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ABSTRACT

We estimate the extensive and intensive margin labor supply response to the monthly Child Tax Credit disbursed in 2021 as a part of the American Rescue Plan Act. Using Current Population Survey microdata, we compare labor supply outcomes among households who qualify for varying relative increases in household income, as a result of their income level and household size. We do not find strong evidence of a change in labor supply for families receiving the credit. The results are robust to alternative labor supply models, where households respond mainly to cash on hand or changes in the annual budget set.

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1 Introduction

A general feature of tax and transfer programs is some trade-off between the value of redistribution, the fiscal costs of these transfers, and potential economic distortions. These inputs, along with subjective decisions regarding the relative weights placed on the wellbeing of different people or groups, and normative preferences over intermediate outcomes, such as labor supply, and/or the form of transfers, e.g. in-kind or cash, are required to carry out policy evaluations. In this study, we estimate the short-term labor supply response to cash transfers for low- and middle-income households, in the form of a fully-refundable child tax credit (CTC).

The 1990s featured a marked shift from so-called "traditional" welfare programs—such as AFDC, which featured cash transfers as a part of the safety net and potential work disincentives—to cash transfers that required recipients to work and have positive earnings, such as the Earned Income Tax Credit (EITC) and the Child Tax Credit (CTC). The contrast between these approaches is related to debates about two features of a nonlinear tax and transfer system: (1) the amount of transfers given to households with zero income, e.g. a guaranteed income, and (2) whether there should be negative marginal tax rates—also known as wage subsidies or "phase-ins"—along some portion of the tax schedule (see Saez, 2002; Rothstein, 2010, for example).

Federal policy in 2021 offered an opportunity to study these alternative models, as the American Rescue Plan temporarily moved the CTC from a policy with a work requirement and phase-in to a fully-refundable design, extending eligibility to families with no earnings. We compare labor market outcomes—labor force participation and hours worked—for families who qualified for smaller and larger CTC transfers, before and after the credit was paid out. Using a difference-in-differences and triple difference approach, we do not detect significant labor supply differences in response to variation in the size of the CTC. When framed as an increase in cash-on-hand, our confidence intervals rule out a Labor force participation rate decline of 0.3 percentage points in response to a 10 percentile increase in CTC relative

to income, or when cast as a change in the return to entering employment, we rule out an extensive labor supply elasticity of 0.005.

2 Background

Originally established in 1997, the Child Tax Credit (CTC) reduced tax liability by $500 per child for families with children under 17 years of age. The tax credit was nonrefundable at its inception—meaning filers had to have federal income tax liability to claim it—but a portion became refundable after the Economic Growth and Tax Relief Reconciliation Act of 2001 (EGTRRA). Families could receive a credit for initially 10 percent, and later 15 percent, of earnings above a minimum threshold. In 2019, total spending on the CTC was $118 billion, comparable to the total federal amount spent on children within Medicaid and the Child Health Insurance Program, combined (Hahn, Lou and Isaacs, 2020).

The last major, pre-pandemic change to the CTC occured with the Tax Cuts and Jobs Act of 2018 (TCJA). The TCJA doubled the maximum credit to $2,000 per child and lowered the minimum earnings threshold to $2,500. Lower-income households with no tax liability however could receive at most $1,400 per child. The CTC under TCJA law phased out for single-parent households at an income of $200,000 (which the TCJA raised from $75,000), and for married households at $400,000 (raised from $110,000). As a result, nearly one-third of children lived in households that did not receive the CTC, with the share among Black and Latino children at one-half and virtually no children in the bottom decile of incomes qualifying for the credit (Goldin and Michelmore, 2020).

In 2021, the CTC was temporarily re-designed as part of the American Rescue Plan (ARP). The ARP CTC allowed for full refundability of the credit, even for families with zero earnings. In addition, the maximum benefit per child increased to $3,000 for children between the ages of 6 and 17, and to $3,600 for children ages 0 to 5, for single filers earning below $75,000 and married filers earning below $150,000. Above those thresholds, the CTC

phased back down to the TCJA-$2,000 level, which then phased-out again per pre-2021 law at $200,000 fir single filders and $400,000 for married filers. In addition, advance, monthly payments were made to families beginning in July of 2021, with the remainder applied to one's tax balance or refund during early 2022. These new ARP parameters were put in place for only one year, although there was discussion of potentially instituting them more permanently. This version of the CTC was more akin to so-called Child Allowance policies of Australia, Ireland, or Canada.

