burkhauser, McNichols and Sabia 2025). We include unemployment and per-capita income only in sensitivity analyses because contemporaneous economic conditions may both influence policy adoption and be affected by labor market changes.

Our pre-analysis plan is available at https://doi.org/10.17605/OSF.IO/MCBTA.

2.5 Primary stacked difference-in-differences design

We use a stacked difference-in-differences design to address concerns about conventional TWFE estimation when states adopt policies at different times and treatment effects vary across states or over time (Cengiz et al. 2019; de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Wing, Freedman and Hollingsworth 2024). We create cohort-specific sub-experiments defined by the ASEC treatment initiation year and the maximum eligible child age under the policy at adoption. States sharing the same initiation year and child age rule can enter the same sub-experiment. For example, California's Young Child Tax Credit cohort is defined by having a child age 0–5 at adoption. The treated observations are working-age adults in the focal adopting state or states who satisfy the policy-specific sample definition for child age. The resulting estimand therefore averages cohort-specific effects among parents satisfying each policy's sample definition for child age at adoption rather than an effect among all parents in adopting states.

The main comparator set consists of states that never adopt a CTC during the analysis period and states whose adoption occurs after the end of the focal sub-experiment's post-treatment window. Thus, a state already exposed—or exposed soon enough that its outcomes would be contaminated during the focal post period—does not serve as a control. We also estimate versions restricted to never-treated controls and, separately, to not-yet-treated controls. Another sensitivity analysis excludes states that border any treated state in the focal sub-experiment to reduce potential contamination from cross-border labor markets or policy spillovers.

The primary stacked sample uses a balanced event window from 10 years before through 2 years after ASEC initiation, with event year -1 serving as the reference. Requiring complete support at each event time improves comparability but excludes early adopters without 10 pre-policy years and recent adopters without two post-policy years. An unbalanced-support

sensitivity analysis incorporates additional cohorts, and a longer balanced specification extends follow-up through +5 where feasible (Appendix Table A4). With ASEC data through 2026, 12 of 16 adopting states—Idaho, Maine, Arizona, California, Massachusetts, Colorado, New Jersey, Vermont, New Mexico, Minnesota, Maryland, and Oregon—contribute treatment variation to the primary balanced stack.

Following Wing, Freedman and Hollingsworth (2024), the regressions include fixed effects for treatment status and relative event time and corrective weights that equalize control contributions across sub-experiments relative to treated observations at each event time. We do not additionally apply CPS population weights because our primary estimand is the trimmed aggregate ATT rather than a population-weighted ATT (Solon, Haider and Wooldridge 2015; Wing, Freedman and Hollingsworth 2024). In a sensitivity analysis, combining survey weights with the stacked-design corrective weights yields similar estimates for employment and labor force participation, although the hours estimates are more sensitive to weighting. Standard errors are clustered by state. Because 12 focal adopting states contribute treatment variation, we supplement conventional inference with state-level wild cluster bootstrap p-values (MacKinnon and Webb 2018). The principal estimate averages effects over event years 0 through +2 relative to −1.

We use event-study models to examine how outcomes evolved before and after adoption. Joint Wald tests assess whether the pre-treatment coefficients differ from zero over the full window (years −10 through −2). We also report tests for the more recent pre-treatment period (years −5 through −2), but do not use these supplementary tests to select the preferred specification or redefine the identifying assumption. Because such tests may have limited power and conditioning inference on a pre-trend test can itself introduce distortions, we interpret them alongside the magnitude and pattern of the coefficients and their confidence intervals rather than applying a mechanical significance threshold (Roth 2022; Roth et al. 2023). For the primary employment and labor force participation outcomes, we use the sensitivity analysis of Rambachan and Roth (2023) to assess how robust the stacked

and TWFE event-study estimates are to departures from parallel trends. We focus on the average effect over event years 0 through 2 relative to event year −1. This method bounds the size of possible post-treatment departures from parallel trends relative to those observed before treatment. For example, at $\overline{M} = 1$, permitted post-treatment departures can be as large as the largest observed pre-treatment departure.

2.6 Secondary TWFE, monthly, and falsification analyses

Secondary CPS-ASEC models use conventional TWFE regressions with state and ASEC year fixed effects, the same individual and state covariates, and standard errors clustered by state. These models use all available pre- and post-policy years and are not population weighted. Although TWFE is not the preferred estimator under staggered adoption, it provides a benchmark and uses a different weighting of the available policy variation.

The Basic Monthly models include state and year-by-month fixed effects. Binary-treatment analyses are estimated in both the full monthly sample and the subset of monthly observations matched to detailed ASEC records. Continuous monthly models use the donor-cell mean or maximum simulated benefit described above.

We use childless adults in two additional analyses. In the first, we assign state CTC adoption timing to childless adults as a placebo test; these adults are not directly eligible for the credit. In the second, a difference-in-difference-in-differences (DDD) model compares parents with childless adults within states over time. The DDD specification absorbs state-by-year, state-by-parent-group, and year-by-parent-group fixed effects, so the identifying term captures the differential post-policy change among adults with children relative to childless adults within the same state-year. These analyses are designed to probe state-specific shocks that affect parents and non-parents similarly.

Additional sensitivity analyses change fixed effects, comparator sets, population weighting, covariate adjustment, event-time support, event windows, child age restrictions, and the set of treated states (Appendix Tables A5 and A6). We also estimate specifications restricted

to treated states whose CTC was refundable in its first effective tax year. Nonrefundable adopting states are excluded from this specification rather than reclassified as controls because they were exposed to a CTC and therefore do not provide untreated comparisons.

Pre-specified subgroup analyses examine heterogeneity by family income, family structure, number of earners, education, race/ethnicity, and age of the youngest child. These domains capture differences in economic resources, caregiving constraints, and potential responsiveness to work incentives. We treat subgroup results as exploratory and rely on joint tests across subgroup categories rather than isolated significance within individual categories, given smaller sample sizes.

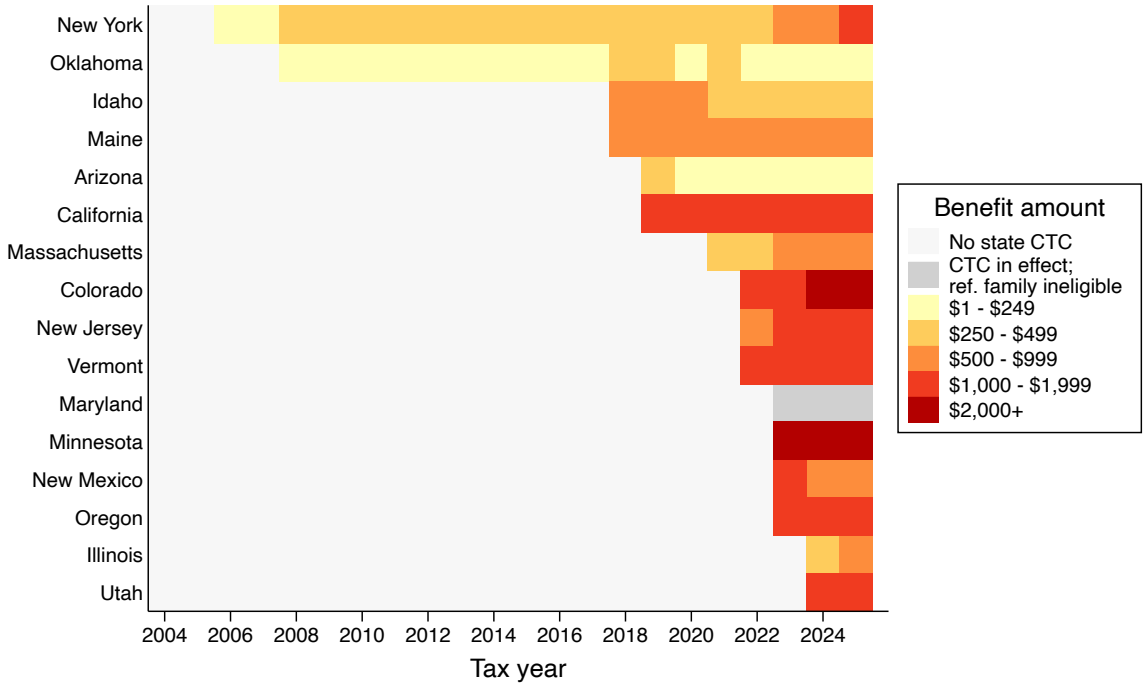
3 Results

3.1 Policy variation and analytic samples

Figure 1 illustrates the variation in state CTC adoption timing and simulated credit generosity through tax year 2025; Appendix Table A1 summarizes the policy rules in effect in 2025. For a reference family consisting of a single parent with children ages 3 and 8 and annual earnings at the 2025 poverty threshold ($25,938), the simulated 2025 credit ranges from zero in Maryland, where this family exceeds the eligibility threshold, to $5,747 in Colorado. The empirical analysis includes tax years through 2025. The substantial differences in statutory schedules motivate our examination of adoption and simulated generosity as separate treatment measures.

Appendix Table A2 describes the 1,005,413 person-year observations in the primary ASEC TWFE employment sample; 377,151 are from states that adopt a qualifying CTC during the study period. The Basic Monthly sample contains 7,661,939 person-month observations (Appendix Table A3). Twelve adopting states have complete support for the primary stacked -10 to $+2$ event window (Appendix Table A4).

Figure 1: Adoption and simulated generosity of state Child Tax Credits through tax year 2025



Note: States are ordered by the first tax year in which a state child tax credit was available. Benefit amounts are simulated for a single working parent with two children, ages 3 and 8, and annual earnings of $25,938 (i.e., at the federal poverty level for a three-person family with two children). Benefits are expressed in 2025 dollars. Gray cells identify years in which a state CTC was in effect but the reference family was ineligible; in Maryland, the reference family's income exceeded the state's eligibility limit.

3.2 Effects of state CTC adoption

Table 1 reports average effects of CTC adoption over event years 0–2 from the primary stacked design and corresponding static TWFE estimates. The stacked employment estimate is 0.05 percentage points (SE 1.03; 95% CI, −2.02 to 2.12), against a treated pre-policy mean of 74.00%. The labor force participation estimate is −0.46 points (SE 0.82; 95% CI, −2.11 to 1.19), against a mean of 78.20%. Estimates for usual weekly hours at current jobs and in the prior calendar year are 0.04 hours (SE 0.28) and −0.02 hours (SE 0.24), respectively. None of the estimates is statistically distinguishable from zero, and each is small ($\leq 0.6\%$) relative to its pre-policy outcome mean. However, the confidence intervals for employment and labor force participation encompass changes of roughly one to two percentage points.

Table 1: Estimated effects of state Child Tax Credits on parental labor-market outcomes

	Stacked DiD (primary)		TWFE DiD (secondary)	
	Adoption	Per $100	Adoption	Per $100
	(1)	(2)	(3)	(4)
Panel A: Employment (percentage points)				
Estimate	0.05	0.15	−0.12	0.22
	(1.03)	(0.17)	(0.23)	(0.20)
Treated pre-policy mean	74.00	74.00	75.59	75.59
Estimate as % of pre-policy mean	0.1%	0.2%	−0.2%	0.3%
Observations	2,366,798	2,366,798	1,005,413	1,005,413
Panel B: Labor force participation (percentage points)				
Estimate	−0.46	0.17	−0.18	0.20
	(0.82)	(0.13)	(0.17)	(0.18)
Treated pre-policy mean	78.20	78.20	79.65	79.65
Estimate as % of pre-policy mean	−0.6%	0.2%	−0.2%	0.3%
Observations	2,366,798	2,366,798	1,005,413	1,005,413
Panel C: Usual weekly hours at current jobs (hours per week)				
Estimate	0.04	−0.07**	0.12*	−0.05**
	(0.28)	(0.03)	(0.07)	(0.02)
Treated pre-policy mean	39.78	39.78	39.94	39.94
Estimate as % of pre-policy mean	0.1%	−0.2%	0.3%	−0.1%
Observations	1,685,365	1,685,365	717,285	717,285
Panel D: Usual weekly hours in prior calendar year (hours per week)				
Estimate	−0.02	−0.07***	0.06	−0.09***
	(0.24)	(0.02)	(0.08)	(0.02)
Treated pre-policy mean	39.32	39.32	39.39	39.39
Estimate as % of pre-policy mean	−0.1%	−0.2%	0.2%	−0.2%
Observations	1,910,956	1,910,956	816,639	816,639
Individual and state covariates	Yes	Yes	Yes	Yes
Wing corrective weights	Yes	Yes	No	No
Fixed effects	Treatment status; event time	Treatment status; event time	State; ASEC year	State; ASEC year

Note: Columns 1 and 2 report stacked difference-in-differences estimates averaged over event years 0–2, relative to the pre-adoption event year −1. Columns 3 and 4 report static two-way fixed effects coefficients, using all available pre- and post-adoption observations. Adoption is a binary indicator; column 2 uses simulated benefit generosity under each state's first-year rules held fixed across event time, while column 4 uses the simulated benefit under the rules in force in each state-year. Continuous-treatment coefficients are per $100 of simulated benefit; employment and participation are measured in percentage points and hours among workers in hours per week. Individual covariates and state covariates measured in the calendar year preceding each ASEC survey are included in all models. Stacked models use Wing corrective weights, treatment-status and relative-event-time fixed effects; TWFE models use state and ASEC-year fixed effects. None uses CPS population weights. State-clustered standard errors in parentheses. Percentage-of-mean calculations use unrounded coefficients. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Static TWFE adoption estimates are −0.12 points (SE 0.23) for employment, −0.18 points (0.17) for labor force participation, 0.12 weekly hours at current job (0.07), and 0.06 hours

(0.08) for usual weekly hours in the prior year. Wild cluster bootstrap inference for the primary stacked models closely tracks conventional state-clustered inference, with bootstrap p-values of 0.968, 0.620, 0.883, and 0.927, respectively (Appendix Table A10). Neither estimator yields a consistent negative average effect on extensive-margin labor supply.

