

# INVESTING IN INFANTS: THE LASTING EFFECTS OF CASH TRANSFERS TO NEW FAMILIES\*

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We provide new evidence that cash transfers following the birth of a first child can have large and long-lasting effects on that child's outcomes. We take advantage of the January 1 birthdate cutoff for U.S. child-related tax benefits, which results in families of otherwise similar children receiving substantially different refunds during the first year of life. For the average low-income single-child family in our sample, this difference amounts to roughly \$1,300, or 10% of income. Using the universe of administrative federal tax data in selected years, we show that this transfer in infancy increases young adult earnings by at least 1%–2%, with larger effects for males. These effects show up at earlier ages in terms of improved math and reading test scores and a higher likelihood of high-school graduation. The observed effects on shorter-run parental outcomes suggest that additional liquidity during the critical window following the birth of a first child leads to persistent increases in family income that likely contribute to the downstream effects on children's outcomes. The longer-term effects on child earnings alone are large enough that the transfer pays for itself through subsequent increases in federal income tax revenue. *JEL Codes:* I38, J13, J62.

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## I. INTRODUCTION

One in five children in the United States grows up in poverty. On average, they will have dramatically lower educational attainment and earnings and greater involvement with the criminal justice system than their peers from more affluent families (Black, Devereux, and Salvanes 2011). Recent evidence suggests that the period of early childhood may be particularly important in determining these socioeconomic divides. Indeed, correlational evidence suggests that family income in early childhood is strongly related to later child outcomes (Duncan, Ziol-Guest, and Kalil 2010). However, the inherent difficulties in separating the effects of family resources from other aspects of a child's environment (e.g., parenting style, neighborhood characteristics) have limited our understanding of whether this relationship is causal and, by extension, whether providing additional resources to families will improve social mobility. In this article, we explore whether cash transfers to poor families following the birth of a first child generate improvements in that child's long-run outcomes.

Much evidence indicates large long-term effects of education and health interventions in early childhood, but the effectiveness of cash transfers remains uncertain (Hendren and Sprung-Keyser 2020). Indeed, a recent summary of the literature concludes that "it is premature to advocate income transfer policies as effective policies for promoting child development" (Heckman and Mosso 2014, 23). Perhaps the closest evidence comes from the rollout of the Food Stamp Program during the mid-twentieth century, with several studies suggesting an important role of county-level availability of food stamps during early childhood in influencing subsequent adult outcomes (e.g., Hoynes, Schanzenbach, and Almond 2016; Bailey et al. 2020; Barr and Smith 2021). However, this program differs substantively from pure cash transfers, making it unclear to what extent the results can be generalized, particularly for more recent cohorts. Evidence from more recent evaluations of cash distributions via lottery winnings and casino profit disbursements is mixed and tends to focus on resources provided when children are older (Akee et al. 2010; Cesarini et al. 2016).<sup>1</sup>

1. Several studies have found positive effects of cash assistance from small welfare-to-work experiments (Hill, Yeung, and Duncan 2001; Gennetian and Miller 2002; Clark-Kauffman, Duncan, and Morris 2003; Morris and Gennetian 2003), increases in the generosity of the EITC schedule (Dahl and Lochner 2012; Hoynes, Miller, and Simon 2015; Bastian and Michelsmore 2018), or increased benefits due

We focus on the effect of cash transfers provided during very early childhood. Leveraging eligibility rules for child-related tax benefits received by tens of millions of households each year, we provide new causal estimates of the long-term effects of cash transfers during the first year of life. Using variation affecting relatively recent birth cohorts (people born in the 1980s and 1990s), we estimate effects for an array of educational, behavioral, and labor market outcomes. Because we are using tax-based variation and administrative tax data, we can infer with a relatively high degree of confidence the size of the transfer received. In addition, our use of multiple panel data sources, including the universe of tax data for selected years, allows for a deeper investigation of how these transfers influence (i) the early childhood environment, and (ii) intermediate child outcomes that likely contribute meaningfully to the long-run effects we observe.

We employ a regression discontinuity (RD) design that leverages the U.S. federal tax code's January 1 birthdate eligibility cutoff for the determination of a dependent child. This cutoff results in families of otherwise similar firstborn children receiving substantially different tax-based cash refunds in the following year. In the 1980s and early 1990s, low-income families with a single child born before January 1 could receive additional federal tax benefits from the Earned Income Tax Credit (EITC) and dependent exemption that averaged 10% of their income, and amounted to as much as 20% for some. As a result, families with children born before January 1 experience a significant increase in liquid resources following the birth of their child.

We focus on children born into families who we predict to be income-eligible for the EITC and who will therefore be most affected by the discontinuity in tax benefits. We link these children to their adult tax filings and find substantial increases in adult earnings at the birth date cutoff for additional tax benefits in the first year of life. Overall, we find that children earn 1%–2% more during their twenties per \$1,000 received during infancy. These effects persist to older ages. For the earlier cohorts for which we can observe longer-term outcomes, we continue to estimate substantial increases in earnings at ages 29–31 and

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to a family's location on the EITC schedule (Manoli and Turner 2018). However, these studies are unable to isolate the effect of income from changes in the incentive to work and, except for Bastian and Micheltore (2018), are mainly focused on near-term outcomes.

32–34 of 2%–3% per \$1,000 received during infancy. Per dollar spent, the effects of additional cash provided during infancy on subsequent child earnings are larger than those generated by the Perry Preschool program, a resource-intensive early childhood intervention targeted at low-income families (Heckman et al. 2010).

Additional evidence supports the conclusion that our estimates identify the effects of cash transfer eligibility and are not confounded by changes in the composition of births. Prior work suggests minimal manipulation of the timing of birth around the January 1 discontinuity, particularly for first births among low-income families (Schulkind and Shapiro 2014; LaLumia, Sallee, and Turner 2015). To further circumvent concerns related to the manipulation of birth timing, we exclude a donut of eight days around the January 1 cutoff in our primary specification. We observe no evidence of bunching or differential composition of births once we impose this donut exclusion. Our results are robust to varying the size of the donut, the size of the bandwidth, and the baseline controls that we use to estimate effects. Consistent with the discontinuity at the eligibility threshold reflecting the causal effect of the cash transfer, we find that samples of children whose families faced larger increases in tax benefits at the birthdate cutoff exhibit larger improvements in adult earnings at the cutoff.

We observe larger effects for males, with 2%–3% increases in earnings per additional \$1,000 received during infancy. The larger effects for males may reflect heterogeneity in the effects of cash transfers by sex, consistent with recent work suggesting that the early childhood environment, including the availability of resources, is particularly important for boys (Bertrand and Pan 2013; Autor et al. 2019; Laird, Nielsen, and Nielsen 2020). We see limited evidence of heterogeneity across other margins.

Our estimates of substantial effects of cash transfers provided during infancy on adult earnings are consistent with a growing body of work on the long-run effects of early childhood resources and environments.<sup>2</sup> Even increases in early childhood

2. See recent studies on the long-run effects of food stamp availability (Hoynes, Schanzenbach, and Almond 2016; Bitler and Figinski 2019; Barr and Smith 2021), early childhood education (Ludwig and Miller 2007; Heckman et al. 2010; Campbell et al. 2012; Thompson 2017; Johnson and Jackson 2019; Bailey, Timpe, and Sun 2020; Barr and Gibbs forthcoming; Anders, Barr, and Smith forthcoming), increased access to health insurance (Meyer and Wherry 2012; Brown, Kowalski, and Lurie 2015; Goodman-Bacon 2016), and housing assistance (Chetty, Hendren, and Katz 2016; Chyn 2018).

resources that are a small share of lifetime family resources appear to have a substantial longer-term effect on child outcomes. For example, the direct increase in the net present value of lifetime family income is small from receipt of the tax benefits soon after birth (at most 0.2%), but the increase in annual family income in the first year of life is substantial (more than 10% for many recipients and up to 20% for some). The results suggest that these transfers may provide increased liquidity for families during a critical window for both parents and children. The first year of a child's life may be critical from the perspective of the parent(s) due to the heightened expenses, reduced incomes, and additional stress that comes with the birth of a child. Increased liquidity may provide a cushion for families that allows them to avoid adverse events common to low-income families. It may also produce more general reductions in stress that lead to changes in interactions with children (Milligan and Stabile 2008; Evans and Garthwaite 2014; Schmidt, Shore-Sheppard, and Watson 2021). The effects of these changes may be magnified for young children, as this window may be critical from the perspective of the child due to the importance of this period for cognitive, physical, and socioemotional development (Cunha et al. 2006).

We find evidence that the liquidity increase during the critical period following childbirth appears to result in persistent increases in family income. While these sustained improvements in the childhood environment likely contribute to the positive effects of the cash transfer provided in infancy on a child's long-run outcomes, back-of-the-envelope calculations using existing intergenerational elasticity of earnings estimates suggest that these improvements only account for roughly a third of the observed effects on children.<sup>3</sup>

To better understand how the cash transfer during infancy generates improved adult earnings, we turn to detailed administrative education data from North Carolina to trace the effect through later childhood and adolescence. Given that eligibility for the EITC depends on income, we focus our analysis on children who are ever eligible for free and reduced-price lunch (FRL), a

3. Of course, we cannot rule out larger contributions from the sustained increases in family earnings. Furthermore, it is important to note that our research design does not allow us to separately identify the contribution of specific post-transfer changes in families (e.g., changes in parents' earnings) that may influence a child's eventual earnings.

proxy for likely EITC eligibility. We find that the effect of being born prior to January 1 is a 0.05 standard deviation increase in an index of child outcomes (including math and reading test scores, suspension, and high-school graduation), translating into an effect of 0.03 standard deviations per \$1,000 provided during infancy. The effect represents over 6% of the gap between those eligible for FRL and those who are not. The effects on our summary index are driven by significant increases in third through eighth grade math and reading test scores, reductions in the likelihood of suspension, and increases in the likelihood of high-school graduation. Taken together, the observed human capital effects explain our estimated effects on earnings.

Our results suggest that transfers to poor families may be especially effective after the birth of a child. Perhaps by providing a financial cushion during a period of high stress, these transfers result in persistent increases in family income. In combination, these effects result in improved academic and behavioral outcomes for the child that ultimately result in improved earnings. Following [Hendren and Sprung-Keyser \(2020\)](#), we find that the discounted stream of additional tax receipts associated with these higher earnings in adulthood exceeds the amount of the initial transfer, implying a negative net cost to the federal government and thus an infinite marginal value of public funds.

## II. DATA AND DESCRIPTIVE STATISTICS

We use the administrative tax data housed at the U.S. Census to explore the long-term effects of cash transfers provided during infancy. We observe IRS 1040 data for every filer in the United States in 1979, 1984, 1989, 1994–95, and 1998–2018. We also observe date of birth, sex, and state of birth for nearly every person born in the United States after 1969 from the Social Security Administration (SSA) Numident File. Using information on family composition, income, and exact date of birth, we can trace a person back to their early childhood family environment to calculate the size of the cash transfer available from tax benefits (and how this varied across the January 1 date of birth eligibility threshold).