In a standard model of labor supply, the effect of this policy change on the annual budget generates two forces. First, the policy removes the phase-in of the credit, which had previously created an incentive to work through substitution effects, all things equal. In other words, a negative marginal tax rate, or wage subsidy, was replaced with a more neutral implicit marginal tax rate of zero for low-income households. In addition, the increase in the level of the credit generates an income effect, reducing labor supply when leisure is a normal good. A secondary impact of the policy was to increase effective marginal tax rates over the range where the credit was phased back down from $3,000 or $3,600 to $2,000.

Because the CTC is applied to annual earnings or income, the preceding incentives operate at the level of the annual budget set. In that case, the timing of the payment of the advance monthly CTC within the year under the ARP regime is immaterial. However, for households who are credit-constrained, the more immediate impact of the CTC may have operated through its effect on monthly income, as the advance began to be paid out in July. At that frequency, the policy would generally produce an income effect by increasing liquidity for cash-constrained households. We may therefore expect to observe a labor supply response following the initial advance payments in July. In either case, the standard model predicts some reduction in labor supply in response to these changes. However, in a search model, more cash on hand could lead to higher quality (higher wage) matches, as discussed in Nekoei and Weber (2017), or, as described in Autor, Dube and McGrew (2023), the benefits could raise reservation wages and increase "job shopping." In theory, the net effect of this

dynamic on employment is ambiguous if workers remain at higher-paying jobs longer.

Prior studies have estimated the impact of the 2021 extension to the CTC on labor supply. Our study is closest to that of Ananat et al. (2021, 2022), who also used monthly CPS data in one of their specifications to look at labor supply before and after the onset of advance payments. Our findings generally replicate theirs of no significant labor supply response. We extend the analysis by further looking at whether there are labor supply changes when the policy expires at the end of 2021, and by using data from 2019 to conduct additional placebo analysis and to flexibly control for group-specific seasonal trends. A number of other studies, using different sources of data, some admiinstrative and some survey-based, similarly fail to find significant labor supply responses (Roll, Hamilton and Chun, 2022; Lourie et al., 2022; Karpman et al., 2022; Pilkauskas et al., 2022). However, Han, Meyer and Sullivan (2022) find slower employment growth among families with lower levels of education and children, relative to similar families without children during the period when CTC payments were made.

A related set of studies ask the more generally how a permanent and fully-refundable CTC would effect employment. A report by a National Academies of Science, Engineering, and Medicine panel estimated a reduction of 150,000 jobs, but only considered the income effects of the CTC. Studies that also consider the substitution effects of the policy change predict larger employment reductions, ranging from 296,000 (Brill, Pomerleau and Seiter, 2021) to 386,000 (Goldin, Maag and Michelmore, 2022) to between 358,000 and 411,000 (Bastian, 2022) to 1.5 million (Corinth et al., 2022). In contrasts to these studies, we focus on the response to a temporary CTC change, which is likely to entail a smaller behavioral response. These studies also rely on simulations, which require the authors to select a labor supply elasticity, while our estimates use realized outcomes and allow us to remain agnostic regarding that choice.

3 Data and Methods

Our analysis primarily draws on the basic monthly Current Population Survey (CPS), a monthly survey of about 60,000 U.S. households with detail on demographics and labor market outcomes. While the monthly CPS includes measures of wages for a subset of respondents, it only contains one measure of total income (HEFAMINC). This family income variable is coded as a categorical variable rather than a continuous one, with family income over the prior 12 months classified among 16 ranges. CPS questionnaires show that the concept of income for this variable is in principle Census money income: that is, CPS interviewers are instructed to exclude tax credits (such as the CTC) and tax liabilities as well as non-cash transfers when asking about family income.

