NBER WORKING PAPER SERIES

REAL-TIME POVERTY, MATERIAL WELL-BEING, AND THE CHILD TAX CREDIT

Jeehoon Han
Bruce D. Meyer
James X. Sullivan

Working Paper 30371
http://www.nber.org/papers/w30371

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2022

This paper was prepared for the December 2022 National Tax Journal Forum. We would like to thank Laura Kawano, an anonymous referee, Raj Chetty, Kevin Corinth, John Friedman, Ilina Logani, Angela Rachidi, Mandana Vakil, and Scott Winship for comments. We also thank the NSF for financial support for an earlier, related project on poverty measurement, and the Russell Sage Foundation, Alfred P. Sloan Foundation, Charles Koch Foundation, and the Menard Family Foundation for their support of the Comprehensive Income Dataset Project. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Real-Time Poverty, Material Well-Being, and the Child Tax Credit
Jeehoon Han, Bruce D. Meyer, and James X. Sullivan
NBER Working Paper No. 30371
August 2022
JEL No. C42,C81,H2,I32,I38,J20

ABSTRACT

In response to the COVID-19 pandemic two new timely poverty measures have been developed to monitor fast-changing economic conditions for the most deprived. The Han et al. near real-time poverty measure uses responses to a global income question on the Monthly Current Population Survey (CPS) that is available for a subsample of those surveyed. The CPSP monthly poverty measure, widely cited in the media, uses data from the Annual Social and Economic Supplement to the CPS and other sources to impute poverty in the Monthly CPS sample based on demographic and employment variables. This paper evaluates the two measures and their estimates of child poverty around the 2021 temporary changes to the Child Tax Credit (CTC). We argue that conceptually the measure based on responses rather than the one based on imputations is preferable, though both measures suffer from important drawbacks. We also conclude that widely publicized claims that child poverty fell by 25 percent when the Advance CTC payments started and subsequently rose by 41 percent when they ended are based on weak evidence and are overstated. The best evidence, though still imperfect, suggests poverty was relatively stable in 2021 and the first half of 2022. Part of the explanation for the lack of change appears to be a compensating decline in employment among low-skilled workers with children. Other evidence tying changes in well-being to the tax credit is confounded by other policy changes.

Jeehoon Han
School of Economics
Zhejiang University
Yuhangtang Road No.866
Hangzhou, 310058
China
jeehoonhan3@gmail.com

James X. Sullivan
Department of Economics
3108 Jenkins Nanovic Halls
University of Notre Dame
Notre Dame, IN 46556
James.X.Sullivan.197@nd.edu

Bruce D. Meyer
Harris School of Public Policy
University of Chicago
1307 E 60th Street
Chicago, IL 60637
and NBER
bdmeyer@uchicago.edu

As the COVID pandemic has made clear, the resources available to vulnerable households can change suddenly. Over the past few years, we have seen sharp changes in employment as well as dramatic swings in government transfers. Timely information about how these fluctuations affect the economic well-being of U.S. households is critically important for appropriately targeting and calibrating policy responses in a timely fashion. Unfortunately, official statistics, such as the official poverty rate are only updated on an annual basis. Furthermore, that they are made available after a considerable lag renders them of little use to federal, state, and local policymakers faced with making quick policy decisions. Other, more direct measures of economic well-being, such as consumption statistics, are likewise only available after a considerable lag.

In response to these data limitations, recent work has developed income-based poverty measures that can be computed on a near real-time basis. In particular, Han, Meyer and Sullivan (2020) construct a measure of income poverty that can be updated on a monthly basis using data on reported income over the past 12 months from the Monthly Current Population Survey (CPS). This paper has been extended with updated results reported each month at www.povertymeasurement.org. Researchers at the Columbia University Center on Poverty and Social Policy (CPSP) have taken a very different approach. They define a monthly poverty indicator based on imputed monthly income constructed from annual income from a prior year of the CPS Annual Social and Economic Supplement (CPS-ASEC), and then use this indicator to impute the poverty status out-of-sample for observations in the Monthly CPS (Parolin et al. 2022). Updated estimates for the CPSP are provided on a monthly basis at www.povertycenter.columbia.edu/forecasting-monthly-poverty-data. A key distinction between these two indicators, in addition to the methodological differences, is that the Han et al. measure

defines poverty using an annual measure of resources, while the CPSP indicator defines poverty based on a prediction of resources for a single month.

In this paper we discuss the strengths and weaknesses of these different approaches to measuring poverty in near real-time and examine how recent changes in real-time poverty differ between these approaches. We also consider how near real-time poverty estimates can inform our understanding of the effects of government policies on income poverty, focusing on recent changes in the Child Tax Credit (CTC). Unprecedented changes to the Child Tax Credit were implemented on a temporary basis in 2021. These changes eliminated the existing Child Tax Credit that incentivized work and replaced it with a child allowance, available regardless of parental work. Part of this allowance was paid out on a monthly basis during the second half of 2021 under what was called the Advance Child Tax Credit.

We show that the two different approaches to measuring real-time poverty suggest sharply different short-run effects of the policy change on child poverty. Parolin et al. (2022) conclude that child poverty was 25 percent lower in July 2021 as the result of the CTC expansion. The CPSP researchers subsequently claimed that poverty rose by over 40 percent in January after the expiration of the monthly payments.[1] These findings have been frequently cited by policymakers and the press in discussions of extending CTC benefits.[2] The Han et al. measure, in contrast, shows only a small decline in poverty during the period of monthly CTC payments and no rise after the elimination of the payments. This measure is expected to move more slowly given the 12-month reference period for income, but even after considering six

[1] See https://www.povertycenter.columbia.edu/publication/monthly-poverty-july-2021 and https://www.povertycenter.columbia.edu/publication/monthly-poverty-january-2022 . https://www.nytimes.com/2022/05/02/opinion/child-tax-credit.html .

[2] See https://www.newyorker.com/news/q-and-a/the-devastating-effects-of-losing-the-child-tax-credit and https://www.nytimes.com/2022/04/17/opinion/biden-child-tax-credit.html and https://www.nytimes.com/2022/05/02/opinion/child-tax-credit.html .

months of payments followed by six months without payments the near real-time poverty measure does not show much movement. The Han et al. measure does clearly register other pandemic tax credits, specifically the Economic Impact Payments, but shows little effect of the Advance CTC. In addition, we show that the differences in reference periods across measures cannot fully explain the different patterns. Other evidence tying changes in well-being to the tax credit changes is also weak.

One of the key differences between the poverty measures that we emphasize is that the CPSP measure allows a very limited effect of behavioral responses to the substitution of a child tax credit with a child allowance. Using CPS data, we examine changes in employment around the time of the CTC payments. We find a decline in the employment of adults with children (the group expected to be affected by the change in tax credits based on prior research) relative to those without children beginning shortly after the passage of the American Rescue Plan Act in March 2021. This decline is only apparent for those with a high school education or less. There is little change for those with at least some college education. The decline begins to reverse in the last quarter of 2021, and by early 2022, the difference between those with and without children disappears. A second source of employment data from Opportunity Insights shows a decline in low-wage employment during a similar period, but little change in middle and high wage employment. The CPSP calculation also ignores other potential short run behavioral responses, such as changes in private transfers or living arrangements.

1. How is Real-Time Poverty Measured?

In the context of the pandemic and recent policy changes there has been great interest in concurrently tracking how poverty and well-being have changed in response. The difficulty with

timely poverty measurement is the requirement of obtaining quality data produced at a high and regular frequency that is publicly released. Ideally, the data would come from a long-running survey so that historical comparisons could be used to validate the measures. To resolve this difficulty, two groups of researchers have constructed timely poverty measures that rely on the Monthly CPS, which collects information on labor market outcomes and demographic characteristics from a representative sample of about 60,000 households each month. In this section we describe the different approaches taken to measuring near real-time poverty, emphasizing the key differences: the reference period over which income is measured (twelve months versus one month), the income concept (one similar to the official poverty measure versus one similar to the Supplemental Poverty Measure (SPM)), and the use of reported versus predicted values. In the following section, we assess the strengths and limitations of the different approaches.

Han et al. Measure

The Han et al. real-time poverty measure relies on a global question in the Monthly CPS intended to capture all money income. Responses to this income question are simply compared to official thresholds, adjusted monthly for inflation, to determine poverty status for families. Specifically, the global income question asks the respondent to report the:

"total combined income during the past 12 months…of all members [of the family]. This includes money from jobs, net income from business, farm or rent, pensions, dividends, interest, social security payments and any other money income received…by members of [the family] who are 15 years of age or older."[3]

[3] "Basic CPS Items Booklet: Labor Force Items," https://www2.census.gov/programs-surveys/cps/techdocs/questionnaires/Labor%20Force.pdf.

Housing units selected to be in the CPS are typically only asked this question in the first- and fifth-month interviews in the survey (housing units are in the CPS sample for eight months over a 16-month period—surveyed for four months, eight months off, and then surveyed another four months). This global income question is asked only in reference to the family income of the householder's family, so we do not observe this income information for individuals in the household who are outside the householder's family (i.e., unrelated individuals and unrelated subfamilies), which accounts for about 5 percent of individuals in the first or fifth interview month.

Rather than reporting a specific amount for total income, respondents in the Monthly CPS choose among 16 categorical income ranges. For the bottom part of the income distribution, the income ranges are fairly small. Below $15,000 there are five categories: less than $5,000, 5,000 to 7,499, 7,500 to 9,999, 10,000 to 12,499, 12,500 to 14,999; and from $15,000 to $40,000 the intervals are $5,000 wide (the entire list of bins is reported in the Data Appendix).[4] We convert the categorical response to a continuous measure by randomly selecting values of family income from families in the CPS ASEC from the prior survey year who have incomes that fall in that same income range and who have some similar demographic characteristics (see Appendix I and Section VI of Han et al. (2020) for more details and analyses of the validity of the income measure from the Monthly CPS).

This global family income question aligns fairly closely with the definition of total cash income that is used for official poverty and income statistics. An important difference is that the concept of money income used to measure official poverty does not include tax liabilities or credits. The extent to which respondents include taxes in their responses to the global income

[4] Because the income bins are much wider high up in the distribution, the responses to this income question are less suitable for studies of top incomes or high percentiles.

question is unclear. However, as we discuss below, there is considerable evidence that pandemic tax credits are largely included.

Interviews for the Monthly CPS take place in the week that includes the 19th of the month, making it unclear whether income in the first few weeks of the interview month is included in the answers of respondents to this question. This distinction is important for determining when we should expect to see this measure of family income reflect changes in income. In practice, we interpret the responses as including income received in the first few weeks of the interview month, however we acknowledge that this may not be the case when the timing of the transfers is close to when the interview takes place.

Han et al. (2020) perform several checks of their near real-time poverty measure. The results show that the 10th and 25th percentiles of the income underlying the poverty measure closely match (but are slightly lower than) those for pre-tax money income in the CPS ASEC at a point in time. This CPS ASEC income measure is used to determine the official poverty and income distribution statistics of the U.S. Changes in percentiles of the near real-time income measure also closely match the changes over time in the CPS ASEC income measure during the 2005-2019 period (Figure 4 of Han et al. 2020). As a result, the poverty rates from their real-time poverty measure closely track those from the official poverty measure over the same period (Figure 3 of Han et al. 2020).

CPSP Measure

Parolin et al. (2022) construct their predicted monthly poverty measure as follows. First, they begin with a measure of calendar year income in the CPS ASEC for the year two years prior (for some months only one year) to the month of their measure. The measure from the CPS ASEC relies on the income concept used for the SPM (Fox and Burns, 2021), so it includes

reported annual values of pre-tax money income (the income concept used for the official poverty measure) as well as reported or imputed values for in-kind benefits such as SNAP, WIC, housing benefits, school lunches, and some education aid; imputed state and federal tax liabilities and credits; reported medical out-of-pocket expenses and imputed work-related expenses.

Next, they convert this annual measure of income to a monthly measure within the CPS-ASEC by imposing several assumptions (their Table 1 provides a summary). For the bulk of the money income components of their measure—as well as for components such as WIC, heating assistance, housing assistance, out-of-pocket medical expenses, and state and federal taxes (excluding tax refunds)—they assume that the income is received evenly throughout the year, so they simply include one-twelfth of the annual value of each income component in each month. A notable exception is the case where the individual is not employed in the survey month but was employed during the previous calendar year. In that case, components such as earnings, unemployment insurance, work-expenses, and FICA taxes are adjusted using information on current employment status. For example, positive earnings values are recoded to zero if the individual is unemployed for five or more weeks. It is important to note that the question about current employment status is reported at the time of the interview, which is typically in March for the CPS-ASEC, but the reference period for the annual earnings questions in the CPS-ASEC is for the previous calendar year. So, earnings, unemployment insurance, and other components are being adjusted for employment status using an employment status reported several months after the end of the reference period for the income received. The labor force status questions in the Monthly CPS and the CPS-ASEC also refer to a different length of time and have different wording so the imputation of monthly income from annual measures of these sources is necessarily based on a rough translation of the monthly responses to the annual ASEC responses.