We use the Numident file to focus on a sample born within one month of January 1 in 1981–82, 1985–86, and 1991–92. We focus on these years because of the availability of 1040 tax information (in 1979, 1984, and 1989), which we use to predict eligibility for the EITC. We link these children with their parents

using any 1040 tax form on which a child (identified by their SSN) is reported as a dependent. To determine likely eligibility for the EITC, we follow the linked parents backward to the closest pre-birth year in which we have the universe of 1040 tax information. We use this information (including whether an individual's parents filed a 1040) to predict adjusted gross income (AGI) during the tax year ending with or just before the birth of a new child.

To illustrate this variation, we can think about a child born in December 1980 or January 1981. We refer to this child as being born in the 1981 recentered birth year. For a child born in the 1981 recentered birth year, we link them to their parental income information from 1979, including whether their parents filed a 1040. We then predict AGI during the 1980 tax year using lagged earnings measures (which are available for the 1979 tax year).<sup>4</sup>

We estimate adult earnings effects for children born into families that we deem income-eligible for the EITC. We use predicted AGI and define eligibility based on the point of EITC phaseout. This introduces some measurement error in our determination of income eligibility for the EITC. Although this may attenuate our estimates somewhat, it further circumvents concerns related to the endogeneity of AGI or filing because we are using prior income information to predict current-year income eligibility. This approach also allows us to conduct additional balance checks and explore subgroup heterogeneity using the information in the tax returns filed prior to birth.<sup>5</sup>

With the parent-child linkage, we can track children from EITC income-eligible households forward to their 1040 tax filings in adulthood and use this information to explore the long-term effects of a cash transfer in infancy on adult earnings outcomes. Our key earnings outcome measure is three-year average earnings, including missing earnings (i.e., nonfilers) as zeroes (Table I).<sup>6</sup> Because our earnings measures are at the level of the filing unit, the estimated effects are at the level of the tax filing unit; this combines the effect on individuals and their

4. See [Online Appendix B](#) for details of AGI predictions.

5. We do not include children born on or around January 1 of 1980, 1985, and 1990 in our primary analysis sample because the differential 1040 filing incentives on either side of the January 1 cutoff could yield a spurious imbalance in baseline covariates simply due to differentially observing family income on either side of the cutoff. Using these years could also raise concerns of endogenous AGI.

6. When there are years in the three-year range where the universe of tax filings are not observed, the remaining observed years are averaged.



TABLE I  
SUMMARY STATISTICS

Panel A: Tax data	
Outcomes	
Earnings (23–25)	\$20,050
Earnings percentile (23–25)	47.22
Earnings (26–28)	\$27,180
Earnings percentile (26–28)	45.90
Baseline	
Family income	\$4,030
Family poverty	0.79
File 1040	0.36
Male	0.50
Predicted AGI	\$12,530
Predicted EITC	\$745.4
Cash transfer in infancy	\$1,291
Observations	625,000
Panel B: North Carolina education data	
Student outcome index	−0.06
Test score index	0.03
HS graduation	0.75
Any suspension	0.20
Black	0.41
Limited English proficiency	0.09
Male	0.52
Observations	44,992

*Notes.* In Panel A, the sample is restricted to individuals born within 28 days of January 1 in 1981–82, 1986–87, and 1991–92 and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding a first birth. Cash transfer in infancy reflects the mean child-related tax benefit eligibility for families in the given group whose child was born prior to January 1. It is constructed using information from prior tax filings to predict AGI in the relevant tax year and then using NBER’s TAXSIM program to determine tax benefit eligibility (see [Online Appendix B](#) for more details). In Panel B, the sample consists of FRL-eligible students born within 28 days of January 1 in 1993–1998 who entered a North Carolina public school by grade 5. Test score index is constructed as the mean of normalized (mean zero, standard deviation one) math and reading test scores in grades 3–8. Student outcome index is constructed as the mean of normalized test scores, high-school graduation, and any suspension in middle or high school. See text for additional details on sample and variable construction. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.

spouses if present. We focus on effects at ages 23–25 and 26–28, for which we can observe all cohorts. We also present effects at ages 29–31 and 32–34 (where available).

II.A. North Carolina Education Data

To better understand the channels through which the earnings effects are operating, we use administrative education data from North Carolina. Critical for our empirical strategy,



these data include students' exact birth dates, among other demographic, behavioral, academic achievement, and attainment information. To best use the set of available outcomes, we focus on students born in the 1993 through 1998 recentered birth years, slightly later than the cohorts available in the tax data.<sup>7</sup> We focus our analysis on children eligible for free and reduced-price lunch (FRL).<sup>8</sup> We use this as a proxy for likely EITC eligibility because the income thresholds for the programs are similar.<sup>9</sup> Among those eligible for FRL based on family income, the rate of EITC eligibility is roughly 75%. After restricting our sample to those FRL students born within 28 days of January 1 (the threshold date in our RD design), but excluding 8 days on either side of January 1, there are 44,992 students in our analytical sample. We construct our key measures of aptitude using mean normalized math and reading test scores from grades 3 through 8. These scores are normalized to have a mean of zero and a standard deviation of one within grade and year. We also construct a measure of behavioral issues, an indicator variable equal to 1 if an individual is ever observed as suspended in middle or high school. Our third key measure is high-school graduation.

To draw general conclusions about the effect of cash transfers, we combine our measures of aptitude, behavior, and educational attainment into an index following Kling, Liebman, and Katz (2007). The aggregation improves statistical power to detect effects that go in the same direction. We construct our index using a weighted average of the  $z$ -scores of its components, with the sign of each measure oriented such that the beneficial outcomes have higher scores than the adverse outcomes (e.g., a decrease in suspensions would contribute to an increase in the index). The  $z$ -scores are generated by subtracting off the control group mean and dividing by the control group standard deviation.<sup>10</sup>

7. We choose these cohorts as they are early enough to observe high-school graduation outcomes and late enough to observe FRL status in middle school or earlier (FRL status is not available in the data prior to 2006).

8. We include any student that we ever observe as FRL eligible in this category. We see no evidence that the cash transfer affects the likelihood of being included in this sample.

9. For example, for a family of three with one child in 2000, the income cutoffs for eligibility were \$25,600 (FRL) and \$27,400 (EITC).

10. Adapting Kling, Liebman, and Katz (2007) to our context, we impute missing index component values using the below- or above-cutoff mean. This results in differences between below- and above-cutoff means of an index being the same

Consistent with the lower levels of resources available to them, children eligible for FRL are 0.51 standard deviations worse off on an index of academic and behavioral outcomes. These differences are driven by large differences in math and reading test scores (0.71 std. dev.), rates of suspension (0.12), and rates of high-school graduation (0.15).

### III. EMPIRICAL STRATEGY

To obtain an estimate of the causal effect of additional resources in early childhood, we take advantage of a natural experiment that resulted in the families of otherwise similar children receiving substantially different child-related tax benefits in their child's first year of life. The families of children who are born on December 31 are eligible to receive substantial increases in tax benefits in the following year, whereas the families of children who are born on January 1 are not eligible for these benefits for an additional year.<sup>11</sup> This source of variation allows us to examine the effect of a pure cash transfer rather than one that is coupled with changes to work incentives, and it directs our focus to changes in family resources during very early childhood.

#### *III.A. Increases in Resources during Infancy*

During our sample period, these changes in resources come primarily via the EITC and, to a lesser extent, the dependent

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as the average of below- and above-cutoff means of the components of that index (when the components are divided by their group standard deviation and have no missing value imputation), so that the index can be interpreted as the average of results for separate measures scaled to standard deviation units. [Online Appendix](#) Table A.XI shows that results are robust to alternate approaches to handling missing components, namely, reweighting the index using only observed components and using only students where all components are observed.

11. This source of variation has been used previously by [Schulkind and Shapiro \(2014\)](#) to examine effects on C-section birth timings and health consequences for infants, [LaLumia, Sallee, and Turner \(2015\)](#) to examine effects of birth timing and tax reporting, [Meckel \(2015\)](#) to examine effects on birth spacing, [Wingender and LaLumia \(2016\)](#) to examine effects on maternal labor supply, and [Jones \(2013\)](#) to examine effects on number of hours worked by single mothers already in the labor market. In a new working paper, [Cole \(2021\)](#) uses a similar strategy with data from the American Community Survey (ACS), finding that children in families eligible for child-related tax benefits (stemming somewhat from the EITC, but also significantly from the dependent exemption due to the focus on all families) are more likely to be on grade for their age.

exemption.<sup>12</sup> Initially intended to be a modest tax credit that provided assistance to low-income working families with children, the EITC has grown into one of the federal government's largest antipoverty programs. During our sample period in particular, the maximum EITC credit grew significantly while income eligibility requirements were also relaxed. In addition to the EITC, the birth of a child during this period generates a dependent exemption that allows families to reduce their taxable income.

Because a child only counts for tax purposes if they were born during the tax year, some children whose families look the same on average receive a cash transfer based entirely on the luck of being born slightly earlier.<sup>13</sup> To obtain an understanding of the magnitude of these benefits, we use information from prior tax filings combined with the National Bureau of Economic Research (NBER)'s TAXSIM program. Specifically, we use the information available to us from taxes filed in the year or two prior to birth to predict AGI for the relevant tax year. We use this prediction, combined with information on marital status and number of dependents, to recover the taxes owed and credits due to each family when claiming one child compared with no children.<sup>14</sup> We calculate this difference for every child's family in our sample. [Figure I](#) illustrates that the average tax benefit provided by a child is around \$1,300 for the full sample, with little change in the implied transfer during infancy as we move across dates of birth until we reach the January 1 threshold, when it drops to \$0.<sup>15</sup> This creates an increase in family resources during infancy for those children born to the left of the threshold.

12. Where relevant for scaling our effects into dollar terms, we also take into account variation in additional child-based benefits, such as those provided by the head of household filing status for single filers and the childcare tax credit. The child tax credit, beginning in 1998, isn't available for most of our cohorts and outcomes.

13. During this period, 86% of all tax refunds were received by May ([Souleles 1999](#)), though EITC recipients typically filed earlier and therefore likely received their refunds earlier than the average taxpayer ([Slemrod et al. 1997](#)).

14. See [Online Appendix B](#) for additional detail and discussion of how we use a similar strategy to estimate the implied discontinuity in child-related benefits during infancy in our sample of North Carolina students.

15. In [Online Appendix Figure A.III](#), we illustrate the average value of an additional dependent child on both sides of the threshold. This figure provides further evidence of limited manipulation of the timing of birth to take advantage of tax benefits. If this type of manipulation were occurring, we would expect to see individuals with greater potential gains manipulating their birth timing to the left of the threshold.