To allow for a continuous measurement of income, we impute a continuous measure of family income with the aid of the CPS Annual Social and Economic Supplement (ASEC). The CPS ASEC is a once-a-year supplement, conducted for most respondents in March, which asks about prior year components of income. We classify both our basic monthly and ASEC CPS samples into demographic cells by marriage status (2 categories), number of children (4 categories), and elderly status (2 categories). We then randomly draw, on a weighted basis, continuous values of family income from the CPS ASEC conditional on an individual's discrete family income category and demographic categories in the basic monthly CPS (see Han, Meyer and Sullivan, 2022, for example). We also draw the value of adjusted gross income (AGI) and total earnings from the same CPS ASEC record that provides family income. For basic monthly CPS samples in calendar years up to and including 2021, we draw from the CPS ASEC sample corresponding to the same reference year. For example, the 2022 CPS ASEC refers to outcomes in calendar year 2021. For 2022 monthly data, we repeat our use of the 2022 CPS ASEC, as the 2023 ASEC is not yet available.

We impute CTC eligibility using the drawn continuous values of AGI, reported number of own-children in the monthly CPS, and the relevant tax parameters of the CTC. We do not adjust the CTC level for incomplete take-up. Once we have calculated a CTC to income

ratio for each household, we order households in their percentile of this ratio, which serves as our main regressor.

The outcomes of interest are (1) labor force participation, using the official labor force definition of being employed, actively seeking a job, or being on temporary layoff/furlough; and (2) actual hours worked unconditional on employment (non-employed individuals and absent workers are coded as 0 hours worked), which provides a continuous measure of labor utilization.

We estimate difference-in-differences (DD) specifications using data from before and after the payment of the advance CTC in July 2021[1]:

$$y_{it} = \alpha + \beta_0 C\hat{T}C_i + \beta_1 H2_t + \beta_{DD} C\hat{T}C_i \times H2_t + \beta_X X_i + \varepsilon_{it} \tag{1}$$

where β_{DD} is the coefficient of interest, $H2_t$ is an indicator for post-July observations, and $C\hat{T}C_i$ is the percentile rank of the household's CTC-to-income ratio. One potential confounder is that households who qualify for different levels of CTC may have different trends in outcomes from the first half to the second half of the year, violating the parallel trends assumption. We therefore, include data from 2019 and estimate a triple difference regression (DDD) as follows:

$$\begin{aligned} y_{it} = \alpha_0 &+ \beta_0 C\hat{T}C_i + \beta_1 H2_t + \beta_3 C\hat{T}C_i \times H2_t \\ &+ \mathbb{1}\{t \in 2021\} \times \left(\alpha_1 + \beta_4 C\hat{T}C_i + \beta_5 H2_t + \beta_{DDD} C\hat{T}C_i \times H2_t\right) + \beta_X X_i + \varepsilon_{it} \end{aligned} \tag{2}$$

where β_{DDD} is the coefficient of interest, and allows us to address differential seasonal trends for those with smaller and larger CTC benefits. Finally, we re-estimate these models using

[1]We also use regression estimates to calculate the implied labor supply elasticity from changing cash-on-hand. In this specification, we estimate the main specifications with the change in return-to-work incentives as the main regressor of interest. We define define change in return-to-work incentives as the negative of the Tax Cuts and Jobs Act (TCJA) CTC benefit, divided by income. This definition reflects that the ARP modified the CTC to be refundable, whereas it was non-refundable in the TCJA. See Ananat et al. (2022) for further discussion.

data from before and after the end of 2021, to capture the impact of expiration of the temporary extension to the CTC.

When households are not cash-constrained, the relevant incentives are the impact of the change in CTC on the annual budget constraint, as measured by the return to work, which is the change in after-tax income when moving from not working to earning a positive amount. Under the TCJA CTC, one can only receive the CTC when earning a positive amount, incentivizing entry. Under the ARP CTC, the CTC is received at low levels of income regardless of one's earnings level, removing this incentive. As explained in Corinth et al. (2022), the reduction in this incentive for a household considering earning z will be:

$$\begin{aligned}
\triangle(\text{Return to Work}) &= \underbrace{\frac{[z - T_{ARP}(z) - T_{ARP}(0)]}{z}}_{\text{Return to work under ARP}} - \underbrace{\frac{[z - T_{TCJA}(z) - T_{TCJA}(0)]}{z}}_{\text{Return to work under TCJA}} \\
&= \frac{\overbrace{[CTC_{ARP}(z) - CTC_{ARP}(0)]}^{=0}}{z} - \frac{[CTC_{TCJA}(z) - \overbrace{CTC_{TCJA}(0)]}^{=0}}{z} \\
&= -\frac{CTC_{TCJA}(z)}{z}
\end{aligned} \tag{3}$$

where T is the tax or transfer under either the ARP or TCJA. In the second line, the level of earnings and other features of the tax code generally cancel out, leaving just the change in the CTC. Furthermore, with no phase in, the ARP CTC, at lower levels of earnings, is unchanged upon entering. We therefore regress labor force participation on this measure, using either our DD or DDD specifications above, and rescale to interpret as an elasticity.