How these current employment status questions are used to impute monthly income based on annual income measures is not clearly stated in Parolin et al. (2022) or other related papers that describe their methods.[5]

The CPSP measure also allows some components to vary within the year. For example, they allow SNAP and TANF benefits to vary within a year by using information on both imputed monthly benefits (using reported monthly earnings and state program rules) and reported annual benefits (see Table 1 of Parolin et al. 2022 for more details). The CPSP measure also includes in its income measure predicted values of key COVID-related income transfers such as the Economic Impact Payments and expanded unemployment insurance benefits using information on potential eligibility status as well as the distribution schedules of these payments (see Parolin et al. 2020 and Bitler et al. 2020). Parolin et al. then compare this predicted monthly income measure to one-twelfth of the SPM threshold provided in the ASEC files for each SPM unit to determine the monthly poverty status of each individual in the CSP ASEC.

The CPS-ASEC is released on an annual basis. So, to generate a poverty estimate that is updated on a monthly basis, Parolin et al. (2022) use their imputed monthly poverty status from the CPS-ASEC to impute the poverty status for a separate sample in the Monthly CPS. For each month, they use the most recent CPS ASEC available. Because the CPS ASEC, which is administered mostly in March of each year, collects income data for the previous calendar year and these data are not released publicly until September of the survey year, the income data from the CPS ASEC is available with a significant lag. For example, to impute poverty status for individuals in August 2021, the most recent CPS ASEC available—the 2020 survey—provides data from households interviewed 17 months prior to August 2021 who are reporting on income

[5] We requested the code used for the studies but did not receive it and were told that it is not currently being made public.

received from January through December 2019, up to 32 months prior to August 2021. Since the income used is for a different sample altogether, Parolin et al. use a combined-sample multiple imputation technique to predict poverty status in the Monthly CPS using their imputed poverty status from the CPS ASEC. However, they note that they get similar results when they estimate a logistic regression in the CPS ASEC and use the parameter estimates from this regression to generate out-of-sample predictions in the Monthly CPS. Given this imputation approach, month-to-month variation in their poverty estimate will be determined by the variables that are in both data sources that are used in the out-of-sample prediction. These variables include demographic characteristics (such as age, sex, education, race, etc.), geographic information, and information on employment status. For a complete list of these variables see Table 2 of Parolin et al. (2022).

2. The Pros and Cons of the Different Approaches

Both of these approaches allow poverty estimates to be updated monthly, which has the potential to provide important information to policymakers on a near real-time basis. In addition, these measures rely on the Monthly CPS, which has been collecting information for a nationally representative sample of U.S. households for more than 70 years, allowing us to compare data used in the timely poverty measures to those from historical data. For income specifically, the Monthly CPS has included the global income question for over 40 years, which allows us to observe the cyclical patterns of income and its association with other variables over a long period of time and compare these patterns to those from other surveys (see Han et al. 2020).

An important limitation of both approaches is that they rely on survey reports of income. Income has been shown to be substantially under-reported in surveys, especially for those with few resources, and the extent of under-reporting has increased over time (Meyer and Sullivan

2003; 2011; Meyer, Mok, and Sullivan 2015). These studies argue that survey measurement issues are less problematic for consumption-based measures of well-being. In addition to measurement issues, consumption may better reflect economic well-being for conceptual reasons. Consumption better reflects long-run resources and is more likely to capture disparities that result from differences across families in the accumulation of assets or access to credit. Consumption will also reflect changes in uncertainty about future income streams, which may be particularly important during periods of crisis. Nevertheless, as we have argued in the past, these issues do not suggest that consumption should be used exclusively. Income and consumption data are complements and there are situations in which one is likely to be more informative than the other. Given that detailed, comprehensive and representative consumption data are not available in a timely fashion, the income data are an important source to use.

To assess the relative advantages or disadvantages of these two timely poverty measures we focus on the three most substantive ways in which they differ: the reference period over which income is measured (twelve months versus one month), the income concept (one similar to the official poverty measure versus one similar to the SPM measure), and the nature of values used (reported versus predicted values).

The Reference Period

A key decision in the poverty measurement literature is the time period over which to measure resources received or consumed. The most common reference period both in the U.S. and elsewhere is a year. Both the official U.S. poverty measure and the Census Bureau's SPM rely on income received over a 12-month period. Consumption poverty measures (Slesnick, 1993; Meyer and Sullivan, 2012) have tended to rely on 3 months or 12 months of consumption. However, consumption over such a time period reflects the inflow of resources over a much

longer period than 3 or 12 months. One might argue that a shorter reference period is preferred because families are limited in their ability to save or borrow, while certain payments, such as rent or other monthly bills, must be made regularly. Consequently, longer time periods would miss that well-being may be affected by variation in income received within the period. The literature on the SNAP benefit cycle suggests that a minority of food assistance recipients experience negative consequences from short term fluctuations in resources, including worse nutrition (Shapiro, 2005) and lower school performance in weeks of low resources (Bond et al., 2021). On the other hand, it is not clear that individuals highly value smooth income flows—a long literature has found little willingness to incur the low administrative costs of smoothing out payments of tax credits through the Advance Earned Income Tax Credit (Romich and Weisner 2000; Jones 2010). In addition, research has shown that low income over several years is a much stronger predictor of deprivation than one-year's income (Brooks-Gunn and Duncan 1997; Duncan et al. 1998; Sullivan et al. 2009). Moreover, a substantial fraction of consumption, including durables such as clothes, furniture, and appliances, depends on spending flows over longer time periods than a month. The ownership of other durables including vehicles and homes is common even for those near the poverty line (Meyer and Sullivan, 2012).

In the end, there is usefulness in understanding changes in poverty using both short time periods such as a month and longer time periods such as a year. The National Research Council panel that was charged with assessing official poverty measurement concluded "that it makes most sense to continue to calculate the official poverty statistics on an annual basis." However, they also acknowledged that measuring poverty over a period shorter (or even longer) than a year may be appropriate in specific circumstances (Citro and Michael, 1995, p. 295). The relative usefulness of such measures depends on how the measure is being employed, and what types of

policies it is intended to inform. An important limitation of very short-term poverty measures, however, is that they may significantly overstate changes in well-being in months where lump sum transfers are made. As we show below, as families disproportionately file taxes in the first quarter of the year, we see dramatic fluctuations in the CPSP poverty estimates around tax-filing time. For example, the CPSP child poverty rate fell by 55 percent in March 2021 only to rise by 78 percent in the following month. It is also worth emphasizing that it is often not made clear in summaries of the CPSP monthly poverty numbers that a different time period is being used in their measures than in other poverty measures, even though the distinction is made clear in the body of several of the CPSP publications. For example, policymakers and the media cited the CPSP's headline finding that the expiration of the Advance Child Tax Credit led to a 41 percent increase in poverty without ever mentioning that this was an increase in poverty based on one month's income rather than the much more standard annual reference period.[6]

The Income Concept

The income concepts on which the two poverty measures are based also differ. A conceptual advantage of the CPSP measure is that it intends to include in-kind benefits and taxes, while the Han et al. measure is based on a question about money income. Most researchers prefer adjusting for taxes and in-kind benefits, because these transfers can account for a substantial share of resources for disadvantaged families, suggesting that an income measure that includes them would be more closely associated with their well-being. However, in practice, all of these taxes and transfers are imputed in the CPSP measure, introducing the potential for substantial error. While conceptually preferable, if taxes or in-kind benefits are not actually measured and

[6] For example, see https://news.yahoo.com/nearly-4-million-children-fall-into-poverty-as-expanded-child-tax-credit-ends-163644670.html and https://www.nytimes.com/2022/05/02/opinion/child-tax-credit.html.

are imputed poorly, they may produce a measure that is less well tied to actual well-being (Meyer and Sullivan 2015).

Given that we are considering changes in real-time poverty in the context of changes in tax policy, the treatment of taxes in the income concept is particularly important. A potential limitation of the Han et al. measure is that it is unclear whether respondents include taxes, and in particular the Advance Child Tax Credit, in their responses to the global income question in the Monthly CPS. However, as we discuss below, the wording of the global income question suggests that respondents should include common cash payments from the government. We also present evidence that similar tax credits from the government that were not periodic, specifically Economic Income Payments (EIPs or stimulus checks), appear to be reported in the monthly CPS. Nevertheless, if the ambiguity of the question leads to under-reporting of receipt of tax credits, the Han et al. measure would understate the impact of these transfers on poverty.

Reported versus Imputed Income

While both measures rely on the same surveys to construct an estimate of real-time poverty, they are constructed using very different methods. In addition to relying on a different reference period discussed above, the most important distinction between these two measures is that Han et al. rely on actual reports of income in the Monthly CPS, while the CPSP measure is entirely imputed—no actual reported income data from the month the measure intends to represent is used to determine poverty status.[7] The income data that the CPSP measure uses comes from prior years for completely different individuals. This distinction between reported

[7] Although Parolin et al. (2022) note that their measure is predicted, this point is not emphasized in their reports. Furthermore, this feature is rarely, if ever, noted in the frequent references to their poverty estimates in the popular press.

and imputed income has important implications for how one should interpret changes in the CPSP measure.

One implication is that the CPSP approach does not allow for any behavioral response to a policy change, such as Advance Child Tax Credit payments, other than the behavioral responses captured in month-to-month changes in the observed characteristics from the Monthly CPS used to impute poverty status, such as employment status and demographic characteristics. Thus, their measure will not reflect behavioral responses through changes in labor supply (because the relationship between employment and poverty is likely to change when there are significant changes in policy); changes in interfamily transfers; or other behavioral responses. Even if behavioral responses change variables used in prediction, the changes in predicted income that result will be predicted for different individuals than those whose behavior was altered.

Another implication is that while both approaches are subject to error due to misreporting in surveys, the CPSP measure relies on several stages of imputation that introduces additional potential for error. For example, the CPSP measure relies on imputed values for many components of their measure of annual income, such as taxes, housing subsidies, school lunches, etc. These imputations, which are provided by the Census and made available in the public use files, are themselves measured with error, as has been documented in Meyer, Wu et al. (2021a, b). On top of these imputations, the authors add additional imputations to reflect policies that changed between the most recent CPS ASEC survey and time of the Monthly CPS survey such as the expansions of SNAP, Unemployment Insurance, and the Child Tax Credit.

The CPSP measure also imputes monthly income from annual income within the CPS ASEC from usually a year or more earlier, and imputes poverty status out of sample in the

Monthly CPS using imputed monthly poverty status in the CPS ASEC. These multiple stages of imputation rely on many untestable assumptions and introduce a series of errors that are difficult to assess. One might also be concerned that imputation based on one state of the economy or under one policy regime might be ill-suited to predict poverty when underlying circumstances change. Unfortunately, it is exactly in these types of changing situations when timely measures of well-being are especially useful that predictions are likely to be less valid.

Parolin et al. (2022) report some checks on the accuracy of the monthly imputations on average. They compare the OPM poverty rate in the Survey of Income and Program Participation (SIPP) to a version of the monthly rate that does not account for taxes or in-kind transfers over the 2004-2016 period (see Figure 2 of Parolin et al. 2022). While the patterns are roughly similar, there are long periods when the monthly measure deviates sharply from the SIPP benchmark. These periods of divergence include most of 2004 and 2005, 2013, and 2014-2016. Even these comparisons necessarily understate the gap between the CPSP monthly poverty measure and true monthly income for two reasons. First, many of the largest imputed components of the CPSP measure (such as tax refunds) are not part of the OPM so the full CPSP measure is not validated by comparisons of a simplified version of the CPSP measure to the OPM in the SIPP. Second, many of the SIPP income components were collected on an annual basis starting in 2013 so are not truly monthly and are converted to monthly values in the same way the CPSP measure does.

In the end, the validity of estimates of the impacts of policies depends on the accuracy of the simulations of the policies themselves, as well as the accuracy of the base income to which credits are added and from which taxes are subtracted. Even if aggregate amounts are correct, it matters to whom various income sources are imputed, particularly when the aim is to estimate a

point on the cumulative distribution function for the overall population or for particular subgroups. It is also unclear what the implication is of imputing full amounts for income received by families for one policy, when most other sources of income tend to be underreported. This approach could very well lead to an overestimate of the effect of the simulated policy.

