Investing in Infants

The Lasting Effects of Cash Transfers to New Families

Andrew Barr — Texas A&M University

Jonathan Eggleston — U.S. Census Bureau

Alexander A. Smith — U.S. Military Academy

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Abstract

We provide novel evidence that cash transfers following the birth of a child can have large and long-lasting effects. We take advantage of the January 1 birthdate cutoff for child-related tax benefits, which results in families of otherwise similar children receiving substantially different refunds during the first year of life. Using the universe of administrative tax data in selected years, we show that a $1,000 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. Examining effects on shorter-run parental outcomes suggests that additional liquidity during the critical window following the birth of a child leads to persistent increases in family income that likely contribute to the downstream effects on children. The downstream effects on child earnings alone are large enough that the transfer pays for itself through subsequent increases in federal income tax revenue.

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1 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.[1] Recent evidence suggests that the period of early childhood may be particularly important in determining these socio-economic divides. Indeed, correlational evidence suggests that once family income in early childhood is controlled for, the intergenerational relationship in income disappears (Duncan et al. 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, etc.) 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 paper, we explore whether cash transfers to poor families following the birth of a child generate improvements in that child's long-run outcomes.

While growing evidence suggests large long-term effects of education and health interventions in early childhood, the effectiveness of cash transfers remains uncertain.[2] 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). 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 et al. 2016; Barr and Smith 2021).[3] However, this program differs substantively from pure cash transfers, making it unclear to what extent the results can be generalized,

[1] Estimates of the intergenerational correlation in income are dramatic, between 0.3 and 0.6 (Black and Devereux 2011; Chetty, Hendren, Kline, Saez and Turner 2014b; Mazumder 2005, Solon 1999; Jones et al. 2020).

[2] See, for example, recent studies on the long-run effects early childhood education (Ludwig and Miller 2007; Thompson 2017; Johnson and Jackson 2019; Campbell et al. 2014; Heckman et al. 2010; Anders et al. 2021; Bailey, Sun, Timpe 2020 Garcia et al. 2020) and increased access to health insurance (Meyer and Wherry 2012; Brown, Kowalski, and Lurie 2015; Goodman-Bacon 2016).

[3] Aizer et al. (2016) similarly indicate that the children of accepted applicants to the Mother's Pension program (born between 1900 and 1925) have better outcomes than those who were not accepted. These children were treated at a variety of ages (mean=8.74).

particularly for more recent cohorts.[4,5] Evidence from more recent evaluations of quasi-cash transfers, such as lottery winnings and casino-revenue distributions, is mixed and only available at later ages (Cesarini et al. 2016; Akee et al. 2010).

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 the only causal estimates, of which we are aware, of the long-term effects of cash transfers during the first year of life. Using variation affecting relatively recent cohorts (individuals born in the 1980s and 1990s) we estimate effects across an array of educational, behavioral, and labor market outcomes. Because we are using tax-based variation and the tax data, we are able to infer with a relatively high degree of confidence the size of the transfer received. Finally, 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 (1) the early childhood environment, and (2) intermediate outcomes that likely contribute meaningfully to the long-run effects we observe.

We employ a regression discontinuity (RD) design that leverages the tax code's January 1 birthdate eligibility cutoff for the determination of a dependent child. This cutoff results in families of otherwise similar children receiving substantially different tax-based cash refunds in the following year. During our primary sample period 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 EITC and dependent exemption worth up to 20 percent of their income (roughly $3,000 in 2015 dollars).[6] As a result, families with children born before January 1

[4]Especially during its initial rollout, Food Stamps were not equivalent to a cash transfer; for some families, more than half by some estimates (Barr and Smith 2021), the benefits were not inframarginal and for others the stamps may have resulted in an increase in food consumption as a result of mental accounting (Hastings and Shapiro 2018).

[5]While a handful of small welfare-to-work experiments demonstrate the positive effect of cash assistance, these studies focus on the near-term effects of increased income on the behavior and schooling outcomes of children; furthermore, they are generally unable to isolate the effect of income from changes in the incentive to work (Gennetian and Miller 2002; Morris and Gennetian 2003; Hill et al. 2001; Clark-Kauffman et al. 2003). This is also true of the literature exploring the effect of changes in the Earned Income Tax Credit (EITC) schedule, which also tends to find positive effects of increases in schedule generosity (Hoynes et al. 2015; Dahl and Lochner 2012; Bastian and Michelmore 2018) or increased benefits due to a family's location on the schedule (Manoli and Turner 2018).

[6]We use NBER's Taxsim program to calculate the value of the additional benefits from claiming 1 child compared with no children.

experience a significant increase in liquid resources following the birth of their child.

Using the universe of administrative tax data in selected years, we focus on children born into families who we predict to be income-eligible for the EITC and who will therefore be most impacted by the discontinuity in tax benefits. We then link these children to their adult tax filings and find substantial increases in adult earnings at the birthdate cutoff for additional tax benefits in the first year of life. Overall, we find that a cash transfer of $1,000 in infancy results in children earning 1-2% more during their twenties. 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 32-34 of 2-3% per $1,000 received during infancy. 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.

Additional evidence supports the conclusion that these 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 (LaLumia 2015 and Schulkind and Shapiro 2014), particularly for first births among low-income families, the group on which we focus.[7] 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. Our results are robust to varying the size of the donut and the bandwidth we use to estimate effects. They are also robust to the inclusion of day-of-the-week, family demographic and predicted income controls. 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

[7] While not itself 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 which might give pause (Buckles and Hungerman 2013). While we replicate some of these differences for the sample of all births for all families in our data, we see no evidence of these average differences in our analysis sample of first births for low-income families (Appendix Table A2).

birthdate cutoff exhibit larger improvements in adult earnings at the cutoff. We observe larger effects for subgroups with larger expected transfers (e.g., those with a history of filing taxes or those born in later cohorts) and null effects for those with small expected increases (e.g., non-first-born children when the EITC only increased for first-born children).

We observe somewhat larger effects for males, with 2-3% increases in earnings per additional $1,000 received during infancy.[8] The larger effects for males may stem from a lower noise to signal ratio among males in our earnings data. Because males are less likely to be married at the observed ages and account for a larger share of earnings among married couples, they provide a stronger signal of the long-run individual effects of cash transfers during infancy on adult earnings at the tax unit-level. Alternatively, these results may reflect heterogeneity in the effects of cash transfers by sex, consistent with recent work suggesting that the early childhood environment is particularly important for boys (Autor et al. 2019; Bertrand and Pan 2013; Laird, Nielsen, and Nielsen 2020). We see limited evidence of heterogeneity across other margins. While the point estimates are often somewhat larger for black individuals, we cannot reject similar effects across race and ethnicity.

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 the early childhood environment.[9] As is often true with the broader body of work relating early childhood resources to later outcomes, these large effects are initially surprising given the relatively small increase in lifetime family resources involved. While the direct increase in the net present value of lifetime family income is tiny (at most 0.2%), 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).[10] The results suggest that these transfers may provide increased

[8] The difference in effect size between males and females is less apparent at older ages.

[9] See for example, recent studies on the long-run effects of Food Stamp availability (Hoynes et al. 2016; Barr and Smith 2021; Bitler and Figinski 2019), early childhood education (Ludwig and Miller 2007; Thompson 2017; Johnson and Jackson 2019; Campbell et al. 2014; Heckman et al. 2010; Anders et al. 2021; Bailey, Sun, Timpe 2020), increased access to health insurance (Meyer and Wherry 2012; Brown, Kowalski, and Lurie 2015; Goodman-Bacon 2016), housing assistance (Chyn 2018; Chetty, Hendren, and Katz 2016), etc.

[10] These percentages may be even higher if family income falls during the year after childbirth, which seems likely.

liquidity for families during a critical window for both parents and children.[11] The window may be critical from the perspective of the parent(s) due to the heightened expenses, reduced incomes, and additional stress that comes alongside 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 (e.g. Despard et al. 2015 find that, among EITC recipient households, 38% experienced unemployment , 33% experienced a hospitalization, 12% had legal expenses, and 42% had a major car repair within six months of filing taxes).[12] It may also lead to more general reductions in stress that lead to changes in interactions with children (Evans and Garthwaite 2014; Schmidt, Shore-Sheppard, and Watson 2021; Milligan and Stabile 2008). 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 socio-emotional development (Cunha et al. 2006).

While we cannot observe effects on adverse events or household stress directly, we can estimate effects on parental earnings and marital status that would likely be affected by these types of changes. We find evidence that the liquidity increase provided during the critical period following childbirth appears to result in persistent increases in family income.[13] 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 imply that these improvements only account for roughly a third of the observed effects on children. These results suggest an important role for liquidity in the year after birth and underscore the relative importance of resources during early childhood period.

[11] This is consistent with dynamic estimates from Hoynes et al. (2016) suggesting that the period between conception and age 2 to 3 is critical to their estimates of the impact of Food Stamp access during early childhood on later adult economic self-sufficiency. Dynamic estimates from Bailey et al. (2020) find a similar pattern for effects on human capital, but not for effects on economic self-sufficiency.

[12] Descriptive evidence from the Survey of Household Economics and Decisionmaking suggests liquidity is particularly constrained during the period after the birth of a family's first child (Appendix Figure A1b and A1a).

[13] The evidence, which we discuss in Section XX, is consistent with this liquidity explanation. However, it is also possible that EITC receipt after birth results in an information shock that incentivizes additional work. While there is little evidence to support this alternative, if true it would imply that our results may overestimate the effect of cash alone.

To better understand how the cash transfer provided 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 (the primary driver of additional tax benefits at the birthdate cutoff) depends on income, we focus our analysis on children who are ever eligible for free and reduced price lunch (FRL), a proxy for likely EITC eligibility.[14] We find that the effect of being born prior to January 1 is a 0.05 standard deviation increase in an index of outcomes that includes math and reading test scores, suspension, and high school graduation. This translates to an effect of 0.03 standard deviations per $1,000 provided during infancy. This represents over 6 percent 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 3rd through 8th grade math and reading test scores, reductions in the likelihood of suspension, and increases in the likelihood of high school graduation.[15] Taken together, the observed human capital effects can entirely explain our estimated effects on earnings.

Our results suggest that transfers to poor families may be especially impactful 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 of the policy to the federal government. Taken together, these conclusions may have important implications for how to best assist low-income families and promote social mobility. These

[14] We estimate that 55 to 75 percent of free and reduced price lunch (FRL) recipients are simultaneously eligible for the EITC (authors' calculations using all households with at least one child age 0 to 10 in the 1992-1999 CPS (1) who received FRL or (2) who were below 150 percent of the poverty line). We use the tax data to demonstrate that there is a significant discontinuity in tax benefits across the threshold within this group. We see no evidence that the cash transfer effects the likelihood of ever being eligible for FRL.

[15] Average math and reading test scores increase by 0.03 standard deviations per $1,000. There are larger effects (-1.4 percentage points per $1,000) on the likelihood of suspension. These represent 4 percent and 9 percent, respectively, of relevant gaps between those eligible for free lunch and those who are not.

implications are particularly salient at a time when child-related benefits are a focus of national policy debate.

2 Data and Descriptive Statistics

We use restricted individual-level data from IRS tax returns in addition to individual-level school data from North Carolina. These data allow us to explore the effect of additional resources across the life course (from kindergarten through early adulthood) and across a variety of meaningful measures of success (school performance, school behavior, educational attainment, earnings, etc.). These data also allow us to better understand the channels through which effects may be operating.

2.1 Linked Tax Data

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 have access to IRS 1040 data for every filer in the United States in 1979, 1984, 1989, 1994-95, and 1998-2018. We obtain date of birth, sex, and state of birth for nearly every individual born in the United States after 1969 from the Social Security Administration (SSA) Numident File.[16] Using information on family composition, income, and exact date of birth, we can look back at an individual's 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 due to 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

[16] For the purposes of our main analyses, we are missing the small number of individuals born after 1980 who do not show up in the Numident file (i.e., lack SSNs). Our outcome information is limited to that observed in the tax data in later years, in which we observe all filers.

the linked parents backwards to the closest pre-birth year in which we have the universe of 1040 tax information.[17,18] We use this information (including whether or not an individual's parents filed) to predict AGI during the tax year ending with or just prior to the birth of a new child.

To fix ideas, 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 or not 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).[19]

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.[20] While 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 contained in the tax returns filed prior to birth.[21]

With the parent-child linkage discussed above 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

[17] In cases where a child has two parents on their later 1040 tax form that do not remain in the same household as we track them backward, we follow the mother.

[18] We identify a child as first-born if the 1040 in which they are linked to their parent contains no older siblings and the closest pre-birth year tax return contains no other children.

[19] AGI predictions apply the relationship between AGI and AGI t years later for maximum parent age by $1,000 income bins (2015 dollars) in later tax years when the universe of 1040s is observed every year. We focus on cohorts born one or two years after the years for which we have available tax data. Our income predictions do a substantially worse job if we predict using income with more than a two-year lag. Additional information is provided in Appendix B.

[20] The classification error will tend to bias the effect toward zero for the income-eligible group because we are incorrectly including non-income eligible individuals as a result of prediction error.

[21] 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.

key earnings outcome measure is 3-year average earnings, including missing earnings (i.e., non-filers) as zeroes.[22] 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 spouses if present. We focus on effects at ages 23-25 and 26-28, for which we are able to observe all cohorts. We also present effects at ages 29-31 and 32-34 (where available). Because males are (1) less likely to be married at the observed ages, and (2) account for a larger share of earnings among married couples, they provide a stronger signal of the long-run individual effects of cash transfers during infancy. As a result, we place some emphasis on estimated effects for them in our discussion of the results.

