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Employment and Labor Supply Responses to the Child Tax Credit Expansion: Theory and Evidence

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Abstract

The 2021 Child Tax Credit (CTC) expansion increased government benefits to families, and especially to families with the lowest incomes. Economic theory predicts that this policy intervention would have led to a reduction in labor supply among adults in those families. The researchers' review of available research suggests that employment within broadly defined demographic groups was not reduced by the 2021 CTC changes. However, they present evidence that employment was reduced among mothers with relatively low levels of education — the demographic group that was most affected by the CTC expansion. For the 2021 CTC expansion, theory and evidence were in the strongest alignment when the research design that produced the evidence was most focused on the demographic groups most likely to be affected by the expansion.

The Child Tax Credit (CTC) has become an important income support policy for low- and moderate-income families with children. Established as part of the 1997 Taxpayer Relief Act, the CTC was originally a tax credit worth $400 per child younger than age 17 and was nonrefundable for most families, meaning that the tax credit reduced the amount of taxes that families owed, but only up to the amount of their tax liability. The CTC has grown incrementally since then, was made partially refundable in 2001, and then expanded to $1,000 per child in 2012. In 2017, the maximum benefit was increased to $2,000 per child, and the refundable portion was increased to $1,400.

Working in conjunction with the Earned Income Tax Credit (EITC), the CTC helps to reduce child poverty. Together, the EITC and CTC reduced overall child poverty (relative to what it would be if these programs did not exist) by 3.8 percentage points in 2020—and by 5.6 percentage points among Black children and 6.4 percentage points among Hispanic children (Bitler, Hoynes, and Schanzenbach 2023). Because the structure of the CTC provides no benefit to children in families with very low incomes, 10 percent of children live in families receiving no credit and 25 percent live in families receiving only a partial credit. (This goes back to the issue of refundability: families with little or no tax burden are not given a credit against taxes that they do not pay.) Among Black children, 25 percent receive no credit and 19 percent receive a partial credit, and among Hispanic children, the rates are 30 percent and 39 percent, respectively (Goldin and Michelmore 2022).

The American Rescue Plan Act of 2021 made several temporary changes to the CTC. Maximum tax credits were raised to $3,600 per child younger than age 6 and $3,000 per child aged 6 or older. Seventeen-year-old children were made eligible for the credit for the first time. The credit was made fully refundable, and half of the benefits were paid monthly through the advance CTC provision. As a result, families receiving the full benefit amount received a monthly payment of $250 or $300 per child from July to December 2021, the period during which the provisions were in place.

Basic economic theory suggests that adults in families receiving additional unearned income will not be as motivated as others to find and hold onto paid work, therefore contributing to a reduction in the overall labor supply. More importantly, as we describe below, the 2021 CTC replaced an existing program with work incentives built into it. Eliminating these work incentives is a key reason to expect a reduction in work effort after the new policy took effect.

In this article, we lay out the economic theory predicting the labor supply response to the policy change, and then we review the existing literature on the contemporaneous impacts of the advance CTC payments. We present empirical explorations of our own that probe the robustness of the findings in some of the extant data and conclude with a discussion of the consensus that is emerging from current evidence.

Theory

Textbook economic theory argues that individuals maximize their well-being by making decisions about how much of their time and resources will be dedicated to the consumption of goods and services and how much will go into leisure time to enjoy. The consumption of goods and services requires work because goods and services are purchased using the earnings generated by working, but the more an individual works, the less time she has available to enjoy leisure. Individuals decide the optimal mix of labor and leisure based on their preferences and on prices, including their wage rate while working.

When income support programs like the Child Tax Credit are introduced or changed, they can affect how much someone decides to work through changing the after-tax wage and their unearned income. In the language of economic theory, changes in wages induce two effects: a "substitution effect" and an "income effect." When the (after-tax) wage goes down, the benefit from working an extra hour is smaller and the cost of enjoying an extra hour of leisure is lower. In this circumstance, the substitution effect implies that the worker will work fewer hours and enjoy more leisure. At the same time, though, an "income effect" asserts itself. A decrease in the wage rate reduces overall income, which reduces the demand for leisure (and for consumption goods). An additional important consideration occurs when unearned income increases. This increases the demand for leisure (and for consumption goods), and economic theory predicts it will lead to a reduction in work hours.

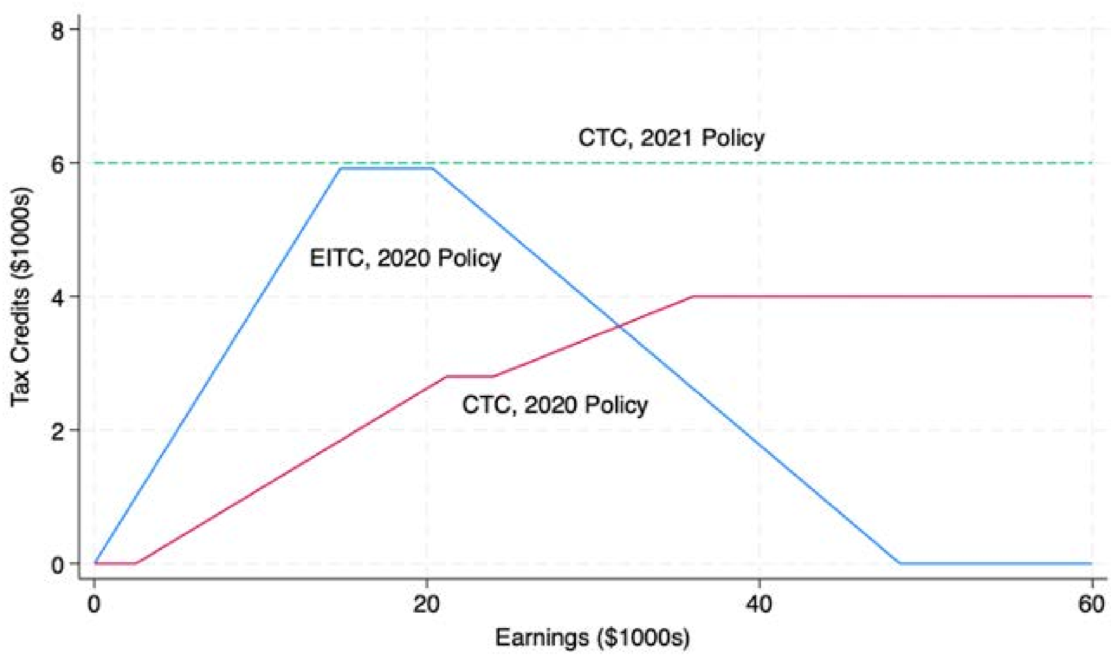
An expansion of the Child Tax Credit can change the after-tax wage rate of workers if the change affects the rates at which the credit phases in or out. It also increases unearned income. Note that often the substitution and income effects work in opposite directions, with one pushing individuals to work more and the other pushing individuals to work less. In some cases, however, both effects push in the same direction, and we can predict whether employment and hours worked are likely to increase or decrease in response to the policy change.