3. Major Policy Changes during the Pandemic

There were many different components to the federal government's response to the pandemic. The two largest initial federal policies that worked to offset the decline in earnings due to the pandemic were the Economic Impact Payments or EIPs and the expansion of Unemployment Insurance (UI) benefits. Additional temporary policies were particularly targeted at families with children. These policies included the transformation of the Child Tax Credit into a child allowance, a special food assistance program for children, the Pandemic EBT program, expansion of childcare assistance, and changes to the Child and Dependent Care Tax Credit. In this section, we briefly describe these policy changes, which are important to keep in mind as they confound attempts to pin any changes in poverty or other economic outcomes on a single policy.

Economic Impact Payments

The initial EIPs were part of the CARES Act, which was passed on March 25, 2020. These payments provided $1,200 to individuals with income less than $75,000 and to single parents (heads of household) with income below $112,500, and they provided $2,400 to married couples with income less than $150,000. Recipients were also eligible to receive an additional $500 for each qualifying child. For those with income above these thresholds, the payments were

reduced by 5 percent of the income that exceeded the threshold. The EIP benefits started the second week of April 2020, with early checks going to those with the lowest adjusted gross income. The Internal Revenue Service had sent EIPs to nearly 90 million individuals by April 17, 2020, and to an additional 63 million individuals over the next five weeks.

A second, less generous round of stimulus payments of up to $600 per person were included in the Consolidated Appropriations Act, which passed in December 2020. These payments started going out on December 29, 2020. The American Rescue Plan Act, which passed on March 11, 2021, provided a third round of stimulus payments of up to $1,400 per person.

Unemployment Insurance

Additional relief to those who lost their job was made available through expanded UI benefits. The CARES Act created the Pandemic Unemployment Compensation (PUC) program, which provided an additional $600 per week to claimants on top of the usual benefit. These PUC payments expired at the end of July 2020. The CARES Act also extended eligibility for benefits to groups not covered by the traditional UI program, such as the self-employed, part-time workers, and those who did not have a long enough work history to qualify for the traditional program (Pandemic Unemployment Assistance, PUA), and it extended by thirteen weeks the duration of UI benefits for a regular claim (Pandemic Emergency Unemployment Compensation, PEUC). UI payments totaled $4.2 billion in March 2020, rose to $48.4 billion in April, nearly doubled to $93.7 billion in May, and $115.7 billion in June.

The UI benefits were partially extended through the Consolidated Appropriations Act. This extension included revived PUC payments at a lower amount ($300 per week rather than

the $600 per week supplement that expired in July 2020). The American Rescue Plan Act extended the $300 PUC payments to September 2021, although some states stopped providing these supplemental benefits a few months earlier.

Together, these expansions of UI led to very substantial increases in transfers to those whose work was affected by the pandemic. Spending on UI jumped from $28 billion in 2019 to $581 and $323 billion in 2020 and 2021, respectively.[8]

Child Tax Credit

To understand the changes to the Child Tax Credit under the American Rescue Plan it is essential to understand how the CTC operated prior to the pandemic. Under the Tax Cuts and Jobs Act (TCJA), the CTC starting in 2018 offered tax filers a partially refundable credit of up to $2,000 per dependent child under age 17. The refundable portion of the CTC (called the Additional CTC or ACTC) does not require tax filers to have any federal income tax liability. It begins phasing in at $2,500 of earned income, at a rate of $0.15 per dollar of earned income.[9] A maximum of $1,400 per dependent child can be claimed as a refundable credit, and the remaining portion of the maximum $2,000 credit must be claimed as a non-refundable credit that

[8] See https://fiscaldata.treasury.gov/datasets/daily-treasury-statement/deposits-and-withdrawals-of-operating-cash.
[9] For example, a tax filer with one dependent child, no tax liability and $3,500 of earned income would receive a refundable credit of $150 (15% of the difference between $3,500 and $2,500).

offsets federal income tax liability. In Figure 1, the solid line shows the credit amount as a function of earnings for a family headed by a single parent with two children.[10]

Child Allowance

The American Rescue Plan Act temporarily replaced the CTC with a child allowance for 2021 only. The child allowance increased the maximum per-child credit from $2,000 for all children under age 17 to $3,600 for children aged 0 to 5 and $3,000 for children aged 6 to 17. It was also fully refundable, meaning families with little or no tax liability would qualify for the entire amount. Notably, children aged 17 qualified for the full $3,000 under the child allowance, whereas they were only eligible for up to $500 under prior law through the Other Dependent Credit. The higher maximum payment began to phase out starting at $75,000 for single filers, $112,500 for head of household filers, and $150,000 for married filers. The $2,000 credit was still available to higher-income tax filers who had under prior law been eligible for the CTC.

In addition to changing benefit amounts, the child allowance was administered differently than the CTC. Families received child allowance payments on a monthly basis during the last six months of 2021. Typically, they received half of the payment they would be eligible for based on prior tax filing. For example, a family with two children between the ages of 6 and 17 would be eligible for a $6,000 credit for tax year 2021, so half of this credit would be paid in advance through six monthly payments of $500 each from July through December of 2021. These advance payments were called the Advance Child Tax Credit or Advance CTC. Total CTC

[10] An additional $500 credit (called the Credit for Other Dependents or ODC) can be claimed on behalf of dependents aged 17+ who are not eligible for the $2,000 credit. The ODC is non-refundable and phases out at the same rate as the non-refundable CTC for dependents under age 17.

payments were over $100 billion in 2020 and were expected to be over $200 billion for 2021 (Corinth et al. 2022).

Changes in Work Incentives Due to the Child Allowance

Since it was fully refundable, the child allowance temporarily eliminated the work incentives of the CTC when passed in March 2021. We focus on the return to work—what is relevant for the work participation decision—because the literature on labor supply for low-income families, particularly single mothers, has focused on the work/non-work decision and found it to be highly responsive to tax incentives. The return to work is equal to earnings net of taxes and reduced transfer benefits when an individual moves from not working to working. The return to work would be lower under the child allowance because families with no earnings would receive the full benefit, whereas such families would receive no benefit from the CTC. Figure 1 showing the payment amounts also shows the loss in incentives of eliminating the CTC that had been in place at the start of 2021. To indicate the magnitude of the work incentives, we show the incremental work incentives for the same single parent family with two children under a 40 percent increase in the Earned Income Tax Credit (EITC). The EITC and CTC are similar tax credits that have their strongest work incentives for the same population, low-earning workers with children, despite the CTC affecting a broader population. One can see in Figure 1 that the work incentive of the CTC and 40% of the EITC are similar through about $15,000 in earnings, after which the CTC work incentive continues to grow until over $30,000 in earnings and then remains constant, while the EITC plateaus and then phases out. The National Academy of Sciences, Engineering and Medicine (NASEM), in an influential report (NASEM 2019) simulated a 40 percent increase to the EITC, arguing that the increase in work incentives would

lead nearly 800,000 single mothers to enter employment. Thus, the elimination of the larger incentive of the 2020 law CTC shown in the figure would be expected to have an even larger effect on employment in the opposite direction. The NASEM report left out this effect in its calculations of the effect of a child allowance. In addition to this substitution effect, one should add a smaller, but not unimportant income effect, a reduction in employment due to higher incomes. See Corinth et al. (2022) for more discussion of the tax incentives of a child allowance.

Other Notable Policy Changes

One of the challenges of determining the impact of a specific policy change, such as the Advance CTC, on poverty is that there were several other policy changes implemented during the pandemic that targeted low-income families and therefore may have affected poverty or other outcomes. For example, 39 billion dollars in expanded food benefits were paid out under the Pandemic EBT program between October 2020 and April 2022, with the vast majority (32 billion dollars) paid out between May and December of 2021 (USDA, 2022). The overlap in timing of these payments with those for Advance CTC will confound any analysis of the effects of the Advance CTC on poverty, food security, or related outcomes.

Another policy change that potentially affected the economic well-being of low-income households around the time of the Advance CTC was the greatly enhanced Child and Dependent Care Credit for 2021, which was part of the American Rescue Plan Act. This change included a 39-billion-dollar expansion in direct childcare funding as well as an expansion of the Child and Dependent Care Tax Credit for 2021.[11]

[11] https://www.whitehouse.gov/briefing-room/statements-releases/2021/04/15/fact-sheet-biden-harris-administration-announces-american-rescue-plan-funding-to-rescue-the-child-care-industry-so-the-economy-can-recover/

4. Trends in the Timely Poverty Measures During the Pandemic

In this section we present estimates for the timely poverty measures discussed above for the period from just prior to the start of the pandemic through the first half of 2022. We highlight the sharply different patterns between these two measures, particularly the differences that are evident around the implementation of major policy changes, such as the Advance CTC.

Han et al. (2020) discusses how the near real-time poverty measure changed during the early months of the pandemic. In Figure 2, we update these estimates through June 2022. We report the rate for all individuals as well as for children because many of the policy changes, such as the Advance CTC, targeted families with children. These estimates indicate that the poverty rate was drifting slowly downward prior to the COVID-19 Pandemic. The rate fluctuated quite a bit on a monthly basis, which is not surprising given that the sample is about one-sixth the size of the CPS ASEC sample. The rate for children is about one-third higher than the rate for all individuals. One can see a pronounced fall in the rate for all individuals and especially for children in April of 2020, which coincides with the distribution of the first EIPs. The rate continues to fall with additional payments in May and the ramping up of expanded unemployment insurance. For all individuals the rate is 1.6 percentage points lower in May than at the start of the year, and for children the rate is 2.3 percentage points lower.[12] Han et al. (2020) showed that the entire decline in poverty through June 2020 can be mainly accounted for by the

[12] Due to the moderately sized samples, estimates of month-to-month changes in poverty are imprecise. Larger changes, such as the fall in poverty between January and May 2020 or the subsequent rise in poverty between May and December, are statistically significant at the 5% level for the full sample. The data behind these figures, including standard errors, are available on the NTJ website hosted by DATAVERSE (https://dataverse.harvard.edu/dataverse/ntj).

initial round of EIPs, issued predominantly in April and May 2020, and secondarily by the expansion of unemployment insurance eligibility and benefits.

In the last 6 months of 2020, however, poverty rose sharply, as some of the benefits that were part of the initial government relief package expired. Between June and December 2020, poverty rose by 2.2 percentage points for all individuals and 2.7 percentage points for children. Poverty rose each month between June and November (October for children) even though the unemployment rate fell by 40 percent (from 11.1 percent to 6.7 percent) during this period. This disconnect between poverty and unemployment is not surprising given that many government benefits expired, and unemployment insurance benefits typically only cover about half of pre-job loss earnings. By December 2020, the rates for both all individuals and children were about one percentage point higher than they were at the start of the year.

During 2021, both the rates for all individuals and children showed little systematic change. The rate for all individuals drifted up slowly, rising almost a full percentage point over the year. The rate for children fluctuated between 15.7 and 17.7 percent given the fairly small sample but showed little trend or difference in the second half of the year compared to the first. The rate for children was 0.4 percentage points lower on average in the first half of 2022 compared to all of 2021, but this difference is small.

In Figure 3 we superimpose the CPSP poverty measures (starting in January of 2020) on those of Han et al., to show how the patterns compare. Unsurprisingly given the one-month reference period for resources, the CPSP monthly poverty rate swings dramatically from month to month. These large changes occur despite the much larger sample size used by the CPSP measure than the Han et al. measure. For example, consider how the CPSP poverty rate fluctuated in the early months of the pandemic. The poverty rate for all individuals fell sharply

by 3.4 percentage points (22 percent) in March of 2020, and then rose by 4 percentage points (34 percent) over the next three months. The fluctuations in child poverty were even larger; poverty for children fell by 7.5 percentage points (40 percent) in March of 2020 and then rose by 8.7 percentage points (78 percent) over the next three months.