2.2 North Carolina Education Data

To examine earlier effects and better understand the channels through which the earnings effects are operating, we use administrative education from North Carolina (obtained from the North Carolina Education Research Data Center). The schooling data contain detailed individual-level administrative data on the K-12 record of all North Carolina public school students beginning in 1997. Critical for our empirical strategy, these data include students' exact birth dates, among other demographic, behavioral, academic achievement, and attainment information. To best utilize 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.[23] We focus our analysis on children eligible for free and reduced price lunch (FRL).[24] We use this as a proxy for likely-EITC eligibility as the income thresholds for the programs are similar.[25] Among those *eligible* for FRL based on family income, the rate of EITC eligibility is roughly 75 percent. 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

[22] When there are years in the 3-year range where the universe of tax filings are not observed, the remaining observed years are averaged.

[23] 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).

[24] We include any student that we ever observe as eligible for free and reduced price lunch in this category.

[25] 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).

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 verbal test scores from grades 3 through 8. These scores are normalized to have a mean of zero and a standard deviation of one within grades. We also construct a measure of behavioral issues, an indicator variable equal to one 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 also combine our measures of aptitude, behavior, and educational attainment into an index following Kling et al. (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.[26]

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 verbal test scores (0.71 SD), rates of suspension (0.12), and rates of high school graduation (0.15).

3 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

[26] Adapting Kling et al. (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 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. Table A12 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.

life. The families of children who are born on December 31st are eligible to receive substantial increases in tax benefits in the following year, while the families of children who are born on January 1st are not eligible for these benefits for an additional year.[27] 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 at changes in family resources during very early childhood. In the year prior to birth, the as-good-as-random assignment of eligibility for the additional income at the cutoff implies no differential incentive to work to maximize the benefits received. In the year following the child's birth, families on both sides of the cutoff face the same tax benefit schedules and therefore work incentives.

3.1 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 exemption.[28] While 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 experience additional family income for one year based entirely on the luck of being born slightly earlier.[29] To obtain an understanding

[27] 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 et al. (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.

[28] 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.

[29] 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 1997).

of the magnitude of these benefits, we use information from prior tax filings combined with 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 then 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 1 child compared with no children.[30] We calculate this difference for every child's family in our sample. Figure 2 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.[31] This creates an increase in family resources during infancy for those children born to the left of the threshold.

While this measure provides a reasonable indication of the size of the average increase in resources during infancy, significant uncertainty remains. First, there is uncertainty induced by our predicted income measure. Because our predictions are based off of tax filing information from the mid-1990s to early-2000s (when the universe of 1040s is observed in consecutive years), our predictions are likely to bias our estimate of the size of the average increase in resources if the relationship between lagged income and current income changes over time. This may be more problematic in the earlier years of our sample (1981-82 and 1986-87 recentered birth years) due to the labor market fluctuations during this period.[32]

[30] We use a similar strategy to estimate the implied discontinuity in child-related benefits during infancy within our sample of North Carolina students, using the tax data to select families with similar-aged children and incomes in North Carolina before tracking them back to their pre-birth information to estimate the implied effect on resources during infancy. First, we capture the set of children meeting the following conditions: (1) the child is born in NC in recentered birth years within the same sample period where we observe the universe of tax filings (i.e., 1994, 1995, and 1998), and (2) the child is ever eligible for FRL in grade 3-8 (based on their age and the income information on parent's tax filings). Next, we link these children back to their parent's tax filing prior to their birth year or the prior year (for those born in years where we observe the universe of tax filings). Finally, we estimate the implied discontinuity in child-related tax benefits during infancy using the average of the simulated benefit difference at the January 1 cutoff for those born before the cutoff.

[31] In Appendix Figure A3, we illustrate the average value of an additional dependent child on both sides of the threshold. While families with children born on January 1 and after are not eligible, 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.

[32] Specifically, the labor market weakened significantly between 1979 (the base year used for prediction) and the 1980 and 1981 tax years, and strengthened significantly between 1984 (the base year used for prediction) and the 1985 and 1986 tax years.

Second, we are using AGI and predicted AGI to infer eligibility for the EITC. It is possible that earned income and AGI differ for some individuals, which would likely result in an overestimate of the associated credit amount. Third, there is uncertainty induced by our classification of a dependent as a first child. Because the 1040 did not require a child's SSN until 1994, we use these later 1040s to link a child with their parents. While we use the prior 1040 information on number of dependent children claimed (in the 1979, 1984, and 1989 tax years) to improve our classification, there likely remains some misclassification. The primary concern is that an older sibling may not be claimed as a dependent in 1994, which may result in us incorrectly identifying a child as a first child. Because a non-first-born child generates significantly smaller tax benefits during our sample period, any improper classification of them as a first child would result in an upward bias of the implied increase in resources during infancy in our main sample of children that we classify as first-born and EITC income-eligible.[33] While we think this misclassification is modest, it is likely to be more problematic during the earlier years of our sample (1981-82 and 1986-87 recentered birth years) given the implied age of an older sibling by 1994. Finally, but perhaps most significantly, there is uncertainty generated by incomplete filing and take up of the EITC. Our estimated increase in resources during infancy assumes that eligible individuals are filing taxes and claiming the EITC, but take-up has never been 100%. Some estimates suggest that EITC take-up was as low as 50% in the mid-1980s and rose to 81-86% by 1990 (Scholz 1990). This suggests a potentially large upward bias in our estimates of the implied increase in resources during infancy that is likely worse for the earlier years of our sample. As a result of this uncertainty, we draw a number of conclusions. First, we view our estimated average increases as almost certainly 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 conservative lower bounds. Second, due to greater rates

[33] In our samples, we estimate the tax benefit as $306 for non-first born children vs. $1,291 for first-born children. This difference is mainly a result of the EITC schedule being the same for one child and two or more children households until the 1991 tax year (when there was a 3.6% higher maximum credit for two children compared to one).

of misclassification and lower rates of take-up, the extent of upward bias in the estimated income increases during infancy is likely much 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 effect of an increase in resources during infancy for the later years. 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.

3.2 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 1st birthdate cutoff (unrelated to family characteristics) 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:

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

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 1st (z_i is zero for children born on January 1). $1[z_i < 0]$ is an eligibility indicator equal to one if child i is born prior to January 1st. θ_t are recentered birth year fixed effects. The primary coefficient of interest is β_1, which identifies the effect of changes in likely eligibility for additional dependent-based tax benefits among low-income families, rather than

the effect of changes in actual income.[34] This is an intent to treat parameter (ITT). While we 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 the results section.

3.3 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 generate differences in child outcomes rather than the differences in resources generated by the tax credit.

We see little evidence of this type of manipulation in the cohorts in our sample. Figure 1 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 NC data. As seen in Panel A, the distribution of birth dates among those income-eligible for the EITC is largely smooth.[35] Panel B similarly indicates minimal levels of birth timing manipulation in the NC data. These results are consistent with previous studies, which have found little to no impact of incentives on birth timing around New Year's, particularly for first births (LaLumia et al. 2015; Schulkind and Shapiro 2014).[36] Nevertheless, we also follow an approach common in the literature and estimate donut hole RDs, dropping the observations around the January

[34] In the tax data, we use predicted adjusted gross income (AGI) to restrict to children born into families with AGI below the EITC phaseout maximum. In the NC education data we use FRL status to proxy for likely eligibility.

[35] Due to disclosure concerns, the number of observations in each two-day bin is rounded to the nearest 500.

[36] Using 2001-2010 tax return data, LaLumia et al. (2015) find limited evidence that parents shift births to December. A $1000 increase in tax benefits is associated with only a 1 percentage point – or 2% - rise in the probability of a late December birth. They find that this effect is smaller for low-income families and much smaller for first births. Similarly, Schulkind and Shapiro (2014) find that a $1000 increase in tax benefits leads to only a 0.54 percentage-point rise in the likelihood of a December birth.

1 threshold (shaded in gray), to address this concern.

Another conventional "test" of the RD identifying assumption which we employ 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. The intuition here is that if the observable predetermined characteristics appear to be balanced across the threshold then we can be reasonably confident that the unobservable characteristics are as well. Due to the structure of our analytical sample, we are able to observe tax filing information one or two years prior to the recentered year of birth. Consistent with conditionally random assignment, we find no significant differences around the birthdate cutoff in child sex, race, or ethnicity, or pre-birth parent characteristics such as marital status, parent age, whether the parent filed a 1040, or the predicted AGI of the parent (Figure A2 and Table 2).[37,38] We similarly see no evidence of discontinuities across other month pairs for which Buckles and Hungerman demonstrate meaningful mean differences, but we have no reason to expect an important discontinuity in outcomes (Appendix Table A4).[39]

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). To circumvent this particular 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.[40] 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 dependent tax benefits and

[37]We similarly see no imbalance in county of birth characteristics (Appendix Table A1). 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 which might give pause (Buckles and Hungerman 2013). We see no evidence of these average differences in our low-income sample (Appendix Table A2).

[38]We conduct similar exercises 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 (Appendix Table A3).

[39]Specifically, we look at all month pairs for which less than 10% of the U.S. population is affected by a school start date. We further exclude July 1 due to the structure of our sample.

[40]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).

EITC take up rates increased significantly between 1980 and 1990. We compare outcome effect estimates across birth cohorts, with the expectation that the regression discontinuity effects should be larger for later cohorts. Second, we estimate our basic regression discontinuity 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.

4 Results - Adult Outcomes

We use the tax data to explore long-run effects on adult earnings. Our baseline estimates in Table 3 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.[41] 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. As noted previously, given that the estimated increases in cash transfers during infancy are overestimates, we view the implied effect of 1.2-1.3% per $1,000 as a very conservative lower bound.

The results are robust to the inclusion of demographic (parent age and child sex) and pre-birth income controls. Figures 3 illustrates these results graphically, suggesting minimal relationship between the assignment variable and earnings, with a clear jump down as we move across the eligibility threshold.[42,43]

Appendix Figure A5 shows how the basic regression discontinuity estimate (β_1) varies by donut size. The estimates are generally similar across donut size. The slight exception is a donut size of around 4 or 5, which includes the negatively selected set of individuals who

[41] Appendix Table A7 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.

[42] Appendix Figure A4 provides the same graphical evidence without the donuts.

[43] We see similar evidence for effects when examining percentile earnings, with an increase of 0.33-0.47 percentiles within a birth cohort(Appendix Table A5 and Appendix Figure A7).

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 β_1.[44] Appendix Figure A6 shows that the estimates are similarly robust to different window sizes, with generally larger but less precise estimates with smaller windows.

Table 4 illustrates how the results vary across cohorts. While disaggregating reduces power substantially, 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%.[45] 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 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 take up. Adjusting the implied cash transfer discontinuity estimates for the differential take up across cohorts tends to bring these scaled estimates closer together.[46]

[44] 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 re-timing appears to result in families with worse than average expected outcomes to be born on Christmas or the day or two after.

[45] Scholz (1990) provides a central estimate of EITC take-up in the mid-1980s of 70 percent, while Scholz (1994) estimates take-up of 80.5-86.4 percent 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.

[46] As discussed in section 5.1, Appendix Figure A11 shows effects by cohort at older ages as well (we can only observe the earlier cohorts at these ages). At these older ages, we can see that the treatment effects for the 1981-82 and 1986-87 cohorts are larger than at younger ages and fairly similar to each other, consistent with the modest differences in child-related tax benefits experienced by these cohorts. While we cannot reject the equivalence of effect sizes across these cohorts at age 26 to 28, it is somewhat surprising that the estimated effect for the 1981-82 cohort appears to be much smaller. This may result from the temporal point of measurement. At ages 26-28, the 1981-82 cohort is observed primarily during the Great Recession, which may contribute to both the decreased precision and magnitude of the estimated effect for this cohort at these ages.

Appendix Table A6 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 non-first-born children in low-income families. While there are modest discontinuities in the size of the cash transfer for the families of non-first-born children, they are much smaller than among first-born children.[47] For the full set of cohorts, the estimated additional transfer during infancy is $1,291 for first-born children, but only $306 for non-first-born children.[48] As a result, if our estimates for the first-born children are driven by increases in the cash transfer provided during the first year of life, we would expect to see smaller effects for non-first-born children and no pattern of larger effects for later cohorts of non-first-born 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 non-first-born children and no pattern of larger effects in recent cohorts.[49]

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 females are married filing jointly and the lower relative share of earnings that females account for in married households filing jointly at the ages that we observe. Appendix Table A7 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. In Table 5, we explore

[47]The form of the additional transfer also differs between these two groups. While the majority of the additional transfer for first-born children comes as a refundable tax credit, the additional transfer for non-first-born children comes primarily as a reduction in the family's tax burden due to an additional exemption.

[48]The sample of non-first-born children is restricted to parents who filed in the year or two prior to the birth of a non-first-born child. This is to avoid the large increase in the transfer that would result from an income-eligible non-filer with a single child deciding to file as a result of their second child being born prior to January 1.

[49]We note here again that our process for linking dependents to parents may affect our construction of the non-first-born sample. First, because we are using the 1994 and subsequent filing information to link dependents to their parents and determine birth order, a non-trivial share of those born in the 1981-82 and 1986-87 recentered birth years may be improperly captured as first-born children. As discussed previously, this will attenuate our main estimates, particularly for the earlier birth cohorts, because non-first-born children are eligible for significantly smaller tax benefits during our sample window. It may also result in our failure to capture some non-first-born children in a family, with more missingness for the earlier birth cohorts. While this affects non-first-born sample sizes, it should not affect the validity of the approach.

how the results vary by child gender. While we cannot rule out substantial effects for females, the effects of transfers provided during infancy do appear to be much larger among males. We estimate earnings effects for males 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 4.[50] 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 gender. Indeed, we do estimate larger earnings effects for *single* men than *single* women, although these results are subject to a number of caveats regarding potential selection into marriage and heterogeneity in effects between single and married individuals.[51] This type of heterogeneity would be consistent with recent work that suggests that the early childhood environment is particularly important for boys (Autor et al. 2019; Bertrand and Pan 2013; Laird, Nielsen, and Nielsen 2020). However, we see limited evidence of such heterogeneity when exploring effects on earlier outcomes. We return to this in the discussion section of the paper. Table 5 also explores heterogeneity in effects by parent tax filing characteristics. While not statistically significant, effects on children are somewhat larger for those whose parents filed income taxes in the year or two before their birth (i.e., in the tax years we use for our income prediction). This is consistent with the larger predicted transfer size we estimate for filers. Within the smaller set of children whose parents filed prior to their birth, we estimate effects separately for those who were single or married.[52] The point estimates are too imprecise to draw strong conclusions, but the effects appear to be driven by the children of single parents at baseline. This pattern is again consistent with the larger predicted transfer discontinuity for single parents.