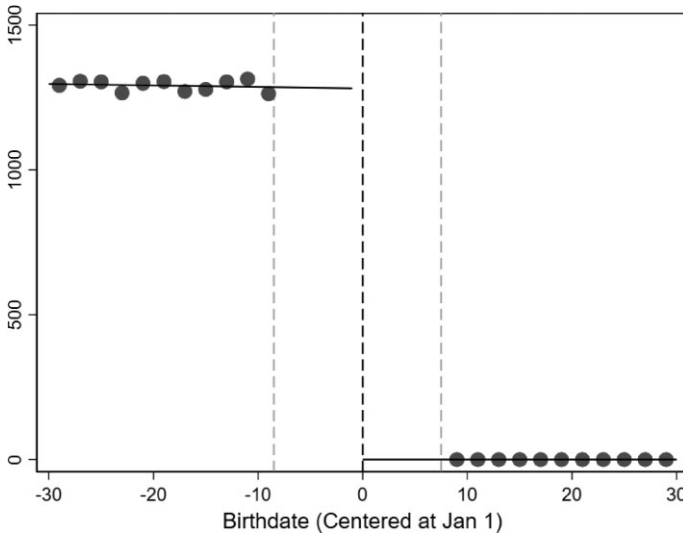


FIGURE I

Effect of Cash Transfer Eligibility on Additional Resources Received during Infancy

The figure displays the mean cash transfer in infancy by two-day birthdate bin for firstborn children who were born within 28 days of January 1 in 1981–82, 1986–87, and 1991–92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. Cash transfer in infancy reflects the child-related tax benefit eligibility for families whose child was born prior to January 1. It is constructed using information from prior tax filings to predict income in the relevant tax year and then using NBER's TAXSIM program to determine tax benefit eligibility (see [Online Appendix B](#) for more details). The horizontal axis represents days relative to the January 1 birthdate cutoff. Birthdates to the left of the dotted line represent those where the child's family could have received additional resources from child-related tax benefits in the following year. See [Table I](#) and text for additional sample restrictions and information on variable construction. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.

This measure provides a reasonable indication of the size of the average transfer during infancy, but significant uncertainty remains. We discuss the factors that contribute to this uncertainty in depth in [Online Appendix B](#) and draw three key conclusions. First, misclassification of dependents and incomplete filing and take-up of the EITC prompt us to view our estimated average increases as likely overestimates of the size of the actual average increase in resources experienced during infancy. Correspondingly, our estimated effects on outcomes per \$1,000

should be viewed as lower bounds. Second, due to greater rates of dependent misclassification and lower rates of EITC take-up, the extent of upward bias in the estimated size of the cash transfer during infancy is likely greater during the earlier years of our sample. In combination with the greater concerns about forecast error during these years, this also leads us to have somewhat greater confidence in our estimates of the implied transfer for the later cohorts. Finally, to the extent that forecast error, misclassification error, or take-up varies across subgroups, there may be meaningful differences in the extent of uncertainty or upward bias related to our estimates of the implied increase in resources during infancy. For example, we would expect lower forecast error for those for whom we have better information (previous filers). We are attentive to these differences as we discuss the results.

### III.B. Main Specification

Our primary empirical model is a regression discontinuity (RD) design that leverages this sudden increase in resources in the first year of life at the January 1 birthdate cutoff to identify the causal effect of early childhood transfers on later outcomes of interest, such as test scores, suspensions, high-school graduation, employment, and earnings. Our basic model is as follows:

$$(1) \quad Y_{it} = \beta_0 + \beta_1 1[z_i < 0] + \beta_2 z_i + \beta_3 1[z_i < 0] \times z_i + \theta_t + \epsilon_{it},$$

where  $Y_{it}$  is an outcome of interest (such as test scores or earnings) for child  $i$  born in recentered birth year  $t$ . Recentered birth year  $t$  includes children born in the days surrounding January 1 of year  $t$ . The “assignment” variable  $z_i$  is the difference between child  $i$ ’s birthdate and January 1 ( $z_i$  is 0 for children born on January 1).  $1[z_i < 0]$  is an eligibility indicator equal to 1 if child  $i$  is born prior to January 1.  $\theta_t$  are recentered birth year fixed effects. The primary coefficient of interest is  $\beta_1$ , which identifies the effect of likely eligibility for child-related tax benefits among low-income families, rather than the effect of changes in actual income.<sup>16</sup> This is an intent to treat (ITT) parameter. While we

16. In the tax data, we use predicted AGI to restrict to children born into families with AGI below the EITC phaseout maximum. In the North Carolina education data, we use FRL status to proxy for likely eligibility.

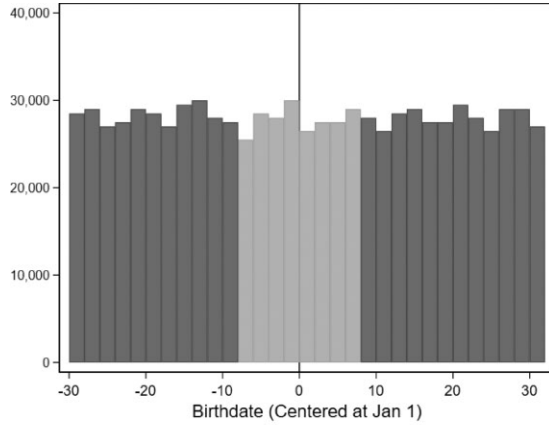
produce estimates of the associated “first-stage” cash transfer during infancy, there is some uncertainty in these estimates. We revisit the discussion of these complications and the associated scaling of our ITT parameter in [Section IV](#).

### *III.C. Evaluating the RD Assumptions*

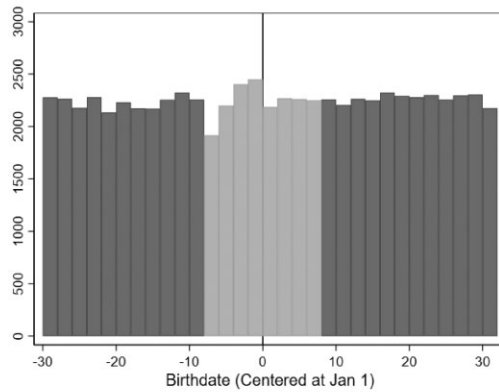
The major assumption underlying the RD design is that treatment assignment is “as good as random” at the threshold for treatment. In our context then, the assumption is that children born just before and just after the January 1 cutoff are the same (on average) in any way that is related to the outcome of interest. It would be a concern, for example, if families were precisely manipulating the date of birth of their children (perhaps to take advantage of the tax credit). If this were the case, unobservable characteristics associated with the decision to give birth prior to January 1 might be responsible for differences in child outcomes rather than the transfer generated by eligibility for child-related tax benefits.

We see little evidence of this type of manipulation in the cohorts in our sample. [Figure II](#) displays the density of birthdates around the January 1 cutoff, plotted separately for all income-eligible first births in the tax data and FRL students in the North Carolina data. As seen in Panel A, the distribution of birth dates among those income-eligible for the EITC is largely smooth. Panel B similarly indicates minimal levels of birth timing manipulation in the North Carolina data. These results are consistent with previous studies, which have found little to no effect of incentives on birth timing around the New Year, particularly for first births ([Schulkind and Shapiro 2014](#); [LaLumia, Sallee, and Turner 2015](#)). Nevertheless, we also follow an approach common in the literature and estimate donut hole RDs, dropping the observations around the January 1 threshold (shaded in gray), to address this concern.

Another conventional test of the RD identifying assumption that we use is to explore whether predetermined characteristics are balanced across the threshold for treatment, analogous to a balancing test in the context of a randomized control trial. Consistent with conditionally random assignment, we find no significant differences around the birthdate cutoff in child sex, race, or ethnicity or prebirth parent characteristics such as marital status, parent age, whether the parent filed a 1040, or the predicted



(A) Tax Data (EITC-Eligible Families)



(B) North Carolina Data (FRL-Eligible Students)

FIGURE II

### Distribution of Birthdates by Sample

Panel A displays the distribution of birthdates (relative to January 1) for first-born children who were born within 28 days of January 1 in 1981–82, 1986–87, and 1991–92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. Due to disclosure concerns, the number of observations in each two-day bin is rounded to the nearest 500. Panel B displays the distribution of birthdates (relative to January 1) for ever-FRL-eligible students born within 28 days of January 1 in 1993–1998 who entered a North Carolina public school by grade 5. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.



AGI of the parent ([Table II](#) and [Online Appendix Figure A.II](#)).<sup>17</sup> In addition, as place of residence is strongly correlated with child outcomes ([Chetty et al. 2014a](#)), we link each child's county of birth to county characteristics in 1980 and find no evidence of imbalance on these measures either ([Online Appendix Table A.I](#)).<sup>18</sup>

Another potential concern is that our treatment is confounded by other treatments that change discontinuously across the January 1 threshold. The only such treatments of which we are aware are school starting ages in some states and years (not North Carolina).<sup>19</sup> To circumvent this confound, we exclude from our analysis births in states where the school age cutoff date falls within our bandwidth around January 1 (or is determined at the district level) at any point during our sample.<sup>20</sup> To further address concerns about other treatments changing discontinuously across the threshold, we take advantage of variation in the size of the transfer across subgroups. First, the generosity of child tax benefits and EITC take-up rates increased significantly between 1980 and 1990. We compare outcome effect estimates across birth cohorts, with the expectation that the RD effects should be larger for later cohorts. Second, we estimate our basic RD specification among individuals with varying magnitudes of discontinuities in the size of the cash transfer across the January 1 threshold due to differences in baseline income or eligibility.

17. We conduct similar covariate balance exercises to [Table II](#) using the North Carolina data and find no significant differences in race, gender, or limited English proficiency (LEP) status in the donuted sample of FRL students ([Online Appendix Table A.III](#)).

18. While not necessarily problematic for our RD strategy (which merely relies on continuity at the cutoff), some prior evidence suggests differences in the average characteristics of parents giving birth in December versus January ([Buckles and Hungerman 2013](#)). We see no evidence of these average differences in our low-income firstborn sample ([Online Appendix Table A.II](#)). We similarly see no evidence of discontinuities across other month pairs for which Buckles and Hungerman demonstrate meaningful mean differences ([Online Appendix Table A.IV](#)).

19. The safety net programs for which having a child affects eligibility (e.g., Food Stamps, Aid to Families with Dependent Children, and Special Supplemental Nutrition Program for Women, Infants, and Children) use point-in-time presence of a dependent to determine eligibility so being born just before versus just after January 1 should not influence eligibility for these programs.

20. This leads us to drop states amounting to 8% of the 1980 U.S. population (CT, CO, DE, DC, LA, MA, MD, NJ, RI, VA, and VT).