These fluctuations in the CPSP measures are driven, in part, by the fact that, as a monthly poverty measure, the material benefits of a lump sum transfer are only recognized in the month that they are received. Thus, poverty fell sharply in March 2020 because the CPSP allocated the bulk of EIP payments as well as tax refunds to families in that month. As a fraction of monthly income, the EIPs were enormous. Monthly income for a family of four with two children and with steady pre-pandemic income at 75 percent of the poverty line increased by more than 200% due to the EIPs. If this family lost all of its non-relief income (due to a lost job, for example) but received the EIP and the expanded unemployment insurance benefits, their monthly income was 4.2 times greater than prior to the pandemic and was more than 200 percent of one-twelfth of the federal poverty line.[13] Did these sharp, transitory changes in income accurately reflect the degree to which well-being changed, as is suggested by a monthly measure of poverty? To conclude that all the economic benefits from income are realized only in the month the income is received, one is implicitly assuming that individuals and families do not save (or borrow) to smooth over very short-term changes in income. Rather, they are implicitly assumed to consume all of their income each month. This assumption seems particularly implausible when the short-run fluctuations in income are particularly large, as was the case in the early months of the pandemic. In fact, research using household level consumption data shows that for those with few resources

[13] These statements are based on a family of four with annual income of $19,445 all of which is from earnings, implying monthly pre-pandemic income of $1,620. An EIP of $3,400 represents a 210% increase over $1,620. If monthly earnings go to 0 but the family receives UI (with a 50% replacement) as well as PUC and EIP, the resulting income is 810 + 2,600 + 3,400 = $6,810, which is 4.2 times greater than $1,620.

consumption did not increase noticeably in the first few months of the pandemic while savings increased (Meyer et al. 2022).

Changes in Real-Time Poverty and the Advance Child Tax Credit

It is important to note the differences in the patterns between the two real-time poverty measures in 2021 and early 2022, because this is the time that spans the period during which the Advance Child Tax Credit was announced and distributed. As noted above, the Advance Child Tax Credit was part of the American Rescue Plan Act, which passed in March 2021. The first advance payments were distributed in July 2021 and these payments continued through December 2021. The patterns for the two poverty measures differ sharply during this period. The CPSP poverty continued to see extraordinarily large changes in 2021. The rate for children fell by 9.4 percentage points (55 percent) in March, presumably driven by imputed receipt of tax refunds in that month. The rate for children fell by 3.9 percentage points (25 percent) in July 2021 and then rose by 4.9 percentage points (40 percent) in January of 2022. These changes, which suggest the CTC had very large, immediate effects on child poverty, for the most part occur by construction—the CTC payments are imputed to families with children during these months, but the measure does not allow changes in other components of income to affect predicted poverty beyond those captured by changes in the inputs used to predict poverty, such as demographics and employment status. The poverty estimates from the Han et al. measure, in contrast, showed much less change during 2021 and early 2022. The Han et al. child poverty rate actually rose in July, by 1.1 percentage points (7.3 percent) and then fluctuated between 16.1 percent and 17.7 percent without a noticeable trend through January 2022.

There are several potential explanations for the stark differences between the two measures of poverty over this period. It is first worth examining the importance of the use of a monthly, as opposed to annual, reference period by the CPSP measure. To get a sense of how important the reference period is for the overall trends, we can take the monthly values of poverty and average them over a year. This does not correspond to income averaged over a year, but it is a useful proxy.[14] We can see in Appendix Figure 1 that the annual averaged CPSP measure still shows a sharp divergence from the Han et al. measure, especially for children. The average of the child poverty rates tends to be about a percentage point above the Han et al. measure in the first half of 2021, but then falls to more than two percentage points below the Han et al. rate in the first half of 2022. Thus, the use of a monthly measure does not seem to explain the different patterns.

It is also worth asking if the large changes from one month to the next that we see for child poverty rates from the CPSP measure are plausibly capturing large changes in child well-being. The comparisons to well-being measures from the Census Household Pulse Survey in Parolin et al. (2022) indicate an association between the monthly poverty measures and well-being measures at the state/month level that is stronger in the simulations that account for policy changes than with poverty measures that do not account for any transfer or a measure of nonemployment.[15] However, this is a fairly weak test given that the alternative poverty measures do not incorporate the trillions of dollars in cash transfers paid out over this period, and nonemployment is a poor proxy for poverty.

[14] Note that even with the monthly predictions of income from the CPSP simulations one could not calculate annual poverty as one would need to know the relationship over time of the predicted incomes over the course of a year for each family.

[15] Most of the measures they examine, such as whether the respondent is anxious, down, lacking interest or is not able to stop worrying, are subjective. They also include food insecurity, which is partly subjective, and missed rent or mortgage.

Another possible explanation would be that the Han et al. measure might not reflect receipt of the Advance CTC if respondents do not include these payments in their responses to the global income question in the Monthly CPS. While this is a serious question, there are good reasons to believe that the CTC payments are captured by the global income question. This issue can be approached in two ways. First, does the nature of the Monthly CPS income question make it likely that the payments were included? Second, what does the empirical evidence say about survey respondents' inclusion of similar payments during the previous year?

The nature of the CPS question is not without ambiguity, but the wording suggests that respondents should believe that the income measure should include the Advance CTC payments. The question asks for all money income received by the family and mentions a common cash payment from the government (social security payments) as an example. While the Census Bureau thinks of this question as capturing pre-tax income, there is no mention of taxes in the income section. The interviewer instructions list that regular cash payments should be included, but not tax refunds. Similar tax payments from the government that were not periodic, specifically Economic Income Payments (EIPs or stimulus checks), appear to be largely reported in the monthly CPS as we show below.

In practice though, many types of government benefit payments are sharply underreported in the CPS and other surveys (Meyer, Mok and Sullivan 2015). The question of empirical evidence comes from two sources. First, the pattern of the Han et al. measure around the payment of the first round of EIPs in April and May of 2020 strongly suggests these EIPs were included in reports of family income. Looking at Figure 2, we see that there was a large dip in poverty of all individuals and children in April of 2020 with a continuing fall in May when the vast majority of the EIPs were paid. The poverty rate of children fell by 2.2 percentage points in

April, and an additional 1.4 percentage points in May. These drops occurred despite a fall in employment and an increase in poverty without the EIPs forecast in Han et al. (2020). UI dollar payments were substantial during this period, though they occurred slightly later. See Han et al. (2020) Table 3 for simulations that indicate that UI was much less important for poverty reduction in April and May 2020 than the EIPs. We also know that UI payments were sharply underreported in the CPS-ASEC in the pandemic (Larrimore et al. 2022). Thus, it seems fairly clear that the EIPs were largely recorded by the monthly money income question in the CPS.

Second, a comparison of the Han et al. measure to the ASEC poverty measure, which explicitly does not account for taxes including EIPs and the Advance CTC, indicates that at least a large share of the EIPs are included in reported family income in the Monthly CPS. We compare calendar year annual ASEC child poverty rates from 1989-2020 to real-time child poverty averaged over the months from November through March, given the size of the child sample. Figure 4 shows the Han et al. measure change from 2019 to 2020 (a fall of 1.7 percentage points) lines up best with the simulated fall in the ASEC adding in most of the EIPs. Incorporating only the first EIP indicates a fall of 1.0 percentage point (triangle marker), while including both EIPs indicates a fall of 2.3 percentage points (diamond marker). On the other hand, the series without the EIPs rises by 1.3 percentage points (circle marker). Thus, again the Han et al. measure appears to largely include the EIPs as the fall in poverty from 2019 to 2020 is of a similar size to that in the ASEC when incorporating them.[16]

Another potential explanation for the different patterns across the two real-time poverty estimates during the period when the Advance CTC payments were distributed is that the Han et

[16] We also compared these measures for all individuals (Appendix Figure 2). The evidence for this full sample is not as strong as it is for the sample of children, but we see that poverty based on the Monthly CPS does not rise noticeably as it does in the CPS-ASEC measure that excludes the EIPs, consistent with some but not all of EIP dollars being reported.

al. measure, which is based on actual survey reported income, allows for a behavioral response to the policy change that would not be reflected in the CPSP measure beyond what is captured in month-to-month changes in the observed characteristics, such as demographic characteristics, and measures of employment that are used to impute CPSP poverty status. Potential behavioral responses that could affect family income, such as changes in labor supply, transfers from family or friends, and changes in living arrangements, would not be directly considered in the CPSP measure.

Because there were many other policy changes that disproportionately affected families with children during this period, it is difficult to determine the extent of the behavioral response to the Advance CTC. However, empirical evidence and past research suggest that the response could be substantive. In general, we would expect the labor supply response in the very short run to be smaller than the long-run response. This may be particularly true in the case of the Advance CTC because the change in incentives was complicated and not particularly emphasized (Corinth et al. 2022). In the case of the EITC, there were extensive outreach campaigns at the national, state, and city level to point out the money gained from working due to the credit. There were also policies that were part of the American Rescue Plan Act that instituted the Advance CTC that would have counteracted the employment reduction effect, in particular the expansion of support for childcare.

Nevertheless, there is evidence from both the Monthly CPS and administrative employment data that employment fell around the time of the Advance CTC. The EITC literature repeatedly finds that the employment of those with low levels of education responds to tax incentives targeted at those with low earnings, while the employment response of those with high levels of education is muted or not present. In Figure 5 we report the employment of adults with

children relative to adults without children by education group after January 2020 (see Appendix Figure 3 for levels of employment). We see a decline in the employment of adults with children relative to those without children beginning shortly after the passage of the American Rescue Plan Act in March 2021. This decline is only apparent for those with a high school education or less. There is little change for those with at least some college education. The decline begins to reverse in the last quarter of 2021 as it became clear that the temporary advance payments were in fact temporary. By early 2022, the difference between those with and without children disappears. In September and October of 2021, both the difference in employment levels between those with and without children and the difference in this difference between those with high and low education are statistically significant. In terms of the magnitude of the employment changes, the decline of 2 percentage points by September 2021 amounts to about 400,000 workers. This change is between 25 and 30 percent of the long-run decline predicted by Corinth et al. (2022) from the elimination of the CTC work incentives.

A similar pattern is evident in administrative employment data. These data come from Opportunity Insights (OI), which combines anonymized paycheck data from two private companies, Paychex and Intuit. The OI series indicates that low-wage employment (the bottom quartile) fell during a similar, though slightly later period than what is indicated by the CPS data. Low earnings employment peaked in July 2021 and did not recover until April of 2022. While the OI series has the advantage of disaggregation by earnings level, we cannot separate the data by education or presence of children (this series can be seen in Appendix Figure 4 and is described in the Data Appendix).

Changes in transfers from family members, friends, and non-custodial parents are another potentially substantive behavioral response. This response could occur in the short run given the

salience and the advertising of these transfers (as opposed to advertising of the work incentives). A substantial literature has found changes in private transfers from family members in response to changes in income or public transfers (see Rosenzweig and Wolpin, 1994; Cox and Jakubson, 1995; Altonji, Hayashi, and Kotlikoff 1997; and Schoeni 2002). Estimates indicate that private transfers are reduced by 10 to 40 cents for every dollar of income received. Nonfamily transfers from partners or fathers of children may be more relevant and potentially an additional source for the families in question. While private transfers may not be a large fraction of income for the typical family, research has indicated that these transfers can account for a large fraction of income for very disadvantaged groups (Edin and Lein, 1997). Finally, changes in living arrangements can also counteract changes in non-market income as those with income increases move out or are less likely to move in with relatives, partners, or friends. Bitler et al. (2006) find large effects of welfare reform on living arrangements for children of low-educated heads averaged over the first years after reforms. The effects of the Advance CTC might be smaller given our short time period.

5. Findings from Other Indicators of Well-Being

Additional evidence on the effects of the CTC comes from two recent studies on changes in measures of well-being during the period of Advance CTC payments. If poverty fell sharply, and then rose sharply six months later, as indicated by the CPSP measure, one might expect to see sharp changes in well-being measures at these times. Furthermore, a key goal of poverty measurement is to identify deprivation which can be directly studied for this period. Two recent papers have examined other measures of well-being besides income poverty before and during the Advance CTC. In the months before and during the Advance CTC Parolin et al. (2021)

examine food insufficiency, missed rent/mortgage payments, and difficulty in meeting essential expenses. In the year before and after the Advance CTC Karpman et al. (2022) examine food insecurity as well as problems paying rent, mortgage payments, or utility bills. The results of both papers suggest an improvement in food security during the period of the Advance CTC. The other well-being measures, which are arguably less subjective, do not show clear improvements.

The lack of consistent effects of the large changes in poverty on family hardship measures is not what one would expect from a 25 percent lower poverty rate. Furthermore, the one outcome that appears to show changes from before the Advance CTC to during its implementation is confounded by the expansions of SNAP and Pandemic EBT. Pandemic EBT expenditures were large (on the order of half of what is distributed in SNAP in a typical year) and were specifically targeted toward poor children during the period of the Advance CTC.