Appendix Table A8 breaks out the estimates by cohort and gender. The effects are

[50] Regression discontinuity plots by gender for percentile wages are in Appendix Figure A8.

[51] We estimate null effects for single women across ages and significant 2-3 percent effects for single men. However, we cannot rule out the possibility that the cash transfer influenced selection into marriage, which could introduce non-trivial bias into these estimates.

[52] We cannot observe parent marital status prior to birth without observing a tax form prior to birth.

most apparent for males, 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 downwards. The estimates are noisier for females, which may be a result of the weaker signal of individual earnings for this group.

In Table 6, 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. While the estimates are too imprecise to draw strong conclusions, the differences across groups are consistent with the significantly lower rates of awareness and take up of the EITC among the Hispanic population.

4.1 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 the multi-year average measures as we do, are strongly predictive of future earnings (Haider and Solon 2006; Chetty et al. 2011).[53] As 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 somewhat smaller for these cohorts, the earnings measures at older ages are somewhat more reliable. In combination with our main

[53] Haider and Solon (2006) suggests that earnings at age 26 (and 27, 28, 29, etc.) are as predictive of lifetime earnings as earnings at age 35. Consistent with this, Chetty et al. (2011) demonstrates that while the correlation between earnings at time t and $t+6$ grows quite quickly between ages 22 and 26, it is fairly high and much slower growing over subsequent ages. We have estimated similar correlations in our data (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-20s are even more predictive of future earnings in our data (Appendix Figure A9).

estimates, observing effects on earnings in the early to mid-30s would boost confidence in our central findings.[54] In Figure 5, we see that the effects persist and perhaps even grow as individuals age, strongly supporting our central finding. Interestingly, we see the emergence of stronger evidence of earnings increases experienced by females at these ages (Appendix Figure A10). Appendix Figure A11 breaks out these effects by cohort, illustrating how the effects change across ages. At these older ages, we can see that the treatment effects for the 1981-82 and 1986-87 cohorts are fairly similar, consistent with the modest differences in child-related tax benefits experienced by these cohorts.

4.2 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 the early to mid-30s. These estimates are likely a conservative lower bound for the effect of cash transfers in infancy, because they do not account for incomplete filing or EITC take-up.[55] The effects for males, for whom we have the strongest signal of individual earnings, are twice as large.

While we are unaware of any causal estimates of the long-run effects of resources in early childhood for recent cohorts, we can benchmark our results against estimates generated from in-kind transfers to cohorts born in the 1960s and early 1970s. For example, Hoynes et al. (2016) report sizable impacts of county-level access to the Food Stamp program in early childhood (in utero to age 5) on metabolic syndrome in adulthood (0.3 sd) and high school graduation (18 percentage points) as well as self-reported good health (30 percentage points) and an index of economic self-sufficiency (0.3 sd) for women. While imprecisely estimated, Hoynes et al. (2016) also report effects on annual earnings (3,610) and log family income (0.247). These are all intent-to-treat estimates for a sample with a 43 percent participation rate, suggesting large effects of FSP availability in early childhood. This results in estimated

[54] Furthermore, observation over a greater range of ages may mitigate concerns related to the different labor markets experienced by different cohorts and how those differences influence the resulting treatment estimates.

[55] However, these estimates may not be a lower bound if receiving additional cash benefits soon after childbirth conveys information about the work incentives such that it affects parents differently than if they received the same information the following year. We discuss this possibility in Section XX.

effects on earnings of $1,460 and on family income of 10 percent per year of Food Stamp participation prior to age 5.[56] Dividing by the average annual Food Stamp benefit among recipients implies an effect on earnings of around $330-430 per $1,000 of Food Stamp benefits. While these level effects are estimated across older ages, they represent 2-3% increases off of the mean, with large 95% confidence intervals that include *reductions* in earnings of similar amounts. While Hoynes et al. (2016) focus on a high-impact sample with a large fraction of families receiving Food Stamps, more recent work using a similar strategy with a significantly larger (but less focused) sample suggests much smaller, but much more precisely estimated effects. Scaling as above, these estimates imply a 0.3-0.4% increase in labor income per $1,000 of Food Stamp benefits (Bailey, Hoynes, Rossin-Slater, and Walker 2020).[57] Bitler and Figinski (2019) use administrative data from the Social Security Administration to estimate similar long-run effects of county-level Food Stamp availability on earnings. They find effects for women that are twice as large as those reported by Bailey et al., with no significant effects for males.

While it is somewhat difficult to compare with the full range of Food Stamp estimates, 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.[58]

While distinct in the nature of treatment, evaluations of early childhood programs may provide alternative benchmarks. Evaluations of the Perry Preschool program estimate earn-

[56] We generate effect sizes for one year of FSP use by dividing by 0.43 (to get the effect of participation) and dividing by 5.75 (to get the per year effect). The 5.75 comes from the exposure between conception and age 5.

[57] Again, this takes Bailey et al.'s estimates of 0.0114, divides by 0.16 (to get the TOT for participation in this sample), divides by 5.75 to get the effect per year of exposure, and by 3.3 to 4.3 to get the effect per $1,000.

[58] Barr and Smith (2021) use the 1960-61 Consumer Expenditure Survey to estimate that only roughly one third to one half of FSP-eligible households would have experienced the stamps as equivalent to cash. This conclusion is consistent with a variety of estimates from the time period which suggest that households used 53 to 86% of food subsidy income for the purchase of additional food (Hoagland, 1977).

ings effects for males at age 27 of around $3,265, significantly larger than our ITT estimates. 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 than our estimated effect of $1,000 provided during infancy. The level of disadvantage in the Perry sample was much greater than in our sample, so the percentage effect on earnings per $1,000 of spending was just under 1%, closer to but still below our conservative full sample estimate and well below our estimate for males. Estimates from evaluations of Head Start imply effects on earnings of 2-3% per $1,000, with wide confidence intervals (Thompson 2018; Johnson and Jackson 2020). Of course, all of these evaluations are of programs operating in the 1960s and 1970s, so we cannot say whether these programs are having similar effects on more recent cohorts. Indeed, recent experimental evidence from the Head Start Impact Study suggests minimal average effects of Head Start participation on test scores, although effects are large and positive for those who would otherwise not attend preschool (Gibbs et al. 2011; Puma et al. 2010; Kline and Walters 2016).

While there are few 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. 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 non-accepted 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. Estimated long-run effects of resource transfers generated by casino revenues also 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 suggests essentially no role for resources in influencing child outcomes (Cesarini et al. 2016). These estimates are generated using a compelling lottery design, but differences in the level

of child disadvantage and the available safety net in Sweden may play an important role in mediating the effect of additional resources. Examining a more dramatic shift in family environment, Sacerdote (2007) uses random assignment of adopted Korean children to American families to estimate the importance of family resources and background. More in line with the lottery study, he finds that parental income is only weakly related to child educational, income, and health outcomes. While there are many other differences between the studies that suggest an important role of resources and those that do not, differences in the level of baseline disadvantage may contribute to the disparate impacts. 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 likely be relatively advantaged compared to the children in the other studies. The role of resources in influencing the outcomes of children may be magnified when resources are scarce.

5 Mechanisms Along the Life Course

While there is compelling evidence that cash transfers during infancy have substantial and long-run implications for child outcomes, in this section we explore why. We begin by exploring potential channels through which the short-term increase in resources could generate the observed effects. 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.

5.1 Effects on Family Structure and Parental Earnings Trajectory in Early 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.[59] This suggests that the large impact 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, while 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 Decisionmaking shows noticeable spikes in the share of families reporting being worse off financially or denied credit during the period after the birth of a first child (Appendix Figures A1a) and A1b).

While 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, and marital status during early childhood. Due to the availability of data and the birth cohorts we use, we are able to observe these outcomes at 1, 2, 6, and 7 years prior to and 3, 4, 9, 10, and 12-18 years

[59] Of course, discounting and changes in eligibility as family size and income increase are likely to make the discounted difference non-trivial.

after the recentered year of birth.[60] 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 6 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.[61] 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. Indeed, 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 see somewhat less evidence that the positive effects observed 3 to 4 years after birth persist. In Appendix Figure A12, we plot analogous estimates 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 3 to 4 years after birth, but no evidence of effects after age 9. We see limited evidence of an effect on 1040 filing, which may be thought of as a crude measure of employment. Appendix Table A9 summarizes the effects across the 18 years following childbirth.[62] While there is a positive effect on total earnings over this period (of roughly 1.8%), there is no evidence that families' earnings are less likely to be below the poverty line.

Returning to Figure A12, we see at most weakly suggestive increases in the likelihood that a child has married parents 3 to 4 years after childbirth, although the point estimates are small and dissipate over subsequent years. Looking cumulatively over the 18 years following 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 (Appendix Table A9). In combination,

[60] See Appendix B and Appendix Figure B1 for additional detail on the available tax years and the construction of the analytical sample.

[61] The standard regression discontinuity plots are presented in Figure A13.

[62] For years where we do not observe the universe of tax records, we use linear interpolation and extrapolation (bounded at zero and the maximum family income from the 18 years following childbirth) to impute family income. For other measures, we use only the subset of 18 years following childbirth where we observe the universe of tax records.

the estimates suggest that the resource boost following the birth of a first child may have had positive and enduring effects on the parents themselves. 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. While not focused on new parents, these studies tend to suggest that EITC receipt generates significant increases in car purchases, major car repair, and other expenses associated with transportation (Goodman-Bacon and McGranahan 2008; Patel 2011; Chicago, Smeeding, Phillips, and O'Connor 2010). 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.[63] This channel is likely to be of somewhat less importance in areas where providing your own transportation is less critical. We therefore examine heterogeneity in our estimates by access to public transportation. While only suggestive, 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.[64] 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 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 (Shore-

[63]This conclusion is consistent with Mello (2018), which suggests that even relatively minor traffic infractions can lead to substantial reductions in subsequent earnings.

[64]We find estimated effects on family income 3-4 years after birth of $215 (standard error of $292) for children born in counties in the top decile of % of commuters using public transportation (1980 Census) and $893 (standard error of $301) for children born in other counties.

Sheppard, and Watson 2021). These changes may have direct effects on a child during a critical window of development as well as indirect effects through persistent increases in family income in subsequent years.

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 individuals of their eligibility has had, at most, limited effects on their subsequent claiming or work behavior (Linos et al. 2020). It isn't obvious why we should expect one-year earlier eligibility for the EITC to generate strong and persistent labor force effects when information designed to shift take up has no effect. Furthermore, if the information about the incentives embedded in the EITC is important enough to generate changes in behavior, it is somewhat surprising that this information isn't shared among parents, the vast majority of whom would have this information (i.e. parents with multiple children or one child born prior to January 1 of the current year).

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). One study demonstrates that parents of children born in December (versus January) are more likely to report earnings in this range for the year ending with their child's birth (LaLumia, Sallee, and Turner 2015). This illustrates that there is at least some awareness of the incentive to *report* earnings in the range that maximizes one's credit *in the year that there is differential eligibility.* If there were a meaningful and persistent difference in information or incentives, we would expect to see this differential bunching

persist past the first year. This is not what is observed. Instead, this 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.[65]

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. While only suggestive, the estimated effects on marital status are 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). While these qualitative results are consistent with the pattern of effects in our study, the magnitude of the estimated effects on marital status in early childhood are too small to explain much of the subsequent earnings effects that we observe.[66] That said, we view the observed changes in marital status as being suggestive of other changes occurring within the household, such as reduced stress, that likely influence child outcomes. A similar exercise using the observed increases in family income across subsequent years suggest a more important role for this channel in contributing to subsequent earnings increases. Combining the estimated increase in annual earnings (1.6 percent) with an intergenerational elasticity of earnings (IGE) of around 0.3 would imply an increase in child earnings of roughly 0.5 percent.[67] Under the

[65] Additional evidence is provided by a recent study using a similar strategy in a less liquidity-constrained population. Mortenson et al. (2018) find no positive 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."

[66] For example, the Laird et al. (2020) study suggests that a delay in the timing of divorce by 4 years would increase the likelihood of high-school graduation by 1.6 percentage points (3.4 percent). In combination with our estimated effects (at most a 2 pp increase in the likelihood of being married at ages 3-4), this would explain less than 5 percent of our estimated high-school graduation effect.

[67] We adopt the IGE from Chetty et al. (2014), which produces IGEs for similar cohorts in Appendix Table A1. They produce IGEs using mean parent income over the five years when the child is 15-19 years old. Our approximation

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 *at most* a third of the observed increase in 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, plays an important role in generating improved outcomes.

5.2 Effects of Income on K-12 Outcomes

We turn to the North Carolina administrative education data to better understand how the short-term effects on income, family structure, and earnings translate to long-run effects on earnings.[68] Table 7 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 percent of the gap between those eligible for FRL and those who are not. It is robust to the inclusion of covariates or controls for the birth day of the week.[69] Figure 7 illustrates these results graphically, with a clear jump down as we move across the eligibility threshold. The estimates are largely stable across donut sizes (Appendix Figure A14) and bandwidths (Appendix Figure A15).[70]

The student outcome estimates presented are all intent-to-treat effects of being born prior to January 1 (and thus more likely to receive additional income). We can scale the effects by the size of the implied increase in resources contained in the bottom row of Table

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 is an upper bound.