Figure 1 displays parameters of the Child Tax Credit for married parents with two children (ages 6 to 16) earning $60,000 or less, under the 2020 policy and the 2021 policy. Because the Earned Income Tax Credit (EITC) is such an important policy impacting low-income families with children, we also include EITC parameters in the figure and start by describing its impacts on work incentives. Note that the EITC provides no benefit to households without earnings but phases in rapidly at a rate of 40 percent. In other words, for every dollar earned until the family reaches the maximum credit amount, the EITC increases by 40 cents. There is a range of earnings—in this case between $14,800 and $19,300—over which the family continues to receive the maximum EITC credit, an amount just below $6,000 per year. Above $19,300 in earnings, the EITC payment is reduced by 21.06 cents for every additional dollar earned until the family is no longer eligible for any EITC benefits if their income is above about $47,500.

Relative to a world in which there is no EITC, the upward-sloping section of the EITC increases the take-home wage of workers. Since EITC benefits increase by 40 cents for every dollar earned in this section, a worker with a $10-per-hour wage will see a net increase of $14 per hour worked between her wage and increased EITC payments. The resulting substitution effect raises the “cost” of not working—equivalently, it increases the financial return from working—and is predicted to draw non-workers into employment. In the flat portion of the EITC graph, the worker brings home an additional $10 in wages for each additional hour worked and loses no EITC benefits. In this range, then, the worker faces no substitution effect but only faces an income effect, which would tend to encourage the worker to consume more leisure (and work fewer hours). In the phase-out range, the worker brings home less than her $10 per hour wage for every additional hour she works, because her additional wage is offset by a $2.11 loss in EITC benefits, making her net hourly wage $7.89. This substitution effect, coupled with the additional income she has from the EITC benefits, encourages her to work fewer hours.

The 2020 CTC policy adds work incentives on top of those caused by the EITC. Unlike the EITC, the 2020 CTC was not refundable from the first dollar but was only refundable for earnings over $2,500 per year. CTC benefits phase in more slowly at a rate of 15 percent, compared with EITC’s 40 percent rate. After a brief plateau, they continue to phase in, offsetting some portion of the phase-out range for EITC benefits before reaching the maximum benefits for two children of $4,000. For married parents, these benefits remain at the $4,000 level before starting to be phased out when joint earnings reach $400,000. The upward-sloping portion of the 2020 CTC policy would induce substitution effects that would tend to increase hours worked, while the flat portions are associated only with an income effect. An income effect is predicted to decrease hours worked but is generally expected to have a smaller distortionary effect than a policy with a substitution effect. The pro-employment incentive effects of the 2020 CTC are eliminated in the 2021 CTC, which would be expected to reduce employment (though the pro-employment impacts of the EITC remain).

Figure 1. CTC and EITC for Married Parents with Two Children (Ages 6–16)



NOTE: Data from Bastian (2024).

The 2021 CTC policy was fully refundable and set at a higher level than the 2020 CTC policy. It remained at this higher level until a married couple with two children earned more than $160,000 per year; it then phased down to the 2020 CTC level. Compared to no policy, the 2021

CTC includes only an income effect in the lower-income range displayed in Figure 1. Compared to the 2020 CTC, however, it eliminates phase-in ranges with substitution effects that provide work incentives. Economic theory predicts that the 2021 CTC will reduce labor supply: it creates both a negative substitution effect (by lowering the marginal return to work over that income range relative to the 2020 CTC) and a negative income effect (by increasing income and therefore increasing the demand for leisure).

Theory can provide insight into the direction of labor supply response, but it alone does not provide strong guidance about the magnitudes of expected changes in labor supply. Below we evaluate a range of studies that attempt to measure the impacts of the 2021 CTC policy and the advance CTC payments on employment outcomes.

Real-Time Evidence

The monthly, refundable advance CTC payments were disbursed during a period when the labor market was recovering from the pandemic shock and other pandemic-era economic supports were undergoing changes.

In July 2021, the national unemployment rate was 5.4 percent, down a percentage point from January and on a path of steady decline to 3.9 percent by December. The employment-to-population ratio among those ages 25 to 54 was also improving, increasing from its pandemic low of 69.6 percent in April 2020 to 77.8 percent in July 2021 and further climbing to 79.2 percent by December.

Unemployment Insurance (UI) had been made more generous through policy reforms that boosted states' UI payments by $300 per week and extended UI coverage to workers who were not typically covered by the program. While these reforms were authorized from March through

September 2021, 18 states opted to terminate them early. Holzer, Hubbard, and Strain (2023) find that in July and August, unemployment declined and employment-to-population ratios increased in the states that terminated benefits early compared to those that did not.

Although the labor market was strengthening and normalizing over this period, the pandemic was still a factor. COVID-19 hospitalizations and deaths were on the upswing (CDC, n.d.). And there was great uncertainty in the fall of 2021 about whether schools and child care centers would open, which would create disproportionately large labor market impacts among parents.

Despite all that was happening, the potential impacts of the monthly, refundable advance CTC can be (for the most part) teased out separately from these other factors. Nonetheless, it is important to interpret the real-time evidence in the context of a rapidly improving labor market with drops in UI generosity occurring around the same time.

A major challenge in evaluating the employment effects of the CTC policy change is a methodological one: how do we properly define the “treatment” and “control” groups in the natural experiment that occurred? For the treatment group, the design of the policy—the credit was available only to households with children under the age of 18 and income below $200,000 for single filers and $400,000 for married filers—implies a clear definition.