TABLE II  
BALANCE ON BASELINE CHARACTERISTICS

	Child male (1)	Child white (2)	Child black (3)	Child Hispanic (4)	Parent max. age (5)	Parent filed 1040 (6)	Parent married (7)	Parent pred. AGI (8)	Parent in poverty (9)
Born before Jan 1	0.005 (0.004)	0.005 (0.006)	− 0.003 (0.003)	− 0.00 (0.005)	0.04 (0.07)	0.002 (0.003)	− 0.003 (0.003)	19.35 (71.67)	0.001 (0.003)
Mean	0.501	0.630	0.134	0.174	24.06	0.364	0.048	12,530	0.788

Notes. Each cell shows the basic regression discontinuity estimate ( $\beta_1$  from [equation \(1\)](#)) from a separate regression where the column denotes the baseline characteristic serving as the dependent variable. Parent/family variables are constructed from prebirth filing information. See the text for additional details on variable construction and sample restrictions. The sample is restricted to firstborn children who were born within 28 days of January 1 in 1981–82, 1986–87, and 1991–92 and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. All regressions exclude observations within an eight-day donut of the January 1 cutoff. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010. Significance levels are indicated by: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

TABLE III  
EFFECT OF CASH TRANSFER ELIGIBILITY ON ADULT EARNINGS

	(1)	(2)	(3)
Earnings (23–25)	318.9** (153.0)	293.0** (152.6)	295.2** (150.1)
Mean	20,050	20,050	20,050
Earnings (26–28)	455.6** (198.4)	429.7** (201.0)	433.4** (198.4)
Mean	27,180	27,180	27,180
Cash transfer in infancy	1,291	1,291	1,291
Observations	625,000	625,000	625,000
Recentered birth year fixed effects	X	X	X
Demographic controls		X	X
Parent predicted AGI control			X

Notes. Each cell shows the regression discontinuity estimate ( $\beta_1$  from [equation \(1\)](#)) from a separate regression where the row denotes the outcome variable. The earnings outcome is constructed as the three-year average of earnings (including nonfilers as zeroes) at the filing unit level. The sample is restricted to firstborn children who were born within 28 days of January 1 in 1981–82, 1986–87, and 1991–92 and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. All regressions exclude observations within an eight-day donut of the January 1 cutoff. Cash transfer in infancy reflects the mean child-related tax benefit eligibility for families in the given group whose child was born prior to January 1. It is constructed using information from prior tax filings to predict AGI in the relevant tax year and then using NBER’s TAXSIM program to determine tax benefit eligibility (see [Online Appendix B](#) for more details). See the text for additional details on variable construction and sample restrictions. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010. Significance levels are indicated by: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

IV. RESULTS: ADULT OUTCOMES

We use the tax data to explore long-run effects on adult earnings. Our baseline estimates in [Table III](#) indicate that eligibility for additional resources during the first year of life generates a \$319 increase in average annual earnings between age 23 and 25 and a \$456 increase between ages 26 and 28.<sup>21</sup> These level effects correspond to around a 1.6%–1.7% increase in average earnings. The average estimated increase in child-related tax benefits during infancy for a child in this sample is \$1,291. The implied effect on earnings at age 23 to 25 is roughly 1.2%–1.3% per \$1,000 provided during infancy. Given that the estimated

21. We see similar effects for percentile earnings, with an increase of 0.33–0.47 percentiles in a birth cohort ([Online Appendix Table A.V](#) and [Online Appendix Figure A.VII](#)). [Online Appendix Table A.VII](#) demonstrates no effect of a cash transfer in infancy on the likelihood of being married as an adult, implying that these results are not driven by changes in household formation.

increases in cash transfers during infancy are overestimates, we view the implied effect of 1.2%–1.3% per \$1,000 as a lower bound.

The results are robust to the inclusion of demographic (parent age and child sex) and parent predicted-AGI controls. [Figure III](#) illustrates these results graphically, showing a clear jump down as we move across the eligibility threshold.<sup>22</sup>

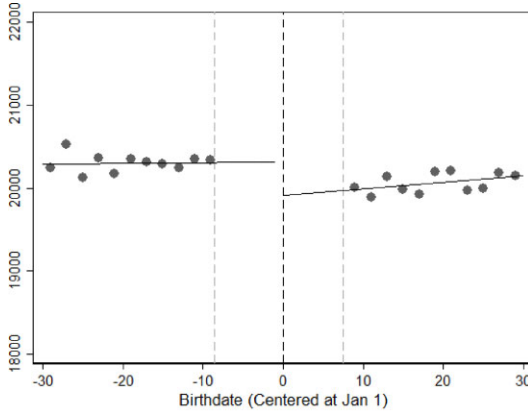
[Online Appendix Figure A.V](#) shows how the basic RD estimate ( $\beta_1$ ) varies by donut size. The estimates are generally similar across donut size. The slight exception is a donut size of around four or five, which includes the negatively selected set of individuals who were born on or just after Christmas on the left-hand side of the discontinuity, pulling the slope down and negatively biasing the estimate of  $\beta_1$ .<sup>23</sup> [Online Appendix Figure A.VI](#) shows that the estimates are similarly robust to different window sizes, with generally larger but less precise estimates with smaller windows.

[Table IV](#) illustrates how the results vary across cohorts. The pattern of estimates is consistent with the differences across cohorts in the magnitude of the increase in resources at the birthdate threshold. The largest effects are for individuals born in the 1991 and 1992 recentered birth years, when the additional transfer provided during infancy is \$1,808, nearly twice the benefit in the earlier cohorts. Given the aforementioned influence of misclassification and incomplete take-up on the increase in transfers actually received during infancy, this difference in the size of the transfer discontinuity across birth cohorts underestimates the true difference. Specifically, estimated EITC take-up over this time period appears to have increased by at least 20%.<sup>24</sup> For individuals born in the 1991 and 1992 recentered birth years, the effect on earnings is around \$665 per year (3.4%) at age 23 to 25 and \$687 (2.6%) at age 26 to 28. Scaled to the effect per \$1,000 of increased resources during infancy, the effects across

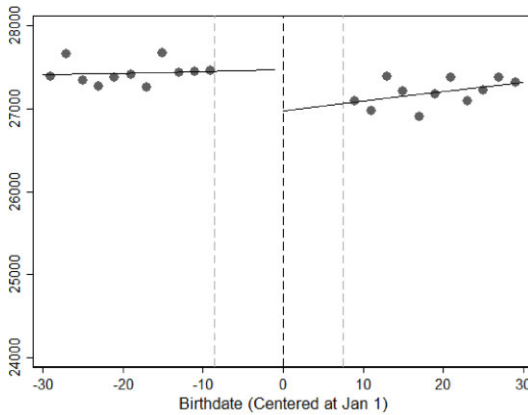
22. [Online Appendix Figure A.IV](#) provides the same graphical evidence without the donuts.

23. There is a modest level of manipulation of birth timing to avoid giving birth on Christmas that could be a result of parent or practitioner preferences. This retiming appears to result in families with worse than average expected outcomes having children born on Christmas or the day or two after.

24. [Scholz \(1990\)](#) provides a central estimate of EITC take-up in the mid-1980s of 70%, while [Scholz \(1994\)](#) estimates take-up of 80.5–86.4% in 1990 (we use the midpoint of this range in our calculation). That said, there is significant uncertainty in these estimates so the increase in take-up may have been significantly higher.



(A) Earnings (23 to 25)



(B) Earnings (26 to 28)

FIGURE III

### Effect of Cash Transfer Eligibility on Adult Earnings

The figure displays mean earnings by two-day birthdate bin for firstborn children who were born within 28 days of January 1 in 1981–82, 1986–87, and 1991–92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. The earnings outcome is constructed as the three-year average of earnings (including nonfilers as zeroes) at the filing unit level. The horizontal axis represents days relative to the January 1 birthdate cutoff. Birthdates to the left of the dotted line represent those where the child's family could have received additional resources from child-related tax benefits in the following year (if eligible based on income). See [Table I](#) and text for additional sample restrictions and information on variable construction. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.

TABLE IV  
EFFECT OF CASH TRANSFER ELIGIBILITY ON ADULT EARNINGS BY COHORT

	1981–82 (1)	1986–87 (2)	1991–92 (3)	All (4)
Earnings (23–25)	103.9 (283.3)	103.9 (259.5)	665.5*** (257.5)	318.9** (153.0)
Mean	21,590	18,910	19,830	20,050
Earnings (26–28)	134.3 (408.2)	475.2 (367.5)	687.3* (372.0)	455.6** (198.4)
Mean	27,750	27,110	26,800	27,180
Cash transfer in infancy	981	954	1,808	1,291
Observations	184,000	202,000	240,000	625,000

*Notes.* Each cell shows the basic regression discontinuity estimate ( $\beta_1$  from [equation \(1\)](#)) from a separate regression where the row denotes the outcome variable. The earnings outcome is constructed as the three-year average of earnings (including nonfilers as zeroes) at the filing unit level. Each column indicates the set of recentered birth years included. The sample is restricted to firstborn children who were born within 28 days of January 1 in the given recentered birth years and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. All regressions exclude observations within an eight-day donut of the January 1 cutoff. Cash transfer in infancy reflects the mean child-related tax benefit eligibility for families in the given group whose child was born prior to January 1. It is constructed using information from prior tax filings to predict AGI in the relevant tax year and then using NBER's TAXSIM program to determine tax benefit eligibility (see [Online Appendix B](#) for more details). See the text for additional details on variable construction and sample restrictions. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010. Significance levels are indicated by: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

1981–82, 1986–87, and 1991–92 cohorts are \$106, \$109, and \$368 at age 23 to 25 and \$137, \$498, and \$380 at age 26 to 28. These scaled estimates do not adjust for differences in child-related tax benefit take-up. Adjusting the implied cash transfer discontinuity estimates for the differential take-up across cohorts tends to bring these scaled estimates closer together. The remaining statistically indistinguishable differences in effect sizes may result from the temporal point of measurement (e.g., at ages 26–28, the 1981–82 cohort is observed primarily during the Great Recession). The differences shrink further in magnitude at older ages (see discussion in [Section V.A](#)).

[Online Appendix Table A.VI](#) provides additional evidence that the size of the discontinuity tracks the size of the cash transfer experienced in the first year after birth. The table contains analogous estimates for nonfirstborn children in low-income families. The discontinuities are much smaller than among

firstborn children.<sup>25</sup> For the full set of cohorts, the estimated additional transfer during infancy is \$1,291 for firstborn children, but only \$306 for nonfirstborn children. As the income of prior-filing parents of nonfirstborn children is substantially higher than parents of firstborn children, the difference in tax benefits understates the difference in the relative importance of these resources to the two groups. As a result, if our estimates for the firstborn children are driven by increases in the cash transfer provided during the first year of life, we would expect to see smaller effects for nonfirstborn children and no pattern of larger effects for later cohorts of nonfirstborn children as the generosity of the EITC increased across birth cohorts of first children. The estimates are somewhat less precise than for first children, but we see little evidence of positive effects for nonfirstborn children and no pattern of larger effects in recent cohorts.