[68] While we are underpowered to detect earnings effects specific to North Carolina (North Carolina only accounts for roughly 3 percent of the United States population and contributes roughly 18,000 observations to our sample), Appendix Table A10 provides the RD estimates specific to individuals born in the state. The estimates are positive, but quite large with the associated confidence intervals including both our full sample estimates and (often) zero.

[69] Appendix Table A11 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 Appendix Table A12 shows robustness to alternate index constructions.

[70] The sole exception is a donut size of 4, 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 β_1.

7 of roughly $1,595.[71] This implies effects of 0.03 standard deviations per $1,000. Put another way, the estimates suggest that the 2020 maximum EITC and child tax credit for a single child closes the gap between those eligible for FRL and those who are not by between a quarter and a half. Appendix Table A13 provides the same estimates from column 1 of Table 7 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 males and females. 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 female 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.[72]

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.[73] Even using our simulated tax benefit (which does not capture incomplete take up or differential take up), we see a slightly larger increase in income 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. 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

[71] As mentioned previously, we use the tax data to estimate the implied discontinuity in transfer size during infancy among this group of students ($1,595). First, we capture the set of children meeting the following conditions: (1) the child is born in North Carolina in recentered birth years within the same sample period where we observe the universe of tax filings up to two years prior to birth (i.e., 1995-1998), and (2) the child is ever eligible for FRL in grade 3-8 (based on their age and the income information on parent's tax filings). Next, we link these children back to their parent's tax filing in the closest year prior to their birth where we observe the universe of tax filings. Finally, we estimate the implied discontinuity in transfer size during infancy using the average of the simulated benefit difference at the January 1 cutoff for those born before the cutoff.

[72] Interestingly, we do 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.

[73] 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.

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 8, we present estimates separately by schooling outcome.[74] Additional resources increases an index of math and reading test scores in 3rd through 8th grade by 0.04 to 0.05 standard deviations. These effects represent roughly 6 to 7 percent 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 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.

5.3 Are Wage Effects Explained by Human Capital Effects?

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 et al. (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 SD) 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 through 8, could entirely account for our observed wage effects at ages 26 to 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 (Appendix Figure A19).[75]

[74] These estimates are presented graphically in Appendix Figure A16, A17, and A18.

[75] In addition to grades 3 through 8, this figure also contains estimates for the standardized tests taken by most students in high school (Algebra and English), labeled as "HS". In Table A14, we summarize the point estimates for high-school measures more generally.

6 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 the first evidence on the effects of cash transfers provided during this period. We do so by taking advantage of a discontinuity in eligibility for child-related tax benefits following the birth of a child. Combined with the universe of 1040 tax data with parent-child linkages spanning four decades and detailed education data from North Carolina, we demonstrate that cash transfers during this sensitive window can have profound and long-lasting effects. For an additional $1,000 in early childhood, earnings at age 23 to 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.

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 during the critical period following childbirth results 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 imply that these improvements only account for roughly a third of the observed effects on children. These results suggest an important role for liquidity in the year after birth and underscore the relative importance of resources during the early childhood period.

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. While we are able to provide convincing evidence of the effect of a few thousand dollars during the first year of life, our results are limited in their ability to inform our understanding of the effects of larger transfers or transfers provided at different ages. With those caveats, our results do suggest that additional resource transfers to poor families around the time of birth would result in substantial improvements in social mobility.

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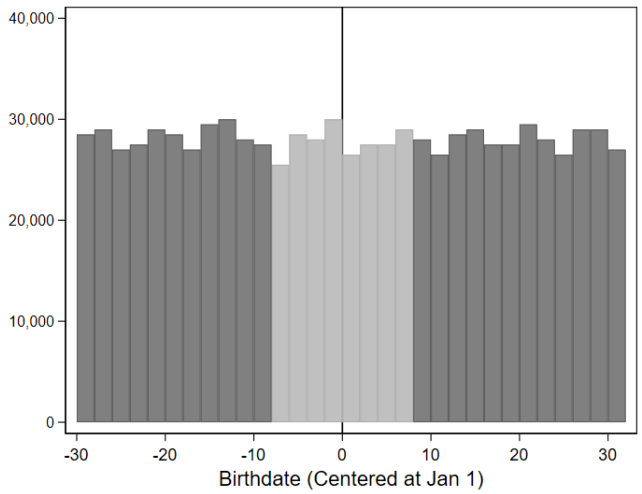
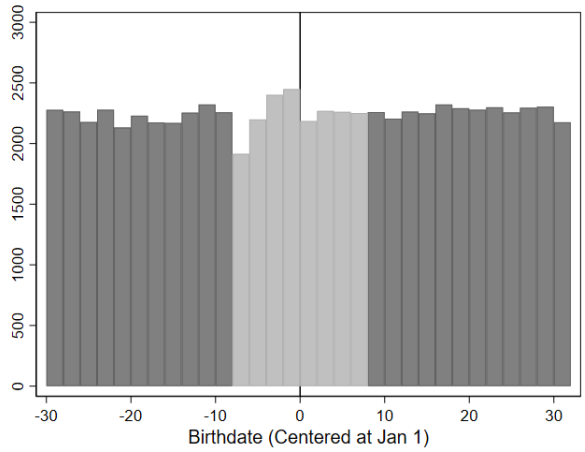
Figure 1: Distribution of Birthdates by Sample



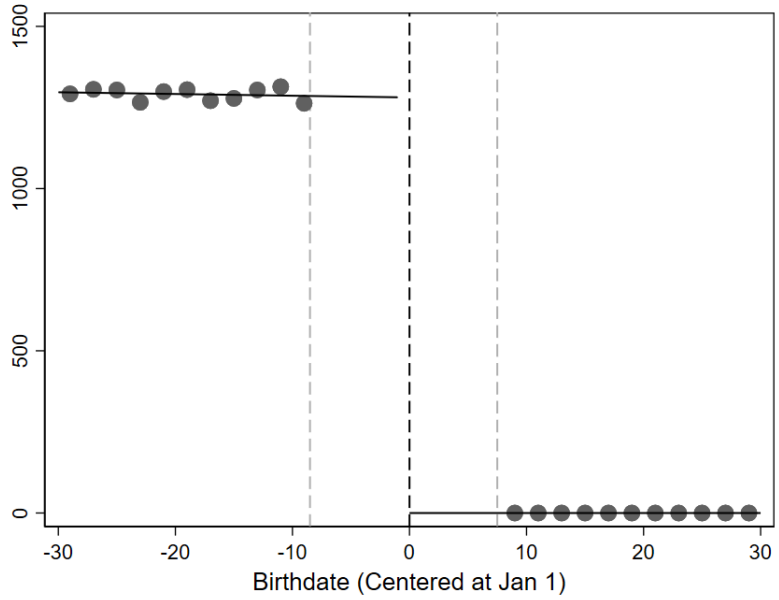
(A) Tax Data (EITC-Eligible Families)



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

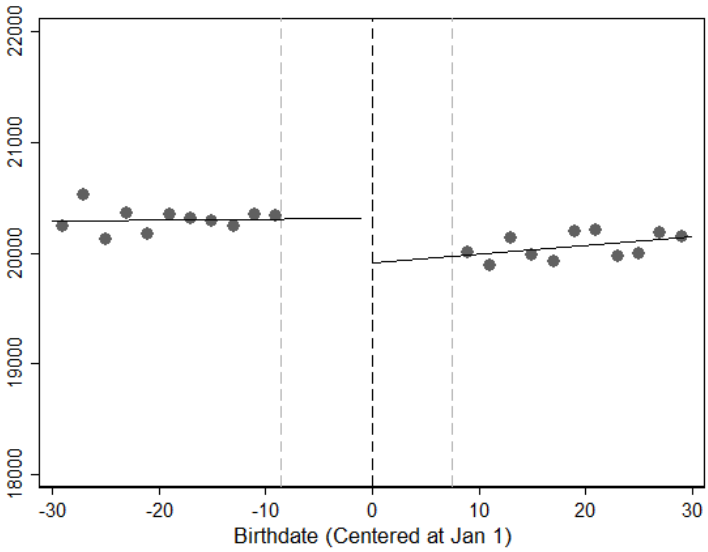
Note: Panel A displays the distribution of birthdates (relative to January 1) for all individuals who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. Panel B displays the distribution of birthdates (relative to January 1) for FRL-eligible students born within 28 days of January 1 in years 1993 to 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.

Figure 2: Effect of Cash Transfer Eligibility on Additional Resources During Infancy

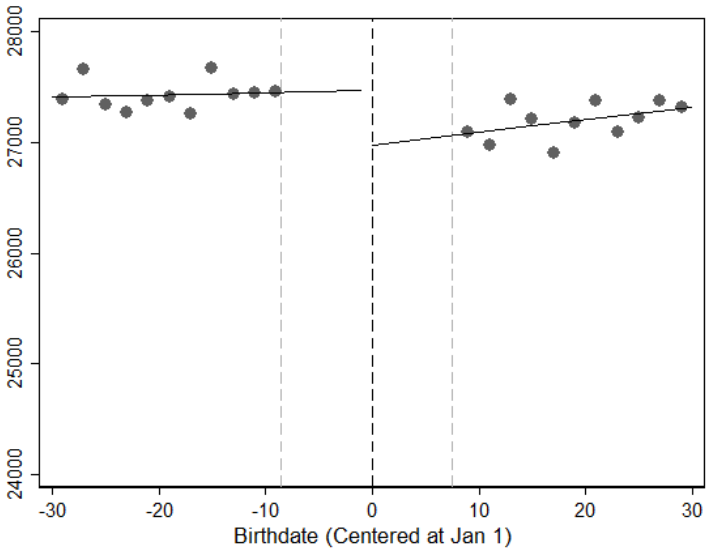


Note: The figure displays mean cash transfer infancy by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. Cash transfer in infancy reflects the 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 income in the relevant tax year and then using NBER's TAXSIM program to determine tax benefit eligibility (see 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 a boost in income in the following year. See Table 1 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.

Figure 3: Effect of Cash Transfer Eligibility on Adult Earnings



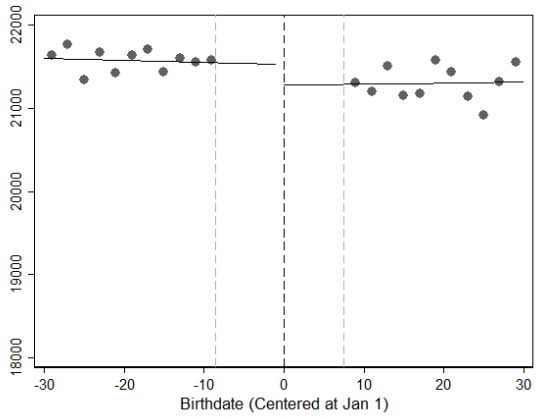
(a) Earnings (23 to 25)



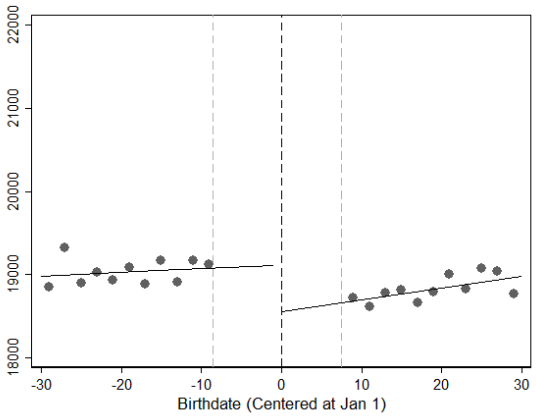
(b) Earnings (26 to 28)

Note: The figure displays mean earnings by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 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 3-year average of the earnings (including non-filers 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 a boost in income in the following year (if eligible based on income). See Table 1 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.

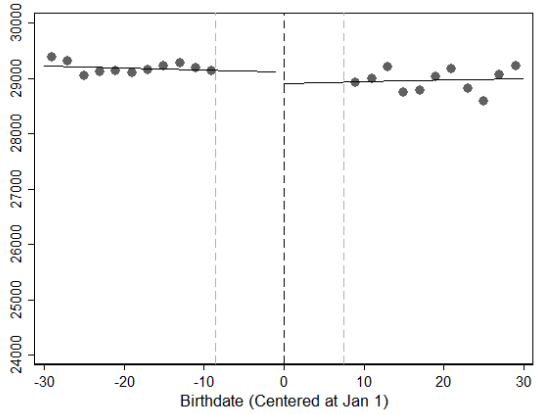
Figure 4: Heterogeneity by Sex in the Effect of Cash Transfer Eligibility on Adult Earnings



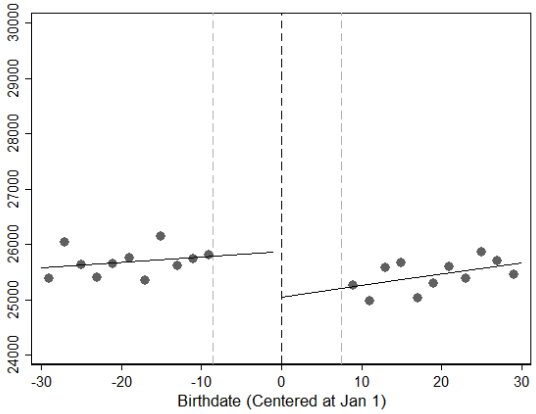
(a) Female Earnings (23 to 25)



(b) Male Earnings (23 to 25)