Definition of a control group is more challenging. Of course, childless adults did not receive the expanded CTC, so they were not treated by the policy. It is not clear, however, to what extent their employment trends serve as a valid counterfactual to those of parents, even during periods of time outside of a pandemic. Furthermore, even if employment trends evolved similarly for households with and without children prior to 2021, both labor force attachment and the experience of living through the pandemic were very different for those groups. Many of the studies we describe below compare outcomes between adults with and without children, before and after the CTC expansion, to measure impacts of the policy. If the employment trends between adults with and without children diverge for reasons other than the CTC expansion, the resulting estimates will be biased.

In our earlier work, we find that the estimated impact of the EITC is sensitive to considerations of sample selection criteria, inclusion of specific control variables, and how variables are measured (Schanzenbach and Strain 2021). The expanded CTC differs from the EITC in some important ways—most notably, the EITC is only available to those with earnings, but the expanded CTC was made fully refundable and thus available to those with no earnings. But the policies also share many features in common, including their relative generosity to low-income families with children. As a result, it is important for research studies to probe the sensitivity of their findings to reasonable alternative approaches.

Below we describe a range of studies investigating the contemporaneous impacts of the advance CTC on labor market outcomes, using a variety of data sources and research designs. With one notable exception, the studies generally find no impact of the CTC policy changes on employment or hours worked. We organize our discussion of these studies by data source.

Evidence from the Current Population Survey

The monthly Current Population Survey (CPS) is the source of the official unemployment rate, and several studies use this data set to study the effects of the CTC policy changes. Although the survey response rate fell and there were unusual patterns in reporting of employment in the immediate wake of the March 2020 shutdowns (Rothbaum and Bee 2021), those data irregularities were short-term and had been resolved by 2021.

Intention-to-treat estimates. Most studies estimate some version of an intention-to-treat (ITT) impact of the advance CTC, with treatment generally defined as whether the respondent was eligible for advance CTC payments (i.e., they had at least one child aged 17 or younger, and the period of observation was between July and December 2021). The estimated ITT coefficient is interpreted to be the impact of the advance CTC payments on the outcome of interest, compared with the control group in the sample and conditional on the included covariates.

In a paper primarily exploring impacts on monthly poverty rates, Han, Meyer, and Sullivan (2022) investigate changes in aggregate employment levels among adults aged 18 to 54 with and without children, separately by education level, from January 2020 through June 2022. Using a sample of adults with a high school diploma or less, they find a sharp drop in relative employment between parents and childless adults that starts in May 2021, reaches a trough in September 2021, and then rebounds through April 2022. Among those with higher levels of education, there is a smaller dip in relative employment between parents and childless adults that occurs between July and October 2021. In the text, comparing parents to childless adults, they describe difference-in-differences results and these differences across groups with higher versus lower levels of education. They report relative employment declines in September and October that are statistically significant and reflect a relative employment decline of 2 percentage points among parents with a high school diploma or less, or about 400,000 fewer workers employed. They do not control for any other labor market factors (such as differences across states or individual-level characteristics, such as sex or marital status).

Also using the monthly CPS on a sample of adults aged 18 to 65, Ananat et al. (2022) explore employment effects of the advance CTC payments and find consistent null effects. In their primary analysis, they restrict the data to the period surrounding the policy change, April to

December 2021, and omit the month of July. They control for individual-level characteristics that influence employment outcomes (including age, level of education, and sex) and, by including state and fixed effects, account for other labor market factors in their models.

Their “binary treatment” model, an ITT approach, finds that adults in households with children are no less likely to be employed during the period of advance CTC payments.[1] The point estimate on the treatment effect suggests a statistically insignificant 0.2 percentage point decline. They also explore labor force participation—including those who are employed plus those who are unemployed but looking for work—as an outcome, with similarly small and statistically insignificant findings.

They add innovations to this baseline model, including whether the treatment effect differs across household income, which may be expected because the 2021 CTC increase is larger (both in levels and as a share of pre-transfer income) for the lowest-income households due to the simultaneous effects of the increased benefit levels and the full refundability. They find consistently insignificant impacts on employment across income levels with point estimates ranging from approximately 0 to -1 percentage point. They also show that their results are robust to defining the timing of the CTC treatment to when the expansion was passed in March instead of when the monthly payments began in July (and expanding their study period to January through December 2021).

Pac and Berger (2024) take a similar approach, using monthly CPS data among adults aged 18 to 64 and extending the observation period to include January 2018 through December 2021. Because their study identifies a specific caregiver of the child or children, they are able also to explore impacts on caregivers of children living in households without either parent.

[1] They also explore whether impacts vary across months and find that point estimates are remarkably stable over time for both labor force participation and employment.

(There are approximately 50,000 such children in their data.) They find small, statistically insignificant negative effects on employment among caregivers compared to childless adults in 2021. The results are nearly identical when they expand the analysis to include 2018 and 2019 (they always omit 2020), which helps them estimate seasonal patterns of employment. On the other hand, when they restrict the comparison only to the second half of the year and examine 2018, 2019, and 2021, they find a statistically significant 0.6 percentage point decline in employment among caregivers relative to childless adults, or nearly 500,000 caregivers. As they note, by omitting observations from January to June 2021, this approach does not allow the impacts of the pandemic to differentially affect employment among caregivers. They argue that this likely leads to upwardly biased estimates.

They conduct a series of tests for heterogeneous treatment effects. For example, they separately investigate impacts on caregivers to 17-year-olds because the 2021 CTC expansion extended eligibility to their children (previously children aged out after age 16). They also separately investigate impacts on caregivers of infants, who both may be more marginally attached to employment and also received the higher advance CTC to children aged zero to five. For both types of caregivers, across all specifications, the estimated effects of the CTC policy change are small and statistically insignificant.[2]

They further present estimates for these samples (overall, for caregivers of infants, for caregivers of 17-year-olds, and among caregivers only comparing those with two or more versus one child) separately by sex and marital status. The prior literature suggests that the impacts are likely to be largest among unmarried females and larger among females than males. Pac and

[2] They also find small, inconsistently signed estimates on hours worked, and some positive estimates of being "on leave" from work among caregivers with an infant (estimated coefficient of 2.4 percentage points on a base employment rate of 23.3 percent).