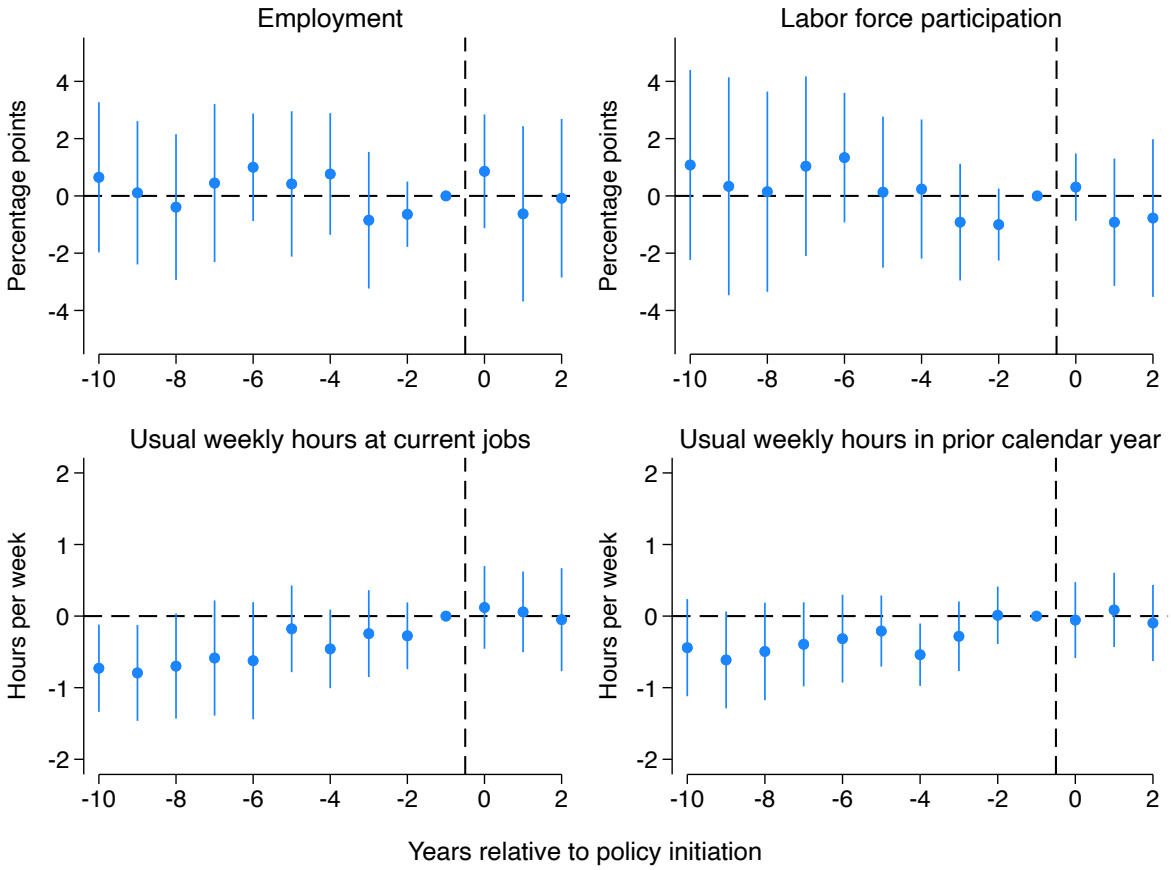
The stacked event studies in Figure 2 provide a more qualified picture of identification. Joint tests reject zero pre-treatment coefficients over years -10 to -2 for all four outcomes (employment, $p = 0.0014$; labor force participation, $p < 0.001$; hours at current job, $p = 0.0014$; hours in prior year, $p = 0.0035$). However, the deviations for the extensive-margin outcomes are concentrated primarily in more distant pre-treatment years. Joint tests restricted to years -5 to -2 do not reject for employment ($p = 0.253$) or participation ($p = 0.421$). The corresponding p-values over the shorter window are 0.076 and 0.102 for hours at current job and in the prior year. The corresponding TWFE tests likewise show nonzero coefficients in earlier pre-policy years, but do not reject for any of the four outcomes over years -5 through -2 (Appendix Figure A5).

Allowing for possible violations of parallel trends widens the range of compatible employment responses (Appendix Table A8). The conventional event-study interval for stacked employment is approximately $[-1.97, 2.07]$ percentage points. Under the relative-magnitude restriction of Rambachan and Roth (2023), it widens to $[-3.97, 3.58]$ when $\overline{M} = 0.5$ and to $[-6.60, 5.99]$ when $\overline{M} = 1$. The corresponding stacked participation interval at $\overline{M} = 1$ is $[-5.77, 4.96]$. These bounds make clear that substantively important changes cannot be ruled out under departures from strict parallel trends.

3.3 Continuous treatment estimates

Table 1 also reports associations between labor supply outcomes and simulated CTC generosity, using variation in statutory CTC generosity. In the primary stacked specification, an additional \$100 of simulated benefit under a state's first-year rules is associated with a 0.15 percentage-point change in employment (SE 0.17) and a 0.17-point change in labor force

Figure 2: Dynamic effects of state Child Tax Credits on parental employment and work hours



Note: Data are from the Current Population Survey Annual Social and Economic Supplement (CPS-ASEC), 2004–2026. Points show estimates from stacked difference-in-differences event-study models. Event time is measured relative to the first ASEC survey year in which outcomes from the state credit's initial tax year are observed; the vertical line at t = −1 is the omitted reference period. Employment and labor force participation are measured in percentage points, and work hours are measured in hours per week. Bars indicate 95% confidence intervals. Models adjust for the full set of individual- and state-level covariates, include treatment-status and event-year fixed effects as recommended by Wing et al. (2024), apply Wing corrective weights, and use standard errors clustered at the state level. Outcome-specific sample sizes are reported in Table 1.

participation (0.13). Both confidence intervals include zero. The corresponding coefficients for hours conditional on working are −0.070 hours (SE 0.033; $p = 0.040$) at current jobs and −0.068 hours (SE 0.024; $p = 0.006$) in the prior calendar year. Static TWFE models using the current simulated benefit produce estimates of 0.22 percentage points (SE 0.20) for employment and 0.20 points (0.18) for labor force participation, together with −0.046

hours (0.023) and −0.087 hours (0.023) on the two work hours outcomes.

The continuous-treatment event studies complicate a causal interpretation of these estimates. In both the stacked and TWFE specifications (Appendix Figures A6 and A7), hours coefficients are negative through much of the pre-adoption period but move toward zero as adoption approaches, before becoming more negative again after implementation. The post-adoption decline could therefore represent a departure from the immediately preceding trend. At the same time, the substantial pre-treatment associations indicate that benefit generosity is correlated with hours even before the policy takes effect. Moreover, simulated entitlements depend partly on observed earnings and statutory phase-ins and phase-outs, so higher simulated benefits can be mechanically associated with lower earnings or hours even without a behavioral response. We therefore interpret the negative estimates for conditional hours cautiously rather than as direct estimates of an income effect.

Appendix Table A7 rescales the continuous-treatment coefficients to express the estimated labor supply response relative to family income, providing a rough benchmark against conventional labor supply elasticities. We calculate these normalizations using mean family income among treated parents and, alternatively, among treated parents who would receive a positive first-year benefit. The income-normalized estimates are large and imprecise for the extensive-margin outcomes. For work hours, the income-normalized estimates are negative and statistically distinguishable from zero, reflecting the significance of the underlying continuous-treatment coefficients. Because the normalizations simply rescale those coefficients, they do not provide independent evidence of a labor-supply response. A $100 benefit increment is small relative to annual family income, which can generate relatively large normalized estimates. More importantly, state credit schedules combine potential income effects with earnings phase-ins and phase-outs, and the simulated benefit measure itself depends partly on observed earnings. We therefore interpret these quantities as descriptive income normalizations rather than structural labor supply elasticities (Chetty et al. 2013).

3.4 Robustness and falsification analyses

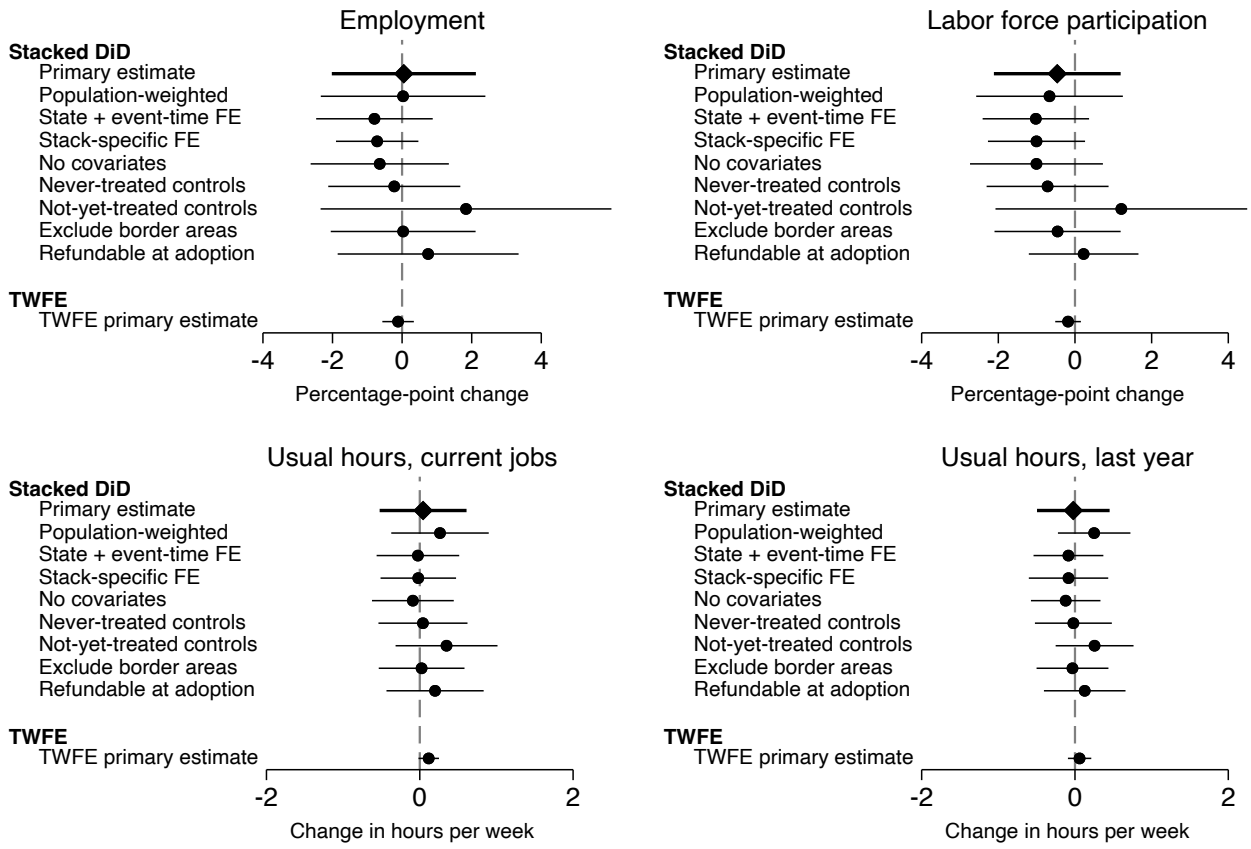
Figure 3 and Appendix Table A5 summarize alternative stacked specifications. Across alternative specifications that change the fixed effects, comparison groups, border exclusions, population weighting, covariate set, event-time support, event windows, and sample restrictions, the binary stacked estimates remain small and the confidence intervals include zero. Employment estimates range from −0.79 to 1.84 percentage points. The not-yet-treated-only comparison is especially imprecise for the extensive-margin outcomes. Restricting the treated group to credits that were refundable at adoption does not materially change the employment finding: the stacked estimate is positive (0.75 percentage points; SE 1.29), while the corresponding TWFE estimate is small and negative (−0.23 points; SE 0.27); neither is statistically distinguishable from zero. Extending the balanced window to five post-policy years changes the contributing cohorts; the resulting event study is shown in Appendix Figure A8.

In secondary analyses (Appendix Table A6), the full Basic Monthly CPS adoption estimate for employment is 0.09 percentage points (SE 0.36). The childless adult placebo estimate is 0.25 points (SE 0.34), and the DDD estimate including parents versus childless adults is −0.27 points (SE 0.29). None differs significantly from zero. The smaller ASEC-matched monthly sample gives somewhat larger estimates, but the overall extensive-margin results are not suggestive of a systematic employment decline. The corresponding event studies appear in Appendix Figures A9–A11. These checks address some possible common state-level shocks.