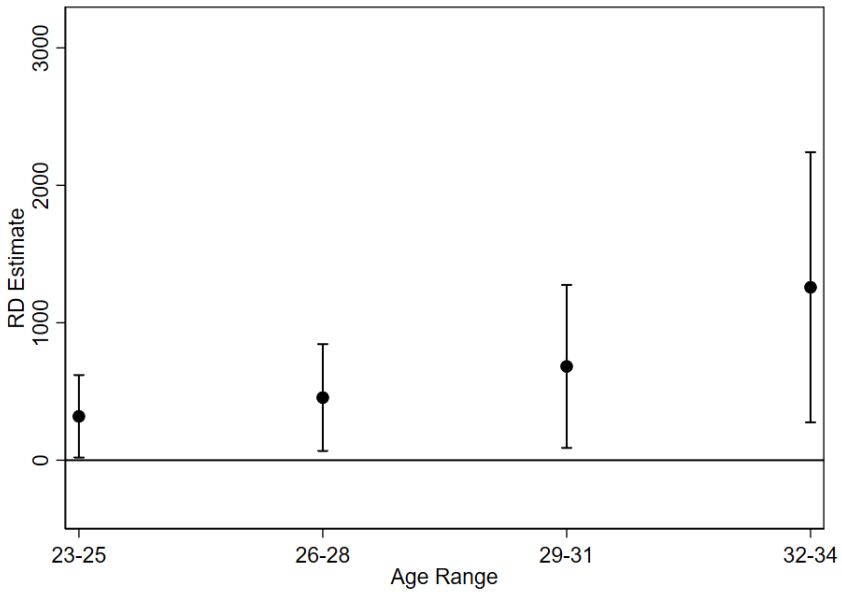
(c) Female Earnings (26 to 28)



(d) Male Earnings (26 to 28)

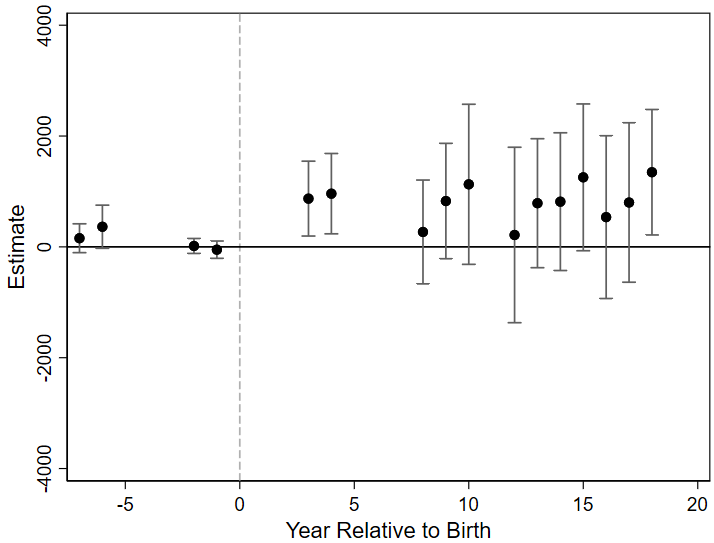
Note: The figure displays mean earnings by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. The earnings outcome is constructed as the 3-year average of the earnings (including non-filers 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 a boost in income in the following year (if eligible based on income). See Table 1 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.

Figure 5: Effect of Cash Transfer Eligibility on Adult Earnings: By Age

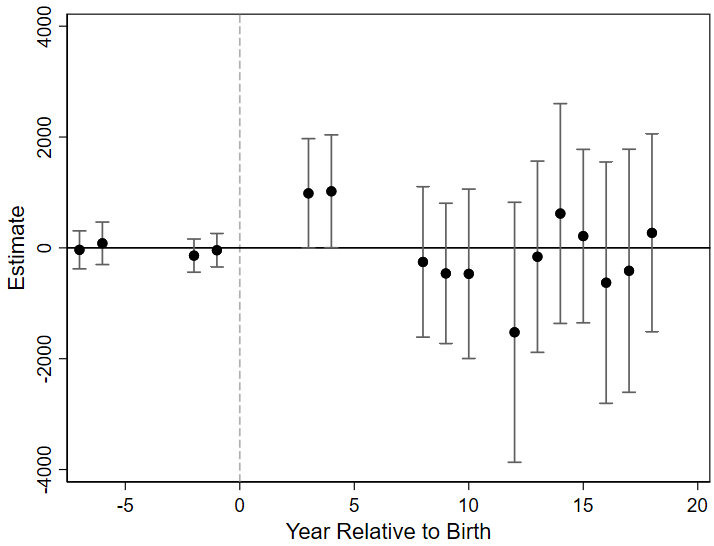


Note: The figure displays the basic regression discontinuity estimate by age range. Sample changes across estimates due to particular cohorts not having reached the observed ages. See Table 1 and the text for additional details on sample restrictions, specification, and construction of outcome variables. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.

Figure 6: Effect of Cash Transfer Eligibility on Family Resources Before and After Birth



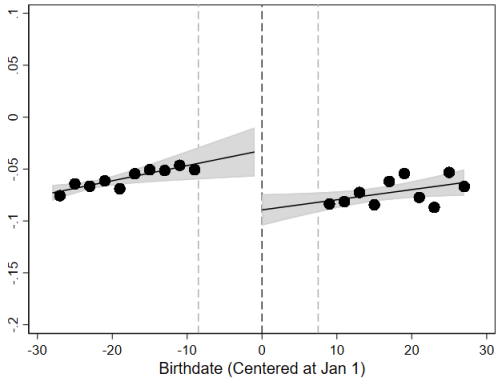
(a) Full Sample



(b) Filers

Note: The figure displays the basic regression discontinuity estimate 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 prior to the birth of their child. Observed years are limited by tax data availability. See Table 1 and the text for additional details on sample restrictions, specification, and construction of outcome variables.

Figure 7: Effect of Cash Transfer Eligibility on Student Outcome Index (North Carolina)



Note: The figure displays the mean student outcome index by 2-day birthdate bin for FRL-eligible students born within 28 days of January 1 in years 1993 to 1998 who entered a North Carolina public school by grade 5. Student outcome index is constructed as the mean of normalized test scores in grade 3-8, high school graduation, and any suspension. The horizonal 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 a boost in income in the following year (if eligible based on income). The shaded area shows the 95% confidence interval.

Table 1: Summary Statistics

	(1)
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

Note: In Panel A, the sample is restricted to individuals born within 28 days of January 1 in years 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 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 Appendix B for more details). In Panel B, the sample consists of FRL-eligible students born within 28 days of January 1 in years 1993 to 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 verbal test scores in grades 3 through 8. Student outcome index is constructed as the mean of normalized test scores, high school graduation, and any suspension. 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.

Table 2: Balance on Baseline Characteristics

	(1) Child Male	(2) Child White	(3) Child Black	(4) Child Hispanic	(5) Parent Max. Age	(6) Parent Filed 1040	(7) Parent Married	(8) Parent Pred. AGI	(9) Parent In Poverty
Born Before Jan 1	0.005	0.005	-0.003	-0.00	0.04	0.002	-0.003	19.35	0.001
	(0.004)	(0.006)	(0.003)	(0.005)	(0.07)	(0.003)	(0.003)	(71.67)	(0.003)
Mean	0.501	0.630	0.134	0.174	24.06	0.364	0.048	12,530	0.788

Note: Each cell shows the β_1 coefficient (RD) estimate from a separate regression where the column denotes the baseline characteristic serving as the dependent variable. Parent/family variables are constructed from pre-birth filing information. See the text for additional details on variable construction and sample restrictions. The sample is restricted to individuals born within 28 days of January 1 in years 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 8 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 indicated by: * ($p<0.10$), **($p<0.05$), ***($p<0.01$).

Table 3: Effect of Cash Transfer Eligibility on Adult Earnings

	(1)	(2)	(3)
Earnings (23 to 25)	318.9**	293.0**	295.2**
	(153.0)	(152.6)	(150.1)
Mean	*20,050*	*20,050*	*20,050*
Earnings (26 to 28)	455.6**	429.7**	433.4**
	(198.4)	(201.0)	(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
Lagged Income Control			X

Note: Each cell shows the β_1 coefficient (RD) estimate from a separate regression where the row denotes the outcome variable. The earnings outcome is constructed as the 3-year average of the earnings (including non-filers as zeroes) at the filing unit level. The sample is restricted to individuals born within 28 days of January 1 in years 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 8 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 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 indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table 4: Effect of Cash Transfer Eligibility on Adult Earnings By Cohort

	1981-82 (1)	1986-87 (2)	1991-92 (3)	All (4)
Earnings (23 to 25)	103.9	103.9	665.5***	318.9**
	(283.3)	(259.5)	(257.5)	(153.0)
Mean	*21,590*	*18,910*	*19,830*	*20,050*
Earnings (26 to 28)	134.3	475.2	687.3*	455.6**
	(408.2)	(367.5)	(372.0)	(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

Note: Each cell shows the β_1 coefficient (RD) estimate from a separate regression where the row denotes the outcome variable. The earnings outcome is constructed as the 3-year average of the earnings (including non-filers as zeroes) at the filing unit level. Each column indicates the set of re-centered birth years included. The sample is restricted to individuals born within 28 days of January 1 in the given re-centered 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 8 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 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 indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table 5: Heterogeneity in the Effect of Cash Transfer Eligibility on Adult Earnings

	All (1)	Female Child (2)	Male Child (3)	Single[a] Parent (4)	Married[a] Parent (5)	Filer[a] Parent (6)	Non-filer[a] Parent (7)
Earnings (23 to 25)	318.9**	110.6	559.6***	571.4*	-229.1	451.2	228.7
	(153.0)	(165.4)	(209.7)	(329.1)	(559.9)	(294.5)	(155.7)
Mean	*20,050*	*21,280*	*18,830*	*21,790*	*24,400*	*22,140*	*18,860*
Earnings (26 to 28)	455.6**	168.3	781.9***	976.3**	-1086.0	676.3	307.3
	(198.4)	(210.9)	(293.7)	(423.2)	(1083.0)	(413.8)	(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

Note: Each cell shows the β_1 coefficient (RD) estimate from a separate regression where the row denotes the outcome variable and the column denotes the subsample. *a* - parent information (i.e., married, single, filer, non-filer) derived from pre-birth tax filing information (see Appendix B for more information). The earnings outcome is constructed as the 3-year average of the earnings (including non-filers as zeroes) at the filing unit level. Parent/family variables are constructed from pre-birth filing information. The sample is restricted to individuals meeting the given subsample criteria who were born within 28 days of January 1 in years 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 8 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 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 indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table 6: Heterogeneity by Race/Ethnicity in the Effect of Cash Transfer Eligibility on Adult Earnings

	Black			Hispanic			White (Non-Hisp)		
	All	Male	1991-92	All	Male	1991-92	All	Male	1991-92
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Earnings (23 to 25)	944.0**	1,075.0*	1836.0**	614.6	67.8	276.0	55.3	628.7*	624.0*
	(472.7)	(601.0)	(753.2)	(521.0)	(582.2)	(691.2)	(201.8)	(337.5)	(327.7)
Mean	*21,460*	*20,220*	*20,970*	*21,280*	*20,200*	*20,500*	*21,570*	*20,450*	*20,910*
Earnings (26 to 28)	1,237.0	883.3	1,644.0	617.0	-394.4	120.0	519.8	1370.0***	1048.0**
	(810.5)	(1048.0)	(1266.0)	(727.8)	(1,005.0)	(978.4)	(325.6)	(483.2)	(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

Note: Each cell shows the β_1 coefficient (RD) estimate from a separate regression where the row denotes the outcome variable and the column denotes the subsample. The earnings outcome is constructed as the 3-year average of the earnings (including non-filers as zeroes) at the filing unit level. The sample is restricted to individuals meeting the given subsample criteria who were born within 28 days of January 1 in years 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 8 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 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 indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table 7: Effect of Cash Transfer Eligibility on Student Outcome Index (North Carolina)

	(1)	(2)	(3)
Born Before Jan 1	0.051***	0.051***	0.047***
	(0.016)	(0.016)	(0.016)
Obs	*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

Note: Each cell shows the β_1 coefficient estimate 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 (grade 3-8), high school graduation, and any suspension. 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 years 1993 to 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 eligible for FRL based on their reported 1040 AGI at the relevant ages. See the text for additional details on variable construction and sample restrictions. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

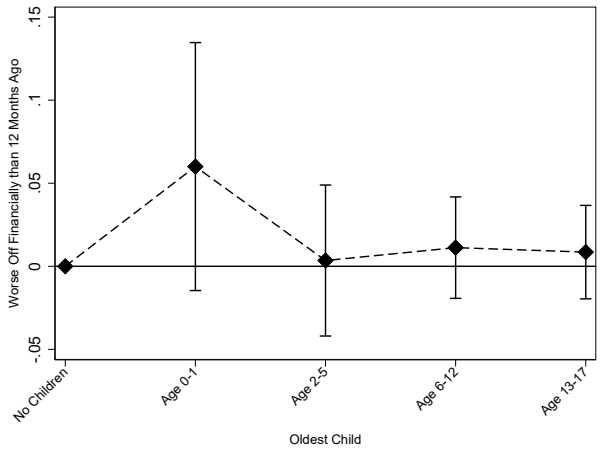
Table 8: Effect of Cash Transfer Eligibility on Individual Student Outcomes (North Carolina)

	(1)	(2)	(3)
Test Score Index	0.046**	0.044**	0.036*
	(0.020)	(0.021)	(0.020)
Obs	*44,984*	*44,984*	*44,984*
Mean	*0.035*	*0.035*	*0.035*
Graduate HS	0.022**	0.022**	0.023**
	(0.011)	(0.011)	(0.011)
Obs	*36,519*	*36,519*	*36,519*
Mean	*0.748*	*0.748*	*0.748*
Ever Suspended	-0.020*	-0.021*	-0.020*
	(0.011)	(0.011)	(0.011)
Obs	*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