Berger find small and statistically insignificant impacts across all adults, as well as among caregivers of infants and 17-year-olds, with a few notable exceptions: the estimates are positive—indicating higher employment rates among caregivers during the period of advance CTC payments—and statistically significant among the married male sample restricted to infant caregivers and the single female sample restricted to caregivers to 17-year-olds. We suggest caution in interpreting the estimates that show employment increasing, as it is not clear on theoretical grounds why single females (married men) should respond differently to the advance CTC if they are caring for 17-year-olds (infants) relative to children of all ages. Nonetheless, their findings do not provide evidence that employment declines in response to the advance CTC payments.

Next, leveraging the facts that advance CTC payments were larger for children aged zero to five, and parents' (especially mothers') employment decisions may be more elastic when children are younger than school-age because of the costs of child care, Pac and Berger examine policy effects separately by number (one, two, three or more) interacted with age (0–5, 6–17) of children. The results are generally small and statistically insignificant, with some estimates (implausibly) positive and significant. Consistent with theory, results for single female caregivers with one child aged zero to five indicate a significant, negative 2.7 percentage point impact on employment; estimates for those with two and three or more children aged zero to five are similarly negative but not statistically significant. On the other hand, results for married female caregivers with one, two, or three or more children aged zero to five are positive and, in the case of two children, statistically significant. Overall, these results suggest no substantial real-time decline in employment during the period in which advance CTC payments were made.

Continuous treatment. The binary treatment approach ignores potentially useful variation in the magnitude of the increase in advance CTC payments, which may influence employment effects. As described above, per-child benefits increased for most families, from $2,000 to $3,000 on an annual basis. For children aged zero to five, per-child benefits increased more, to $3,600 annually. Benefits were also extended to 17-year-old children, raising their benefits from zero to $3,000. Of course, the number of children in the household will also determine the total CTC received and its increase during 2021. Furthermore, since prior to 2021 the CTC was only partially refundable, the increase in the value of payments due to full refundability in 2021 was thus larger in dollar terms for families that did not previously receive the full CTC amount. As a share of baseline income, the effect would be larger still (because a larger benefit increase is divided by a lower income level).

While it is useful to account for this variation in estimating the impacts of the CTC policy changes, the monthly CPS data do not provide enough information to directly measure the change at the individual household level. Ananat et al. (2022) circumvent this problem by using data from the pre-pandemic 2019 CPS Annual Social and Economic Supplement (ASEC), which collects comprehensive information on income, to predict expanded CTC benefit levels using program parameters and information on family income, the number and ages of children, and other relevant characteristics. They directly observe pre-reform CTC benefits in the ASEC and take the difference between predicted 2021 benefits and observed pre-reform benefits to calculate the expected net CTC benefit per family in 2021. They then take the mean of this net CTC calculation at the level at which they can merge the information back to the monthly CPS: income bracket by number of children by number of adults. The resulting measure is the 2021 increase in net CTC benefits in (hundreds of) dollars. In some specifications, they take the ratio of the net CTC increase to the baseline income (measured as earnings plus the baseline CTC payments) to estimate the percentage change in relative wage.

The Ananat et al. (2022) continuous treatment estimates consistently find no impacts on employment or labor force participation, as measured by the interaction of the net change in CTC payments in 2021 interacted with the indicator variable for August to December, or in any of the robustness checks that are analogous to those described above for the binary treatment indicator.

Enriquez, Jones, and Tedeschi (2023) take a similar approach to estimating continuous CTC payment levels but use updated 2022 CPS ASEC data (which refer to 2021 income) to estimate payments in 2021. They construct percentile ranks based on households' CTC-to-income ratios and interact those with an indicator variable for data collected from August to December to construct continuous difference-in-differences estimates. They augment these to estimate triple-differences by including either: (1) pre-pandemic observations from 2019, or (2) post-policy observations from 2022. They find small and consistently insignificant estimates of the impact of the advance CTC payments in 2021 on labor force participation or hours worked across a range of samples, including subsets divided by gender, educational attainment (college degree, less than a college degree), number of children (one, two, or more), and racial group (white, not white). They also estimate the impacts of the termination of the advance CTC payments, finding no impact on labor force participation when the treatment period is compared to a control period either before payments began or after they ended.

Evidence from other data sources

Several additional data sources have been used to estimate impacts of the advance CTC payments on labor market outcomes, and they generally find small (and significantly insignificant) impacts of the policy change.

Ananat et al. (2022) also use data from the Census Bureau's Household Pulse Survey to estimate employment effects. The Pulse data come from an online survey launched in April 2020 to collect real-time information on social and economic outcomes during the COVID-19 pandemic. There are several drawbacks relative to the CPS data that make the Pulse less preferable to measure employment outcomes. The primary employment question is less comprehensive than the CPS measure, and the survey is regularly redesigned and the position of the questions about employment are reordered within it, inducing changes in rates of non-response to those questions. On the other hand, respondents are asked whether they received advance CTC payments, with only 53 percent of those with children reporting that they did in the final survey before the expanded credit's expiration. The addition of this variable allows researchers to estimate a treatment-on-treated impact, using eligibility as an instrumental variable to predict receipt. In both the reduced-form ITT and the instrumented treatment-on-treated approaches, they find small, insignificant, positive-signed estimates on employment.[3]

Evidence from the Urban Institute's Well-Being and Basic Needs Survey (WBNS), collected in 2020 and 2021, is provided in Karpman et al. (2022). The WBNS is a nationally representative panel survey, meaning it follows the same adults over time and therefore can identify changes in individuals' behavior after the receipt of CTC payments. The researchers compare those who do and do not receive CTC payments, regardless of CTC eligibility. In other

[3] Roll, Hamilton, and Chun (2021) also find no impact of advance CTC payments on employment in the Pulse data in the three survey waves (collected July 21–August 16, August 18–September 13, and September 15–October 11) after the payments began.

words, the nonrecipient group includes both childless adults and those with children who did not receive benefits for some reason (e.g., they did not file a tax return). They find small, insignificant increases in both employment rates and the share of workers employed full time between December 2020 and December 2021 for both CTC recipients and nonrecipients, indicating no evidence of a decline in employment in response to the enhanced CTC payments.