4 Results

Figure 1 displays a binscatter of labor force participation on our main regressor of interest, percentiles of CTC-to-income ratios. Panel A displays this for five months pre-CTC extension

(February-June 2021) and five months post-CTC extension (August 2021-December 2021). This figure shows that while labor force participation does decline for higher levels of the CTC, this pattern is nearly identical pre- and post-CTC extension. The figure suggests that the CTC did not reduce labor force participation.[2]

The same conclusion is echoed in Figure 1, Panel B, which displays a binscatter for the five months of extended CTC in 2021 and three months post-expiration (January-March 2022). Again, this figure shows the relationship between labor force participation and CTC eligibility is nearly identical pre- and post-CTC expiration, suggesting the extended CTC did not reduce labor force participation.[3]

Figure 2 tests for placebo effects. Panel A shows a binscatter of labor force participation on percentiles of CTC-to-income ratios for five months before early in 2019 (February-June 2019) and five months later in 2019 (August 2019-December 2019), treating July of 2019 as a placebo CTC rollout. Panel B displays the binscatter for the five months of the placebo 2019 CTC rollout and the three months of 2020 (January-March 2020), which mirror the expiration of the CTC extension. As in Figure 1, both figures show little difference in the relationship between LFPR and CTC eligibility, suggesting the estimated relationship between LFPR and CTC eligibility was similar in 2019 and 2021.

Table 1, Panel A shows regression estimates of the effects of the effect of CTC-to-income percentiles on labor force participation; we show these estimates for both the rollout and the expiration.[4] Theoretically, this effect is likely to be negative in both main specifications (double difference and triple difference, columns (1), (2), (4), and (5)); if leisure is a normal

[2]In addition, we imprecisely estimate a positive effect on employment and a negative effect on unemployment—again failing to detect a reduction in labor supply.

[3]One caveat to this analysis: the Bureau of Labor Statistics modified the CPS survey weights beginning with the January 2022 CPS based on the results of the 2020 decennial Census. This population control resulted in a slight increase in employment and labor force participation rates for some cohorts. To the extent this affects our analysis, it will shift down the CTC eligibiliy-LFPR gradient in the post-extension period. Given our null finding, under the assumption that mismeasurement is greatest for lower-income households, this would imply that the CTC caused increased labor force participation - which again runs opposite to the hypothesis that CTC disincentivized work.

[4]For the expiration regressions, we use the post-expiration period as the base period, showing the coefficient on the interaction term between the CTC-to-income percentiles and an indicator for the expanded CTC time period, that is, August to December 2021.

good, the income effect of an increase in cash-on-hand could likely decrease labor supply. Yet, across our main specifications, and when using either the rollout or the expiration of the expanded CTC as the experiment of interest, we fail to detect a significant negative effect of the expanded CTC on labor force participation. One of the four (double difference, column (1)) is statistically significant and opposite-signed from our theoretical prediction. The placebo estimates (columns (3) and (6)) are small and positive, but statistically significant. That the placebo estimates are significant suggests there may be seasonal patterns in labor force participation that vary by income level - for example, if seasonal work during the December holiday season is concentrated among lowest wage workers. Such seasonal patterns would also confound the double difference specification (columns (1) and (3)), and bias the results against finding a negative income effect. Yet, when we run our triple difference specification (columns (2) and (4)) to adjust for baseline seasonal differences, the coefficient of interest is statistically indistinguishable from zero.