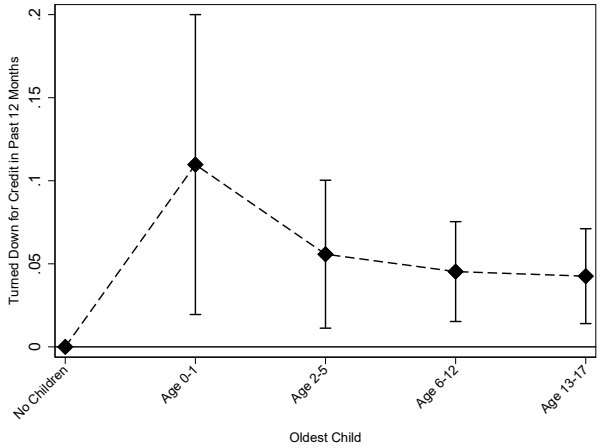
Note: Each cell shows the β_1 coefficient estimate from a separate regression where the row denotes the student outcome and the column denotes the inclusion of different controls. The test score index is constructed as the mean of normalized (mean zero, standard deviation one) math and verbal test scores in grades 3 through 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 years 1993 to 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 1 and text for additional sample restrictions and information on variable construction. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Supplemental Figures and Tables

Figure A1: First Birth and Potential Liquidity Constraints



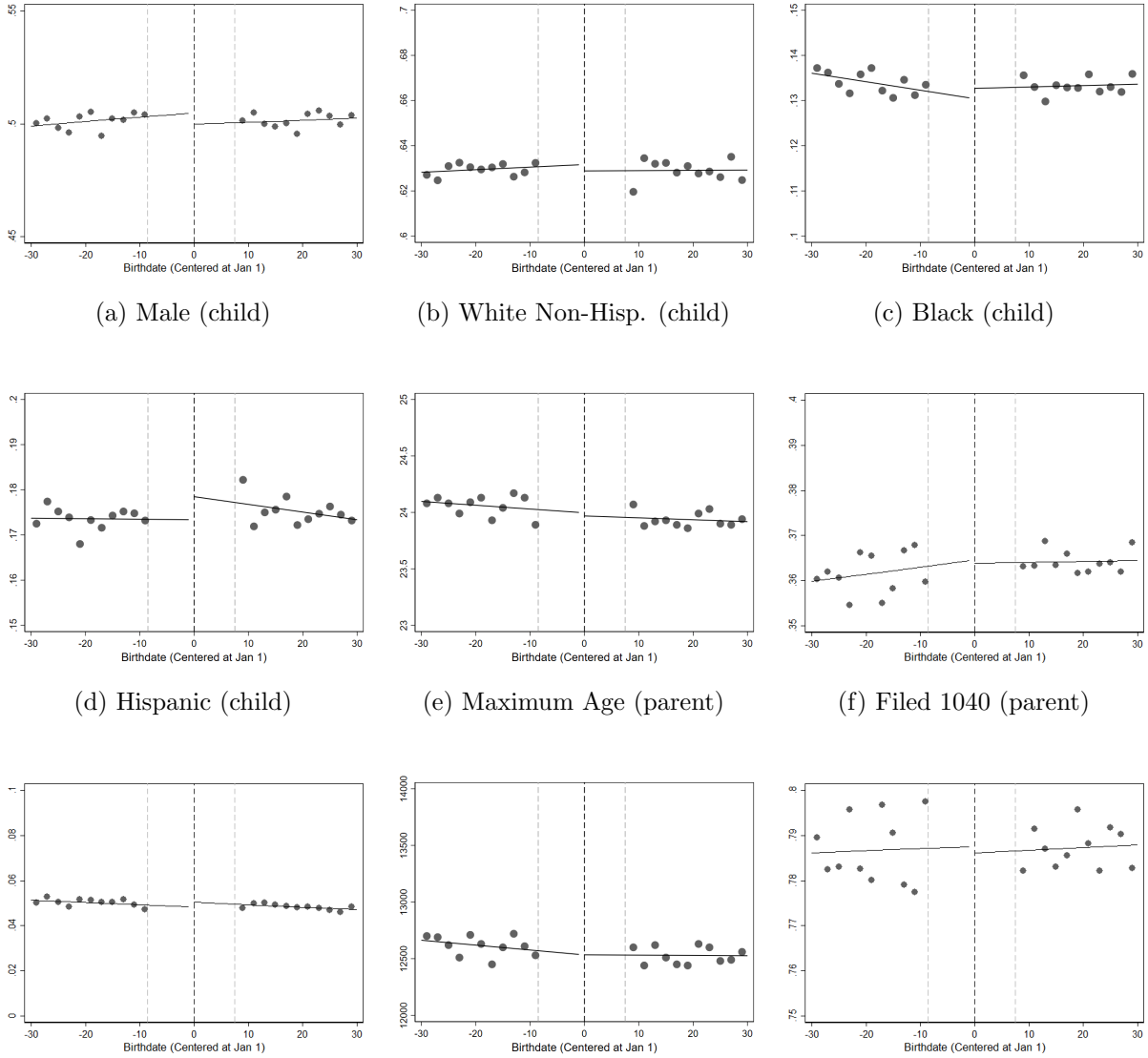
(a) Worse Off Financially



(b) Denied Credit

Note: The figure displays the coefficient estimates (and 95% confidence intervals using robust standard errors) for indicators of age group of oldest child (relative to no children) from a regression that also includes parent age category, region, and year fixed effects. In panel (a), the dependent variable is an indicator equal to one if a respondent reports being "Much worse off" or "Somewhat worse off" financially than 12 months ago. In panel (b), the dependent variable is an indicator equal to one if a respondent reports being "Turned down for credit" in the past 12 months. The sample includes respondents under age 50 from waves 2015-2019 of the Survey of Household Economics and Decision-making.

Figure A2: Balance on Baseline Characteristics



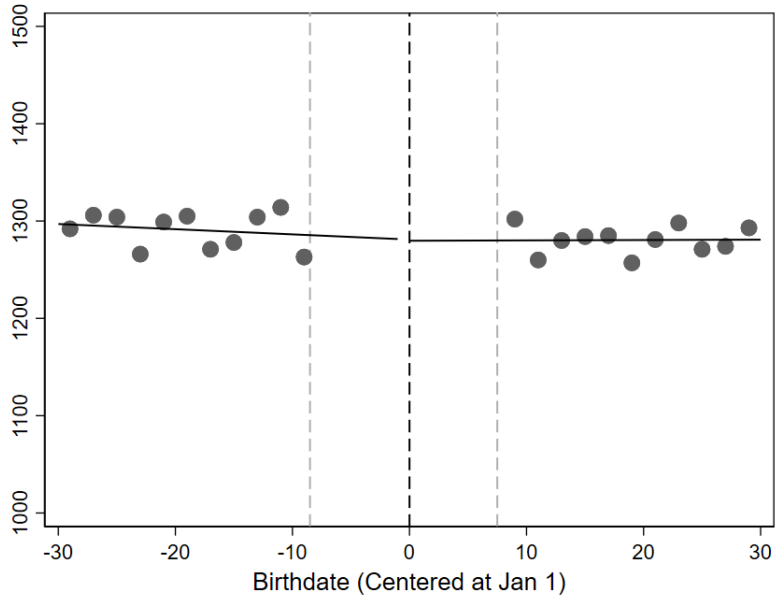
(a) Male (child) (b) White Non-Hisp. (child) (c) Black (child)

(d) Hispanic (child) (e) Maximum Age (parent) (f) Filed 1040 (parent)

(g) Married (parent) (h) Predicted AGI (parent) (i) In Poverty (parent)

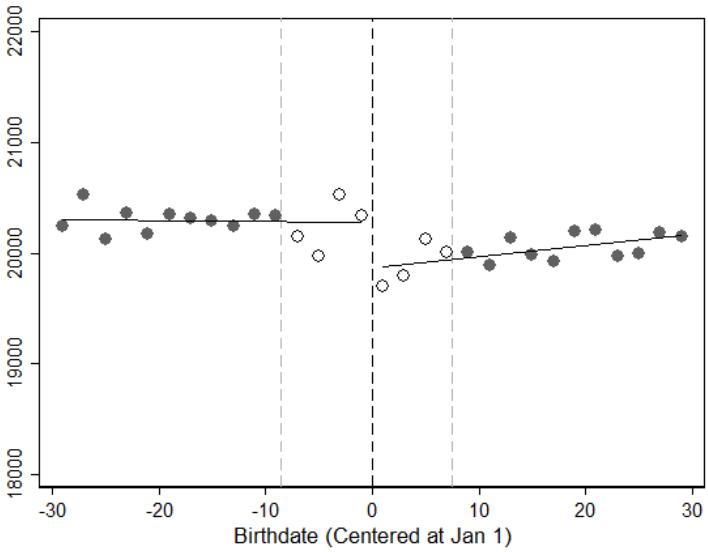
Note: The figure displays the mean of each baseline covariate by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. Parent/family variables are constructed from pre-birth filing information. 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 a boost in income in the following year. See Table 2 for the RD estimates associated with each graph. See Table 1 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.

Figure A3: Balance on Potential Additional Resources During Infancy had Child been Born Prior to Jan 1

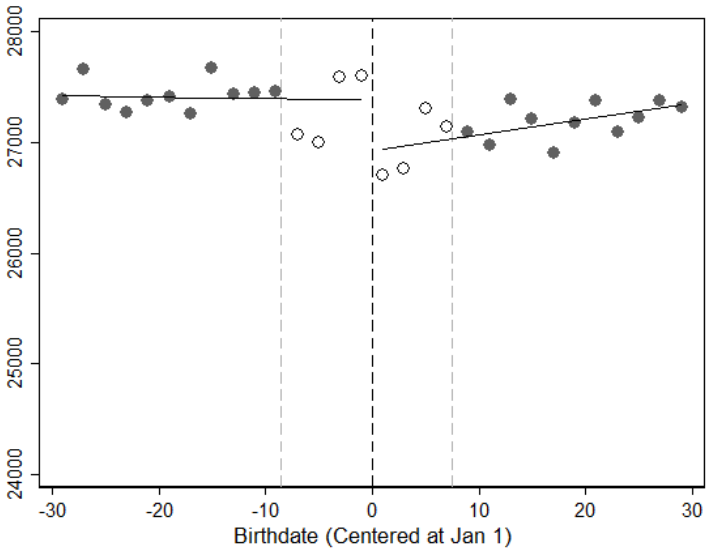


Note: The figure displays the mean potential additional income by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. Potential additional income is defined as the extra income from child-related tax benefits that a child's family would have received if they had been born prior to January 1 (regardless of when they were actually born). It is calculated using information from prior tax filings combined with NBER's TAXSIM program. The figure displays the estimated tax benefit of an additional dependent child for all birthdays, while only those to the left of the threshold are eligible. 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 a boost in income in the following year. See Table 1 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.

Figure A4: Effect of Cash Transfer Eligibility on Adult Earnings (No Donut)



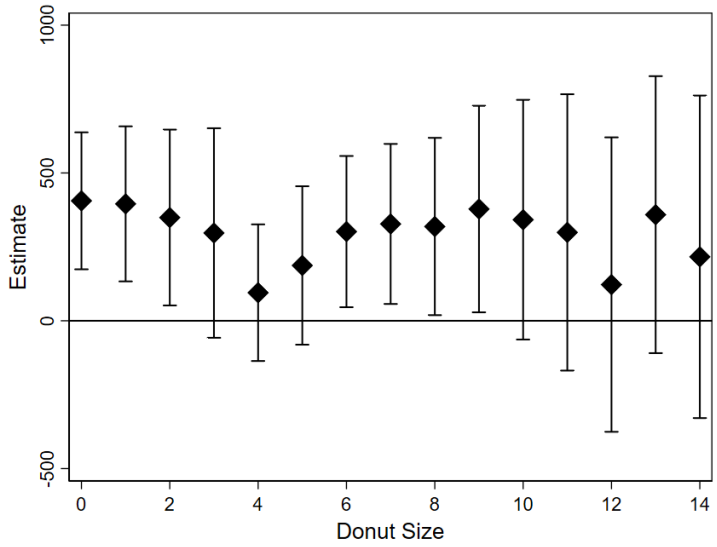
(a) Earnings (23 to 25)



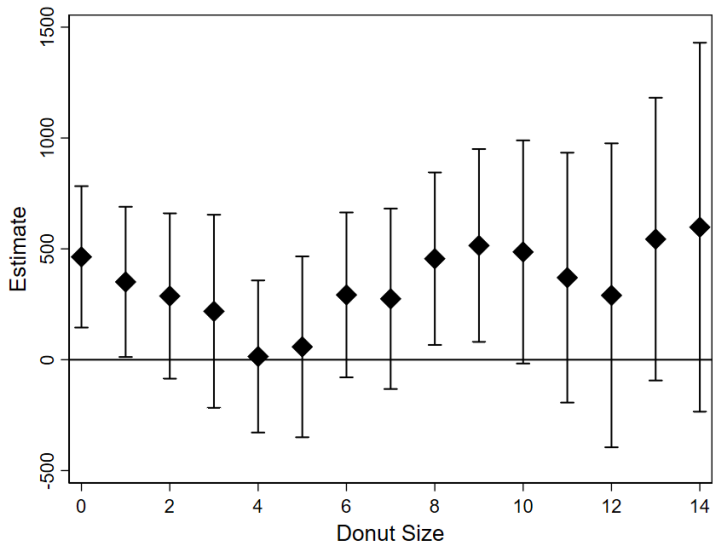
(b) Earnings (26 to 28)

Note: The figure displays mean earnings by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 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 3-year average of the earnings (including non-filers as zeroes) at the filing unit level. The 8 days on either side of January 1 are not excluded. 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 a boost in income in the following year. See Table 1 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.