3.5 Heterogeneity

Figure 4 presents the pre-specified subgroup estimates for employment, and Appendix Table A9 reports joint tests across the seven domains. In the stacked specification, equality is rejected across race/ethnicity groups ($p = 0.0022$); the result survives Holm correction for multiple testing (adjusted $p = 0.015$). The point estimates are positive for non-Hispanic

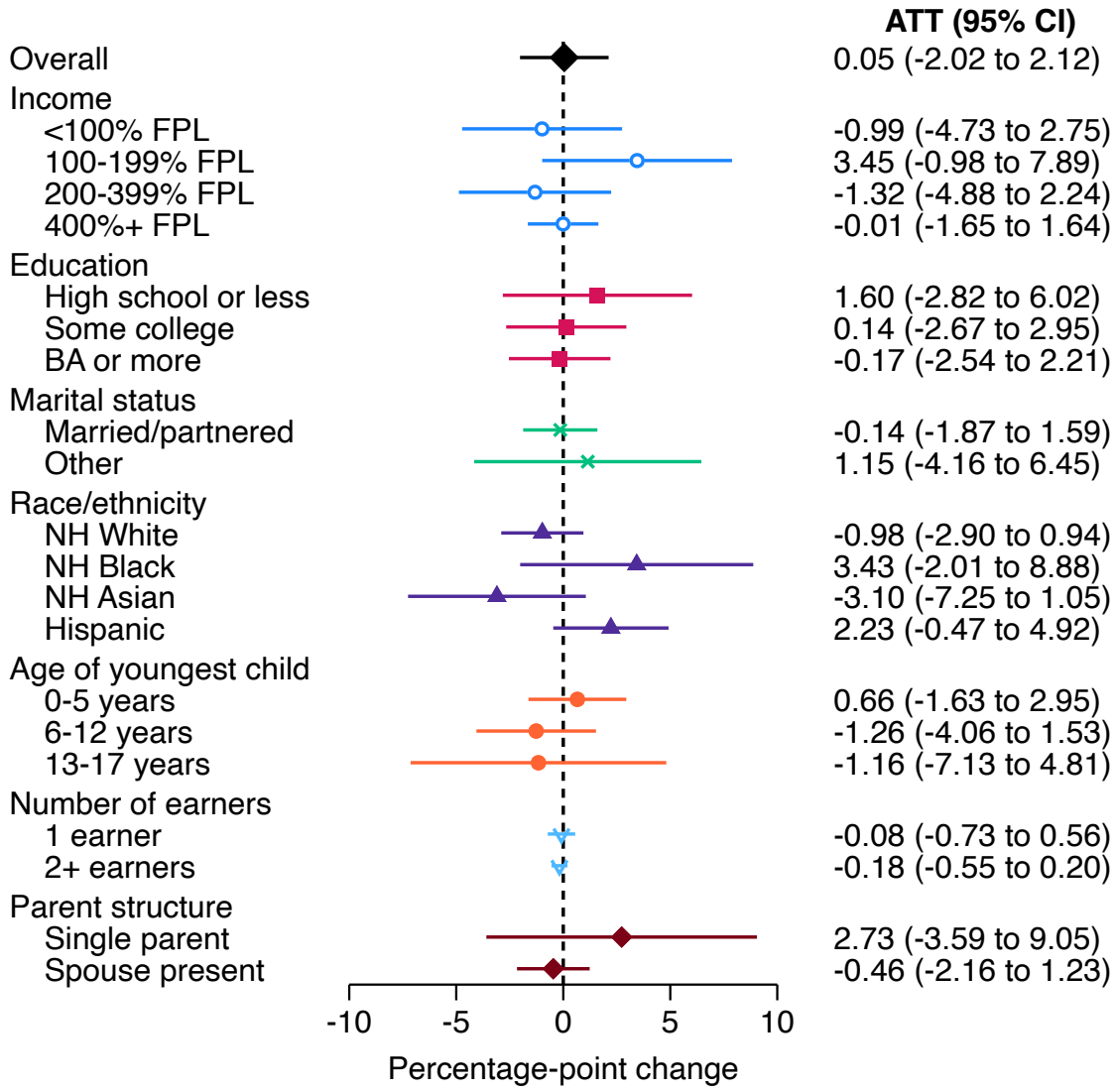
Figure 3: Robustness of estimated effects of state Child Tax Credits on parental employment and work hours



Note: Data are from the Current Population Survey Annual Social and Economic Supplement (CPS-ASEC), 2004–2026. Points show estimated average effects over event years 0 through 2, and horizontal bars indicate 95% confidence intervals. Panels report employment, labor force participation, usual weekly hours at current jobs, and usual weekly hours in the prior calendar year. Employment and labor force participation estimates are expressed in percentage points; work-hours estimates are expressed in hours per week. The primary specification is a stacked difference-in-differences model that adjusts for the full set of individual- and state-level covariates, includes treatment-status and event-year fixed effects, applies the corrective weights recommended by Wing et al. (2024), and clusters standard errors by state. Alternative specifications vary the indicated feature of the primary model while otherwise retaining the primary specification. The full results for the sensitivity estimates are provided in Appendix Tables A5 and A6.

Black parents (3.43 percentage points) and Hispanic parents (2.23), negative for non-Hispanic Asian parents (−3.10), and smaller in magnitude for non-Hispanic White parents (−0.98), although the subgroup estimates are imprecise and their confidence intervals include zero. The joint test for family income also rejects before adjustment for multiple testing ($p = 0.021$), but not afterward (adjusted $p = 0.123$). No other domains survives the adjustment.

Figure 4: Heterogeneity in the effects of state Child Tax Credits on parental employment



Note: Points show estimated average effects of state child tax credit adoption on parental employment from stacked difference-in-differences models over event years 0 through 2; horizontal bars indicate 95% confidence intervals. Estimates are expressed in percentage points. The full-sample estimate is shown first, followed by estimates for prespecified subgroups. Models use the population-unweighted stacked difference-in-differences specification, adjust for the full set of individual- and state-level covariates, include treatment-status and event-year fixed effects, apply the corrective weights recommended by Wing et al., and cluster standard errors by state. Joint tests of equality across subgroup categories are reported in Appendix Table A9.

4 Conclusions

State CTCs have become an increasingly important source of financial support for families with children in the absence of a permanent federal child allowance (National Conference

of State Legislatures 2026; Vinh et al. 2025). Using two decades of CPS data, we examine how parental labor supply has changed following the adoption of state CTCs with varying generosity and work incentives. We find little indication that the credits implemented to date produced meaningful reductions in parental employment or labor force participation. The preferred stacked estimates for employment and labor force participation are close to zero, and the binary adoption models show no consistent reduction in work hours. Secondary TWFE, monthly CPS, placebo, triple-difference, and refundable-only analyses yield broadly similar results for employment. The point estimates are consistent with recent evidence from the 2021 federal expansion and California's Young Child Tax Credit, which suggest smaller extensive-margin employment responses than some ex ante simulations predicted (Ananat et al. 2024; Goldin et al. 2024; Pac and Berger 2024). The average adoption effect nevertheless combines credits with different earnings requirements, phase-outs, refundability rules, and benefit levels. It should not be interpreted as the response to any one CTC design.

The findings for hours worked and demographic subgroups are less straightforward. In the continuous-treatment models, larger simulated benefits are associated with modestly lower hours among employed parents, even though the binary adoption estimates remain close to zero. These associations warrant caution: the event-study coefficients are also negative during much of the pre-adoption period, and simulated benefit generosity depends partly on contemporaneous earnings and statutory phase-ins and phase-outs. The subgroup analyses provide some evidence of heterogeneity by race and ethnicity. In the stacked design, the joint test remains significant after adjustment for multiple testing, with positive point estimates for non-Hispanic Black and Hispanic parents, a negative estimate for non-Hispanic Asian parents, and a smaller estimate for non-Hispanic White parents. Individual subgroup estimates are imprecise, however, and the corresponding TWFE joint test does not reject equality across groups. We find no consistent heterogeneity by family income, parent structure, number of earners, or the other pre-specified domains. These patterns raise potentially important distributional questions, but they do not establish that particular demographic

groups respond differently to state CTC adoption.

The study has several limitations. First, our binary policy estimates are intent-to-treat effects for parent samples defined by state, timing, and child age, not treatment effects among actual CTC recipients. Actual receipt is unavailable in the CPS, and some parents in the treated samples may not qualify under the applicable income or other eligibility rules. The intent-to-treat design can attenuate policy-level estimates relative to effects among recipients, although conditioning on observed receipt could itself introduce selection bias. Second, for several nonrefundable credits, simulated statutory benefits may also exceed the amount families can use after accounting for state income tax liability, which may overstate usable benefits in the continuous-treatment models. Third, the balanced design draws treated variation from 12 of the 16 adopting states, excluding early and recent adopters that lack complete event-time support, and provides relatively limited post-adoption follow-up. Fourth, state policy adoption may also coincide with other economic or social policy changes, which is only partially addressed by our inclusion of time-varying state-level covariates and the robustness checks including childless adults. Fifth, the full-window event studies reject zero pre-treatment coefficients for all four primary outcomes. Although the deviations for employment and labor force participation are concentrated in more distant leads rather than a clear monotonic pretrend, the shorter-window diagnostics do not eliminate concerns about parallel trends. Sensitivity analyses allowing moderate departures from this assumption leave room for substantively meaningful effects in either direction.

The results, therefore, offer a qualified perspective on the labor-supply implications of state CTCs. The point estimates do not suggest large average employment declines following the policies observed here, but neither can meaningful employment gains or losses be ruled out under less favorable assumptions about counterfactual trends. The distinction is relevant to ongoing debates over how CTC generosity and work requirements should be balanced in designing tax credits for families with children (Holzer 2026; Michelmore 2026; National Academies of Sciences, Engineering, and Medicine 2026). Labor supply is also only one

component of the broader welfare consequences of child benefits: our analysis does not measure effects on poverty, material hardship, child development, or public budgets, which are important considerations in evaluating these policies (Garfinkel et al. 2022). Future research linking administrative tax records to labor-market outcomes could help distinguish the effects of refundability, benefit size, and earnings conditions while measuring actual receipt and usable tax benefits. Longer follow-up for recent state adopters would also provide evidence on whether employment responses change as these policies become more established.

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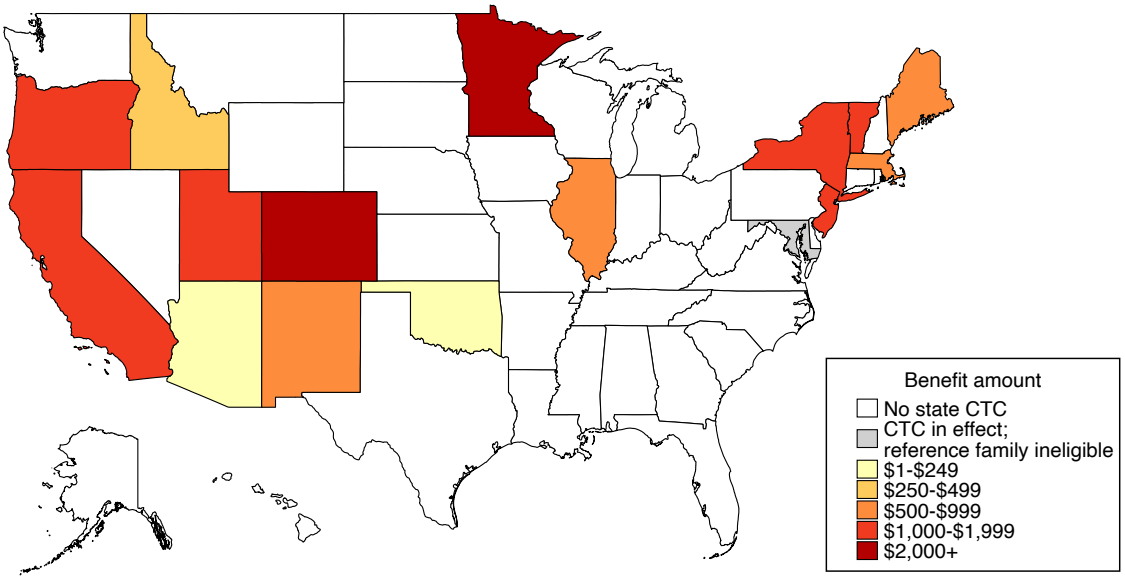
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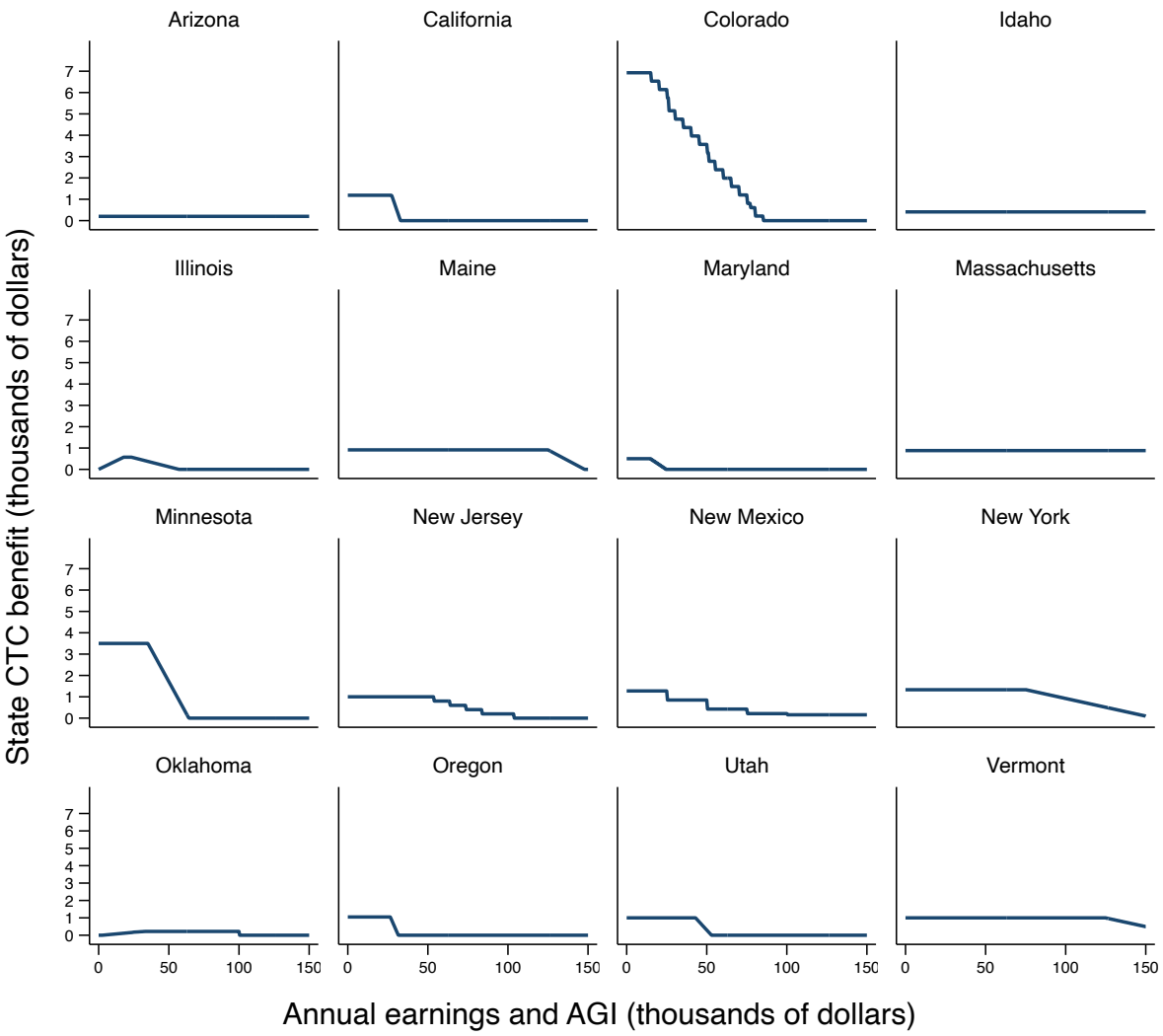
Supplemental Appendix