Table 1, Panel B shows regression estimates of the effects of changes in return-to-work incentives on labor force participation. Theoretically, these coefficients should be positive. And since the ARP CTC removed the income phase-in from the TCJA CTC, the net income from earning a positive amount, relative to not working, was diminished. The substitution effect implies the ARP's effect on the annual budget set could potentially decrease labor supply. Yet, across both main specifications (columns (1), (2), (4), and (5))—and, again, when using either the rollout or the expiration of the expanded CTC as the experiment of interest—we fail to find significant effects of a temporary change in return-to-work incentives on labor force participation. Furthermore, the effects are comparable in magnitude to our placebo estimates in columns (3) and (6), which are, as expected, insignificant.

To evaluate the magnitude of the implied labor supply response, Table 1, Panel C calculates the elasticity of labor supply with respect to return-to-work incentives. To calculate the largest labor supply elasticity that can be ruled out with our data, we calculate the elasticity at the 95th percentile of the confidence interval for the regression estimates of the

effect of return-to-work incentives on labor force participation (estimated in Panel B). Our estimates rule out a labor supply elasticity of 0.005; this suggests that a temporary 100 percent decrease in return to work generated by the TCJA CTC reduces labor supply by no more than 0.5 percent.

Figure 3 reports results from our triple-difference specification (2) for labor force participation and for total hours worked, broken out by demographic subgroups: gender, education level, number of kids, and race. The effect sizes are scaled by the standard deviation of the outcome variable for comparability. Across demographic groups, we fail to find an effect of the extended CTC on labor force participation.

For total hours worked, we likewise fail to find a significant impact on labor supply, although the point estimates tend to be more positive than those derived from labor force participation. Again, this pattern holds across different demographic subgroups.

5 Conclusion

The American Rescue Plan of 2021 extended the Child Tax Credit, by increasing the maximum benefit per child to $3,000-3,600 for the July 2021-December 2021 period and removing the work requirement to receive this money. This temporary policy provided a unique opportunity to study the effect of swithing from a transfer with work requirements to an unconditional cash benefit on labor force participation. We fail to find effects of the expanded benefit, and temporary removal of the work requirement, on labor force participation and a total hours worked.

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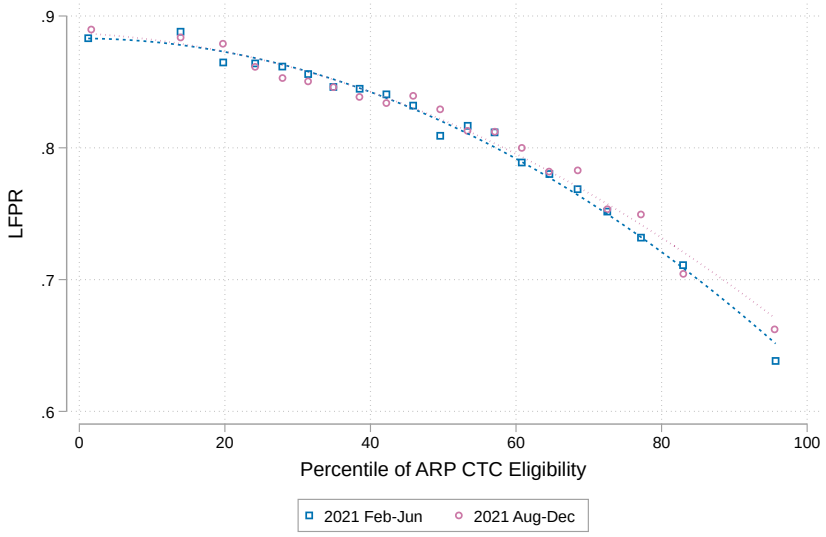
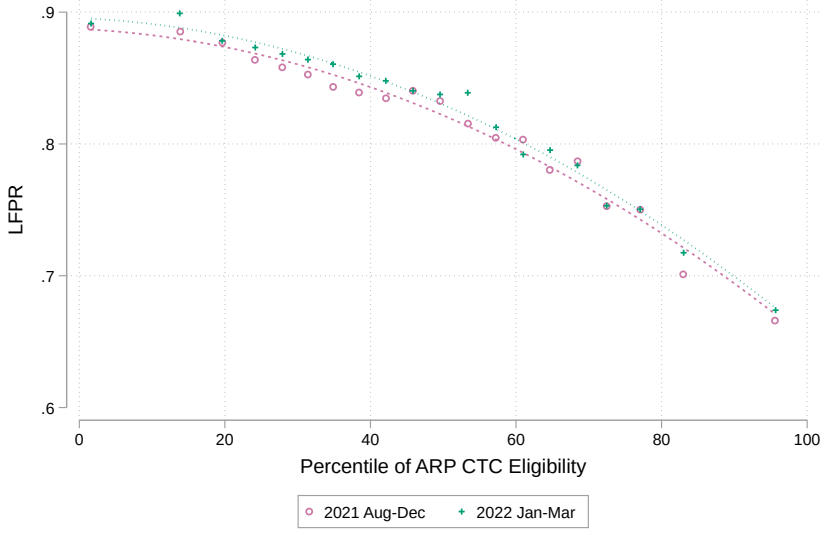
Figure 1: Relationship between ARP Child Tax Credit Eligibility and Labor Force Participation