Lourie et al. (2022) use proprietary bank and credit card transaction data to estimate the impact of the monthly CTC payments. Their rich data allow them to separate families that receive an increase in benefits in 2021 from those who receive the same total payment amount but have their payment timing shifted to monthly payments. They compare CTC recipients (as identified by receipt of payments from the IRS) to nonrecipients, before and after advance CTC payments began, conditional on both individual (user) and state-by-month fixed effects. They find small, insignificant, positive impacts on the overall likelihood of employment and small, positive impacts on overall log wages. When they separate impacts by whether the family experienced a CTC benefit increase or just a shift in the timing of payments, they find (puzzling) small, statistically significant, positive employment and wage impacts among those for whom the timing shifted and small, insignificant impacts among those with benefit increases. As before, findings of unexpected, positive impacts may reflect a problem with the study design; nonetheless, there is no evidence that there is a meaningful decline in short-run employment.

Finally, Pilkauskas et al. (2022) use data from a monthly survey conducted by a free mobile app designed to help families track and manage their Supplemental Nutrition Assistance Program (SNAP) benefits. Each month, users are invited to participate in a survey on their financial stability. They restrict their sample to households with children and predict monthly CTC benefits based on the number and ages of children in the household, estimating an ITT model using variation in the timing of the monthly CTC payments (pre- and post-implementation) and in benefit size. Because only two-thirds of the respondents with children in their sample report receiving CTC payments, they also estimate treatment-on-the-treated effects, using predicted monthly CTC benefit to instrument for reported monthly CTC benefits. They do not find evidence that the CTC expansion reduced employment, though they find some suggestive evidence that the expansion may have led to some shift from full- to part-time employment. Generalizing these findings to the broader population of households with children requires caution, though, as labor force attachment appears to be qualitatively different in their sample than in the broader low-income population. Thirty-one percent of their sample is unemployed and 40 percent is employed. Among those employed, roughly half work part time.

Additional Analyses of Employment Effects of the 2021 CTC Expansion

In Tables A1-A3 in the appendix, we compare estimates of the advance CTC on employment across a variety of samples to test the sensitivity of the results to investigating impacts on women only, on taking various approaches to defining low-income samples, on using different sample age restrictions, and on taking different approaches to controlling for the state employment context. We find that the results are somewhat sensitive to changes in sample restrictions but not very sensitive to changes in control variables.

In Table 1, we test for differential impacts on parents of young children, in a monthly CPS sample from April to December 2021 (dropping July). Recall that the CTC expansion was larger for young children ($3,600 versus $3,000 for older children). Further, mothers' employment rates tend to be lower when they have young children (Black, Schanzenbach, and Breitwieser 2017), and there is some evidence that their employment is more income-elastic compared with mothers of older children (Meyer and Rosenbaum 2001). As a result, we may expect to see a different impact of the CTC expansion on mothers with young children than on those with older children. We test this by including a separate indicator for mothers with children ages zero to five in the household, and that indicator interacted with the presence of the CTC monthly payments. Since, by definition, a mother with a young child also has a child age zero to 17 in the household, the total CTC effect requires summing both treatment measures. We test the joint significance of the two treatment estimates and report the test's p-value.

As shown in column 1, among the overall sample, parents were 0.8 percentage points less likely, and parents of young children were 0.5 percentage points less likely, to be employed while advance CTC payments were being made. While neither estimate is individually significant, the joint treatment effect (representing the total effect for parents of young children) is almost statistically significant at conventional levels, with a p-value of 0.13.

Table 1. Employment Effects of CTC Payments: Differential Impacts for Parents of Young Children

	(1) All	(2) Females only	(3) Some college or less & female	(4) Some colleges & female & unmarried
HH w/ kids <=17	0.035***	0.002	0.012	0.036***
	(0.004)	(0.006)	(0.008)	(0.013)
HH w/ kids <=17 X Post July 2021	-0.008	-0.007	-0.002	0.021
	(0.005)	(0.007)	(0.010)	(0.013)
HH w/ kids <=5	-0.050***	-0.098***	-0.132***	-0.102***
	(0.006)	(0.008)	(0.010)	(0.018)
HH w/ kids <=5 X Post July 2021	-0.005	-0.009	-0.008	-0.045**
	(0.007)	(0.013)	(0.014)	(0.018)
Joint treatment test p-value	0.125	0.259	0.765	0.030
Pretreatment Mean HH w/ children weighted	0.769	0.672	0.597	0.643
Observations	318,293	162,680	94,978	53,160

NOTES: Monthly CPS data, April–December 2021 used; July data dropped. Standard errors in parentheses, clustered by state. Regressions are weighted by final CPS weight. Dependent variable is equal to one if the individual is employed (whether at work or not) the prior week. Ages 20–50 included. All columns include month and state fixed effects and controls (as appropriate) for age, education, female, race and ethnicity groups, and whether the respondent is unmarried. Joint test of treatment reports the *p*-value of a joint test of whether the sum of HH w/ kids <= 17* after July and HH w/ kids <= 5* after July is equal to zero. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Including both males and females in the same employment regression could provide misleading results. For example, if the CTC treatment has a small negative impact on females and no impact on males, the estimated coefficient would be a combination of both impacts. We estimate the impacts on a sample of females only in column 2. Note that the pre-treatment mean among households with children drops to 67 percent, reflecting lower employment rates among women than in the pooled sample. While the treatment effects overall and for mothers with young children are negative, they are neither independently nor jointly statistically significant.

Column 3 repeats the analysis for a sample of women with low levels of education, who are more likely to have incomes in the ranges impacted by the changes in incentives described above. Among women with some college or less, the estimated treatment effects are both independently and jointly small and negative and not statistically significant.

Column 4 further restricts the sample to women with low levels of education who are also unmarried. The treatment effect is negative and statistically significant and implies a 4.5 percentage-point reduction in employment during the advance CTC payments.

We interpret these new findings as additional evidence that there is little to no detectable short-term negative impact of the advance CTC payments on employment broadly defined in August to December 2021. At the same time, among the demographic groups most likely to be affected by the advance CTC, we do see some evidence of employment reductions.