A further limitation of the findings from these studies is that the approach used to identify the effect of the CTC on hardship in many of the analyses compares reporting recipients to reporting nonrecipients, an approach likely to be biased because of non-classical measurement error in who reports receipt. The pronounced underreporting of receipt can be seen by dividing the number of children sent payments (61 million as reported in Parolin et al. 2021) by the number of children covered by the relevant surveys (74 million) yielding a receipt rate of over 82 percent (Parolin et al. report 83 percent). In the Parolin et al. sample, only 67 percent of children were in families with parents reporting receipt. Karpman et al. (2022) cut the sample down to those with incomes that would make them eligible and still find that only 76 percent of adults with children report receipt.[17] Thus, a large share of those not reporting receipt is those who

[17] The rate for families could be lower than that for children but given the surveys' likely under-coverage of nonrecipient undocumented immigrants and children abroad, the survey almost certainly has substantial underreporting.

received but just did not report it, likely a selected sample of those for whom the Advance CTC was not salient.

In the case of the regression analyses employed by Parolin et al. with reported receipt as a binary explanatory variable that is measured with error, there will be substantial bias (Aigner 1977; Meyer and Mittag 2017), that will overstate effects in this case. The nonclassical measurement errors are necessarily correlated with the true values of receipt. Parolin et al. also estimate the effect of CTC receipt using the presence of children as an instrumental variable for receipt. But this approach is also necessarily biased because the nonclassical measurement error must be correlated with any instrument that is correlated with the true values. In this context, the instrumental variable procedure necessarily biases upward estimates of the CTC effects.[18] Comparisons of those with and without children would still be valid, a contrast used in some of the Parolin et al. (2021) analyses. While these studies would be more compelling with additional evidence from the period after the Advance CTC had expired, one should recognize that other policies of this period would continue to confound the analyses.

6. Conclusions

The COVID pandemic sparked innovation in real-time poverty measurement. In this paper, we highlight the differences between the new approaches taken to measure real-time income poverty and show that the different approaches can yield sharply different patterns and conclusions about the effects of policies. Several academic papers and numerous popular reports based on these measures conclude that poverty fell sharply during the temporary implementation of a child allowance in 2021 and rose sharply when it lapsed. What is not evident in these

[18] This logic also applies to a variable like CTC dollars which is the product of a mismeasured binary variable and a continuous one.

sources is that these claims of poverty changes in the range of 40 percent are based on simulations that do not rely on income data from the period in question. Instead, they simulate income relying on income data from prior years rather than actual reports of current income. The simulations also assume behavioral responses to cash transfers are absent. When we examine reported survey income data from the Monthly CPS, our estimates indicate that child poverty rates changed little during and after the period of a temporary child allowance. We explore explanations for the different patterns. Some of the differences are likely due the CPSP simulating monthly as opposed to annual income, to behavioral responses, and the underreporting of government transfers. While we cannot resolve many of the differences, the evidence we present indicates that conclusions that there was a large decrease in poverty while a child allowance was in place in 2021 and a large increase in 2022 when it lapsed merit greater qualification. In fact, evidence from a measure based on reported rather than imputed income for an annual rather than a monthly reference period suggests much more modest changes in poverty.

It is also important to qualify that the relationship between a child allowance and income poverty in the long run likely differs from this relationship in the short run. This may be particularly true when examining changes in income in a single year (2021) that was atypical in many ways and during which other changes confound potential analyses. An assessment of the long run implications of a policy such as a permanent child allowance should consider the potential for behavioral responses including long-run changes in labor supply, living arrangements, fertility, and marriage that might affect the poverty reduction effects, and more broadly the effects on child well-being. Corinth et al. (2022) find that the labor supply responses could reverse most, or all, of the positive effects of a child allowance compared to a CTC. Other

recent papers have found smaller, but still substantial, effects but tend to rely on lower elasticities than have been found in the EITC literature, assuming that elasticities have fallen over time (Bastian 2022). The evidence for a decline in elasticities is mostly from married mothers which is not especially relevant. The one paper that focuses on single women (Bishop et al. 2009), finds little change in the participation elasticities (as opposed to hours elasticities) that are relevant for the CTC and even less change over the period beginning in late 1980s or 1990s from which most of the elasticities in the EITC literature draw their identification. Other work that has directly estimated the labor supply effects of the CTC has found larger effects than Corinth et al. (2022) assumed (Lippold 2019).

Other potential long-run effects of the change to a child allowance are also important to consider alongside short-run effects.[19] Increased support for low-income children could improve their long-run outcomes. Children's access to food stamps in the 1960s and 1970s led to improved outcomes when they became adults, including higher earnings (though not increased employment), better health, less incarceration and less dependence on welfare programs (Hoynes, Schanzenbach, and Almond 2016; Bitler and Figinski 2019; Bailey et al. 2020). Much of this evidence comes from a period when other safety net programs were much less generous than current aid, so the marginal effects might be lower today. Larger EITC payments for children have increased their educational attainment and their employment and earnings as adults (Bastian and Michelmore 2018). In that case, the policy being examined was a combination of more income and higher employment.

Finally, we should emphasize the potential limitations of income-based poverty measures generally due to measurement issues and because income variation may not imply changes in

[19] See Winship (2021) for discussion of alternatives to a child allowance.

economic well-being. For these reasons, previous research has argued that consumption is a better measure of economic well-being, particularly for families with few resources. Nevertheless, this does not suggest that consumption should be exclusively used. Income and consumption data are complements and there are situations where each is likely to be more informative than the other. Given the conflict between income sources that we have highlighted, we expect that consumption data will be especially informative about how the material circumstances of families changed during the pandemic and with the temporary introduction of a child allowance.[20] Though, as indicated earlier in this section, both favorable and unfavorable long-run effects will not likely be immediately apparent.

[20] If the Bureau of Labor Statistics follows the same schedule as in recent years, nationally representative data on consumption for 2021 from the Consumer Expenditure Survey would not be released until September 2022.

References

Aigner, Dennis J., 1973. Regression with a binary independent variable subject to errors of observation. J. Econometrics 1 (1), 49–59.

Altonji, Joseph G., Fumio Hayashi, and Laurence J. Kotlikoff. 1997. "Parental Altruism and Inter Vivos Transfers: Theory and Evidence" Journal of Political Economy, 105(6): 1121-1166.

Bastian, Jacob. 2022. "Investigating the Effects of the 2021 Child Tax Credit Expansion on Poverty and Employment." Working Paper, Rutgers University.

Bastian, Jacob, and Katherine Michelmore. 2018. "The Long-Term Impact of the Earned Income Tax Credit on Children's Education and Employment Outcomes." *Journal of Labor Economics* 36 (4): 1127–63.

Bee, Adam, Charles Hokayem and Daniel Lin, "Imputing 2020 Economic Impact Payments in the 2021 CPS ASEC" SEHSD Working Paper No. FY 2021-18, September 2021.

Bitler et al. Bitler, Marianne P., Jonah B. Gelbach, and Hilary W. Hoynes. 2006. "What Mean Impacts Miss: Distributional Effects of Welfare Reform Experiments." Journal of Human Resources.

Bishop, Kelly, Bradley Heim and Kata Mihaly. 2009. "Single Women's Labor Supply Elasticities: Trends and Policy Implications" *Industrial and Labor Relations Review*, 63(1): 146-168.

Bond, Timothy N. Jillian B. Carr, Analisa Packham & Jonathan Smith 2021. Hungry for Success? SNAP Timing, High-Stakes Exam Performance, and College Attendance WORKING PAPER 28386, January.

Brooks-Gunn, Jeanne, and Greg Duncan "The Effects of Poverty on Children" The Future of Children, Vol. 7, No. 2, Summer-Fall 1997, pp. 55-71.

Chetty, Raj, John Friedman, Nathaniel Hendren, Michael Stepner, and the Opportunity Insights Team. November 2020. "The Economic Impacts of COVID-19: Evidence from a New Public Database Built Using Private Sector Data" Working Paper.

Citro, Constance F. and Robert T. Michael. 1995. Measuring Poverty: A New Approach, eds. Washington, D.C.: National Academy Press.

Corinth, Kevin and Bruce D. Meyer. 2021. "The Child Tax Credit, Labor Supply, and Poverty." CID RESEARCH NOTE SERIES #1: December 6, 2021. https://cpb-us-w2.wpmucdn.com/voices.uchicago.edu/dist/a/3122/files/2021/12/CID-Research-Note-Child-Tax-Credit.pdf

Corinth, Kevin, Bruce D. Meyer, Matthew Stadnicki and Derek Wu. 2022. “The Anti-Poverty, Targeting, and Labor Supply Effects of Replacing a Child Tax Credit with a Child Allowance.”. NBER Working Paper No. 29366, October 2021 (Revised title and text, March 2022).

Cox, Donald and George Jakubson. 1995. "The Connection between Public Transfers and Private Interfamily Transfers" Journal of Public Economics, 57(1): 129-167.

Duncan, Greg, W. Jean Yeung, Jeanne Brooks-Gunn and Judith R. Smith. 1998 “How Much Does Childhood Poverty Affect the Life Chances of Children?” American Sociological Review, Vol. 63, No. 3 (Jun., 1998), pp. 406-423.

Edin, Kathryn and Laura Lein (1997). Making Ends Meet: How Single Mothers Survive Welfare and Low-Wage Work, Russell Sage Foundation, New York, NY.

Fox, Liana and Kalee Burns. 2021. The supplemental poverty measure: 2020. U.S. Census Bureau U.S. Census Bureau, Current Population Reports, P60-275.

Han, Jeehoon, Bruce D. Meyer and James X. Sullivan 2020 “Income and Poverty in the COVID-19 Pandemic,”. Brookings Papers on Economic Activity Summer 2020 Special Edition, pp. 85-118.

Han, Jeehoon, Bruce D. Meyer and James X. Sullivan, 2022. “Real-time Poverty Estimates During the COVID-19 Pandemic through May 2022,” University of Chicago, Harris School of Public Policy and Wilson Sheehan Lab for Economic Opportunities at Notre Dame. http://povertymeasurement.org/wp-content/uploads/2022/02/Monthly_poverty_rates_updated_thru_May_2022_v1.pdf

Hoynes, Hilary, Diane Whitmore Schanzenbach, and Douglas Almond. 2016. “Long-Run Impacts of Childhood Access to the Safety Net.” American Economic Review 106 (4): 903–34.

Jones, Damon. 2010. “Information, Preferences, and Public Benefit Participation: Experimental Evidence from the Advance EITC and 401(k) Savings” American Economic Journal: Applied Economics 2 (April):147-163.

Karpman, Michael, Elaine Maag, Stephen Zuckerman, and Doug Wissoker. 2022. Child Tax Credit Recipients Experienced A Larger Decline in Food Insecurity and a Similar Change in Employment as Nonrecipients Between 2020 and 2021. Tax Policy Center. May 2022.

Larrimore, Jeff, Jacob Mortenson, and David Splinter. 2022. “Unemployment Insurance in Survey and Administrative Data” Working Paper.

Lippold, Kye. 2019. “The Effects of the Child Tax Credit on Labor Supply.” SSRN Electronic Journal.

Meyer, Bruce D. and Nikolas Mittag 2017. “Misclassification in Binary Choice Models” *Journal of Econometrics* 200 (October 2017), 295-311.

Meyer, Bruce D., and Nikolas Mittag. 2019. "Using Linked Survey and Administrative Data to Better Measure Income: Implications for Poverty, Program Effectiveness, and Holes in the Safety Net." *American Economic Journal: Applied Economics* 11 (2): 176–204.

Meyer, Bruce D., Derek Wu, Victoria Mooers, and Carla Medalia. 2021. "The Use and Misuse of Income Data and Extreme Poverty in the United States." *Journal of Labor Economics* 39 (S1): S5–58.

Meyer, Bruce D., Derek Wu, Grace Finley, Patrick Langetieg, Carla Medalia, Mark Payne and Alan Plumley. "The Accuracy of Tax Imputations: Estimating Tax Liabilities and Credits Using Linked Survey and Administrative Data," Forthcoming in Measuring and Understanding the Distribution and Intra and Intergenerational Mobility of Income and Wealth, NBER.

Meyer, Bruce D., and James X. Sullivan. 2012a. "Winning the War: Poverty from the Great Society to the Great Recession." *Brookings Papers on Economic Activity* Fall: 133–200.

Meyer, Bruce D., Connacher Murphy, and James X. Sullivan. 2022. "Changes in the Distribution of Economic Well-Being During the COVID-19 Pandemic: Evidence from Nationally Representative Consumption Data," NBER Working Paper 29878, March 2022.

National Academies of Sciences, Engineering, and Medicine (NASEM). 2019. A Roadmap to Reducing Child Poverty. Edited by Greg Duncan and Suzanne Le Menestrel. Washington, D.C.: National Academies Press.