Panel A. Before/After Extended CTC Introduction



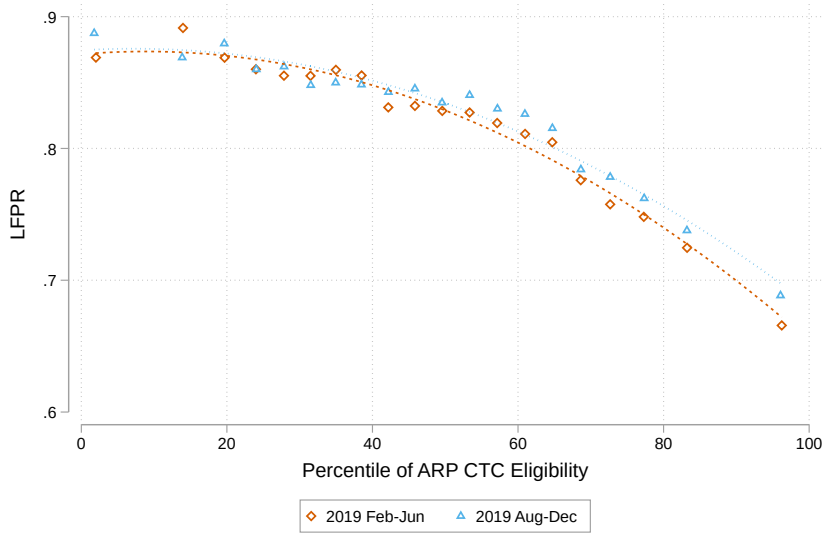
Panel B. Before/After Extended CTC Expiration



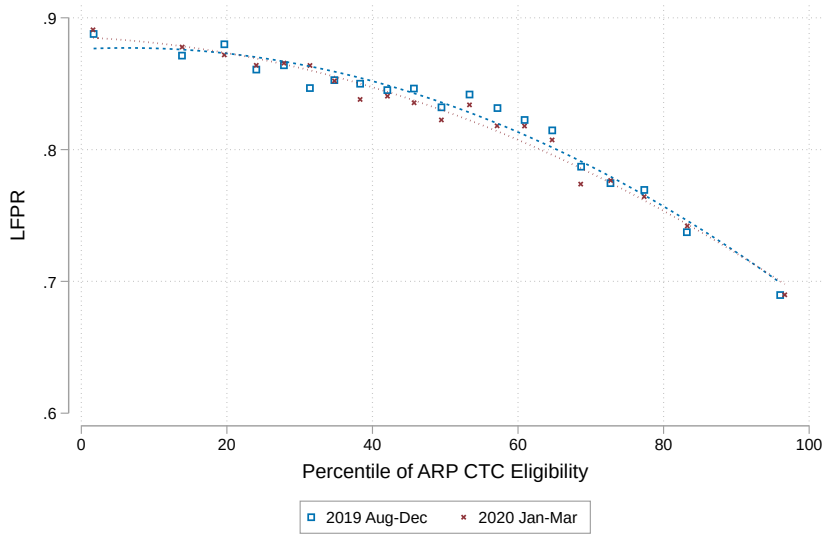
This figure shows the relationship between ARP CTC eligibility and labor force participation. Panel A shows the relationship before (Feb-June 2021) versus after (Aug-Dec 2021) the introduction of the ARP CTC benefit. Panel B shows the relationship before (Aug-Dec 2021) versus after (Jan-Mar 2022) the expiration of the ARP CTC benefit.