Figure A1: State Child Tax Credit benefits for a standardized family, tax year 2025



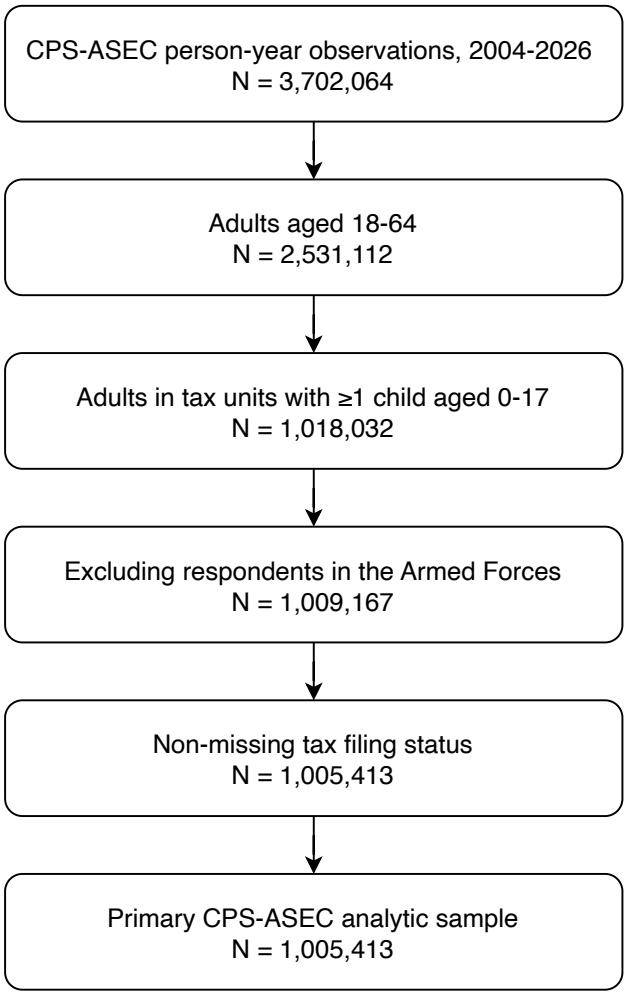
Note: Benefit amounts are simulated for a single working parent with two children, ages 3 and 8, and annual earnings and adjusted gross income of $25,938, the 2025 Census poverty threshold for a three-person family with two related children. Benefits are expressed in nominal 2025 dollars. Gray identifies states in which a state CTC was in effect but the reference family was ineligible; in Maryland, the reference family's income exceeds the state's eligibility limit.

Figure A2: Simulated state Child Tax Credit benefit schedules by income, 2025



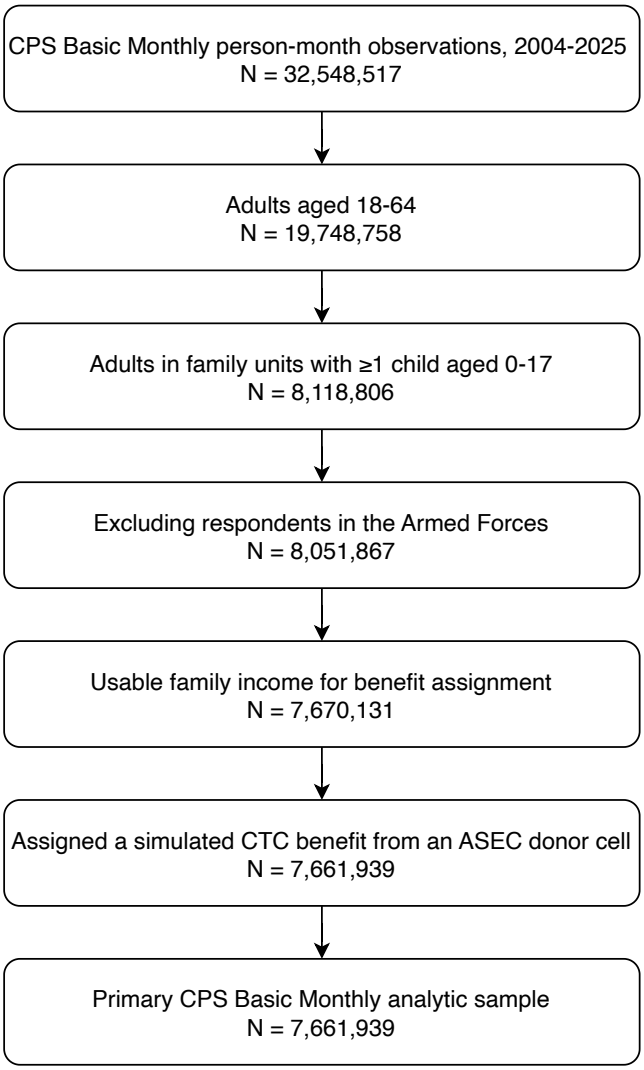
Note: Each panel presents the simulated 2025 state child tax credit benefit schedule for a standardized family consisting of a single head-of-household parent with two children, ages 3 and 8. The schedules show simulated benefits as annual earnings and adjusted gross income vary together across the income levels shown on the horizontal axis, with other family characteristics held fixed. Both income and benefits are shown in thousands of dollars. The schedules incorporate the state-specific maximum credit, income eligibility thresholds, phase-in provisions, phaseout provisions, and other formula parameters used in the benefit simulation. Upward-sloping portions indicate benefit phase-ins, flat portions indicate ranges over which the simulated credit does not vary with income, and downward-sloping portions indicate benefit phaseouts. The figure displays the simulated policy schedule for this standardized family and not the observed distribution of tax units across income levels.

Figure A3: Flowchart of sample generation process (CPS-ASEC data)



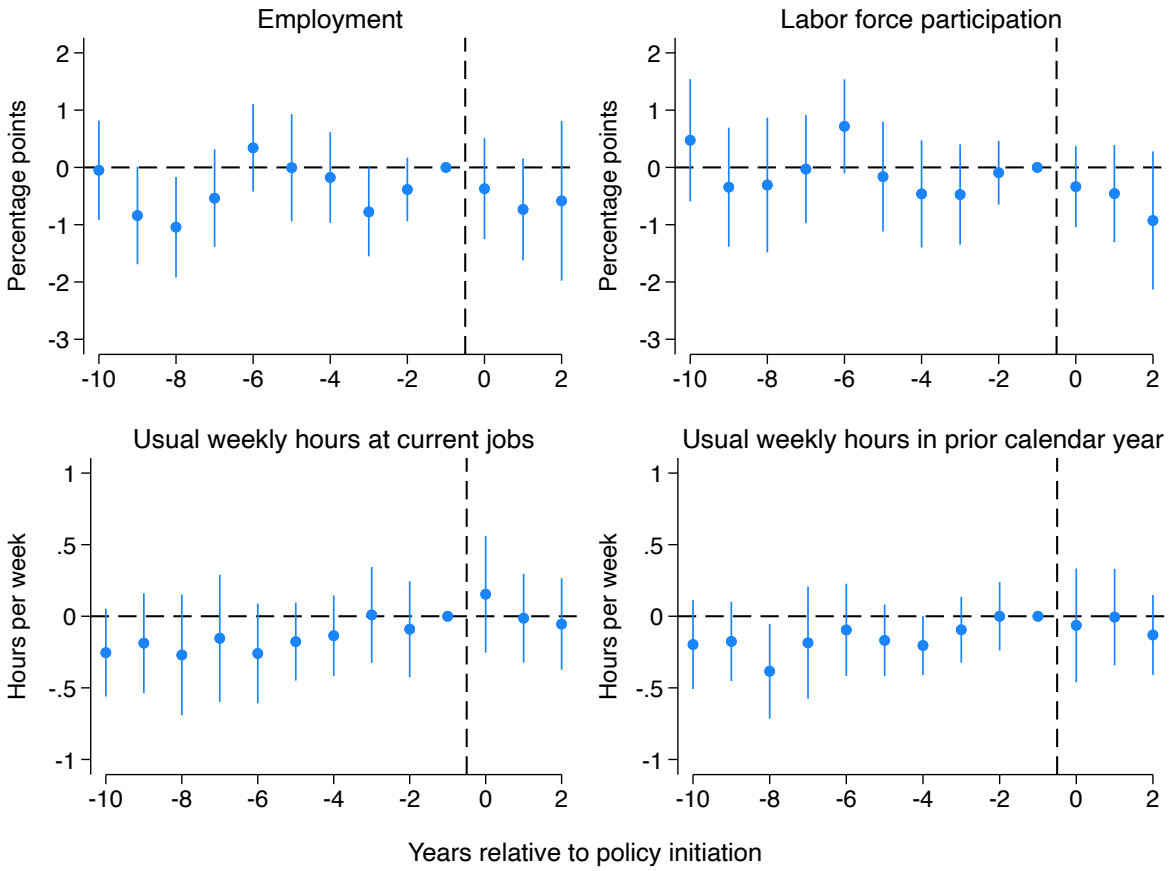
Note: Counts represent unweighted person-year observations remaining after each sequential sample restriction. The analytic sample includes adults aged 18–64 years in tax units with at least one child aged 0–17 years. Respondents in the Armed Forces and those with missing tax filing status are excluded. Because the CPS-ASEC is a repeated cross-section, some respondents may contribute observations in more than one year.

Figure A4: Flowchart of sample generation process (CPS Basic Monthly data)



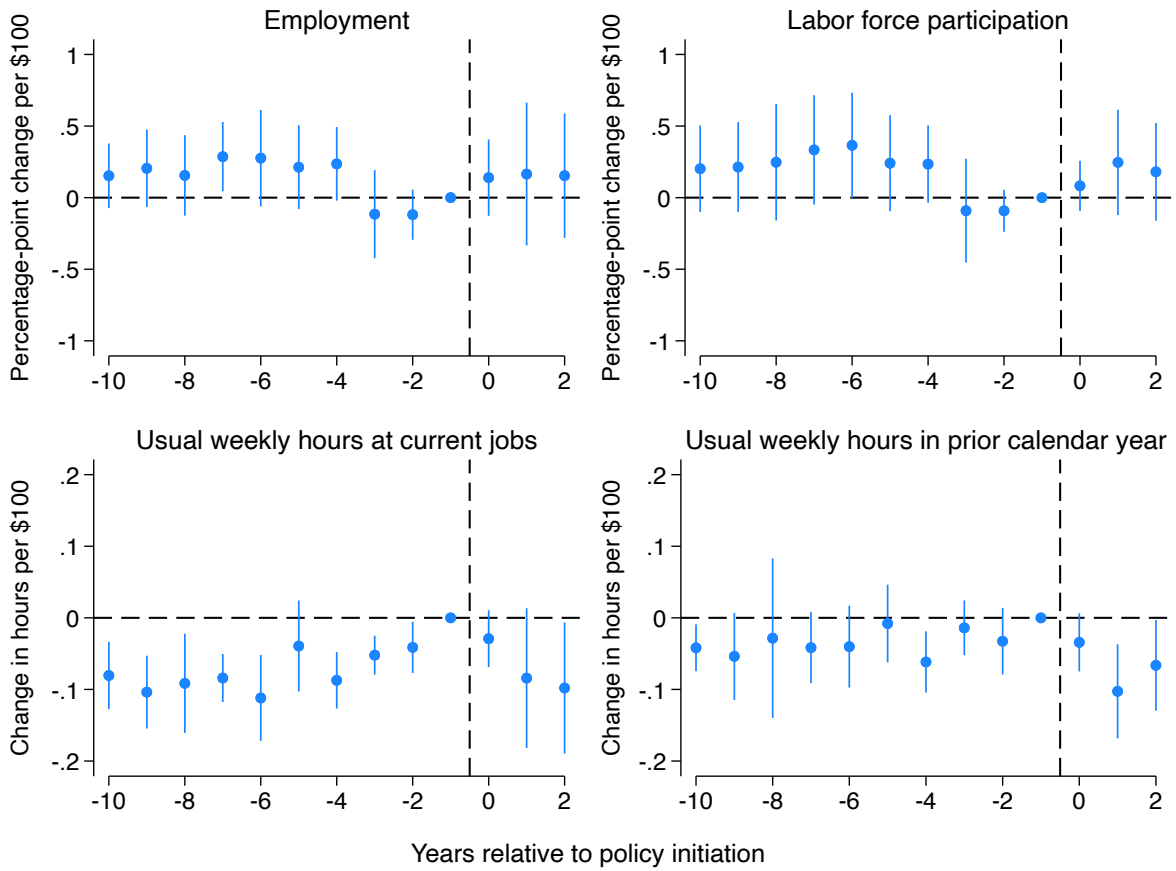
Note: Counts represent unweighted person-month observations remaining after each sequential sample restriction. Family characteristics are constructed using all members of the family unit before restricting analytic respondents to adults aged 18–64 years. The analytic sample includes adults in family units with at least one child aged 0–17 years and excludes respondents in the Armed Forces. Simulated state child tax credit benefits are assigned using CPS-ASEC donor cells defined by calendar year, state, family income category, number and age of children, marital status, and number of adults, with progressively less detailed cells used when an exact match is unavailable. Respondents may contribute observations in multiple months.