Discussion

The 2021 CTC expansion increased benefits, especially to the lowest-income families. Under the prior law, CTC benefits were phased in over a range of low incomes, but because the 2021 CTC eliminated this feature, economic theory predicts that the 2021 reforms would reduce labor supply. Economic theory is relatively silent, though, about the magnitude of this expected reduction. The empirical literature suggests that employment, broadly defined, was not reduced by the 2021 CTC changes. However, we present evidence that employment was reduced among unmarried mothers with relatively low levels of education—the demographic group that was most affected by the CTC expansion.

There are four reasons why the results from the 2021 experiment may differ from any future reform. First, the 2021 expansion was temporary. Second, it was widely reported in the press that it would need to be extended by Congress, adding to the sense that it could permanently expire at the end of 2021. Third, it occurred in the context of a (fading) pandemic, with risks to individual health at the top of mind and for which extraordinary fiscal policy support was enacted that led to a large increase in household savings. Finally, it occurred during a period characterized by uncertainty about the consistency with which school and child care centers would be open. The first two factors could have mitigated the degree to which parents and caregivers responded to the policy change. The second two factors likely affected the degree to which parents and caregivers were attached to the labor force.

Even abstracting from the pandemic-era environment of 2021, the existing evidence captures the effects of the temporary 2021 CTC expansion, while the effects of a permanent CTC expansion may be different and may grow over time. There is evidence that employment impacts of EITC expansions cumulate over time. Hoynes and Patel (2015) find that the employment effects of the 1993 EITC expansion continue to grow for five years after the policy change (and for three years after the expansion was fully phased in). Similarly, Schanzenbach and Strain (2021) find that employment effects grow for two years across all pooled EITC expansions, and Miller et al. (2018) find that employment does not respond to an expanded EITC for single adults until the second year of treatment.

The analogy between the CTC and EITC is limited, however, because, of course, the EITC only provides benefits to workers. The most directly comparable longer-term evidence to the CTC comes from the Baby's First Years (BFY) study. In BFY, mothers of newborns were randomly assigned to receive an unconditional cash gift of either $20 per month, or $333 per month (about $4,000 per year). Sauval et al. (2022) find small and statistically insignificant impacts on mothers' employment during the first three years of her child's life due to receipt of the high cash gift. There is some evidence that mothers receiving the high cash gift were less likely to work full time, and worked fewer hours, during one pandemic year, but the findings are insignificant in the pooled data across the full three years.

Because the 2021 CTC expired shortly after it was enacted, the employment effects of a permanently expanded 2021 CTC can only be predicted using simulation methods, which themselves can be quite sensitive to assumptions. For example, simulation methods have predicted that the range of employment reductions could span from 367,000 (Bastian 2024) to 1.46 million (Corinth et al. 2021) in response to a permanent CTC expansion with the 2021 credit's parameters. The difference in the predictions stems primarily from different assumptions about whether households with relatively high incomes would have their employment status affected by this policy change and the degree to which certain demographic groups (e.g., married men) would be affected.

The 2021 CTC itself was relatively straightforward, providing a constant benefit per child across a wide range of family incomes. Because of the way it changed the CTC, economic theory predicted it would reduce the labor supply of the groups affected by the change. But the magnitude of that effect was an open empirical question. Researchers did an admirable job estimating the actual labor market effects, rapidly producing a range of studies that could help policymakers assess the impacts of the temporary policy change. Because there are often several reasonable approaches to take when analyzing a policy, it is crucial for researchers to probe their estimates for sensitivity and robustness so that we can assess the strength of the evidence.

This article suggests that for the 2021 CTC expansion, theory and evidence were in the strongest alignment when the research design that produced the evidence was most focused on the demographic groups most likely to be affected by the expansion. We argue that this lesson applies to policy analysis more broadly.

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Appendix

Table A1, below, shows how the estimated impact of advance CTC payments on employment varies by sample definition and inclusion of various controls. All samples include monthly CPS observations from April to December 2021, dropping observations from July. Standard errors are clustered by state.

The basic regression estimated in column 1 is as follows:

(1) $emp_{ist} = \alpha + \beta_1 PostJuly_t + \beta_2 CTCElig_i + \beta_3 AdvanceCTC_{it} + \varepsilon_{ist}$

where *emp* represents whether individual *i* living in state *s* in month *t* is employed (whether at work or not). *PostJuly* is an indicator for whether the month of observation falls between August and December, *CTCElig* is an indicator for whether individual *i* lives in a household with a child age 17 or younger, and *AdvanceCTC* is the interaction of *PostJuly* and *CTCElig* and represents the treatment. β_3 is the coefficient of interest and can be interpreted as the impact of the advance CTC payments on an individual's employment status.

Equation 1 does not account for variation in macroeconomic conditions—or other differences in individual characteristics that may influence employment decisions—and may vary between those living with children and not living with children. These may be correlated with advance CTC payments, and failing to account for them would lead to omitted variables bias. Even if they are uncorrelated with advance CTC payments, controlling for these characteristics may improve the model fit and allow us to obtain more precise estimates of β_3. In columns 2 and 3, we augment the regression as follows:

(2) $emp_{ist} = \beta_1 PostJuly_t + \beta_2 CTCElig_i + \beta_3 AdvanceCTC_{it} + X_{ist}\gamma + \alpha_s + \lambda_t + \varepsilon_{ist}$

where α_s is a vector of time-invariant state fixed effects, λ_t is a vector of month fixed effects, and X_{ist} is a vector of individual-level characteristics, including the following indicator variables: education level of high school or less, education level of some college, female, unmarried, individual ages in years, and racial and ethnic group indicators. Column 2 omits the individual characteristics, and column 3 includes them.

We can more flexibly control for macroeconomic conditions by controlling for state-by-month fixed effects, as shown in equation 3 below:

(3) $emp_{ist} = \beta_1 PostJuly_t + \beta_2 CTCElig_i + \beta_3 AdvanceCTC_{it} + X_{ist}\gamma + \mu_{st} + \varepsilon_{ist}.$

When state-by-month effects are controlled, β_3 is identified by comparing within-state employment differences (conditional on control variables) between those living with and without children from August to December 2021.