Parolin, Zachary, Sophie Collyer, Megan A. Curran, and Christopher Wimer. 2021. "Monthly Poverty Rates among Children after the Expansion of the Child Tax Credit." *Poverty and Social Policy Brief* 5(4).
https://static1.squarespace.com/static/5743308460b5e922a25a6dc7/t/612014f2e6deed08adb03e18/1629492468260/Monthly-Poverty-with-CTC-July-CPSP-2021.pdf.

Parolin, Zachary, Megan Curran, Jordan Matsudaira, Jane Waldfogel, and Christopher Wimer. 2020. "Monthly Poverty Rates in the United States during the COVID-19 Pandemic." Poverty and Social Policy Working Paper.
https://static1.squarespace.com/static/5743308460b5e922a25a6dc7/t/6123aff64135f6579e56b0a9/1629728760431/Projecting-Monthly-Poverty-CPSP-2020.pdf.

Parolin, Zachary, Megan Curran, Jordan Matsudaira, Jane Waldfogel and Christopher Wimer. 2022. "Estimating Monthly Poverty Rates in the United States," *Journal of Policy Analysis and Management*, Forthcoming.

Parolin, Zachary, Elizabeth Ananat, Sophie Collyer, Megan Curran, and Christopher Wimer. 2021. "The Initial Effects of the Expanded Child Tax Credit on Material Hardship" NBER Working Paper 29285, September 2021.

Romich, Jennifer L., and Thomas Weisner. 2000. “How Families View and Use the EITC: Advance Payment versus Lump Sum Delivery.” *National Tax Journal* 53:1245–64.

Rosenzweig, Mark R. and Kenneth I. Wolpin. 1994. "Parental and Public Transfers to Young Women and their Children" *American Economic Review*, 84(5): 1195-1212.

Schoeni, Robert F. 2002. "Does Unemployment Insurance Displace Familial Assistance?" *Public Choice*, 110(1-2): 99-119.

Shapiro, J. M. (2005). “Is there a daily discount rate? Evidence from the food stamp nutrition cycle.” Journal of Public Economics, 89, 303–325.

Slesnick, Daniel, 1993, “Gaining Ground: Poverty in the Postwar United States,” *Journal of Political Economy*, vol. 101:1, 1-38.

Sullivan, James, Leslie Turner and Sheldon Danziger (2008), “The Relationship between Income and Material Hardship? *Journal of Policy Analysis and Management*, 27(1): 63-81.

U.S. Department of Agriculture, 2022. National Data Bank Version 8.2 PUBLIC https://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap

Winship, Scott. 2021. “The Conservative Case Against Child Allowances.” American Enterprise Institute.

Data Appendix

Analysis Sample

Our analyses focus on a subset of individuals from the Monthly CPS monthly survey because we do not observe family income for all individuals for several reasons. First, housing units in the CPS are typically only asked this question in the first and fifth interview months that they are in the survey (housing units are in the CPS sample for eight months over a 16-month period—four months on, eight months off, and four months on). Second, the total income question is asked only in reference to the family income of the householder's family, so we do not observe this income information for individuals in the household who are outside the householder's family (i.e. unrelated individuals and unrelated subfamilies), which accounts for about 5 percent of individuals in the first or fifth interview month. Finally, during our sample period, between 20 and 31 percent of individuals in the first or fifth months of the survey do not have a response to the family income question. Although the Census Bureau provides imputed values of income for those who do not respond, we do not include these observations in our analysis. As a result of these restrictions, we observe family income from respondents in their first or fifth month in the survey for a monthly sample of about 8,000 households and 20,000 individuals.

Changes in nonresponse in the pandemic

An important issue to consider for analyses of income that include the period of the COVID-19 pandemic is a concern about how the pandemic may have affected survey responses. Due to health concerns, the Census Bureau shifted the survey collection method for the Monthly CPS from in- person to phone interview for some households in March 2020 and for nearly all households in April 2020. This change in procedure continued through August of 2020, but by September 2020 the methods had returned to baseline. This issue affects both the Monthly CPS and the CPS-ASEC for 2020.

Adjusting Survey Weights

To address concerns about changes in sample representativeness due to the pandemic, we reweight the samples during and after the pandemic so that they closely match a sample prior to the pandemic on observable characteristics. Specifically, for the Monthly CPS samples used for Figures 2 through 3, we first pool the January and February 2020 surveys, and divide our sample into 27 demographic cells defined by age (18-39, 40-64, 65 or above), education (high school dropout, high school degree or some college, College degree or above), and family type (individual 65 or older, non-elderly married person with/without children, non-elderly unmarried person with/without children). We then adjust the weights so that the weighted averages of these demographic characteristics of samples in March 2020-June 2022 surveys match to those of the pooled sample from January and February 2020. For the CPS-ASEC and the Monthly CPS samples used for Figure 4, we repeat the above reweighting procedure but adjusting the weights for survey years 2020-2021 to match the above demographic characteristics of the sample in the 2019 CPS-ASEC.

Imputing a Continuous Measure of Income from Bracketed Income in the Monthly CPS

Rather than reporting a specific dollar amount for family income, respondents in the Monthly CPS choose among 16 categorical income ranges:

1) Less than $5,000
2) 5,000 to 7,499
3) 7,500 to 9,999
4) 10,000 to 12,499
5) 12,500 to 14,999
6) 15,000 to 19,999
7) 20,000 to 24,999
8) 25,000 to 29,999
9) 30,000 to 34,999
10) 35,000 to 39,999
11) 40,000 to 49,999
12) 50,000 to 59,999
13) 60,000 to 74,999
14) 75,000 to 99,999
15) 100,000 to 149,999
16) 150,000 or more

We convert categorical responses into a continuous measure by randomly selecting values of family income from families in the CPS-ASEC from the prior survey year who have incomes that fall in that same income range and who have some similar demographic characteristics. Specifically, we define the cells from which we draw income values based on the 16 income categories and 15 demographic categories defined by family size, number of children, and whether the age of the household head is 65 or older. For example, we assign an income value for a 65-year-old single individual in the Monthly CPS who reports having income between $30,000 and $34,999 by randomly selecting income values from the CPS ASEC sample of single individuals aged 65 and over who report a total income value that is between $30,000 and $34,999. We create and use five imputations for each response to estimate poverty. The key assumption for this imputation approach is that the distribution within a given category is the same in the Monthly CPS as in the CPS-ASEC, which is reasonable given that both questions refer to a twelve-month period and rely on the same definition of income.

Imputing Economic Impact Payments (EIP)

Imputing the EIP is straightforward as eligibility was primarily determined by family income, size and composition, all of which we observe in the Monthly CPS. However, to calculate the EIP, in some cases we have to make assumptions about 1) who is in the tax filing unit and 2) how total family income is divided across families with multiple tax filing units.

1.1 Specifying the tax filing unit
To assign individuals in the Monthly CPS to tax filing units we make four assumptions. First, each family unit within a household is a separate tax unit. Because the monthly income question is only asked of the householder's family, we must focus on the 95 percent of the population that

this question covers. Fortunately, 96.4 percent of these individuals are in households that only have one family and no subfamilies. An example of the remaining cases is when a household has a primary family and a subfamily that we would assume file tax returns separately. For a household with multiple subfamilies, each subfamily is a separate tax unit. Second, a married couple files taxes jointly. Third, a person age 23 or below who is not the head of family or the spouse of family head (i.e. child or other relative of family head) belongs to the family head's tax unit as a dependent. Fourth, a person age 24 or above who is not the head of the family or the spouse of the family head is a separate tax unit.

1.2. Specifying the income of tax filing units

In multiple family households that contain 3.6 percent of the population, we first allocate family income in a household assuming that each family's contribution to household income is proportional to the number of adults in the family. For example, suppose that a household consists of two families where the first family has two adults and the second family has three adults. We assign family income of 2*(total household income/5) to the first family and family income of 3*(total household income/5) to the second family. Similarly, we calculate tax filing unit income as family income multiplied by the percent of adults in a family who belongs to the tax filing unit. Overall, 11.7 percent of people are in households where income is allocated in this way.

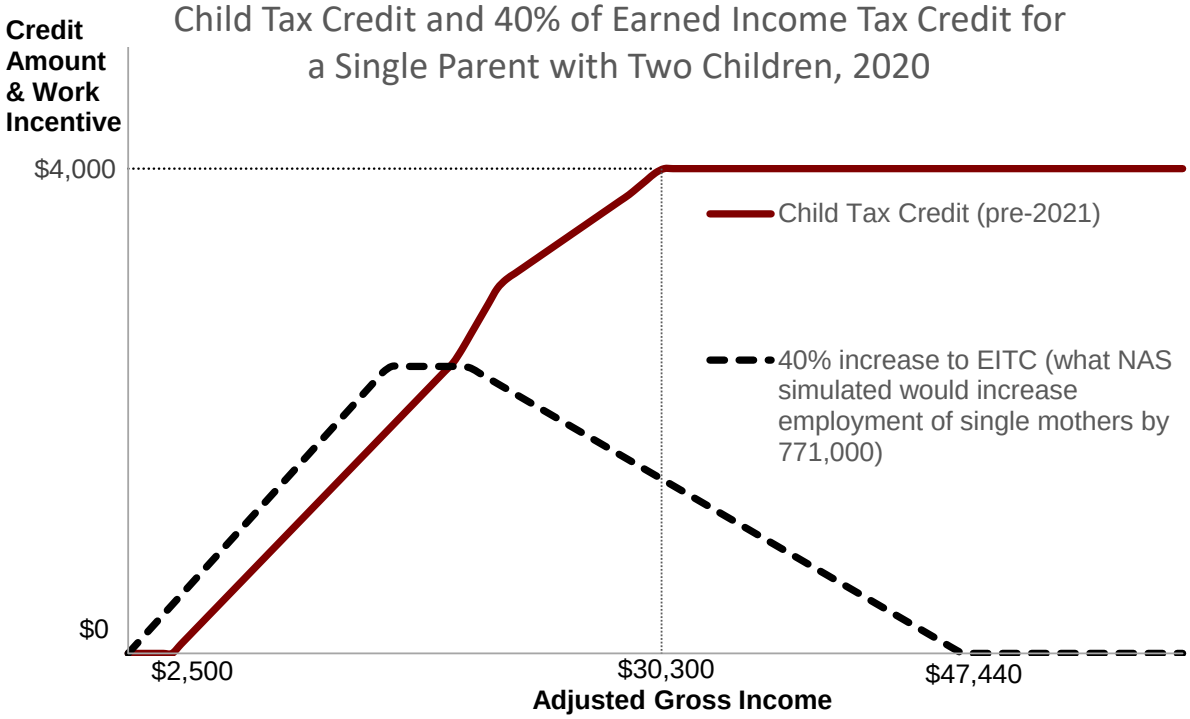
1.3 Household level EIP

Having the imputed tax filing units and their income, we calculate the amount of the first and second round EIPs for each tax filing unit by applying the EIP eligibility/benefit rules. Specifically, for the 1st (2nd) round EIP, we assign $1,200 ($600) to a single tax unit who has income less than $75,000. We apply the benefit reduction rate of 5 percent for each dollar in excess of $75,000. We assign $2,400 ($1,200) to a married couple tax unit with income less than $150,000 and apply the benefit reduction of 5 percent for each dollar in excess of $150,000. For each dependent who is age under 17, we assign an additional $500 ($600) to a tax unit. Finally, we calculate the household-level EIP as the sum of EIPs in all tax filing units of a household.