Given that we measure earnings at the filing unit rather than the individual level, we expect to observe a stronger signal of individual earnings (or potential earnings) for males than for females. This is due to the higher likelihood that women are married filing jointly and the lower relative share of earnings that women account for in married households filing jointly at the ages that we observe.<sup>26</sup> In [Table V](#), we explore how the results vary by child gender. The effects of transfers provided during infancy appear to be much larger among men, though substantial effects for women cannot be ruled out. We estimate earnings effects for men of \$560 per year between ages 23 and 25 and \$782 per year between ages 26 and 28. These level effects correspond to increases of roughly 3% of the mean. Scaling by the discontinuity in the size of the transfer implies an increase in earnings of 2.3% per \$1,000 provided in infancy. Graphical evidence of these effects is provided in [Figure IV](#).<sup>27</sup> While these results are consistent with observing a stronger signal of male earnings, it is also possible that there is heterogeneity in the effects of transfers by sex. Indeed, we

25. The form of the additional transfer also differs between these two groups. The majority of the additional transfer for firstborn children comes as a refundable tax credit, whereas the additional transfer for nonfirstborn children comes primarily as a reduction in the family's tax burden from an additional exemption.

26. [Online Appendix Table A.VII](#) illustrates the difference in mean marriage rates by age and gender and demonstrates that there is no effect of a cash transfer in infancy on the likelihood that an individual is married as an adult.

27. RD plots by gender for percentile wages are in [Online Appendix Figure A.VIII](#).



TABLE V  
HETEROGENEITY IN THE EFFECT OF CASH TRANSFER ELIGIBILITY ON ADULT EARNINGS

	All child (1)	Female child (2)	Male child (3)	Single parent (4)	Married parent (5)	Filer parent (6)	Nonfiler parent (7)
Earnings (23–25)	318.9** (153.0)	110.6 (165.4)	559.6*** (209.7)	571.4* (329.1)	–229.1 (559.9)	451.2 (294.5)	228.7 (155.7)
Mean	20,050	21,280	18,830	21,790	24,400	22,140	18,860
Earnings (26–28)	455.6** (198.4)	168.3 (210.9)	781.9*** (293.7)	976.3** (423.2)	–1,086.0 (1,083.0)	676.3 (413.8)	307.3 (232.9)
Mean	27,180	28,940	25,440	29,200	33,330	29,750	25,710
Cash transfer in infancy	1,291	1,291	1,291	1,737	1,112	1,663	1,081
Observations	625,000	312,000	313,000	196,000	31,000	227,000	398,000

Notes. Each cell shows the basic regression discontinuity estimate ( $\beta_1$  from equation (1)) from a separate regression where the row denotes the outcome variable and the column denotes the subsample. The earnings outcome is constructed as the three-year average of earnings (including nonfilers as zeroes) at the filing unit level. Parent/family variables are constructed from prebirth filing information. The sample is restricted to individuals meeting the given subsample criteria, who were firstborn children who were born within 28 days of January 1 in 1981–82, 1986–87, and 1991–92 and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. All regressions exclude observations within an eight-day donut of the January 1 cutoff. Cash transfer in infancy reflects the mean child-related tax benefit eligibility for families in the given group whose child was born prior to January 1. It is constructed using information from prior tax filings to predict AGI in the relevant tax year and then using NBER's TAXSIM program to determine tax benefit eligibility (see Online Appendix B for more details). See the text for additional details on variable construction and sample restrictions. Parent information (i.e., married, single, filer, nonfiler) was derived from prebirth tax filing information (see Online Appendix B for more information). Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010. Significance levels are indicated by: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

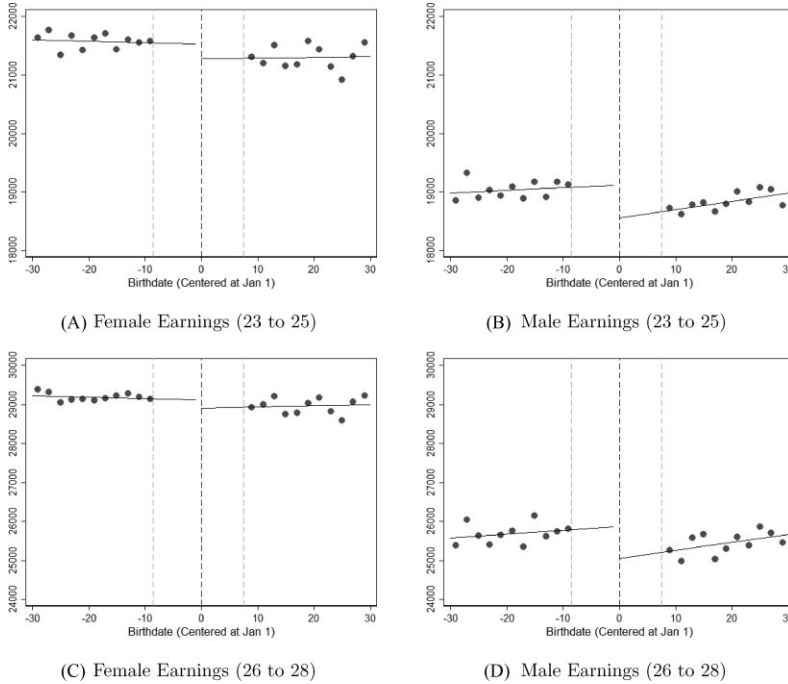


FIGURE IV

Heterogeneity by Sex in the Effect of Cash Transfer Eligibility on Adult Earnings

The figure displays mean earnings by two-day birthdate bin for firstborn children who were born within 28 days of January 1 in 1981–82, 1986–87, and 1991–92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. The earnings outcome is constructed as the three-year average of earnings (including nonfilers as zeroes) at the filing unit level. The horizontal axis represents days relative to the January 1 birthdate cutoff. Birthdates to the left of the dotted line represent those where the child’s family could have received additional resources from child-related tax benefits in the following year (if eligible based on income). See Table I and text for additional sample restrictions and information on variable construction. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.

estimate larger earnings effects for single men than single women, although these results are subject to several caveats regarding potential selection into marriage and heterogeneity in effects between single and married individuals.<sup>28</sup> This type of

28. We estimate null effects for single women across ages and significant 2%–3% effects for single men.

heterogeneity would be consistent with recent work that suggests that the early childhood environment is particularly important for boys (Bertrand and Pan 2013; Autor et al. 2019; Laird, Nielsen, and Nielsen 2020). However, as we discuss in Section VI, we see limited evidence of such heterogeneity when exploring effects on earlier outcomes. Examining heterogeneity in effects across other characteristics, we find that it is generally consistent with larger effects among subgroups with larger predicted transfers (e.g., parents who filed prior to birth and single parents).

Online Appendix Table A.VIII shows the estimates by cohort and gender. The effects are most apparent for men, with the same pattern of larger effects for those cohorts born later. Indeed, the pattern of earnings effects maps closely to the pattern of increased transfers in infancy across birth cohorts. The implied effects per \$1,000 across cohorts are \$531, \$528, and \$672 (or 2.08%, 2.08%, and 2.07%) at ages 26–28, although we note again that the effects for earlier cohorts are likely biased downward. The estimates are noisier for women, which may be a result of the weaker signal of individual earnings for this group.

In Table VI, we present the results separately by race and ethnicity. Among males, where we observe the strongest earnings signal, we see somewhat larger point estimates for black and non-Hispanic white individuals than Hispanic individuals. This is also true for the later birth cohorts for which the transfers were significantly larger. The differences across groups are consistent with the significantly lower rates of awareness and take up of the EITC among the Hispanic population.

#### IV.A. *Do the Effects Represent Likely Shifts in Permanent Income?*

A natural question is the extent to which the observed effects at ages 23–28 are a reliable signal of increases in permanent income. Earnings, particularly as measured in a single year, are highly variable during the early twenties. That said, prior work suggests that earnings by age 26, particularly when using multiyear average measures as we do, are strongly predictive of future earnings (Haider and Solon 2006; Chetty et al. 2011).<sup>29</sup> As

29. We have estimated similar correlations (using the earliest cohorts) and they are even higher than those reported in Chetty et al. (2011), implying that earnings during the mid- and late twenties are even more predictive of future earnings in our data (Online Appendix Figure A.IX).

TABLE VI  
HETEROGENEITY BY RACE/ETHNICITY IN THE EFFECT OF CASH TRANSFER ELIGIBILITY ON ADULT EARNINGS

	Black			Hispanic			White (non-Hisp)		
	All (1)	Male (2)	1991–92 (3)	All (4)	Male (5)	1991–92 (6)	All (7)	Male (8)	1991–92 (9)
Earnings (23–25)	944.0** (472.7)	1,075.0* (601.0)	1,836.0** (753.2)	614.6 (521.0)	67.8 (582.2)	276.0 (691.2)	55.3 (201.8)	628.7* (337.5)	624.0* (327.7)
Mean	21,460	20,220	20,970	21,280	20,200	20,500	21,570	20,450	20,910
Earnings (26–28)	1,237.0 (810.5)	883.3 (1,048.0)	1,644.0 (1,266.0)	617.0 (727.8)	–394.4 (1,005.0)	120.0 (978.4)	519.8 (325.6)	1,370.0*** (483.2)	1,048.0** (510.0)
Mean	29,310	27,590	28,260	29,270	27,700	28,310	29,300	27,690	28,170
Cash transfer in infancy	1,306	1,306	1,808	1,317	1,317	1,808	1,313	1,313	1,808
Observations	66,000	32,500	26,000	87,000	43,000	34,500	313,000	153,000	124,000

Notes. Each cell shows the basic regression discontinuity estimate ( $\beta_1$  from equation (1)) from a separate regression where the row denotes the outcome variable and the column denotes the subsample. The earnings outcome is constructed as the three-year average of earnings (including nonfilers as zeroes) at the filing unit level. The sample is restricted to individuals meeting the given subsample criteria, who were firstborn children, and who were born within 28 days of January 1 in 1981–82, 1986–87, and 1991–92 (or only 1991–92) and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. All regressions exclude observations within an eight-day donut of the January 1 cutoff. Cash transfer in infancy reflects the mean child-related tax benefit eligibility for families in the given group whose child was born prior to January 1. It is constructed using information from prior tax filings to predict AGI in the relevant tax year and then using NBER's TAXSIM program to determine tax benefit eligibility (see [Online Appendix B](#) for more details). See the text for additional details on variable construction and sample restrictions. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, CBDRB-FY2021-CES010-008, and CBDRB-FY2021-CES010-010. Significance levels are indicated by: \*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$ .