Table A1 reports results for adults ages 18 to 65. As shown in column 1 of the first row, among the overall sample, 77 percent of adults in households with children were employed prior to July. Including no controls (as in equation 1, shown in column 2), the treatment effect is estimated to be negative 0.2 percentage points and the estimate is not statistically significant. Adding state and month fixed effects (column 3), individual controls (column 4), or state-by-month fixed effects without (column 5) or with (column 6) individual controls does little to change the magnitude of this estimate, and no estimate is statistically significant.

Row 2 limits the sample to individuals with education levels of some college or less. Among this sample, 71 percent of adults in households with children were employed prior to July. Across the range of controls, the point estimates on the advance CTC treatments range from 0.1 to 0.2 percentage points, and none of the coefficients is statistically significant.

Including both males and females in the same employment regression could provide misleading results if, for example, the advance CTC treatment has a small negative impact on females and no impact on males, making the resulting estimated coefficient an attenuated combination of both effects. We test the sensitivity of the results to estimating the impacts on samples of females only in the subsequent rows. Row 4 is a sample of all females, regardless of education. The pre-treatment employment mean among households with children drops to 67 percent, reflecting lower employment rates among women than in the pooled sample. The estimated advance CTC treatment effect ranges from 0.1 to 0.2 percentage points and the coefficients are not statistically significant.

The next row limits the sample to females with some college or less. These estimates are positive (between 0.1 and 0.9 percentage points) and not statistically significant.

The next two rows examine first all unmarried females, then unmarried females with some college or less education. Here the estimates are consistently positive and not statistically significant (ranging from 0.6 to 1.0 percentage points).

The final two rows examine married females overall and among the subset with some college or less education. The estimates are all statistically insignificant and positive, with point estimates ranging from 0.3 to 1.4 percentage points.

Table A2 repeats the exercise from Table A1, limiting the sample to those aged 18 to 55. Compared to Table A1, the estimates are more likely to be negative, and the positive estimates are smaller. In the overall sample (row 1), the estimated employment effect is negative but not statistically significant with a magnitude of 0.6 to 0.8 percentage points. No other estimated treatment effects for any subgroup is statistically significant.

Table A3 repeats the exercise for a sample aged 20 to 50. In this case, the estimated treatment effects are uniformly negative (or zero), but generally remain imprecisely estimated. In the overall sample, presented in the first row, the advance CTC payments are estimated to reduce employment by a statistically significant 1.1 to 1.2 percentage points.

The estimates are negative and generally significant among the overall sample of females (row 3), ranging from a 1.1 to 1.3 percentage point reduction in employment among women with children while advance CTC payments were being made. When the sample is limited to females with some college or less, the estimates are (surprisingly) smaller but (not surprisingly) less precisely estimated. Among the unmarried female samples, overall and for the samples with low education levels, the estimates are generally small and imprecise. On the other hand, among the married female sample, the estimates overall are negative and occasionally statistically significant.

Overall, the patterns in Tables A1–A3 indicate that advance CTC payment effects are not particularly sensitive to the controls included in the model, as demonstrated by relatively small variability across columns. The results are more sensitive to the age range included in the sample. To provide context into what age ranges are most appropriate to include, Figure A1 shows the distribution of adults overall and women living with children in the CPS sample used in the employment analysis. The share of adults living with children drops off substantially as adults age. For example, only 4 percent of women aged 56 to 65, and 6.3 percent of the overall sample in that age range, live with children aged 17 or younger. Less than 1 percent of the sample (overall or among women) aged 56 to 65 lives with a child aged five or younger. As a result, we argue that including 56- to 65-year-olds in the employment regression is overly broad.

Table A1. Employment Effects of CTC Payments, Ages 18–65: Alternate Sample Selections

	Mean (pre-treat w/ kids) [total N]	No controls	State & month FEs	Ind control, State & month FEs	State-by-month FEs	Ind control, State-by-month FEs
	(1)	(2)	(3)	(4)	(5)	(6)
Overall	0.770	-0.002	-0.002	-0.002	-0.003	-0.002
	[504,364]	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)
Some college or less	0.712	0.001	0.001	0.002	0.002	0.002
	[325,893]	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)
Females only	0.673	0.001	0.001	0.002	0.001	0.002
	[258,491]	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Females, some college or less	0.594	0.009	0.009	0.010	0.009	0.010
	[159,768]	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Unmaried females	0.685	0.007	0.006	0.006	0.006	0.007
	[121,679]	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Unmarried females, some college or less	0.639	0.008	0.008	0.010	0.009	0.010
	[84,952]	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Married females	0.668	0.003	0.003	0.004	0.003	0.004
	[136,812]	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Married females, some college or less	0.562	0.013	0.013	0.013	0.013	0.014
	[74,816]	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)

NOTES: Monthly CPS data, April–December 2021 used; July data dropped. Standard errors in parentheses, clustered by state. Regressions are weighted by final CPS weight. Each cell represents a separate regression, and the reported coefficient is the CTC intention-to-treat (the interaction between the indicator variable of families with children and after July 2021). Column 1 reports the April–June mean employment rate among individuals in households with children for the sample denoted in the row. The number in brackets below the mean employment rate is the regression sample size in columns 2–6. Column 2 includes an indicator for observations after July, families with children, and the interaction of the two, with no additional controls. Column 3 adds state and month fixed effects. Column 4 includes state and month fixed effects, and individual controls (as appropriate) for age, education, female, race and ethnicity groups and whether the respondent is unmarried. Column 5 includes state-by-month fixed effects but no individual controls. Column 6 includes state-by-month fixed effects and individual controls. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A2. Employment Effects of CTC Payments, Ages 18–55: Alternate Sample Selections