Notes on the Opportunity Insight (OI) Employment Tracker

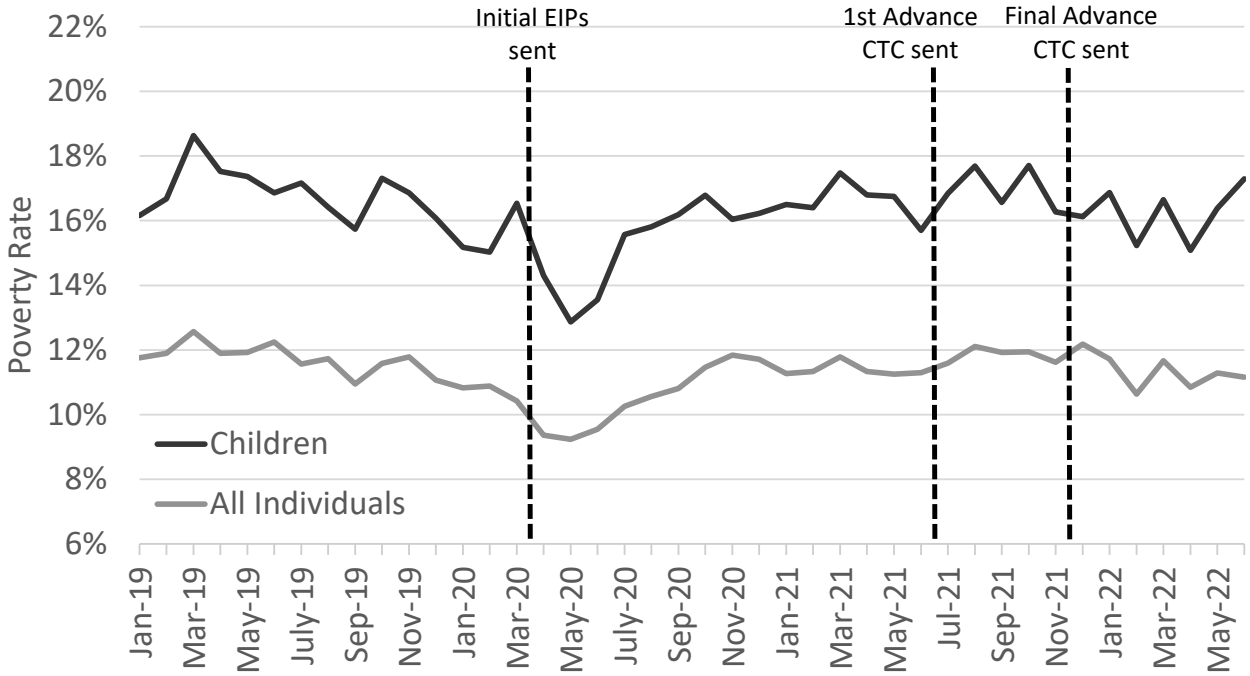
OI reports weekly (seven-day moving average) employment rates by wage quartiles, expressed as a percentage change relative to the period of January 4-31, 2020. The wage quartiles are defined using multiples of the poverty thresholds that are adjusted monthly for inflation using the CPI. The upper threshold for the bottom annual wage quartile is 100% of the federal poverty line, while the upper threshold for the 2nd and 3rd quartiles are 150% and 250% of the federal poverty line, respectively. See Chetty et al. (2020) for more details.

Figure 1. Credit Amount and Work Incentive of Child Tax Credit and 40% of Earned Income Tax Credit for a Single Parent with Two Children, 2020



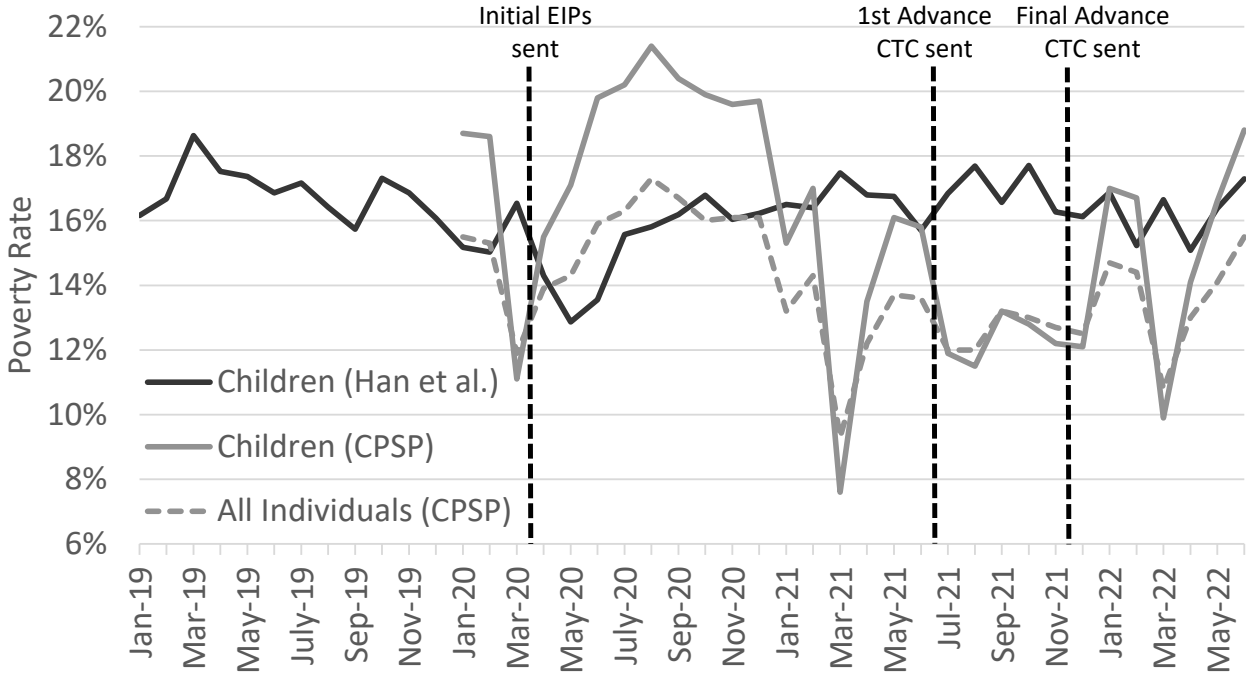
Notes: This figure is from Corinth and Meyer (2021). CTC and EITC parameters are based on 2020 tax law (all dollar values expressed in 2020 nominal terms). All adjusted gross income is assumed to come from earned income, and the family is assumed to take the standard deduction and claim no other non-refundable tax credits.

Figure 2. Han et al. Near Real-Time Poverty Rate, January 2019 – June 2022, All Individuals and Children



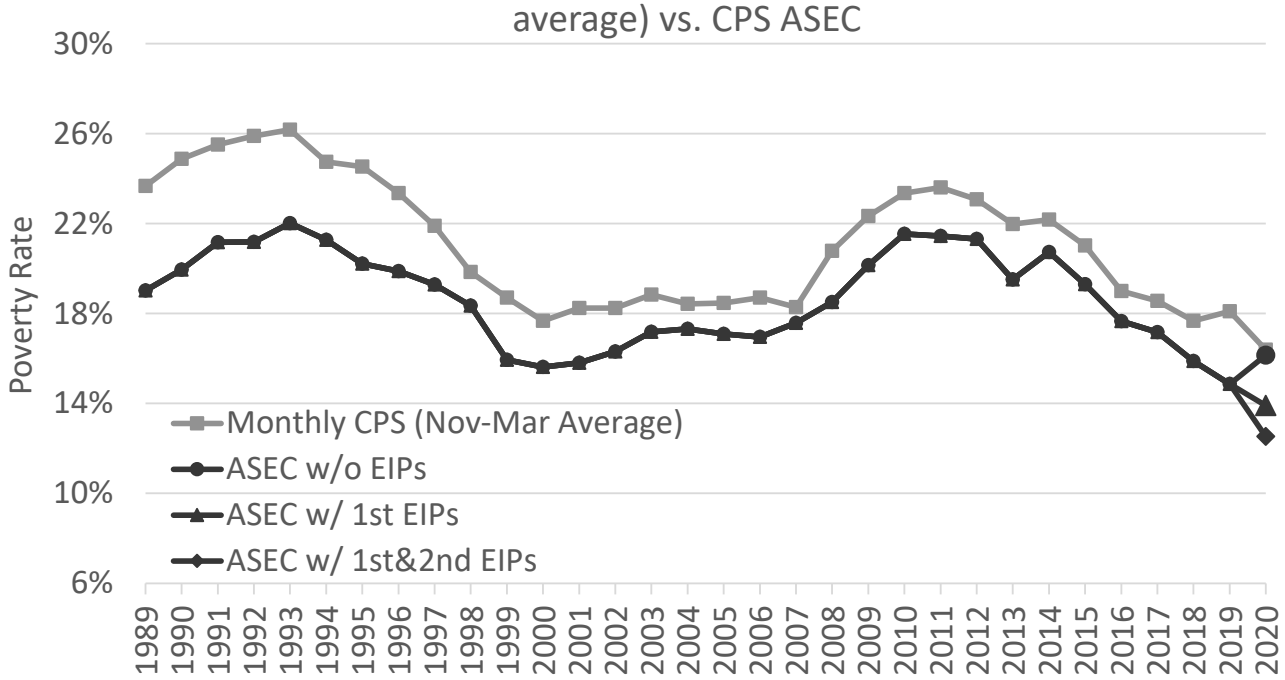
Notes: To determine the poverty status of a family, we compare their income to their official poverty threshold, adjusted monthly for inflation using the monthly CPI. The sample includes individuals who are included in the householders' families and who are in their 1st or 5th month in the Monthly CPS survey. Individuals with imputed income are excluded from the sample. The statistics are weighted using fixed demographic weights since March 2020.

Figure 3. Han et al. Near Real-Time and CPSP Monthly Poverty Rates



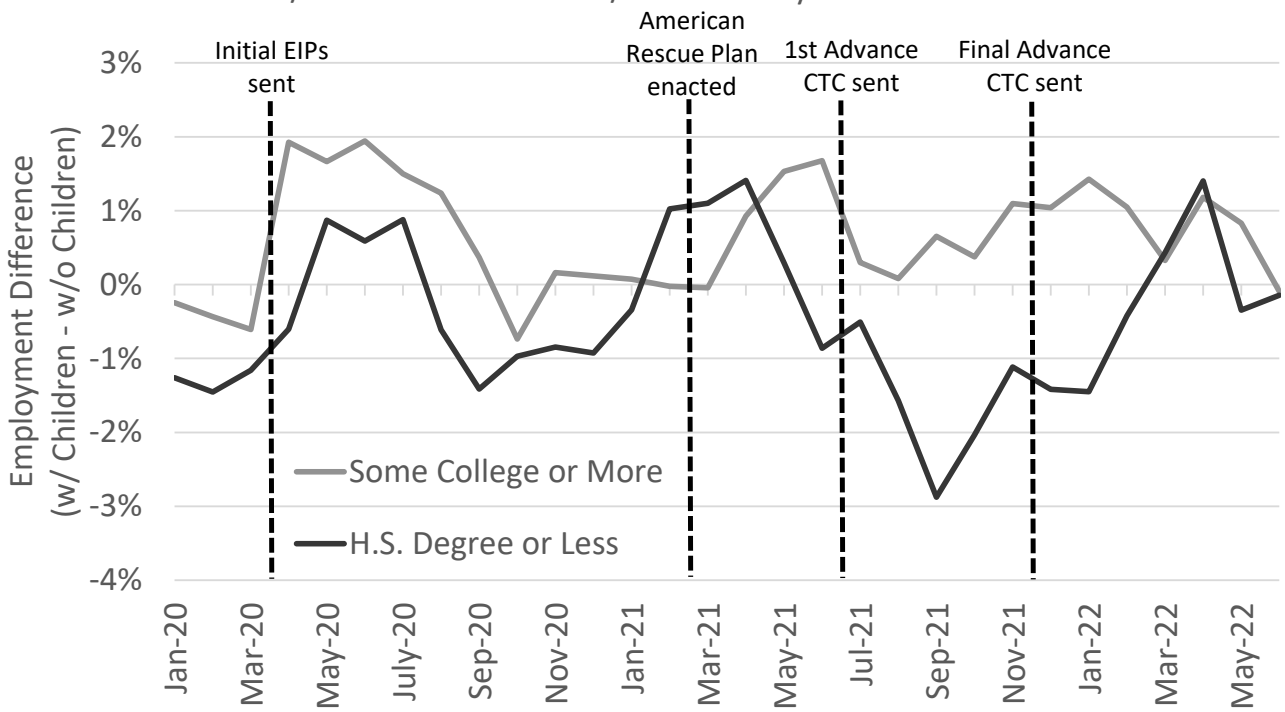
Notes: To determine the poverty status of a family, we compare their income to their official poverty threshold, adjusted monthly for inflation using the monthly CPI. The sample includes individuals who are included in the householders' families and who are in their 1st or 5th month in the Monthly CPS survey. Individuals with imputed income are excluded from the sample. The statistics are weighted using fixed demographic weights since March 2020. The CPSP data are from https://www.povertycenter.columbia.edu/forecasting-monthly-poverty-data

Figure 4. U.S. Child Poverty Rate, Monthly CPS (5-month moving average) vs. CPS ASEC