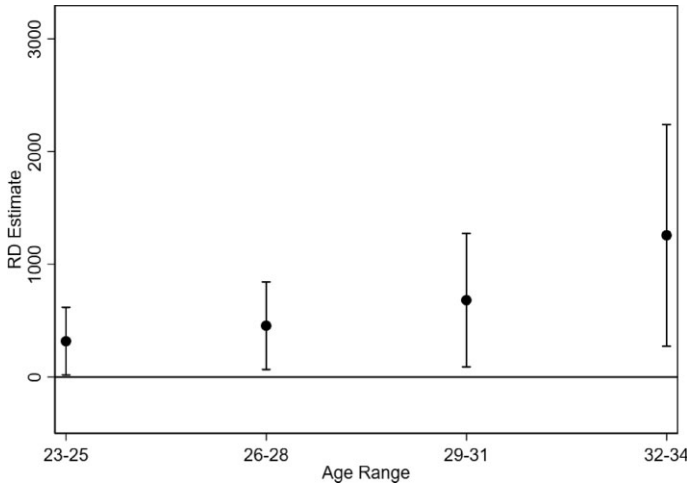


FIGURE V

Effect of Cash Transfer Eligibility on Adult Earnings: By Age

The figure displays the basic regression discontinuity estimate ( $\beta_1$  from [equation \(1\)](#)) by age range. The sample changes across estimates because later cohorts are not yet observed at older ages. See [Table I](#) and the text for additional details on sample restrictions, specification, and construction of outcome variables. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.

further evidence that our estimated effects are a reliable signal of increases in permanent income, we can produce estimates for subsets of cohorts that we are able to observe at older ages. While the treatment (cash transfer in infancy) is smaller for these cohorts, the earnings measures at older ages are somewhat more reliable. In combination with our main estimates, observing effects on earnings in the early to mid-thirties would boost confidence in our central findings. In [Figure V](#), we see that the effects persist and perhaps even grow as individuals age. Interestingly, we see the emergence of stronger evidence of earnings increases experienced by women at these ages ([Online Appendix Figure A.X](#)). [Online Appendix Figure A.XI](#) shows these effects by cohort, illustrating how the effects change across ages. At these older ages, we see that the treatment effects for the 1981–82 and 1986–87 cohorts are similar, consistent with the modest differences in child-related tax benefits experienced by these cohorts.

#### IV.B. Putting the Effect Sizes in Context

Overall, we find that per additional \$1,000 in infancy, children earn at least 1%–2% more during their twenties, with perhaps even larger effects in their early to mid-thirties. The effects for men, for whom we have the strongest signal of individual earnings, are twice as large.

Given the absence of causal estimates of the long-run effects of cash transfers in early childhood for recent cohorts, we can instead benchmark our results against estimates generated from in-kind transfers to cohorts born in the 1960s and early 1970s.<sup>30</sup> Estimated effects of county-level access to the food stamp program in early childhood (in utero to age five) using large-scale data sets imply effects on earnings of 0.3% to more than 1% (for females) per \$1,000 depending on the sample and approach (Bitler and Figinski 2019; Bailey et al. 2020).<sup>31</sup>

Our percentage earnings effects for males (where we have the most precise signal of individual earnings) are larger than those implied by either large-scale study, suggesting the relative importance of very early childhood. Of course, it is not clear whether the effects of earlier access to food stamps during childhood, which were targeted at and in many cases necessitated increased food consumption, are directly comparable to cash provided to a family just after the birth of a first child (Barr and Smith 2021).

An alternative benchmark is provided by evaluations of the Perry Preschool program, which estimate earnings effects for men at age 27 of around \$3,265, significantly larger than our ITT estimates (Heckman et al. 2010). However, the Perry Preschool program costs over \$20,000 a year in current dollars, implying effects per \$1,000 of around \$181 on annual earnings, smaller

30. Aizer et al. (2016) estimate positive effects of cash transfers to widowed mothers in the early 1900s on their male children's educational attainment and earnings by comparing accepted and nonaccepted applicants to the program. While limited information on child ages and program specifics from this time period make it difficult to scale their estimates into comparable figures, the implied effects on education and earnings appear to be very large.

31. These estimates are generated by dividing by food stamp participation rates in each sample (0.16), dividing by 5.75 (to get the per year effect), and dividing by the average annual food stamp benefit among recipients. Earlier work suggested even larger effects (2%–3% per \$1,000), but the survey data supporting these estimates resulted in relatively imprecise estimates with 95% confidence intervals that included reductions in earnings of similar amounts (Hoynes, Schanzenbach, and Almond 2016).

than our estimated effect of \$1,000 provided during infancy. Due to Perry's very low-income sample, these effects translate to an increase in earnings of just under 1% per \$1,000 of spending, closer to but still below our conservative full-sample estimate and well below our estimate for men.<sup>32</sup>

While there are few modern estimates that focus on the effect of resources available during early childhood, a small number of papers have estimated the effects of income or wealth at other ages using quasi-experimental variation. Estimated long-run effects of resource transfers generated by casino profits suggest positive effects, although effects on labor market outcomes are not studied (Akee et al. 2010). In contrast, estimates from Sweden using variation in wealth generated by lotteries suggest essentially no role for resources in influencing child outcomes (Cesarini et al. 2016). Estimates from a more dramatic shift in family environment resulting from the random assignment of adopted Korean children to U.S. families similarly imply only a weak relationship between parental income and child educational, income, and health outcomes (Sacerdote 2007).

Differences in the level of baseline disadvantage may contribute to the disparate effects observed. In both the lottery and adoption study, children receiving the lowest level of resources, that is, children in Swedish families that did not win the lottery, or children assigned to the poorest family adopting a child would still be relatively advantaged compared to many of the children in our study, those in families eligible for the Food Stamp, Head Start, or Perry programs in the 1960s and 1970s, and those in Eastern Band of Cherokee Indian families eligible for casino profit-sharing. The role of resources in influencing the outcomes of children may be magnified when resources are scarce.

## V. MECHANISMS ALONG THE LIFE COURSE

In this section we explore why cash transfers during infancy have substantial and long-lasting implications for child outcomes. We begin by exploring potential channels through which the short-term increase in resources could generate the observed effects.

32. Estimates from evaluations of Head Start imply effects on earnings ranging from less than 1% to 3% per \$1,000, although the larger estimates have wide confidence intervals (Thompson 2017; Johnson and Jackson 2019; Bailey, Timpe, and Sun 2020).



Most of these channels rely on temporary reductions in liquidity constraints allowing families to avoid adverse events or short-term stress with long-term ramifications. We consider changes in parental outcomes such as marital status and subsequent earnings outcomes as providing some indication of the role of resources provided at this critical point in allowing families to avoid these negative shocks. We then attempt to trace the effects of the cash transfer through a set of intermediate outcomes that could explain the longer-term earnings effects we observe for children. To do so we examine effects on K–12 outcomes contained in the North Carolina education data, demonstrating positive effects of cash transfers on test scores, the likelihood of suspension, and high-school graduation. We then conduct a simple accounting exercise to conclude that the observed effects on test scores and educational attainment are sufficient to explain the observed effect on earnings.

#### *V.A. Effects on the Family Environment throughout Childhood*

While the maximum increase in single-year cash transfers at the January 1 birthdate cutoff for additional tax benefits is substantial, as much as 20%, it is modest relative to the stream of lifetime income. Indeed, there may be little gain at all in lifetime income if the families of children born on or after January 1 are still income-eligible for child-related benefits when the child turns 18, one tax year later than those on the other side of the cutoff.<sup>33</sup> This suggests that the large effect of the additional resources may be generated through increased liquidity during a critical window. Increased liquidity may provide a cushion for families that allows them to avoid adverse events such as bankruptcy, eviction, loss of transportation, or food insecurity, or it may lead to more general reductions in stress that lead to changes in interactions with children (Milligan and Stabile 2008). The liquidity injection may be particularly important during the period following childbirth, when stress is high, expenses are increasing, and working is physically difficult or impossible for new mothers. Indeed, descriptive evidence from the Survey of Household Economics and Decision-making shows noticeable spikes in the share of families reporting being worse off financially or denied credit during the time after the birth of a first child (Online Appendix Figures A.1a and A.1b).

33. Of course, discounting and changes in eligibility as family size and income increase are likely to make the discounted difference nontrivial.

Although we cannot observe effects on adverse events or household stress directly, we can observe effects on family formation and parental income that would likely be affected by these types of changes. We use the same regression discontinuity strategy to study effects of the cash transfer on parent earnings, family poverty status, marital status, 1040 filing, and number of dependents. Given the availability of data and the birth cohorts we use, we can observe these outcomes at 1, 2, 6, and 7 years prior to and 3, 4, 9, 10, and 12–18 years after the recentered year of birth.<sup>34</sup> The estimates in years prior to birth serve as another balance check because they occur prior to treatment, while the estimates after birth illustrate changes in the early childhood environment that came about as a result of the cash transfer. [Figure VI](#) plots these estimates for family earnings. We see null effects prior to birth. Three and four years after birth, we see significant increases in parent earnings, with increases of around \$1,000, or 4% of the mean.<sup>35</sup> There is some evidence that these effects persist throughout the 18 years following birth, although the magnitude and precision of these estimates varies across ages. When we look at the sample of parents who filed prior to the birth of a first child (in Panel B), for whom we arguably have a more consistent measure of earnings, we similarly see substantial increases in parent earnings three to four years after birth, although the persistence of these effects over subsequent years is less clear. [Online Appendix Table A.IX](#) summarizes the effects across the 18 years following childbirth, with a positive effect on discounted total earnings over this period of roughly 1.8% in the full sample and just over 1% among filers, although the latter estimate is not significantly different from zero.

In [Online Appendix Figure A.XIII](#), we plot analogous estimates to Panel A of [Online Appendix Figure A.XIV](#) for 1040 filing, poverty status, whether the parents are married, and the number of dependents. The effects on parental poverty status similarly suggest a possible reduction three to four years after birth, but no evidence of effects after age nine. We see some evidence of an effect on 1040 filing, which may be thought of as a crude measure of employment. We see at most weakly suggestive increases in the likelihood that a child has married parents three to four

34. See [Online Appendix B](#) and [Online Appendix Figure B.I](#) for additional detail on the available tax years and the construction of the analytical sample.