	Mean (pre-treat w/ kids) [total N]	No controls	State & month FEs	Ind control, State & month FEs	State-by-month FEs	Ind control, State-by-month FEs
	(1)	(2)	(3)	(4)	(5)	(6)
Overall	0.772	-0.008	-0.008	-0.006	-0.008	-0.006
	[389,051]	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Some college or less	0.715	-0.006	-0.005	-0.003	-0.005	-0.002
	[248,786]	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Females only	0.675	-0.006	-0.006	-0.004	-0.006	-0.004
	[198,400]	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Females, some college or less	0.597	0.001	0.001	0.003	0.000	0.003
	[120,058]	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Unmaried females	0.687	-0.000	-0.000	0.000	-0.001	0.000
	[98,624]	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Unmarried females, some college or less	0.641	0.002	0.002	0.004	0.002	0.004
	[68,516]	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Married females	0.670	-0.005	-0.005	-0.002	-0.005	-0.002
	[99,776]	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)
Married females, some college or less	0.566	0.002	0.003	0.006	0.003	0.006
	[51,542]	(0.011)	(0.011)	(0.010)	(0.011)	(0.010)

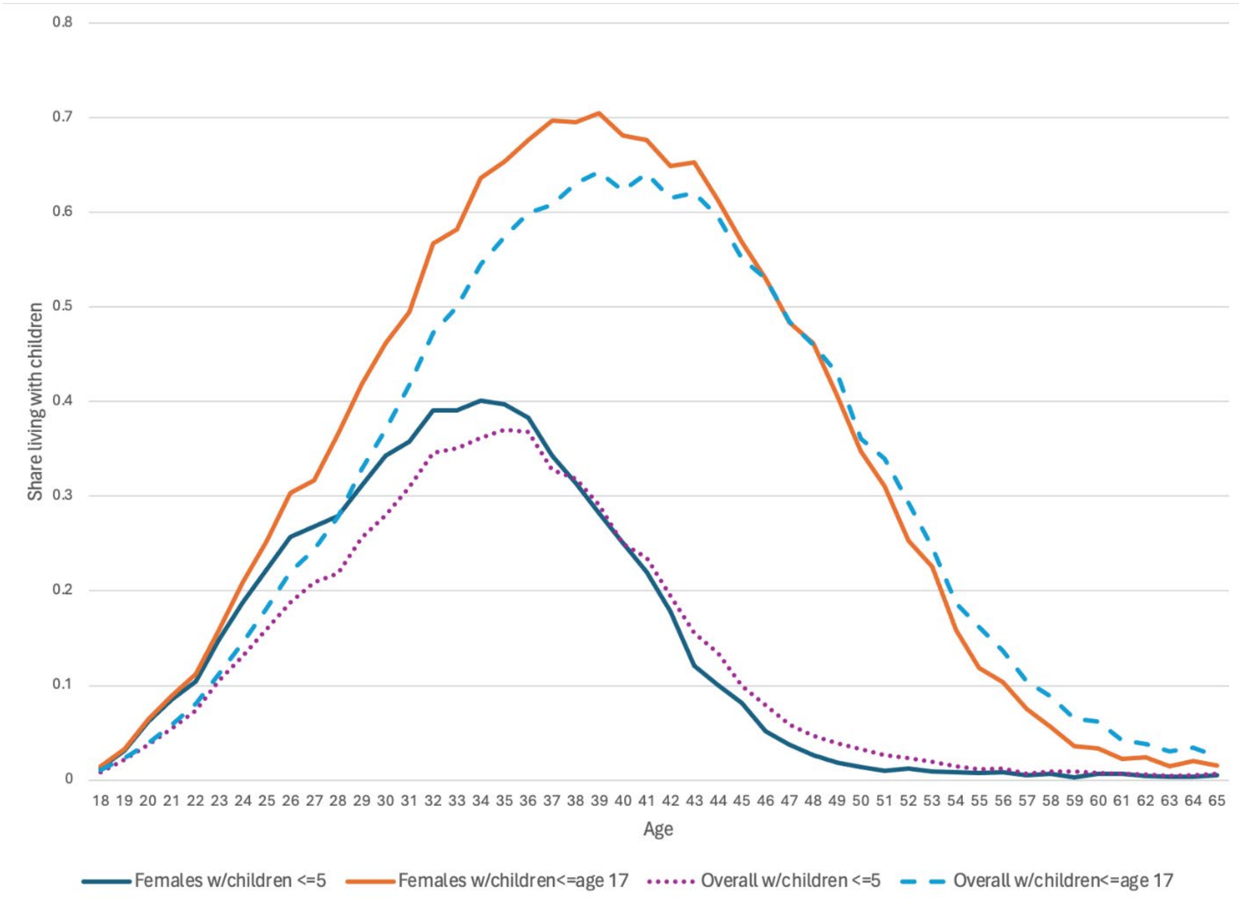
NOTES: See notes to Table A1.

Table A3. Employment Effects of CTC Payments, Ages 18–55: Alternate Sample Selections

	Mean (pre-treat w/ kids) [total N]	No controls	State & month FEs	Ind control, State & month FEs	State-by-month FEs	Ind control, State-by-month FEs
	(1)	(2)	(3)	(4)	(5)	(6)
Overall	0.769	-0.012**	-0.012**	-0.011**	-0.012**	-0.011*
	[318,293]	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Some college or less	0.714	-0.011	-0.011	-0.009	-0.011	-0.009
	[197,783]	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Females only	0.672	-0.013*	-0.012*	-0.012*	-0.012*	-0.011
	[162,680]	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Females, some college or less	0.597	-0.008	-0.008	-0.006	-0.008	-0.006
	[94,978]	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Unmaried females	0.686	-0.002	-0.002	-0.004	-0.002	-0.003
	[80,164]	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)
Unmarried females, some college or less	0.643	-0.004	-0.004	-0.002	-0.003	-0.002
	[53,160]	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Married females	0.666	-0.017*	-0.017*	-0.013	-0.016	-0.013
	[82,516]	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)
Married females, some college or less	0.565	-0.011	-0.009	-0.006	-0.008	-0.004
	[41,818]	(0.014)	(0.014)	(0.012)	(0.014)	(0.013)

NOTES: See notes to Table A1.

Figure A1. Share of Adults Living with Children, by Age



NOTES: Monthly CPS data, April–June 2021 used; means are weighted by final CPS weight. The *x*-axis represents the adult's age in years, and the *y*-axis is the share of adults living with children. Four groups are displayed: adults living with children aged 17 or younger, adults living with children aged 6 or younger, women living with children aged 17 or younger, and women living with children aged 5 or younger.