Figure A5: Dynamic effects of state Child Tax Credits on parental employment and work hours (TWFE event study, binary treatment)



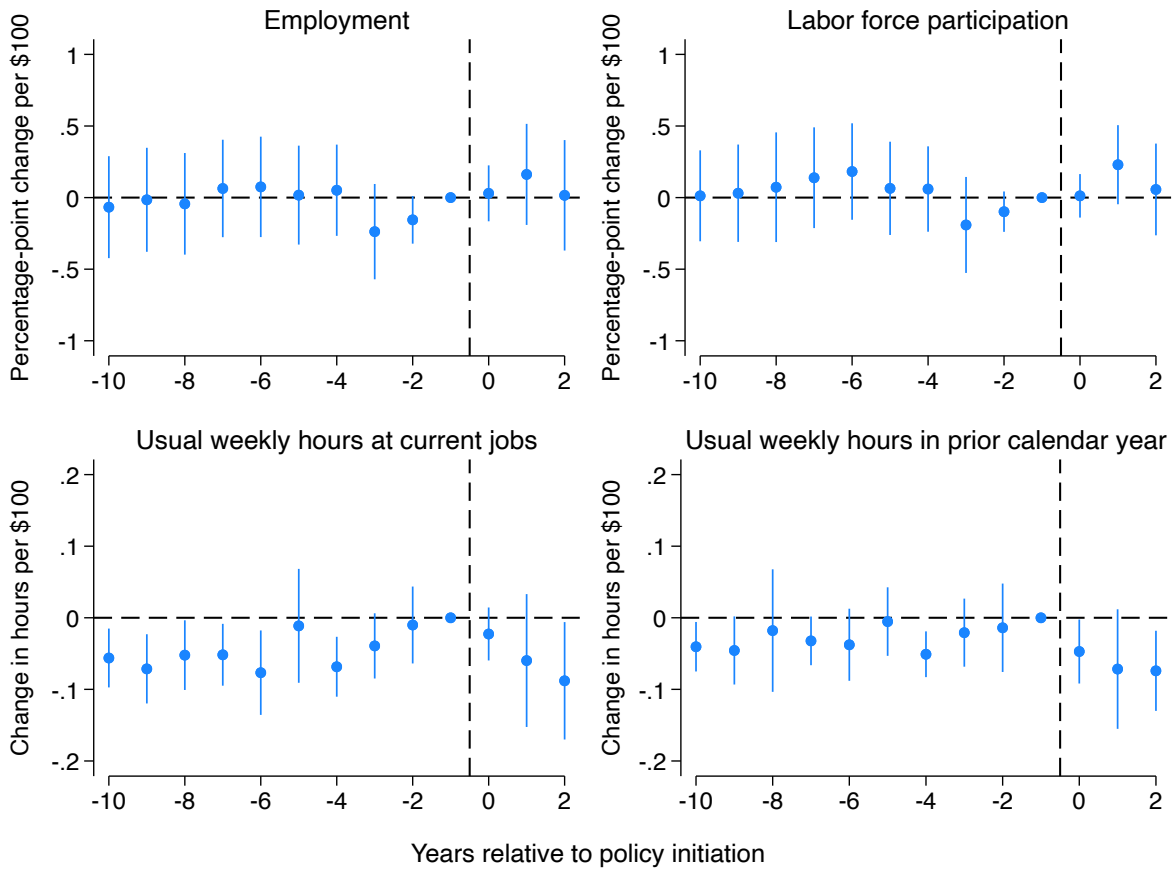
Note: Data are from the Current Population Survey Annual Social and Economic Supplement (CPS-ASEC), 2004–2026. Points show coefficients from binary-treatment two-way fixed effects event-study models. Event time is measured relative to the first ASEC survey year in which outcomes from the state's modeled initial tax year are observed; event year −1 is the omitted reference period. Employment and labor force participation are measured in percentage points, and work hours are measured in hours per week among respondents reporting positive hours. Bars indicate 95% confidence intervals. Models include state and ASEC year fixed effects, adjust for the pre-specified individual covariates and state covariates aligned to the preceding calendar year, and cluster standard errors at the state level.

Figure A6: Dynamic effects of state Child Tax Credits on parental employment and work hours (Stacked DiD event study, continuous first-year treatment)



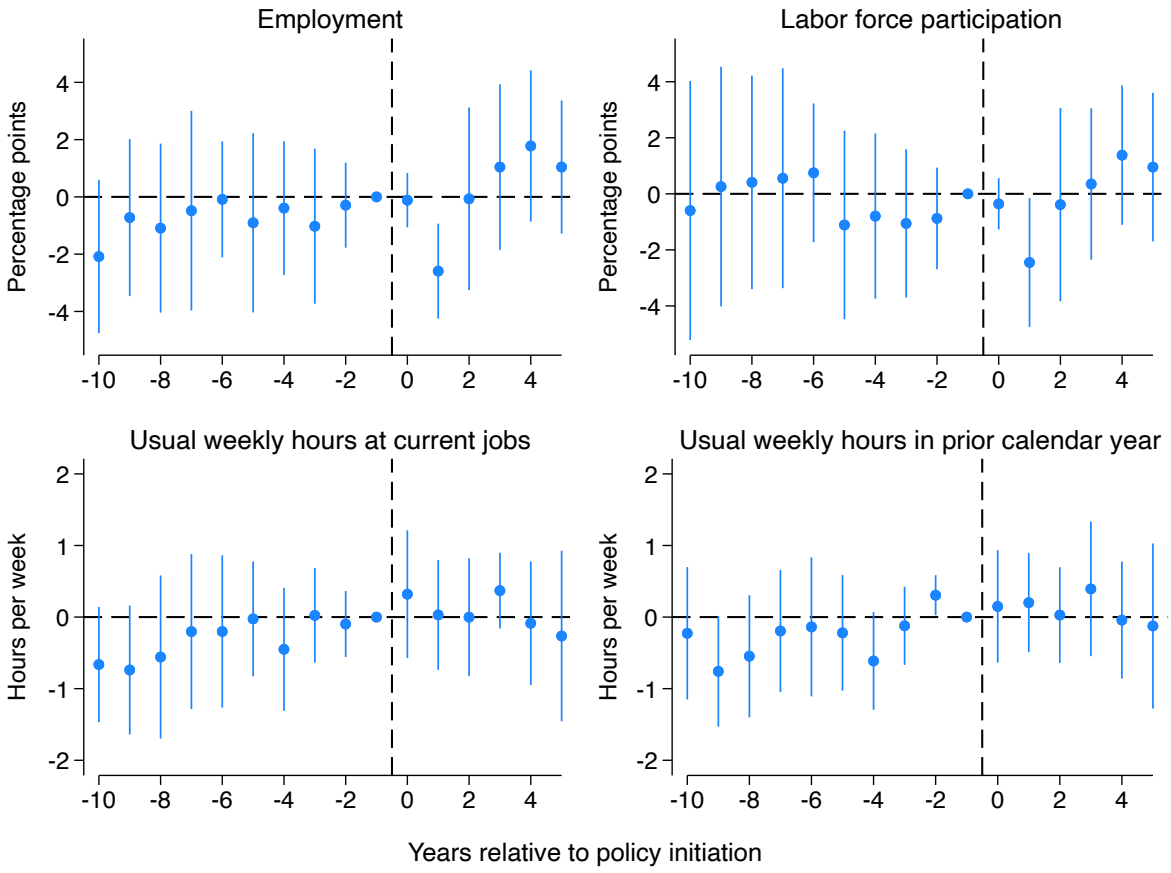
Note: Data are from the CPS-ASEC, 2004–2026. Points show coefficients from continuous-treatment stacked difference-in-differences event-study models. The treatment measure is the simulated benefit under each state's first-year CTC rules, held fixed across event time; coefficients therefore represent the change in the outcome per $100 of simulated benefit. Event time is measured relative to the first ASEC survey year in which outcomes from the state's modeled initial tax year are observed; event year −1 is the omitted reference period. Employment and labor force participation coefficients are percentage-point changes per $100, and work-hours coefficients are changes in hours per week per $100 among respondents reporting positive hours. Bars indicate 95% confidence intervals. Models use the primary balanced −10 to +2 stacked design, adjust for the pre-specified individual and state covariates, include treatment-status and relative-event-time fixed effects, apply Wing corrective weights, and cluster standard errors at the state level.

Figure A7: Dynamic effects of state Child Tax Credit adoption on parental employment and work hours (TWFE event study, continuous treatment)



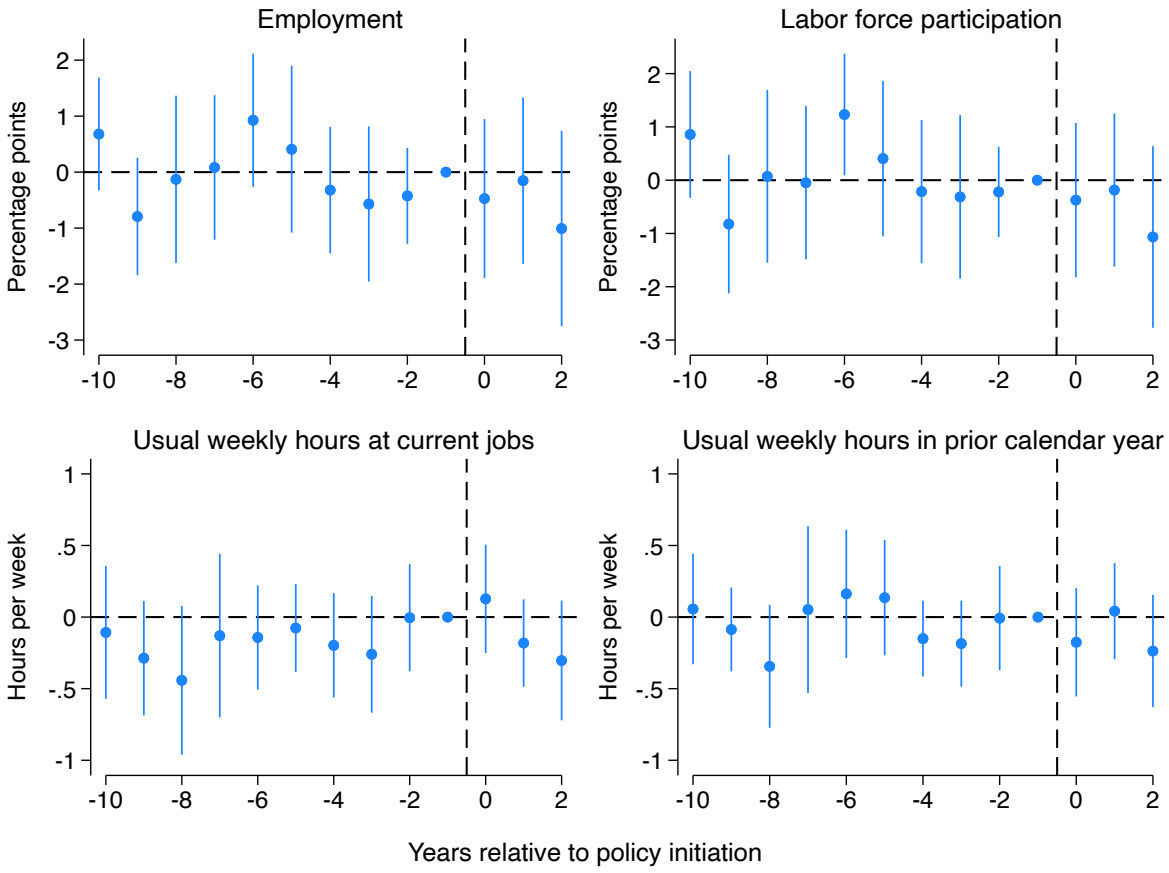
Note: Data are from the CPS-ASEC, 2004–2026. Points show coefficients from continuous-treatment two-way fixed effects event-study models. The treatment measure is the simulated benefit under the CTC rules in force in each state-year; coefficients therefore represent the change in the outcome per $100 of simulated benefit. Event time is measured relative to the first ASEC survey year in which outcomes from the state's modeled initial tax year are observed; event year −1 is the omitted reference period. Employment and labor force participation coefficients are percentage-point changes per $100, and work-hours coefficients are changes in hours per week per $100 among respondents reporting positive hours. Bars indicate 95% confidence intervals. Models include state and ASEC year fixed effects, adjust for the pre-specified individual covariates and state covariates aligned to the preceding calendar year, and cluster standard errors at the state level.

Figure A8: Dynamic effects of state Child Tax Credits on parental employment and work hours (Stacked DiD event study, longer post-treatment window)



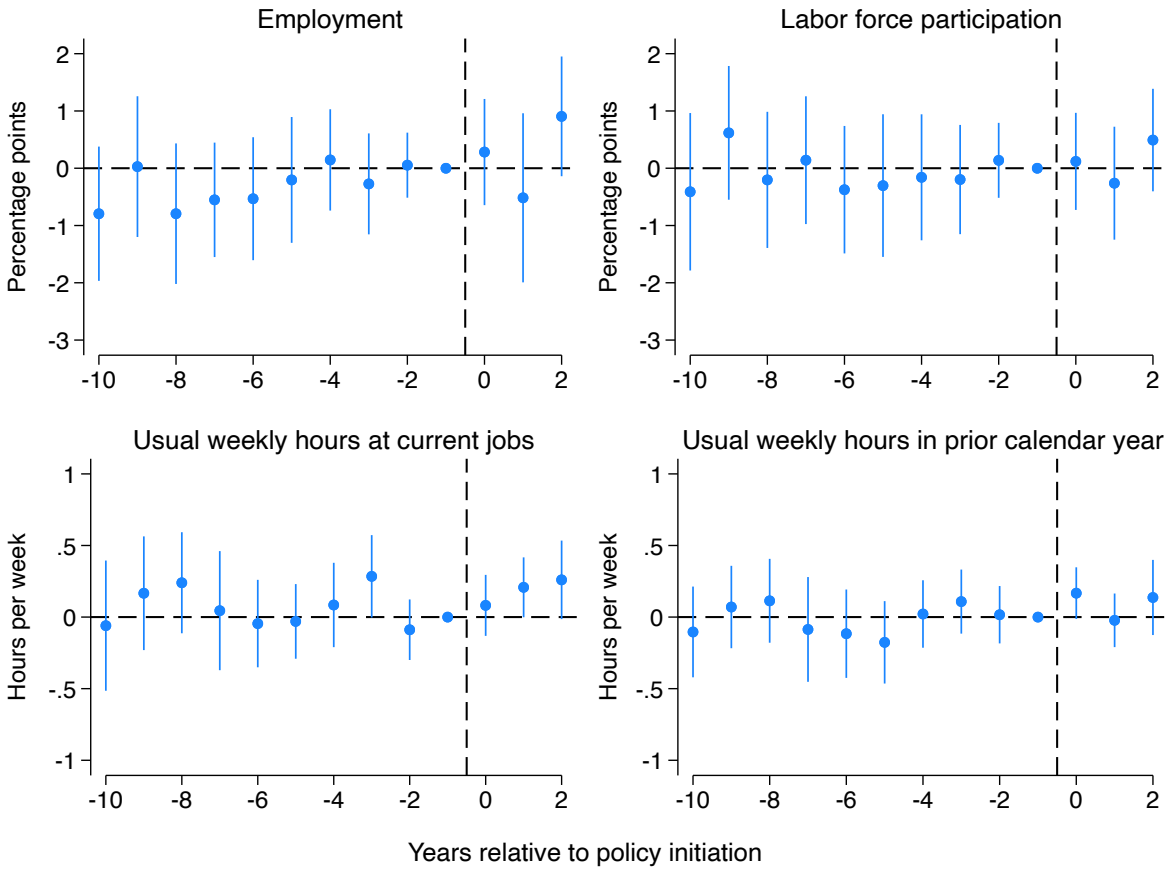
Note: Data are from the CPS-ASEC, 2004–2026. Points show coefficients from binary-treatment stacked difference-in-differences event-study models using a balanced event window from −10 through +5; only treated cohorts with complete support over this window contribute. Event year −1 is the omitted reference period. Employment and labor force participation are measured in percentage points, and work hours are measured in hours per week among respondents reporting positive hours. Bars indicate 95% confidence intervals. Apart from the longer event window, models use the primary stacked specification: the pre-specified individual and state covariates, treatment-status and relative-event-time fixed effects, Wing corrective weights, and state-clustered standard errors.