35. The standard regression discontinuity plots are presented in [Online Appendix Figure A.XIV](#).

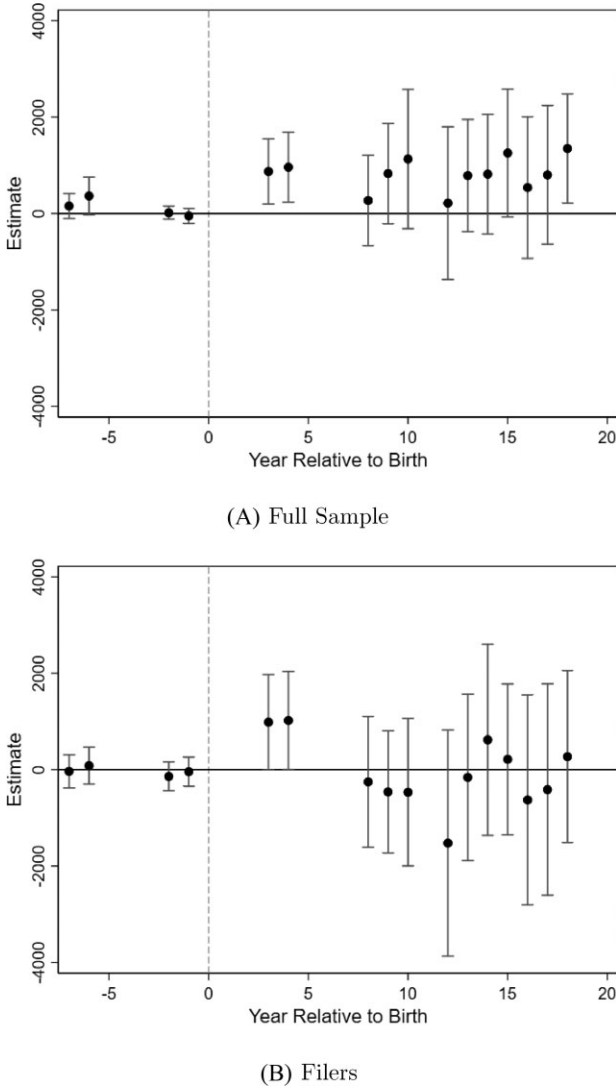


FIGURE VI

Effect of Cash Transfer Eligibility on Family Resources before and after Birth

The figure displays the basic regression discontinuity estimate ( $\beta_1$  from [equation \(1\)](#)) for parental earnings at various years before (i.e., -6, -7, -2, and -1) and after the child's birth (i.e., 3, 4, 8, 9, 10, and 12–18). Panel A contains all families, and Panel B is restricted to families that filed a 1040 in the year or two before the birth of their child. Observed years are limited by tax data availability. See [Table I](#) and the text for additional details on sample restrictions, specification, and construction of outcome variables.

years after childbirth, although the point estimates are small and dissipate over subsequent years.<sup>36</sup> Looking cumulatively over the 18 years after childbirth, there is similarly suggestive evidence of a reduction in shifts out of marriage (parents are less likely to ever be single) as a result of eligibility for child-related tax benefits, but no evidence of any shifts into marriage ([Online Appendix Table A.IX](#)). In combination, the estimates suggest that the resource boost following the birth of a first child may have had positive and enduring effects on the parents. These effects are consistent with the resource boost allowing families to better weather negative shocks or other stressors.

Adverse events are quite common among EITC-recipient households, with one study suggesting that 38% experienced unemployment, 33% experienced a hospitalization, 12% had legal expenses, and 42% had a major car repair within six months of filing taxes ([Despard et al. 2015](#)). The prevalence of car repairs in particular is consistent with the findings from studies that attempt to understand how EITC recipients spend their refunds. These studies tend to suggest that in addition to facilitating spending on basic necessities (e.g., food and housing), EITC receipt generates significant increases in car purchases, major car repair, and other expenses associated with transportation ([Smeeding, Phillips, and O'Connor 2000](#); [Goodman-Bacon and McGranahan 2008](#); [Patel 2011](#); [Despard et al. 2015](#)). Taken together, these results suggest that one potential avenue by which the cash transfer may generate persistent increases in earnings is by facilitating the capacity of individuals to maintain employment. This channel is likely to be of somewhat less importance in areas where providing one's own transportation is less critical. Consistent with this, we see strong evidence that the effects of additional cash benefits on parental earnings three and four years after birth are smaller for individuals living in areas with significant access to public transportation.<sup>37</sup> Given the prevalence of adverse events experienced by EITC-recipient households, the limited cushion available to them to weather these shocks, and the difficulties associated with the period after childbirth, it seems

36. Panel B of [Online Appendix Figure A.XII](#) plots the analogous estimates for the sample of parents who filed prior to the birth of a child.

37. We find estimated effects on family income three to four years after birth of \$215 (standard error of \$292) for children born in counties in the top decile of percent of commuters using public transportation (1980 census) and \$893 (standard error of \$301) for children born in other counties.

reasonable to expect that a transfer that increases household income by 10% could have important effects on levels of stress in the home as well as success at work. Indeed, some very recent evidence indicates that the availability of additional resources for single mothers provides a protective effect on maternal mental health, with these effects driven by simulated tax credit eligibility (Schmidt, Shore-Sheppard, and Watson 2021). These changes may have direct effects on a child during an important period for development as well as indirect effects through persistent increases in family income in subsequent years.

Our preferred interpretation sees an important role for liquidity during a critical window, but an alternative possibility is that having a child born just before January 1, and thus receiving additional cash benefits soon after childbirth, conveys information about the work incentives embedded in the tax code. While parents of children born on or just after January 1 are likely to be eligible for the EITC in the following year (and thus receive any information shock at that point), it is possible that the year just after childbirth is a critical decision point that influences parental work behavior over the subsequent years. It is difficult to test for this type of information effect, but a variety of evidence suggests that it is unlikely to be driving the parental earnings impacts that we observe. For example, attempts to explicitly inform EITC-eligible people of their eligibility has had limited effects on their subsequent claiming or work behavior (Manoli and Turner 2016; Linos et al. 2020).

Perhaps a more direct test of awareness of these incentives is provided by the extent to which subsequent earnings are bunched to take advantage of the maximum EITC credit (just below the plateau). If there were a meaningful and persistent difference in information or incentives, we would expect to see differential bunching persist past the first year. This is not what is observed. Instead, the difference in bunching across birth months disappears the following year, at the point that children born in January are eligible to be claimed (LaLumia, Sallee, and Turner 2015). We observe a similar lack of differential bunching in our own data at three and four years after birth; the point estimates are negative (i.e., suggesting less bunching for parents with children born in December), but are not significantly different from zero.<sup>38</sup>

38. Additional evidence is provided by a recent study using a similar strategy in a less liquidity-constrained population. Mortenson et al. (2018, i) find no positive

An additional question of interest is the extent to which the observed changes in the family environment that stem from the initial cash transfer can explain the subsequent increases in child earnings. The estimated effects on marital status are imprecise but consistent with a delay in the timing of divorce. Some recent evidence suggests that divorce in early childhood has small negative effects on subsequent outcomes relative to divorce at later ages, with larger effects on boys (Laird, Nielsen, and Nielsen 2020). These qualitative results are consistent with the pattern of effects in our study, but the magnitude of the estimated effects on marital status in early childhood is too small to explain much of the subsequent earnings effects that we observe.<sup>39</sup> That said, we view the observed changes in marital status as being suggestive of other changes occurring in the household, such as reduced stress, that likely influence child outcomes. A similar exercise using the observed increases in family income across subsequent years suggests a more important role for this channel in contributing to subsequent earnings increases. Combining the estimated increase in annual earnings (1.6%) with an intergenerational elasticity of earnings (IGE) of around 0.3 would imply an increase in child earnings of roughly 0.5%.<sup>40</sup> Under the assumed parameters and a causal interpretation of the IGE, the increase in parental earnings that stems from the initial cash transfer would appear to account for roughly a third of the observed increase in

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effects of eligibility for child-related tax benefits on earnings or labor supply in the year following birth, when we would expect the incentive effects of differential awareness of EITC eligibility to be strongest. This was true even for subgroups that experienced particularly strong incentives to increase labor supply (i.e., those who experienced a reduction in their marginal tax rate), leading the authors to conclude that their results “suggest that households do not learn about (and respond to) child tax benefits in the first year they are claimed.”

39. For example, the Laird, Nielsen, and Nielsen (2020) study suggests that a delay in the timing of divorce by four years would increase the likelihood of high-school graduation by 1.6 percentage points (3.4%). In combination with our estimated effects (at most a 2 percentage point increase in the likelihood of being married at ages 3–4), this would explain less than 5% of our estimated high-school graduation effect.

40. We adopt the IGE from Chetty et al. (2014b), which produces IGEs for similar cohorts in Online Appendix Table A1. They produce IGEs using mean parent income over the five years when the child is 15–19 years old. Our approximation using mean earnings over the 18 years when a child is 0 to 18 slightly inflates the percentage increase in parental earnings, suggesting that the fraction of the increase in child earnings that the increase in parental earnings can account for may be an upper bound.

TABLE VII  
EFFECT OF CASH TRANSFER ELIGIBILITY ON STUDENT OUTCOME INDEX (NORTH CAROLINA)

	(1)	(2)	(3)
Born before Jan 1	0.051*** (0.016)	0.051*** (0.016)	0.047*** (0.016)
Observations	44,992	44,992	44,992
Mean	−0.059	−0.059	−0.059
Cash transfer in infancy	1,595	1,595	1,595
Recentered birth year fixed effects	X	X	X
Day-of-week fixed effects		X	X
Demographic controls			X

Notes. Each cell shows the regression discontinuity estimate ( $\beta_1$  from [equation \(1\)](#)) from a separate regression where the column denotes the inclusion of different controls. The student outcome index is constructed as the mean of normalized test scores (grades 3–8), high-school graduation, and any suspension in middle or high school. Demographic controls include indicators for race, ethnicity, sex, and limited English proficiency. The sample consists of ever-FRL-eligible students born within 28 days of January 1 in 1993–1998 who entered a North Carolina public school by grade 5. The average cash transfer in infancy is produced using tax data for a similar population of individuals born in North Carolina and observed as ever eligible for FRL based on their reported 1040 AGI at the relevant ages (see [Online Appendix B](#) for additional details). See the text for additional details on variable construction and sample restrictions. Significance levels are indicated by: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

child earnings. This result further suggests that the timing of the additional resources during the critical window of early childhood and not just the total amount of additional resources, may play an important role in generating improved outcomes.

V.B. Effects on Educational Outcomes

We turn to the North Carolina administrative education data to better understand how the short-term effects on family structure and earnings translate to long-run effects on children’s later earnings. [Table VII](#) shows estimates of [equation \(1\)](#) for our index of behavioral and academic outcomes using the sample of FRL-eligible students. The results indicate that likely eligibility for additional cash during the first year of life generates a 0.05 standard deviation increase in the index. This estimated effect represents 11% of the gap between those eligible for FRL and those who are not.<sup>41</sup> [Figure VII](#) illustrates these results graphically, with a clear

41. [Online Appendix](#) Table A.X shows that these results are also robust to school district, school, district by recentered birth year, and school by recentered birth year fixed effects, while [Online Appendix](#) Table A.XI shows robustness to alternate index constructions.