Figure A5: Adult Earnings RD Estimates by Donut Size



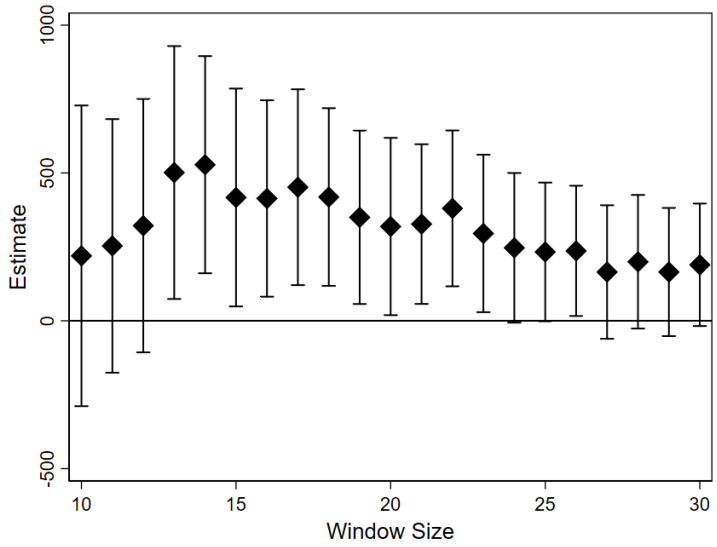
(a) Earnings (23 to 25)



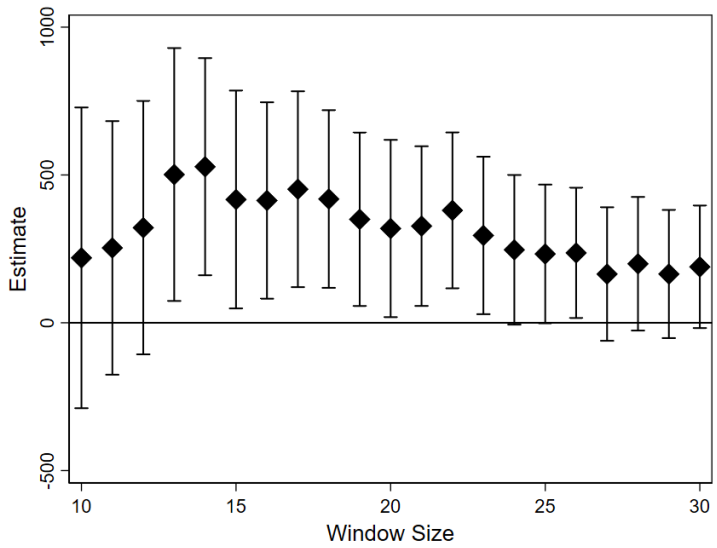
(b) Earnings (26 to 28)

Note: The figure displays an estimate of β_1 (the January 1 RD estimate) of earnings at ages 23-25 and 26-28 for various donut sizes. A bandwidth of 20 days on either side of the donut is used throughout. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.

Figure A6: Adult Earnings RD Estimates by Bandwidth



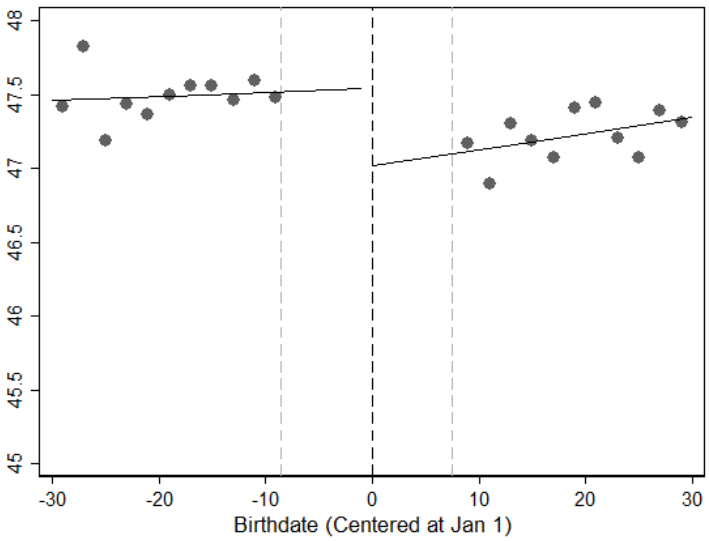
(a) Earnings (23 to 25)



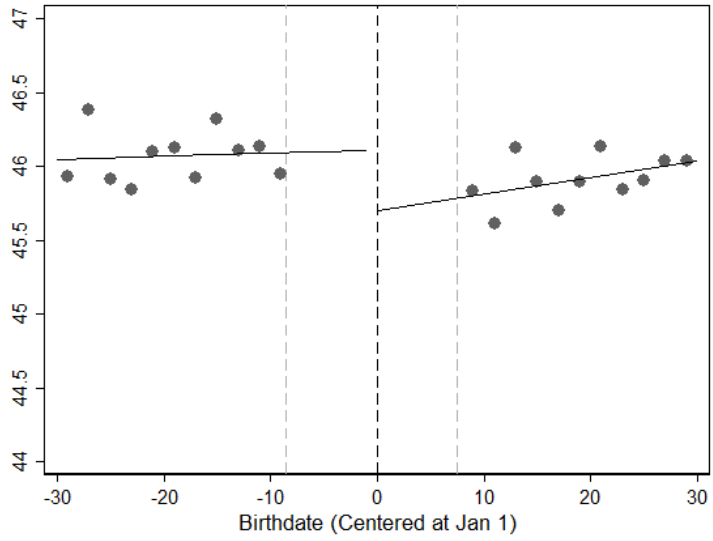
(b) Earnings (26 to 28)

Note: The figure displays an estimate of β_1 (the January 1 RD estimate) of earnings at ages 23-25 and 26-28 for various bandwidth sizes (on either side of the donut). A donut of 8 days on either side of the cutoff is used throughout. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.

Figure A7: Effect of Cash Transfer Eligibility on Adult Earnings Percentile



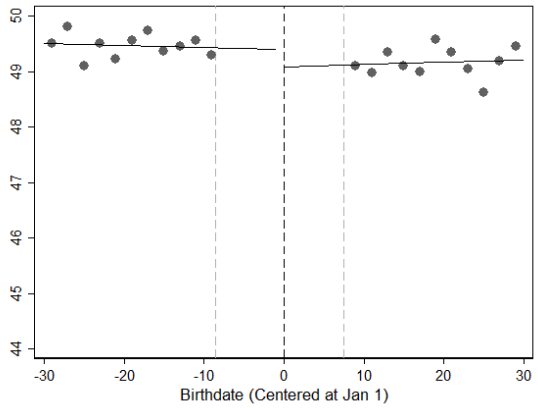
(a) Earnings Percentile (23 to 25)



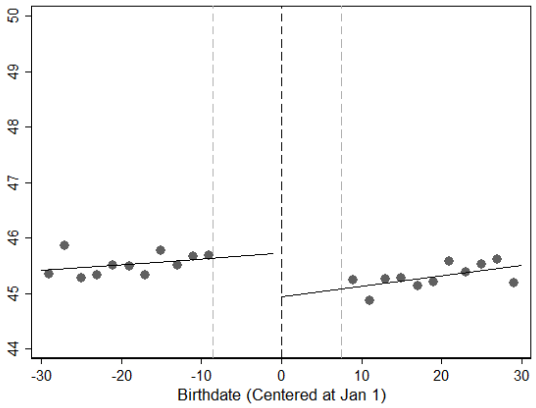
(b) Earnings Percentile (26 to 28)

Note: The figure displays mean adult earnings percentile by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 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. Adult earnings percentile is constructed by taking the 3-year average of earnings at the filing unit level (counting non-filing as zero) and then finding the percentile of that average within a child's cohort. 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 a boost in income in the following year. See Table 1 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.

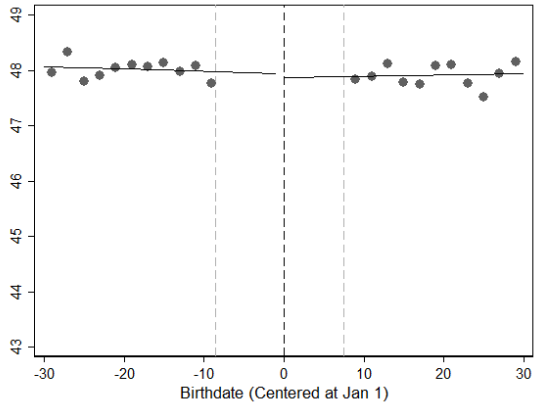
Figure A8: Heterogeneity by Sex in the Effect of Cash Transfer Eligibility on Adult Earnings Percentile



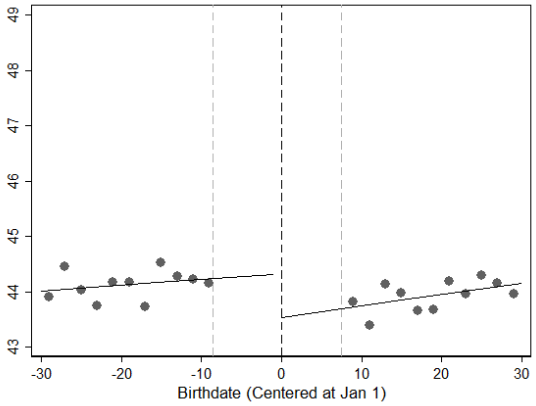
(a) Female Percentile Earnings (23 to 25)



(b) Male Percentile Earnings (23 to 25)



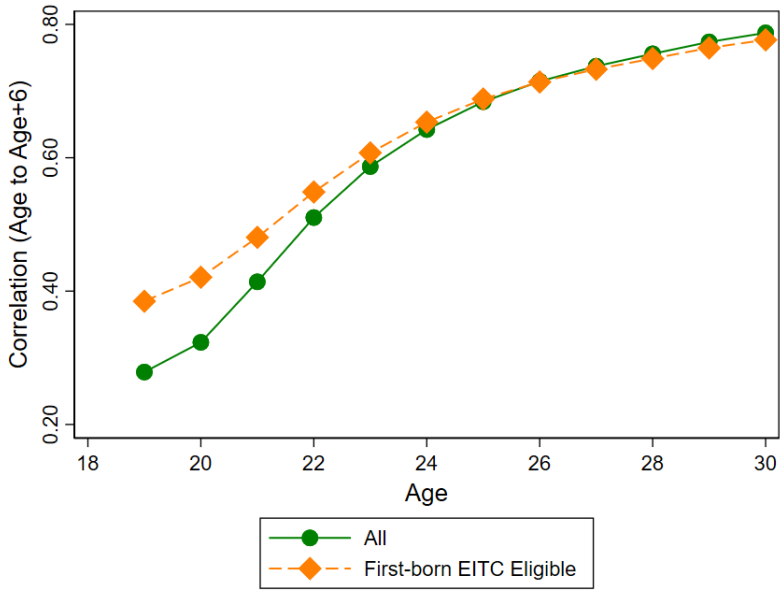
(c) Female Percentile Earnings (26 to 28)



(d) Male Percentile Earnings (26 to 28)

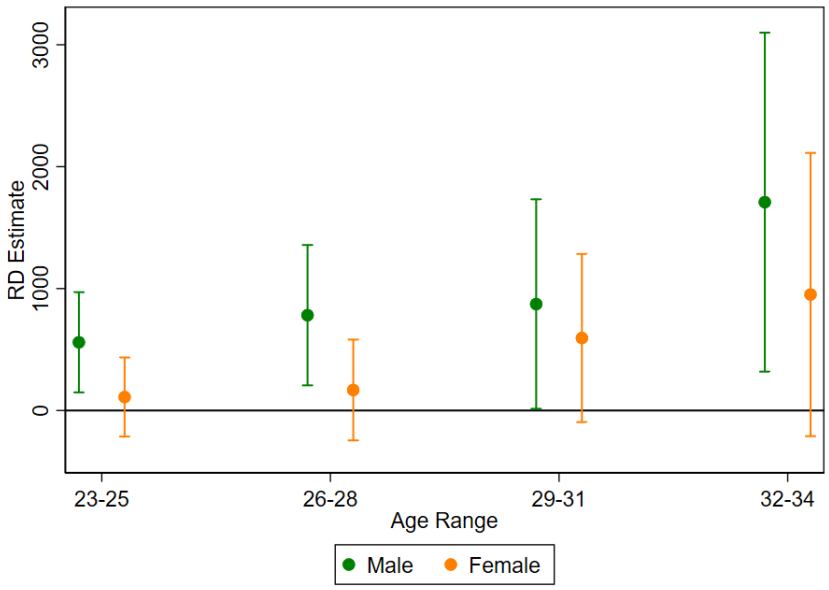
Note: Each figure displays mean adult earnings percentile by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. Adult earnings percentile is constructed by taking the 3-year average of earnings at the filing unit level (counting non-filing as zero) and then finding the percentile of that measure within the distribution of each child's cohort. 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 a boost in income in the following year. See Table 1 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.

Figure A9: Correlation Between Earnings at Age t and Age $t+6$



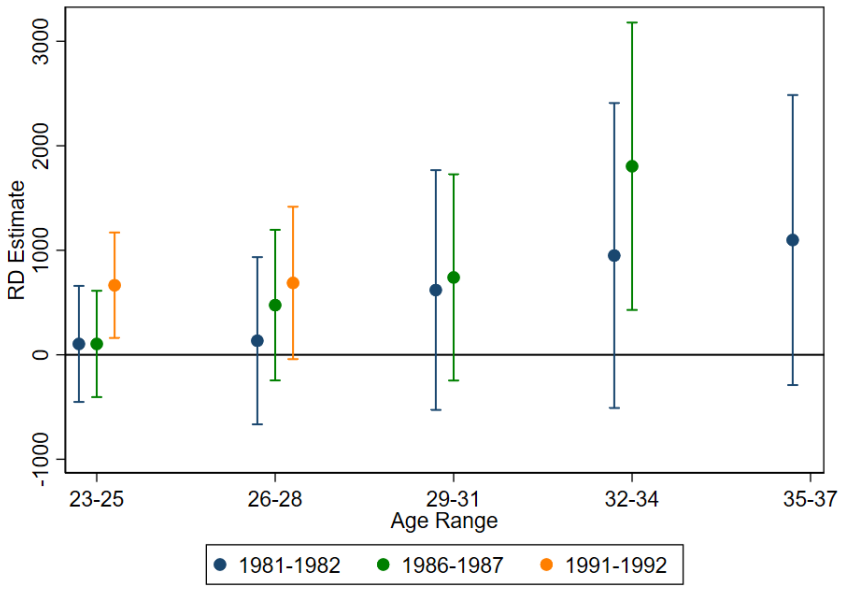
Note: The figure displays the correlation between earnings measured at age t and earnings at age $t+6$ within our analytical sample as well as that sample with parent income restriction removed. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.