Figure A9: Dynamic effects of state Child Tax Credits on parental employment and work hours (DDD event study using childless adults)



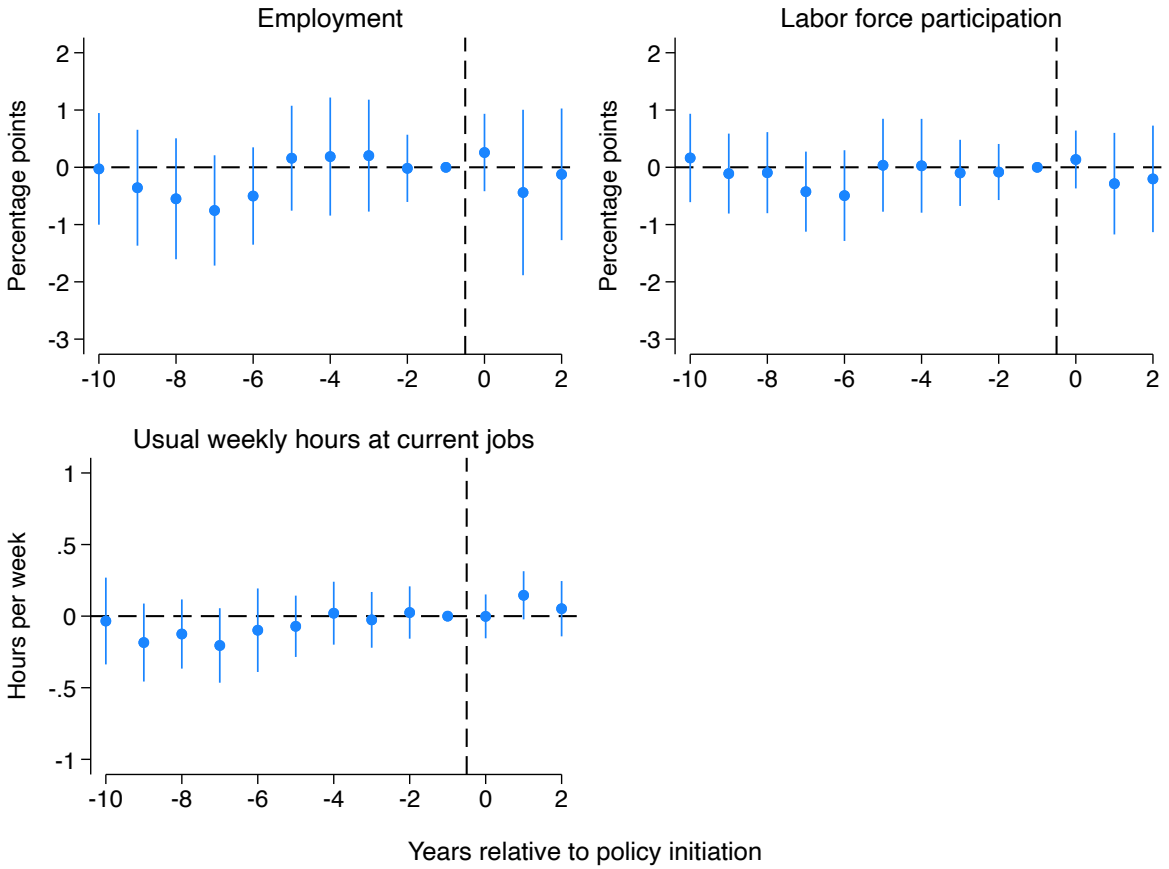
Note: Data are from the CPS-ASEC, 2004–2026. Points show event-time triple-differences estimates that compare changes among adults with children with changes among childless adults in adopting versus comparison states. Event time is measured relative to the first ASEC survey year in which outcomes from the state's modeled initial tax year are observed; event year −1 is the omitted reference period. Employment and labor force participation are measured in percentage points, and work hours are measured in hours per week. Bars indicate 95% confidence intervals. Models include state-by-ASEC year, state-by-parent group, and ASEC year-by-parent group fixed effects, adjust for the pre-specified covariates, and cluster standard errors at the state level.

Figure A10: Dynamic effects of state Child Tax Credits on parental employment and work hours (TWFE event study with childless adult placebo)



Note: Data are from the CPS-ASEC, 2004–2026. The sample is restricted to childless adults, who are not directly eligible for the state CTCs studied. Points show coefficients from binary-treatment two-way fixed effects event-study models that assign treatment timing according to state CTC adoption; the estimates therefore serve as a placebo test for changes coincident with adoption that are not specific to parents. Event year −1 is the omitted reference period. Employment and labor force participation are measured in percentage points, and work hours are measured in hours per week. Bars indicate 95% confidence intervals. Models include state and ASEC year fixed effects, adjust for the pre-specified covariates, and cluster standard errors at the state level.

Figure A11: Dynamic effects of state Child Tax Credits on parental employment and work hours (TWFE event study, Basic Monthly data)



Note: Data are from the Current Population Survey Basic Monthly files, 2004–2025. Points show coefficients from binary-treatment two-way fixed effects event-study models estimated in the full Basic Monthly sample. Event time is indexed to the modeled year of state CTC adoption; event year −1 is the omitted reference period. Employment and labor force participation are measured in percentage points, and usual weekly hours at current jobs are measured in hours per week among respondents reporting positive hours. Usual weekly hours from the prior calendar year are not available in the Basic Monthly CPS. Bars indicate 95% confidence intervals. Models include state and year-month fixed effects, adjust for the pre-specified individual and state covariates, do not use CPS population weights, and cluster standard errors at the state level.

Table A1: State Child Tax Credit policy features and simulated benefits, tax year 2025

State	First tax year used in analysis	Eligible child ages, 2025	Credit calculation, 2025	Income eligibility and phaseout, 2025	Refundability	Simulated benefit for reference family, 2025
New York	2006	Ages 0–3: larger credit; ages 4–16: smaller credit	Fixed credit of $1,000 per child age 0–3 and $330 per child age 4–16; no longer tied to the simulated federal CTC/ACTC.	Full amount through AGI of $110,000 for joint filers and $75,000 for single/HOH; above the threshold, total credit reduced by $16.50 for each completed $1,000 of excess AGI in the code. Separate $55,000 MFS threshold is not modeled.	Refundable	$1,330
Oklahoma	2008	CTC branch: ages 0-16; child-care branch: ages 0-12	Greater of 5% of federal CTC/ACTC or 20% of the federal child and dependent care credit.	Federal AGI $\leq$$100,000. Code assumes Oklahoma AGI equals federal AGI and therefore does not apply the statutory proration.	Nonrefundable; simulated allowable credit is not capped at state tax liability	$\approx$$181.65[†]
Idaho	2018	Ages 0-16	$205 per qualifying child.	No income phaseout in the coded schedule.	Nonrefundable; simulated allowable credit is not capped at state tax liability	$410
Maine	2018	Modeled: ages 0–16 (separate rates for ages 0–5 and 6–16); other dependents not modeled	$610 per qualifying child under age 6; $305 per qualifying child age 6–16.	Total credit reduced by $20 for each $500 (or fraction) of Maine AGI above $150,000 (joint), $125,000 (HOH), or $100,000 (single). The separate $75,000 MFS threshold is defined but not implemented in the CPS filing-status categories.	Refundable	$915
Arizona	2019	Ages 0-16 receive $100; age 17 receives $25	$100 per dependent under age 17 and $25 per dependent age 17.	Reduced by 5% for each $1,000 (or fraction) of federal AGI above $400,000 for joint filers and $200,000 for other filers.	Nonrefundable; code does not cap the simulated amount at state tax liability	$200
California	2019	At least one child age 0-5	Young Child Tax Credit of up to $1,189 per return, irrespective of the number of qualifying children.	Full credit with earned income of $0–$27,425; reduced by $21.71 for each $100 (or fraction) above $27,425; $0 above $32,901. CPS wage income is used as a proxy.	Refundable	$1,189
Massachusetts	2021	Modeled: ages 0-12; policy also covers certain disabled or older dependents	$440 per eligible individual.	No income limit or phaseout.	Refundable	$880
Colorado	2022	Regular CTC: ages 0-5; Family Affordability Tax Credit: ages 0-16	Combined regular CTC and Family Affordability Tax Credit (FATC). Regular CTC: $1,200, $600, or $200 per child age 0–5 by income tier; FATC up to $3,273 per child age 0–5 and $2,455 per child age 6–16.	Nonjoint regular CTC bands: AGI $\leq$$26,000, $26,001–$51,000, and $51,001–$77,000; joint bands $10,000 higher. FATC falls in $5,000 AGI bands: zero above $85,000 (nonjoint) or $96,000 (joint). Federal AGI used as proxy.	Refundable	$5,747
New Jersey	2022	Ages 0-5	Per child: $1,000, $800, $600, $400, or $200 across successive taxable-income bands.	New Jersey taxable income $\leq$$30,000: $1,000; $30,001-$40,000: $800; $40,001-$50,000: $600; $50,001-$60,000: $400; $60,001-$80,000: $200; $0 above $80,000. CPS federal taxable income is used as a proxy; married filing separately is ineligible.	Refundable	$1,000
Vermont	2022	Ages 0–6 (expanded from ages 0–5)	$1,000 per qualifying child.	Reduced by $20 per child for each $1,000 (or fraction) of AGI above $125,000.	Refundable	$1,000
Maryland	2023	Under age 6; ages 6-16 with a qualifying disability are also eligible but not modeled	$500 per qualifying child.	Full credit through federal AGI of $15,000; above $15,000, total credit is reduced by $50 for each $1,000 (or fraction) of excess AGI, down to $0.	Refundable	$0

Table A1 (continued)

State	First tax year used in analysis	Eligible child ages, 2025	Credit calculation, 2025	Income eligibility and phaseout, 2025	Refundability	Simulated benefit for reference family, 2025
Minnesota	2023	Ages 0-17	$1,750 per qualifying child. The simulation defines CTC as the incremental combined CTC + Working Family Credit (WFC) benefit relative to WFC alone.	CTC and WFC phased down jointly by 12% above $31,950 for nonjoint filers and $37,910 for joint filers, using the greater of earned income or AGI. The modeled WFC earnings cap is $9,480; CPS wage income proxies earned income.	Refundable	$3,500
New Mexico	2023	Modeled: ages 0-16; statutory IRC §152(c) definition may include some older qualifying children	Per child: $637, $424, $212, $106, $79, $53, or $26 across increasing AGI bands.	AGI ≤$25,000: $637; $25,001–$50,000: $424; $50,001–$75,000: $212; $75,001–$100,000: $106; $100,001–$200,000: $79; $200,001–$350,000: $53; above $350,000: $26.	Refundable	$848
Oregon	2023	Ages 0-5; maximum of five qualifying children	$1,050 per qualifying child (up to five).	Full credit through modified AGI of $26,550; linear phaseout over the next $5,000; $0 at $31,550 or above. Federal AGI is used as proxy.	Refundable	$1,050
Illinois	2024	At least one dependent child under age 12	40% of the Illinois EITC in 2025. Because the Illinois EITC equals 20% of the federal EITC, the state CTC equals 8% of the federal EITC.	Eligibility and phaseout follow the Illinois EITC/federal EITC schedule; there is no separate CTC income threshold.	Refundable	$528.80[†]
Utah	2024	Ages 0–5 (expanded from ages 1–3)	$1,000 per qualifying child.	Reduced by 10% of modified AGI above $54,000 for joint filers and $43,000 for single/head-of-household filers. The separate $27,000 married-filing-separately threshold is not separately modeled.	Nonrefundable; simulated allowable credit is not capped at state tax liability	$1,000

Note: All policy characteristics describe tax year 2025 (ASEC survey year 2026); the "first tax year used in analysis" is the modeled adoption year, not necessarily the legal first year of any related credit. Massachusetts is coded from tax year 2021 (Household Dependent Tax Credit; the Child and Family Tax Credit replaced it in 2023). Colorado is coded from tax year 2022 (regular CTC), with restructuring and FATC added in 2024. Maryland is coded from tax year 2023, when eligibility broadened to all children under six; its earlier disability-only pathway is not treated as the adoption event. The illustrative reference family is a single head-of-household parent with children ages 3 and 8, annual wage earnings and AGI of $25,938 (the official 2025 Census poverty threshold for a three-person family with two related children), and no child-care expenses. Amounts are nominal 2025 dollars. The policy schedules are those implemented in calculate_CTC_benefit(6).do; the example values are independent illustrative calculations, not re-estimates from the full ASEC 2026 CPS microdata. A zero indicates an effective credit for which the reference family is ineligible at this income. [†] The Oklahoma and Illinois examples require additional federal tax-model inputs not determined by the state-policy do-file alone. For Oklahoma, ≈$181.65 assumes a 2025 federal nonrefundable CTC of $233 (standard-deduction/IRS-tax-table illustration), refundable ACTC of $3,400, and $0 dependent-care credit; its actual sctc equals 5% of CPS CTCCRD + ACTCCRD or 20% of the modeled dependent-care credit, whichever is larger. For Illinois, $528.80 is 8% of the $6,610 federal EITC in the IRS 2025 EIC table for the $25,900–$25,950 band, two children, and HOH filing; actual sctc uses the CPS EITCRED field. Recalculate both from the final ASEC 2026 input if exact data-derived values are required. Nonrefundable credits in the simulation may represent allowable amounts without a state-income-tax-liability cap. Other CPS approximations include non-child/disabled dependent eligibility, certain MFS distinctions, New Jersey taxable income, Minnesota earned income, and Oregon modified AGI. The 2017 federal rule freeze for New York applies only through tax year 2024; the 2025 model uses fixed age-based amounts. DC and Georgia are excluded because their currently coded adoption years begin in tax year 2026, beyond the 2025 analytic period.