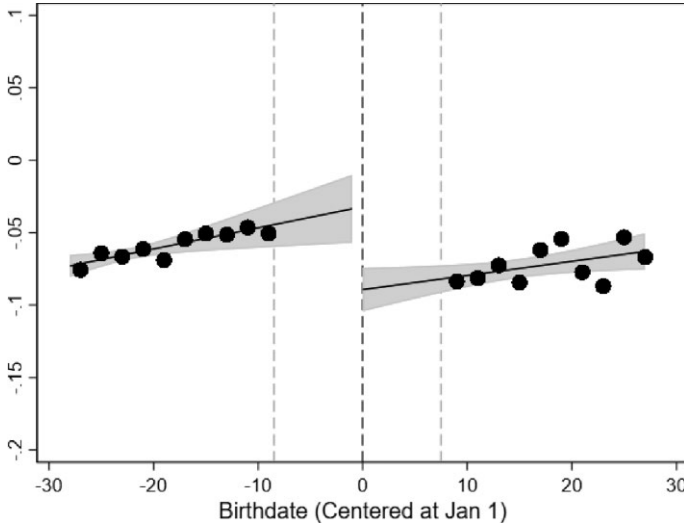


FIGURE VII

Effect of Cash Transfer Eligibility on Student Outcome Index (North Carolina)

The figure displays the mean student outcome index by two-day birthdate bin for FRL-eligible students born within 28 days of January 1 in 1993–1998 who entered a North Carolina public school by grade 5. Student outcome index is constructed as the mean of normalized test scores in grades 3–8, high-school graduation, and any suspension in middle or high school. The horizontal axis represents days relative to the January 1 birthdate cutoff. Birthdates to the left of the dotted line represent those where the child’s family could have received additional resources from child-related tax benefits in the following year (if eligible based on income). The shaded area shows the 95% confidence interval.

jump down as we move across the eligibility threshold. The estimates are largely stable across donut sizes ([Online Appendix Figure A.XV](#)) and bandwidths ([Online Appendix Figure A.XVI](#)).<sup>42</sup>

The student outcome estimates presented are all intent-to-treat effects of being born before January 1 (and thus likely eligible for child-related benefits in infancy). We can scale the effects by the size of the implied increase in resources contained in the bottom row of [Table VII](#) of roughly \$1,595. This implies effects of 0.03 standard deviations per \$1,000. [Online Appendix Table A.XII](#) provides the same estimates from [Table VII](#), column (1)

42. The sole exception is a donut size of four, which includes the negatively selected set of individuals who were born on or just after Christmas on the left side of the discontinuity, pulling the slope down and negatively biasing the estimate of  $\beta_1$ .

for FRL-eligible students separately by subgroup. Unlike for earnings during the twenties (and for single individuals), there is little difference in the estimated effect for men and women. This perhaps further suggests that the differences observed in the tax data may be partially a result of the greater noise to signal ratio in our measure of women's earnings, particularly at young ages. That said, it is possible that these differences reflect real heterogeneity in the effects of the cash transfer by gender, even at the younger ages; the North Carolina estimates are too imprecise to rule out meaningful differences.<sup>43</sup>

The effects appear to be somewhat stronger for white children, although the confidence intervals overlap. If this difference is meaningful, it may be a result of the greater rates of eligibility and take up of the EITC within the poor white versus poor black population, particularly in North Carolina.<sup>44</sup> Alternatively, there could be differences in the effects of cash transfers by race, particularly on the test score margin (e.g., if these effects are mediated by school quality, which differs by race). We see an opposite pattern of results when examining effects on earnings nationwide, although the confidence intervals are again too wide to draw strong conclusions.

In [Table VIII](#), we present estimates separately by schooling outcome.<sup>45</sup> Eligibility for the transfer increases an index of math and reading test scores in third through eighth grade by 0.04 to 0.05 standard deviations. These effects represent roughly 6%–7% of the overall gap between those eligible for FRL and those who are not. There are also large (2.2 to 2.3 percentage points) reductions in the likelihood of suspension and large increases in the likelihood of high-school graduation (2.0 to 2.1 percentage

43. Interestingly, we see some evidence of stronger effects of cash transfers for boys in terms of behavioral outcomes (i.e., reductions in suspensions), which is consistent with the conclusions of [Autor et al. \(2019\)](#) regarding the differential effects of family disadvantage for boys in terms of behavioral outcomes. That said, we lack the statistical power to draw strong conclusions from these estimates.

44. [Phillips \(2001\)](#) finds some evidence of this in the 1999 National Survey of America's Families, where black low-income families were less likely to have heard of the EITC or have ever received the EITC than white families. Even using our simulated tax benefit (which does not capture incomplete take-up or differential take-up), we see a slightly larger estimated transfer for white families. These differences may be exacerbated by differences in take-up if black families in North Carolina were less aware of the EITC during our sample period.

45. These estimates are presented graphically in [Online Appendix](#) Figures A.XVII, A.XVIII, and A.XIX.

TABLE VIII  
EFFECT OF CASH TRANSFER ELIGIBILITY ON INDIVIDUAL STUDENT OUTCOMES (NORTH CAROLINA)

	(1)	(2)	(3)
Test score index	0.046** (0.020)	0.044** (0.021)	0.036* (0.020)
Observations	44,984	44,984	44,984
Mean	0.035	0.035	0.035
Graduate HS	0.022** (0.011)	0.022** (0.011)	0.023** (0.011)
Observations	36,519	36,519	36,519
Mean	0.748	0.748	0.748
Ever suspended	-0.020* (0.011)	-0.021* (0.011)	-0.020* (0.011)
Observations	42,425	42,425	42,425
Mean	0.195	0.195	0.195
Cash transfer in infancy	1,595	1,595	1,595
Recentered birth year fixed effects	X	X	X
Day-of-week fixed effects		X	X
Demographic controls			X

*Notes.* Each cell shows the regression discontinuity estimate ( $\beta_1$  from equation (1)) from a separate regression where the row denotes the student outcome and the column denotes the inclusion of different controls. The test score index is constructed as the mean of normalized (mean zero, standard deviation one) math and reading test scores in grades 3–8. Demographic controls include indicators for race, ethnicity, sex, and limited English proficiency. The sample consists of FRL-eligible students born within 28 days of January 1 in 1993–1998 who entered a North Carolina public school by grade 5. See the text for additional details on variable construction and sample restrictions. The average cash transfer during infancy is produced using tax data for a similar population of individuals born in North Carolina and observed as eligible for FRL based on their reported 1040 AGI at the relevant ages. See Table I and the text for additional sample restrictions and information on variable construction. Significance levels are indicated by: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

points). These effects translate to a 0.02 to 0.03 standard deviation increase in test scores, 1.4 percentage points on high-school graduation, and -1.4 percentage points on having ever been suspended per \$1,000 transfer during the first year of life.

### V.C. Are Adult Earnings Effects Explained by Improvements in Human Capital?

A natural question is whether the observed effects on earnings can be largely accounted for by the increases in academic performance observed in the North Carolina data. Chetty, Friedman, and Rockoff (2014) suggests gains of \$2,500 in age 28 earnings per standard deviation increase in test scores. Multiplying our point estimate per \$1,000 (0.037 std. dev.)

by \$2,500 would suggest an increase in earnings at age 28 of around \$92 per year of increased test scores. This would suggest that the observed effects on human capital accumulation, which average test score effects over grades 3–8, could entirely account for our observed wage effects at ages 26–28 (estimated to be \$353 per \$1,000 in the full sample and \$606 for males). The prospect that human capital plays an important role as a mechanism is further bolstered by the absence of substantial fadeout by age in the effect of transfers during infancy on test scores ([Online Appendix Figure A.XX](#)).<sup>46</sup>

## VI. DISCUSSION AND CONCLUSION

Recent evidence suggests the importance of in-kind transfers in early childhood in positively influencing lifetime success. We contribute to this growing literature by providing new evidence on the effects of cash transfers provided during this period. We take advantage of a discontinuity in eligibility for U.S. child-related tax benefits following the birth of a first child. Combined with the universe of 1040 federal tax data with parent-child linkages spanning four decades and detailed education data from North Carolina, we demonstrate that cash transfers during this window can have profound and long-lasting effects. For an additional \$1,000 in early childhood, earnings at age 23–28 are 1%–2% higher. These effects persist to older ages, with 2%–3% increases at ages 29–31 and 32–34. Per dollar spent, these effects of additional cash provided during infancy on subsequent child earnings are larger than those generated by the Perry Preschool program, a resource-intensive early childhood intervention targeted at low-income families. Examining the discounted stream of additional tax receipts associated with the increased earnings in adulthood, we find that they exceed the amount of the initial transfer, implying a negative net cost to the federal government and an infinite marginal value of public funds.<sup>47</sup>

46. In addition to grades 3–8, this figure also contains estimates for the standardized tests taken by most students in high school (algebra and English), labeled as “HS.” In [Online Appendix Table A.XIII](#), we summarize the point estimates for high-school measures more generally.

47. We calculate the net cost to the government of a cash transfer in the first year after birth to low-income families of firstborn children as the difference between the upfront cost of the transfer (\$1,291 on average) and the increase in discounted future tax revenue through age 65. Following [Hendren and Sprung-Keyser \(2020\)](#), we use a 3% discount rate and the combined tax rate (12.9%) associated

The observed earnings effects appear to be explained by earlier human capital effects. During childhood and adolescence, we find substantial increases in test scores, reductions in behavioral problems, and a greater likelihood of high-school graduation. Estimates of effects on parental behavior in the years after birth suggest that the short-term liquidity increase may allow families to avoid adverse events or reduce stress during a critical window for parents and children. We find evidence that the liquidity increase provided in the critical period after childbirth results in persistent increases in family income. Back of the envelope calculations using existing intergenerational elasticity of earnings estimates suggest that these improvements only account for roughly a third of the observed effects on children. These results point to an important role for liquidity in the year after birth. They also suggest the relative importance of resources during the early childhood period; however, our research design does not allow us to separately identify the contribution of specific posttransfer changes in families (e.g., changes in parents' earnings) that may influence a child's eventual earnings.

These results may have important implications for how to best assist low-income families and promote social mobility, particularly at a time when child-related benefits are the focus of national debate. Although we are able to provide convincing evidence of the effect of a few thousand dollars in the first year of a first child's life, our results are limited in their ability to inform our understanding of the effects of larger transfers, provided at different ages, or provided to nonfirstborn children. With those caveats, our results do suggest that additional resource transfers to poor families around the time of a first birth would result in substantial improvements in social mobility.

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with the second quintile of earnings, in which the average earnings of our sample falls. This calculation uses our estimates from [Figure V](#) for the increase in earnings at ages 26–28, 29–31, and 32–34, and applies our age 32–34 estimates to ages 35–65 (we conservatively assume no increase in tax revenue prior to age 26). The associated sum of discounted future tax revenue substantially exceeds the upfront costs, implying a negative net cost to the government. This back-of-the-envelope calculation ignores the net positive tax revenues associated with the increases in parental earnings (calculated using NBER's TAXSIM program) and any other possible effects of the cash transfer. Although these effects would likely serve to increase the marginal value of public funds, our calculations are by nature limited to effects on outcomes that we can observe.

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### SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at *the Quarterly Journal of Economics* online.

### DATA AVAILABILITY

Code replicating the tables and figures in this article can be found in Barr, Eggleston, and Smith (2022) in the Harvard Dataverse, <https://doi.org/10.7910/DVN/XNVR4G>.

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