Figure 2: Relationship between Placebo ARP Child Tax Credit Eligibility and Labor Force Participation: 2019

Panel A. Before/After Extended CTC Introduction: Placebo (2019)

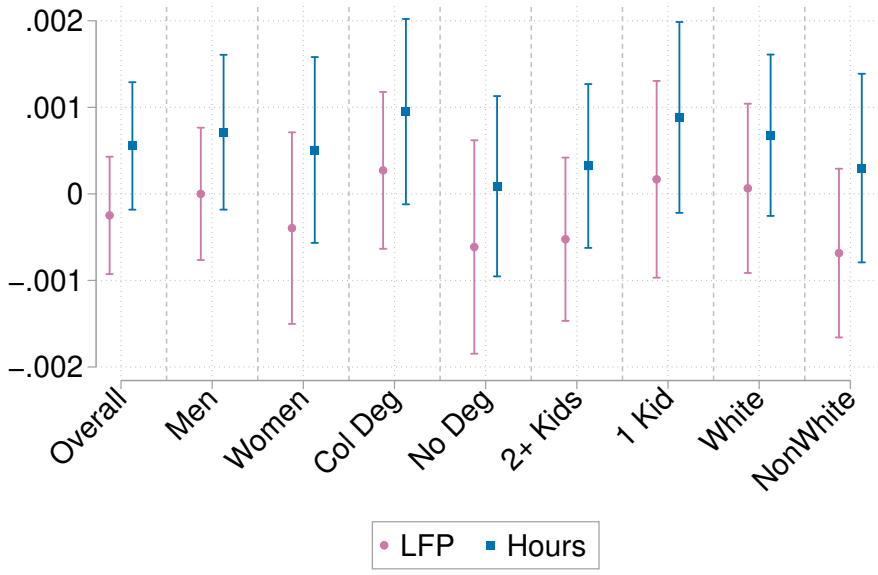


Panel B. Before/After Extended CTC Expiration: Placebo (2019)



This figure shows the relationship between placebo ARP CTC eligibility (in 2019) and labor force participation. Panel A shows the relationship before (Feb-June 2019) versus after (Aug-Dec 2019) the placebo equivalent of the introduction of the ARP CTC benefit. Panel B shows the relationship before (Aug-Dec 2021) versus after (Jan-Mar 2022) the placebo equivalent of the introduction of the ARP CTC benefit.

Figure 3: Effect of ARP Child Tax Credit Extension on Labor Force Participation and Total Hours Worked



This figure shows the effect of the ARP CTC eligibility percentile on labor force participation and hours. The dependent variable is scaled by its standard deviation. Specification detailed in the text. Standard errors are clustered by region and household size.

Table 1: Effect of ARP Child Tax Credit Extension on Labor Force Participation

	(1)	(2)	(3)	(4)	(5)	(6)
	DD Rollout	DDD Rollout	DD Placebo Rollout	DD Expiration	DDD Expiration	DD Placebo Expiration
Panel A: Cash on Hand Model						
100 × CTC-to-Income Pctile	0.017	-0.010	0.026	0.004	-0.011	0.015
	(0.008)	(0.013)	(0.011)	(0.009)	(0.012)	(0.008)
Panel B: Annual Budget Set Model						
100 × Return-to-Work Incentive	-0.049	-0.025	-0.025	-0.059	-0.046	-0.013
	(0.045)	(0.061)	(0.042)	(0.047)	(0.068)	(0.048)
Panel C: Upper Bound Return-to-Work Elasticity						
Elasticity	0.002	0.005	0.003	0.002	0.004	0.004

Notes: Panel A shows regression estimates of the effect of the ARP CTC eligibility percentile on labor force participation. Panel B shows regression estimates of the effect on labor force participation of ARP CTC return-to-work incentives (defined as the negative of the CTC benefit in the Tax Cuts and Jobs Act - see text for details). For the expiration regressions in Panels A and B, we use the post-expiration period as the base period, showing the coefficient on the interaction term between the CTC-to-income percentiles and an indicator for the expanded CTC time period. Panel C shows the labor supply elasticity with respect to return-to-work incentives; we calculate this implied elasticity at the 95th percentile of the estimated confidence intervals of the Panel B estimates. Specification detailed in the text. Standard errors are clustered by region and household size.