Table A2: Summary statistics for working-age adults with children (CPS-ASEC)

Characteristic	All states	CTC states	Control states
Family income (%)			
<100% FPL	114,143 (11.4)	41,806 (11.1)	72,337 (11.5)
100-199% FPL	177,172 (17.6)	67,158 (17.8)	110,014 (17.5)
200-399% FPL	320,196 (31.8)	114,923 (30.5)	205,273 (32.7)
400%+ FPL	393,902 (39.2)	153,264 (40.6)	240,638 (38.3)
Educational attainment (%)			
High school equivalent or less	119,978 (11.9)	50,198 (13.3)	69,780 (11.1)
Some college	538,241 (53.5)	190,505 (50.5)	347,736 (55.3)
Bachelor's degree or more	347,194 (34.5)	136,448 (36.2)	210,746 (33.5)
Marital status (%)			
Married / partnered	777,420 (77.3)	295,832 (78.4)	481,588 (76.7)
Other	227,993 (22.7)	81,319 (21.6)	146,674 (23.3)
Parent structure (%)			
Single parent	204,164 (20.3)	71,687 (19.0)	132,477 (21.1)
Spouse present	801,249 (79.7)	305,464 (81.0)	495,785 (78.9)
Race/ethnicity (%)			
NH White	638,617 (63.5)	216,236 (57.3)	422,381 (67.2)
NH Black	87,381 (8.7)	22,534 (6.0)	64,847 (10.3)
NH Asian	56,387 (5.6)	28,472 (7.5)	27,915 (4.4)
Hispanic	194,306 (19.3)	100,256 (26.6)	94,050 (15.0)
Number of children (%)			
1	428,877 (42.7)	158,436 (42.0)	270,441 (43.0)
2	373,946 (37.2)	141,876 (37.6)	232,070 (36.9)
3	142,851 (14.2)	53,890 (14.3)	88,961 (14.2)
4 or more	59,739 (5.9)	22,949 (6.1)	36,790 (5.9)
Age of youngest child, mean	7.6 (5.3)	7.6 (5.3)	7.6 (5.3)
Number of earners (%)			
0 earners	81,901 (8.1)	29,477 (7.8)	52,424 (8.3)
1 earner	406,164 (40.4)	157,219 (41.7)	248,945 (39.6)
2 or more earners	517,348 (51.5)	190,455 (50.5)	326,893 (52.0)
No. observations	1,005,413	377,151	628,262

Note: The sample consists of adults aged 18–64 years in tax units with at least one child aged 0–17 years and corresponds to the estimation sample for the primary population-unweighted TWFE employment model. The same sample is used for labor force participation; work-hours models additionally restrict the sample to respondents reporting positive hours. CTC states are states that adopted a state child tax credit during the study period and include observations from those states in both pre- and post-adoption years. Control states did not adopt a state CTC during the study period. Statistics are unweighted, and percentages use nonmissing observations for the relevant characteristic as the denominator. Family income is expressed as a percentage of the official federal poverty level. Race/ethnicity categories are mutually exclusive; respondents classified in other racial/ethnic categories are included in the denominator but not displayed separately. Parent structure is defined at the tax-unit level: single-parent tax units contain at least one child aged 0–17 and no spouse present; the comparison category has a spouse present. Observations are person-year records, and some respondents may appear in more than one year.

Table A3: Summary statistics for working-age adults with children (CPS Basic Monthly)

Characteristic	All states	CTC states	Control states
Annual family income category (%)			
Under $25,000	1,162,754 (15.2)	402,126 (14.1)	760,628 (15.8)
$25,000-$49,999	1,704,654 (22.2)	617,822 (21.7)	1,086,832 (22.6)
$50,000-$99,999	2,576,124 (33.6)	924,770 (32.5)	1,651,354 (34.3)
$100,000 or more	2,218,407 (29.0)	903,219 (31.7)	1,315,188 (27.3)
Educational attainment (%)			
High school equivalent or less	938,561 (12.2)	381,695 (13.4)	556,866 (11.6)
Some college	4,346,675 (56.7)	1,532,741 (53.8)	2,813,934 (58.5)
Bachelor's degree or more	2,376,703 (31.0)	933,501 (32.8)	1,443,202 (30.0)
Marital status (%)			
Married / partnered	5,961,761 (77.8)	2,266,400 (79.6)	3,695,361 (76.8)
Other	1,700,178 (22.2)	581,537 (20.4)	1,118,641 (23.2)
Parent structure (%)			
Single parent	1,840,267 (24.0)	637,340 (22.4)	1,202,927 (25.0)
Spouse present	5,821,672 (76.0)	2,210,597 (77.6)	3,611,075 (75.0)
Race/ethnicity (%)			
NH White	4,822,419 (62.9)	1,622,172 (57.0)	3,200,247 (66.5)
NH Black	753,752 (9.8)	195,043 (6.8)	558,709 (11.6)
NH Asian	430,733 (5.6)	214,965 (7.5)	215,768 (4.5)
Hispanic	1,399,258 (18.3)	732,489 (25.7)	666,769 (13.9)
Number of children (%)			
1	3,296,783 (43.0)	1,206,998 (42.4)	2,089,785 (43.4)
2	2,767,860 (36.1)	1,036,601 (36.4)	1,731,259 (36.0)
3	1,097,703 (14.3)	411,018 (14.4)	686,685 (14.3)
4 or more	499,593 (6.5)	193,320 (6.8)	306,273 (6.4)
Age of youngest child, mean	7.6 (5.4)	7.6 (5.4)	7.6 (5.4)
Number of earners (%)			
0 earners	504,747 (6.6)	170,973 (6.0)	333,774 (6.9)
1 earner	2,564,585 (33.5)	958,698 (33.7)	1,605,887 (33.4)
2 or more earners	4,592,607 (59.9)	1,718,266 (60.3)	2,874,341 (59.7)
No. observations	7,661,939	2,847,937	4,814,002

Note: The analytic sample corresponds to the unweighted full-sample Basic Monthly CPS employment model. CTC states are states that adopted a state child tax credit during the study period; observations from these states are included in the CTC state column in both pre- and post-policy periods. Control states did not adopt a state CTC during the study period. CPS Basic Monthly reports family income in broad categories and does not contain the detailed income and poverty-threshold measures available in the ASEC; the income rows therefore report broad annual family-income categories rather than percentages of the federal poverty level. Percentages use non-missing observations for the relevant characteristic as the denominator. Race/ethnicity rows omit respondents classified in other racial/ethnic categories, although they remain in the denominator. Parent structure is defined at the family-unit level using the full family roster before restricting respondents to working age: single-parent family units contain at least one child aged 0–17 and no spouse present; the comparison category has a spouse present. Respondents may contribute observations in multiple months.

Table A4: Treated cohorts and event-time support in stacked difference-in-differences analyses

Treated state(s)	Initial tax year	First ASEC year	Child ages defining cohort	Primary balanced -10 to +2	Unbalanced sensitivity	Event-time support in unbalanced stack
New York	2006	2007	Ages 0-16	No	Yes	-3 to +2
Oklahoma	2008	2009	Ages 0-16	No	Yes	-5 to +2
Idaho, Maine	2018	2019	Ages 0-16	Yes	Yes	-10 to +2
Arizona	2019	2020	Ages 0-17	Yes	Yes	-10 to +2
California	2019	2020	Ages 0-5	Yes	Yes	-10 to +2
Massachusetts	2021	2022	Ages 0-11	Yes	Yes	-10 to +2
Colorado, New Jersey, Vermont	2022	2023	Ages 0-5	Yes	Yes	-10 to +2
New Mexico	2023	2024	Ages 0-16	Yes	Yes	-10 to +2
Minnesota	2023	2024	Ages 0-17	Yes	Yes	-10 to +2
Maryland, Oregon	2023	2024	Ages 0-5	Yes	Yes	-10 to +2
Illinois	2024	2025	Ages 0-11	No	Yes	-10 to +1
Utah	2024	2025	Ages 1-3	No	Yes	-10 to +1

Note: Cohorts are defined by the first ASEC survey year corresponding to the modeled initial tax year and the child age rule used to define the treated sample. The primary stacked analysis retains only cohorts with treated and comparison observations at every event year from -10 through $+2$; twelve focal treated states satisfy this requirement. The unbalanced sensitivity retains supported event years within the same target window and therefore incorporates early and recent adoption cohorts that cannot contribute the full balanced window. States adopting in the same ASEC year with the same child age rule enter the same sub-experiment. Cohort inclusion shown here is determined at stack construction, before outcome-specific restrictions such as limiting work hours analyses to respondents reporting positive hours.

Table A5: Sensitivity analyses using stacked difference-in-differences models

	Alternative fixed effects		Alternative comparators		Other design, weighting, and sample specifications						
	(1) State + event-time FE	(2) Stack-specific FE	(3) Never-treated controls	(4) Not-yet-treated controls	(5) Exclude border areas	(6) Population weighted	(7) No covariates	(8) Unbalanced support	(9) Long event window	(10) Child age ≤ 5	(11) Refundable at adoption
Panel A. Employment (percentage points)											
Estimate	−0.79	−0.72	−0.22	1.84	0.03	0.03	−0.64	0.25	0.18	0.66	0.75
	(0.83)	(0.59)	(0.94)	(1.93)	(1.04)	(1.18)	(0.99)	(0.70)	(0.90)	(1.14)	(1.29)
Treated pre-policy mean	74.00	74.00	74.00	72.23	74.00	73.57	74.00	74.44	71.24	72.69	73.86
Estimate as % of pre-policy mean	-1.1%	-1.0%	-0.3%	2.5%	0.0%	0.0%	-0.9%	0.3%	0.3%	0.9%	1.0%
Observations	2,366,798	2,366,798	2,107,744	314,273	2,142,327	2,366,798	2,366,798	3,282,545	1,106,389	1,320,335	1,427,064
Panel B. Labor force participation (percentage points)											
Estimate	−1.02	−1.00	−0.71	1.21	−0.45	−0.66	−1.00	0.01	−0.09	0.40	0.23
	(0.69)	(0.63)	(0.79)	(1.52)	(0.82)	(0.95)	(0.86)	(0.54)	(1.10)	(0.79)	(0.71)
Treated pre-policy mean	78.20	78.20	78.20	77.09	78.20	78.02	78.20	78.39	76.28	77.13	78.11
Estimate as % of pre-policy mean	-1.3%	-1.3%	-0.9%	1.6%	-0.6%	-0.9%	-1.3%	0.0%	-0.1%	0.5%	0.3%
Observations	2,366,798	2,366,798	2,107,744	314,273	2,142,327	2,366,798	2,366,798	3,282,545	1,106,389	1,320,335	1,427,064
Panel C. Usual weekly hours at current jobs (hours per week)											
Estimate	−0.02	−0.02	0.04	0.35	0.02	0.26	−0.09	0.09	0.06	0.28	0.20
	(0.27)	(0.25)	(0.29)	(0.31)	(0.28)	(0.32)	(0.27)	(0.20)	(0.40)	(0.28)	(0.31)
Treated pre-policy mean	39.78	39.78	39.78	39.75	39.78	39.78	39.78	39.84	39.80	39.70	39.69
Estimate as % of pre-policy mean	-0.1%	-0.0%	0.1%	0.9%	0.1%	0.7%	-0.2%	0.2%	0.2%	0.7%	0.5%
Observations	1,685,365	1,685,365	1,498,427	224,622	1,524,633	1,685,365	1,685,365	2,340,389	783,230	926,082	1,017,415
Panel D. Usual weekly hours in prior calendar year (hours per week)											
Estimate	−0.09	−0.08	−0.02	0.26	−0.03	0.25	−0.12	−0.04	0.10	0.17	0.13
	(0.23)	(0.26)	(0.25)	(0.24)	(0.23)	(0.24)	(0.23)	(0.17)	(0.39)	(0.34)	(0.26)
Treated pre-policy mean	39.32	39.32	39.32	39.25	39.32	39.36	39.32	39.40	39.23	39.27	39.29
Estimate as % of pre-policy mean	-0.2%	-0.2%	-0.1%	0.7%	-0.1%	0.6%	-0.3%	-0.1%	0.3%	0.4%	0.3%
Observations	1,910,956	1,910,956	1,699,982	254,071	1,728,560	1,910,956	1,910,956	2,662,727	891,599	1,053,762	1,149,916
Fixed effects	State; event time	Stack × state; stack × event time	Treatment status; event time	Treatment status; event time	Treatment status; event time	Treatment status; event time	Treatment status; event time	Treatment status; event time	Treatment status; event time	Treatment status; event time	Treatment status; event time
Weights	Wing corrective	Wing corrective	Wing corrective	Wing corrective	Wing corrective	Wing corrective × CPS pop.	Wing corrective	Wing corrective	Wing corrective	Wing corrective	Wing corrective
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Sample or event window	Primary sample	Primary sample	Never-treated controls	Not-yet-treated controls	Exclude border areas	Primary sample	Primary sample	Unbalanced event-time support	Balanced −10 to +5	Child age ≤ 5	Refundable at adoption