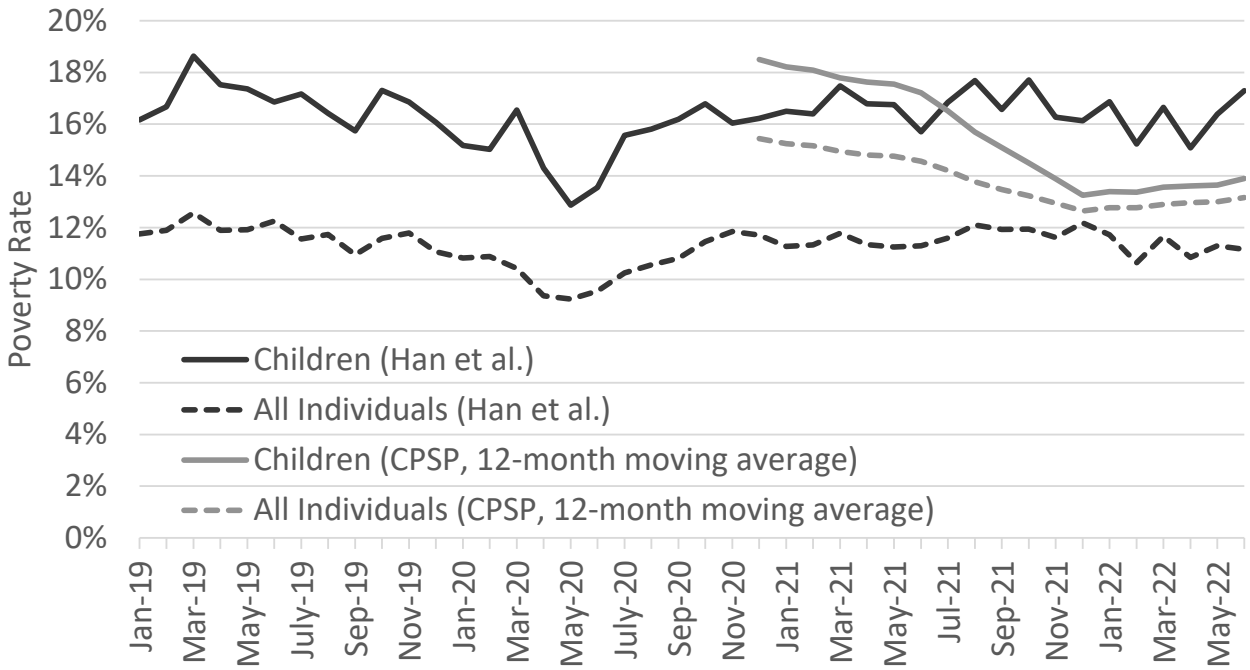
Notes: The Monthly CPS and CPS ASEC samples include individuals who are included in the householders' families. The Monthly CPS sample is restricted to children in households with non-imputed income that are in their 1st or 5th month in the survey. The statistics are weighted using fixed demographic weights since 2019. EIPs are imputed based on methods used in Han, Meyer, and Sullivan (2020).

Figure 5. Employment Rate Difference between Individuals aged 18-54 w/ Children and those w/o Children by Education Level

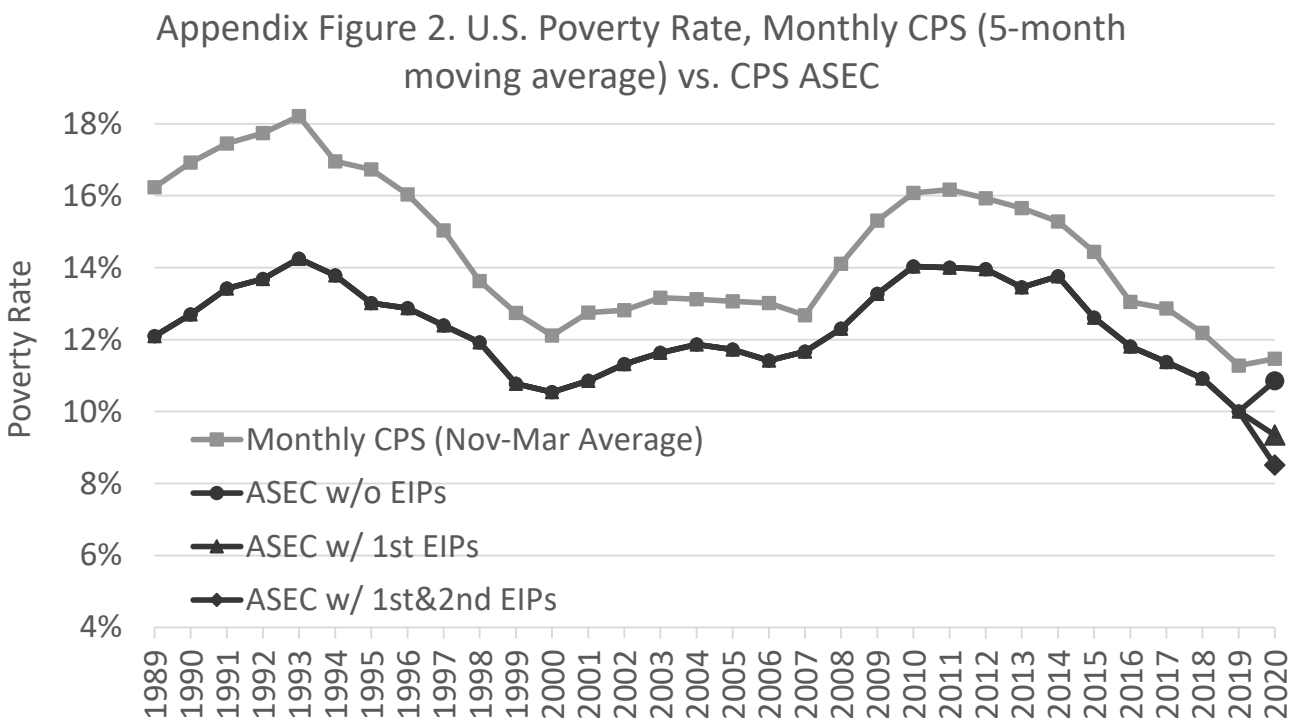


Note: This figure reports the employment rates of adults aged 18-54 with children relative to those without children by education group. The employment data come from the Monthly CPS. The statistics are weighted using survey weights.

Appendix Figure 1. Han et al. Near Real-Time and CPSP Monthly Poverty Rates (12-month moving average)

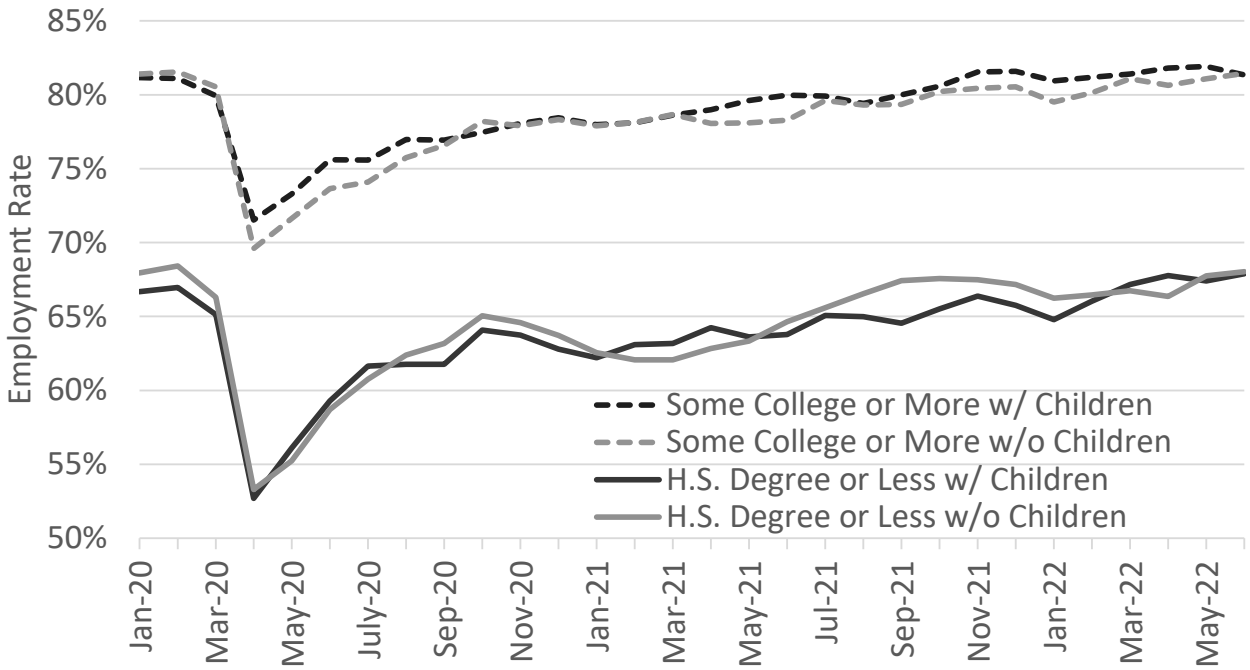


Notes: To determine the poverty status of a family, we compare their income to their official poverty threshold, adjusted monthly for inflation using the monthly CPI. The sample includes individuals who are included in the householders' families and who are in their 1st or 5th month in the Monthly CPS survey. Individuals with imputed income are excluded from the sample. The statistics are weighted using fixed demographic weights since March 2020. The CPSP data are from https://www.povertycenter.columbia.edu/forecasting-monthly-poverty-data



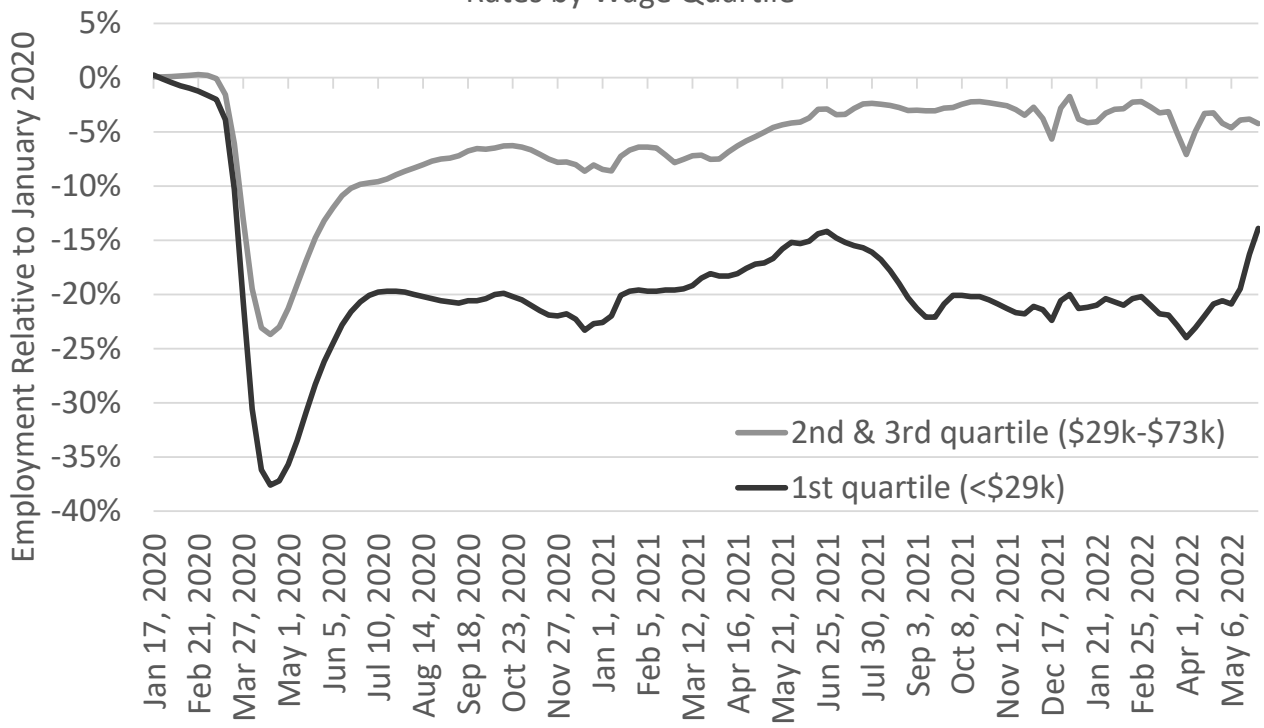
Notes: The Monthly CPS and CPS ASEC samples include individuals who are included in the householders' families. The Monthly CPS sample is restricted to individuals from households with non-imputed income that are in their 1st or 5th month in the survey. The statistics are weighted using fixed demographic weights since 2019. EIPs are imputed based on methods used in Han, Meyer, and Sullivan (2020).

Appendix Figure 3. Employment Rates by Education Level and Presence of Children, Individuals aged 18-54



Note: This figure reports the employment rates of adults aged 18-54 by education level and presence of children in the family. The employment data come from the Monthly CPS. The statistics are weighted using survey weights.

Appendix Figure 4. Opportunity Insights Economic Tracker Employment Rates by Wage Quartile



Notes: This figure reports weekly employment rates from the Economic Tracker at https://tracktherecovery.org. The employment series is constructed using the paycheck data from Paychex and Intuit.