Note: Entries are sensitivity analyses of the binary-treatment stacked difference-in-differences model. Except for model (9), estimates are equally weighted averages over event years 0–2 relative to event year −1. Model (9) uses a balanced event window from −10 through +5 and averages event years 0–5. Model (1) includes state and relative-event-time fixed effects; model (2) includes stack-by-state and stack-by-relative-event-time fixed effects. Model (3) restricts the comparison group to never-treated states; model (4) permits not-yet-treated states as comparators; model (5) excludes border areas; model (6) combines Wing corrective weights with CPS population weights; model (7) omits covariates; model (8) allows unbalanced event-time support; model (10) restricts the sample to adults with at least one child aged 5 years or younger; and model (11) restricts treated cohorts to states whose CTC was refundable in its modeled first tax year. States adopting a nonrefundable credit are excluded from model (11), rather than treated as controls. Unless otherwise indicated, models include treatment-status and relative-event-time fixed effects, the pre-specified individual and state covariates, and Wing corrective weights. Employment and labor force participation estimates are percentage-point changes; work-hours estimates are changes in hours per week among respondents reporting positive hours. Standard errors clustered by state are reported in parentheses. Estimates are also expressed as percentages of the corresponding weighted pre-policy outcome mean for the treated group. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A6: Sensitivity analyses using TWFE models

	Binary treatment: Effect of state CTC adoption								Continuous treatment: Coefficient per $100 of simulated benefit	
	(1) Monthly matched	(2) Monthly full	(3) Childless placebo	(4) DDD: childless	(5) Added macro controls	(6) Population weighted	(7) Zero hours for non-workers	(8) Refundable CTCs	(9) Mean benefit	(10) Maximum benefit
Panel A. Employment (percentage points)										
Estimate	0.49	0.09	0.25	−0.27	−0.05	−0.31		−0.23	−0.07	0.01
	(0.33)	(0.36)	(0.34)	(0.29)	(0.18)	(0.26)		(0.27)	(0.08)	(0.03)
Treated pre-policy mean	75.48	73.80	70.90	75.59	75.48	74.84		75.51	73.80	73.80
Estimate as % of pre-policy mean	0.7%	0.1%	0.3%	-0.4%	-0.1%	-0.4%		-0.3%	-0.1%	0.0%
Observations	990,721	7,661,939	1,463,173	2,468,586	955,580	1,005,413		929,781	7,661,939	7,661,939
Panel B. Labor force participation (percentage points)										
Estimate	0.37	0.11	0.09	−0.33	−0.18	−0.35*		−0.37**	−0.07	0.00
	(0.32)	(0.32)	(0.28)	(0.27)	(0.20)	(0.21)		(0.18)	(0.07)	(0.03)
Treated pre-policy mean	79.78	78.42	76.23	79.65	79.57	79.10		79.77	78.42	78.42
Estimate as % of pre-policy mean	0.5%	0.1%	0.1%	-0.4%	-0.2%	-0.4%		-0.5%	-0.1%	0.0%
Observations	990,721	7,661,939	1,463,173	2,468,586	955,580	1,005,413		929,781	7,661,939	7,661,939
Panel C. Usual weekly hours at current jobs (hours per week)										
Estimate	0.29**	0.12	0.08	0.04	0.14*	0.14*	0.14	0.13	0.00	0.02***
	(0.14)	(0.07)	(0.09)	(0.09)	(0.08)	(0.07)	(0.10)	(0.09)	(0.02)	(0.01)
Treated pre-policy mean	39.66	39.61	39.28	39.94	39.91	39.92	29.67	39.98	39.61	39.61
Estimate as % of pre-policy mean	0.7%	0.3%	0.2%	0.1%	0.4%	0.3%	0.5%	0.3%	0.0%	0.0%
Observations	700,399	5,289,974	961,930	1,679,215	681,536	717,285	953,942	664,024	5,289,974	5,289,974
Panel D. Usual weekly hours in prior calendar year (hours per week)										
Estimate			0.04	0.02	0.03	0.07	−0.00	0.09		
			(0.08)	(0.09)	(0.09)	(0.09)	(0.09)	(0.10)		
Treated pre-policy mean			38.67	39.39	39.38	39.42	30.02	39.44		
Estimate as % of pre-policy mean			0.1%	0.1%	0.1%	0.2%	-0.0%	0.2%		
Observations			1,123,957	1,940,596	775,005	816,639	990,246	755,567		
Specification	Monthly matched to ASEC	Monthly full sample	Childless DiD placebo	DDD with childless	Added macro controls	Population weighted	Nonworkers set to zero	Refundable at adoption	Monthly: mean benefit	Monthly: max. benefit
Fixed effects	State; year-month	State; year-month	State; calendar year	State×year; State×group; Year×group	State; calendar year	State; calendar year	State; calendar year	State; calendar year	State; year-month	State; year-month
CPS population weights	No	No	No	No	No	Yes	No	No	No	No

Note: Models (1)–(8) are binary-treatment models and report the effect of state CTC adoption. Models (9)–(10) are continuous-treatment models and report the coefficient per $100 of the current simulated state CTC benefit, using the mean and maximum simulated benefit assigned to each Basic Monthly CPS income cell, respectively. Model (1) uses Basic Monthly CPS observations matched to ASEC records; model (2) uses the full Basic Monthly CPS sample; model (3) is a difference-in-differences placebo analysis among childless adults; model (4) is a triple-difference model comparing adults with children with childless adults; model (5) additionally adjusts for the state unemployment rate and per-capita income; model (6) applies CPS population weights to the primary CPS-ASEC model; model (7) re-estimates the work-hours outcomes after retaining nonworking adults and assigning them zero hours; and model (8) restricts treated states to those whose CTC was refundable at adoption. States whose credit was nonrefundable at adoption are excluded from model (8), while never-treated states remain in the comparison group. Employment and labor force participation are not reported for model (7). Basic Monthly CPS models include state and year-month fixed effects; other models include the fixed effects shown in the table. All models adjust for the pre-specified individual and state covariates unless otherwise indicated. Employment and labor force participation estimates are percentage-point changes; work-hours estimates are changes in hours per week. Work-hours models are restricted to respondents reporting positive hours except in model (7). Prior-calendar-year hours are unavailable in the Basic Monthly CPS, and the corresponding cells are left blank. Standard errors clustered by state are reported in parentheses. Estimates are also expressed as percentages of the corresponding pre-policy outcome mean for the treated group. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: Alternative income normalizations of continuous-treatment labor supply estimates

			Income-normalized coefficient	
Outcome	Estimator / benefit measure	Coefficient (SE)	All treated parents	Positive-benefit parents
Employment	Stacked DiD: first-year simulated benefit	0.15 (0.17)	2.08 [-2.45, 6.60]	1.69 [-1.99, 5.38]
Employment	TWFE DiD: current simulated benefit	0.22 (0.20)	2.96 [-2.25, 8.17]	2.13 [-1.62, 5.89]
Labor force participation	Stacked DiD: first-year simulated benefit	0.17 (0.13)	2.19 [-1.22, 5.59]	1.78 [-0.99, 4.55]
Labor force participation	TWFE DiD: current simulated benefit	0.20 (0.18)	2.56 [-2.07, 7.18]	1.84 [-1.49, 5.18]
Weekly hours at current jobs	Stacked DiD: first-year simulated benefit	-0.07^{**} (0.03)	-1.99 [-3.89, -0.09]	-1.58 [-3.08, -0.07]
Weekly hours at current jobs	TWFE DiD: current simulated benefit	-0.05^{**} (0.02)	-1.26 [-2.50, -0.01]	-0.90 [-1.78, -0.01]
Weekly hours in prior year	Stacked DiD: first-year simulated benefit	-0.07^{***} (0.02)	-1.89 [-3.21, -0.57]	-1.48 [-2.51, -0.44]
Weekly hours in prior year	TWFE DiD: current simulated benefit	-0.09^{***} (0.02)	-2.35 [-3.62, -1.08]	-1.65 [-2.54, -0.76]

Note: The stacked estimates use the simulated benefit under each state's first-year CTC rules, held fixed across event time, and average event years 0–2 relative to event year −1. The TWFE estimates use the current simulated benefit under the CTC rules in force in each state-year. Continuous-treatment coefficients are reported per $100 of simulated benefit. The first income-normalized coefficient uses mean family income among all treated pre-policy parents in the outcome-specific regression sample. The alternative uses mean family income among treated pre-policy parents who would receive a positive benefit under their state's first-year CTC rules. The treatment coefficient and treated-group pre-policy outcome mean are unchanged; the second normalization therefore changes only the income denominator and is not a treatment effect re-estimated among potential recipients. Stacked income means use Wing corrective weights; TWFE income means are unweighted. Family income is measured in nominal dollars to match the nominal-dollar treatment measure. For employment and labor force participation, the normalizations scale extensive-margin estimates; for work hours, they scale conditional intensive-margin estimates because hours models are restricted to respondents reporting positive hours. Brackets show 95% confidence intervals obtained by applying the same scaling to the regression coefficient confidence intervals and treating the outcome and income normalization means as fixed. These quantities are descriptive income normalizations, not structural labor supply elasticities or pure income-effect parameters, because simulated benefits are not exogenous transfers and state CTC schedules may include earnings phase-ins and phase-outs.

Table A8: Pre-trend diagnostics and sensitivity to violations of parallel trends

		Joint pre-trend Wald tests		Sensitivity to violations of parallel trends			
Estimator	Outcome	p-value, years -10 to -2	p-value, years -5 to -2	Conventional 95% CI	HonestDiD: $\bar{M} = 0.5$	HonestDiD: $\bar{M} = 1$	HonestDiD: $\bar{M} = 2$
Stacked DiD	Employment	0.0014	0.253	[-1.97, 2.07]	[-3.97, 3.58]	[-6.60, 5.99]	[-12.21, 11.50]
Stacked DiD	Labor force participation	0.0001	0.421	[-2.07, 1.15]	[-3.66, 2.67]	[-5.77, 4.96]	[-10.62, 9.87]
TWFE event study	Employment	< 0.0001	0.100	[-1.13, 0.00]	[-2.23, 1.03]	[-3.58, 2.41]	[-6.46, 5.28]
TWFE event study	Labor force participation	0.0002	0.643	[-1.17, 0.02]	[-2.38, 0.97]	[-3.83, 2.32]	[-6.81, 5.28]

Note: The joint pre-trend tests report Wald tests of the null that the indicated pre-treatment event-study coefficients equal zero relative to event year -1. The -5 through -2 tests are supplementary diagnostics. The sensitivity intervals correspond to the equally weighted average of event years 0–2 relative to event year -1. Conventional intervals use standard event-study inference. HonestDiD intervals apply the relative-magnitude sensitivity analysis of Rambachan and Roth (2023), under which $\bar{M}$ scales the allowable magnitude of post-treatment departures from parallel trends relative to the largest pre-treatment departure. Values of $\bar{M} = 0.5$, 1, and 2 allow post-treatment violations up to one-half, equal to, or twice the corresponding pre-treatment benchmark. All models use the primary binary-treatment specifications and cluster standard errors at the state level.

Table A9: Joint tests of heterogeneity in employment effects across pre-specified subgroup domains

	Stacked DiD		TWFE	
Domain	p-value	Holm p-value	p-value	Holm p-value
Family income	0.0206	0.1234	0.7147	1.0000
Education	0.5639	1.0000	0.0335	0.2344
Marital status	0.5864	1.0000	0.4157	1.0000
Race/ethnicity	0.0022	0.0152	0.5128	1.0000
Age of youngest child	0.2495	1.0000	0.0473	0.2836
Number of earners	0.7937	1.0000	0.8978	1.0000
Parent structure	0.2848	1.0000	0.3084	1.0000

Note: Entries are p-values from joint Wald tests of the null that the estimated effect of state CTC adoption on employment is equal across all categories within the indicated subgroup domain. For the primary stacked difference-in-differences design, category-specific effects are equally weighted averages over event years 0–2 relative to event year −1. The pooled interacted stacked models allow event-study treatment effects, treatment-status and event-time fixed effects, and covariate coefficients to vary across subgroup categories; Wing corrective weights are recalculated within each category, and standard errors are clustered by state. The secondary TWFE models allow treatment effects, state and ASEC year fixed effects, and covariate coefficients to vary across subgroup categories, with standard errors clustered by state. The pooled specifications are used for the formal equality tests so that the covariance of subgroup estimates within state is retained. The domain for number of earners compares tax units with exactly one earner with those with two or more earners; zero-earner tax units are excluded from that domain. Holm's step-down correction is applied across the seven pre-specified subgroup domain tests separately within each estimator. Race/ethnicity tests include the four displayed categories; respondents in other racial/ethnic categories are excluded from that domain-specific test.

Table A10: Wild cluster bootstrap inference for primary stacked difference-in-differences estimates

Outcome	Estimate	Clustered SE	Conventional p-value	Bootstrap p-value	N
Employment (percentage points)	0.05	1.03	0.962	0.968	2,366,798
Labor force participation (percentage points)	-0.46	0.82	0.577	0.620	2,366,798
Usual weekly hours at current jobs	0.04	0.28	0.879	0.883	1,685,365
Usual weekly hours in prior calendar year	-0.02	0.24	0.927	0.927	1,910,956

Note: Entries report the primary binary-treatment stacked difference-in-differences estimates, averaged equally over event years 0–2 relative to event year −1. Employment and labor force participation estimates are percentage-point changes; work-hours estimates are changes in hours per week among respondents reporting positive hours. Conventional standard errors and p-values use state-clustered inference. Wild cluster bootstrap inference is based on the null-imposed wild bootstrap-t, with state as both the inference and bootstrap cluster, 9,999 replications, Rademacher weights, and symmetric p-values. Models use the primary balanced −10 to +2 stack, treatment-status and relative-event-time fixed effects, the pre-specified individual and state covariates, and Wing corrective weights. The outcome-specific estimation samples contain 49 distinct state-level clusters, including 12 states that contribute as focal treated states in at least one balanced sub-experiment. States that adopt during the broader study period may also contribute as clean later-treated controls when their adoption occurs after the focal sub-experiment's post-treatment window.