Figure A10: Effect of Cash Transfer Eligibility on Adult Earnings: By Age and Sex



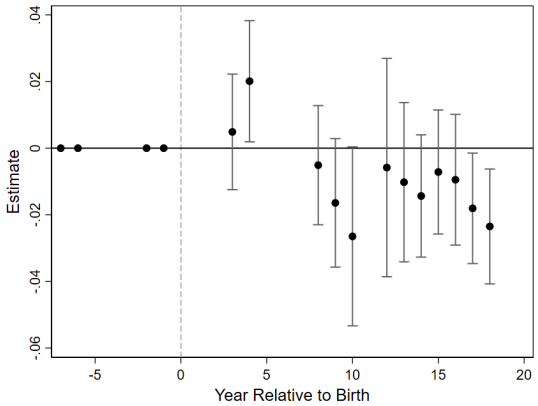
Note: The figure displays the basic regression discontinuity estimate by age range and sex. Sample changes across estimates due to particular cohorts not having reached the observed ages. See Table 1 and the text for additional details on sample restrictions, specification, and construction of outcome variables. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.

Figure A11: Effect of Cash Transfer Eligibility on Adult Earnings: By Age and Cohort

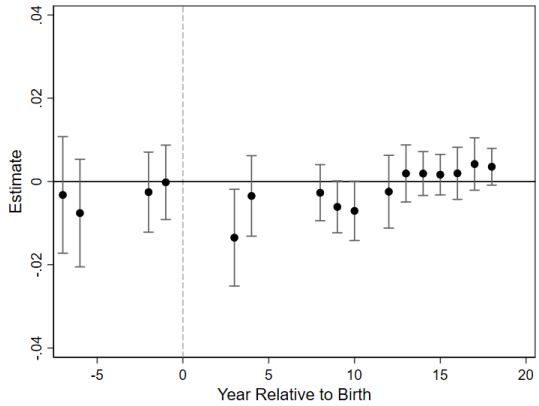


Note: The figure displays the basic regression discontinuity estimate by age range and cohort. For cohorts that are not observed at all ages in a given age range, we take the mean of ages at which they are observed. See Table 1 and the text for additional details on sample restrictions, specification, 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.

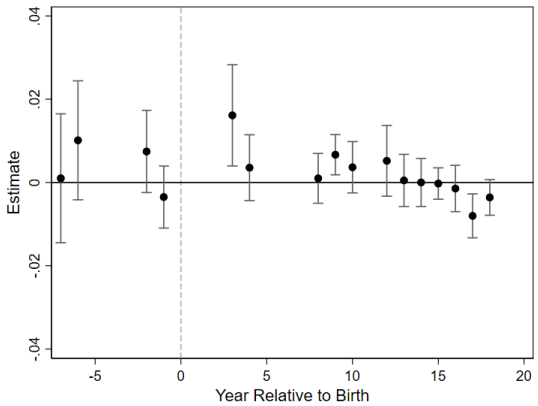
Figure A12: Effects of Eligibility on Early Family Environment Before and After Birth



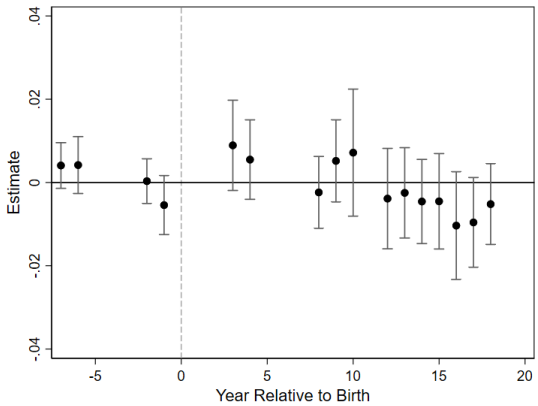
(a) Dependents



(b) Parent Poverty Status



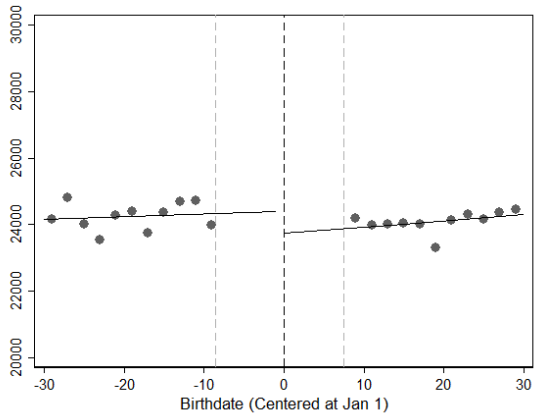
(c) Parent File 1040



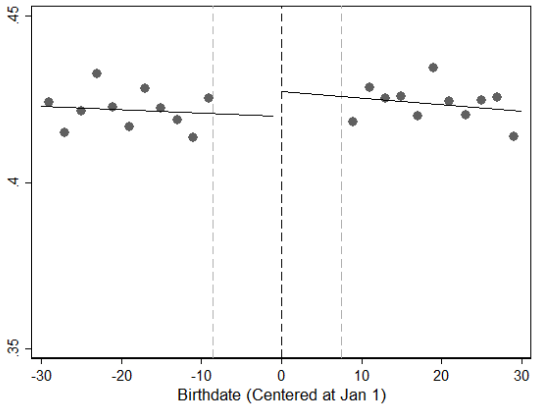
(d) Parent Marital Status

Note: The figure displays the basic regression discontinuity estimate for parental outcomes 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). Observed years are limited by tax data availability. See Table 1 and the text for additional details on sample restrictions, specification, and construction of outcome variables.

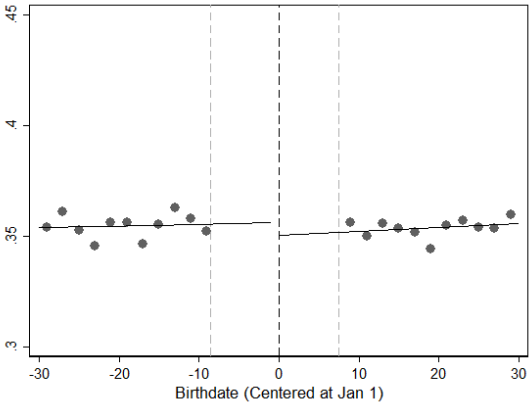
Figure A13: Effects of Eligibility on Early Family Environment (3-4 Years After Birth)



(a) Parent Earnings



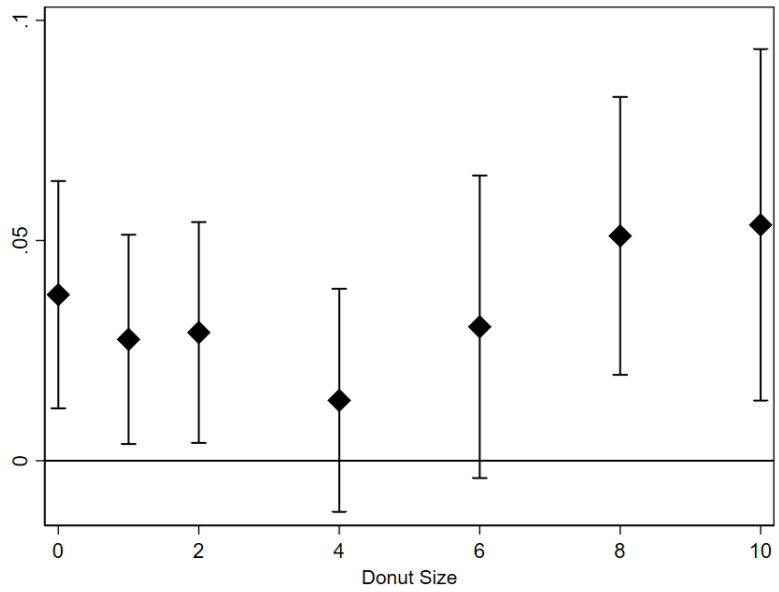
(b) Parent Poverty Status



(c) Parent Marital Status

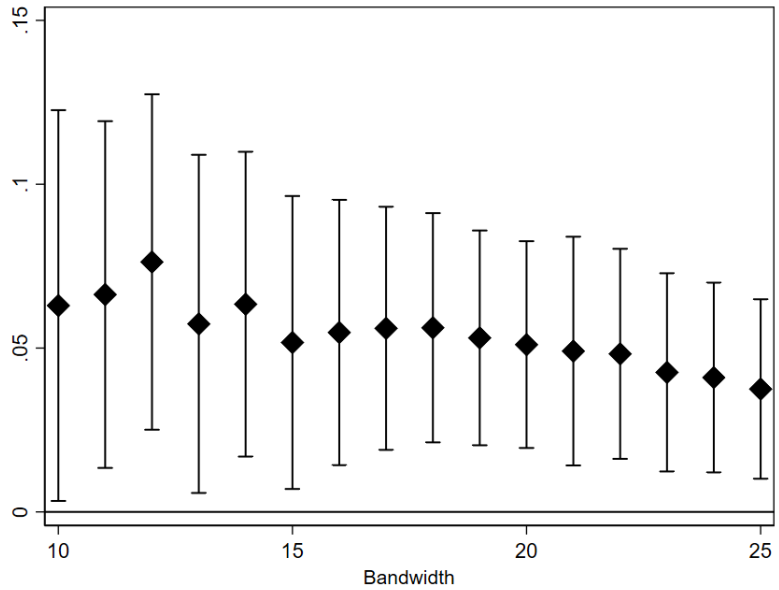
Note: The figure displays each outcome by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. The horizontal axis 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 a boost in income in the following year . Each panel reflects a different parental outcome observed 3-4 years after the child's birth. See Table 1 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.

Figure A14: Student Outcome Index RD Estimates by Donut Size (North Carolina)



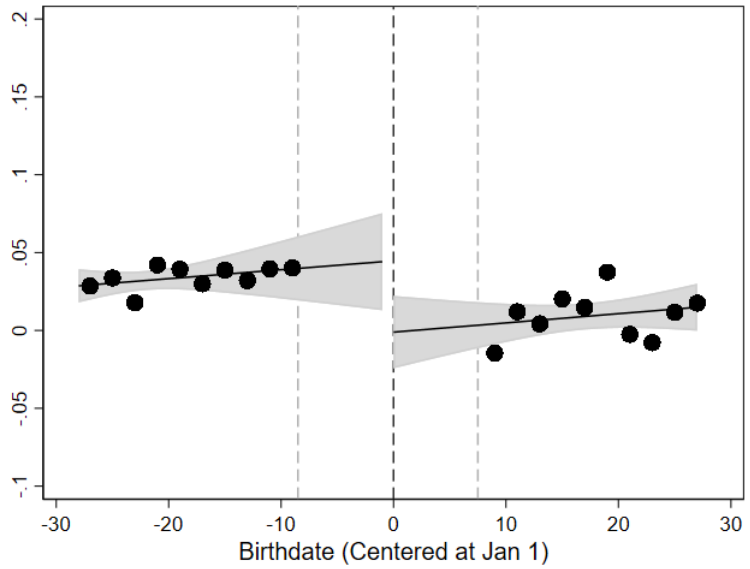
Note: The figure displays an estimate of the January 1 discontinuity in the student outcome index for various donut sizes. The sample consists of FRL-eligible students born within 28 days of January 1 in years 1993 to 1998 who entered a North Carolina public school by grade 5. See the text for additional details on variable construction and sample restrictions..

Figure A15: Student Outcome Index RD Estimates by Bandwidth (North Carolina)



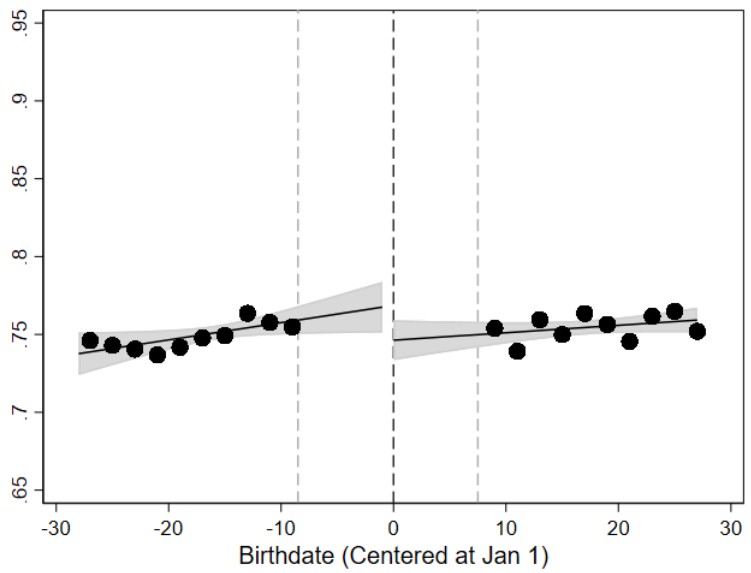
Note: The figure displays an estimate of β_1 (the January 1 RD estimate) of earnings at ages 23-25 for various bandwidth sizes on either side of the 8-day donut. The sample consists of FRL-eligible students born around January 1 in years 1993 to 1998 who entered a North Carolina public school by grade 5. See the text for additional details on variable construction and sample restrictions.

Figure A16: Effect of Cash Transfer Eligibility on Student Test Score Index (North Carolina)



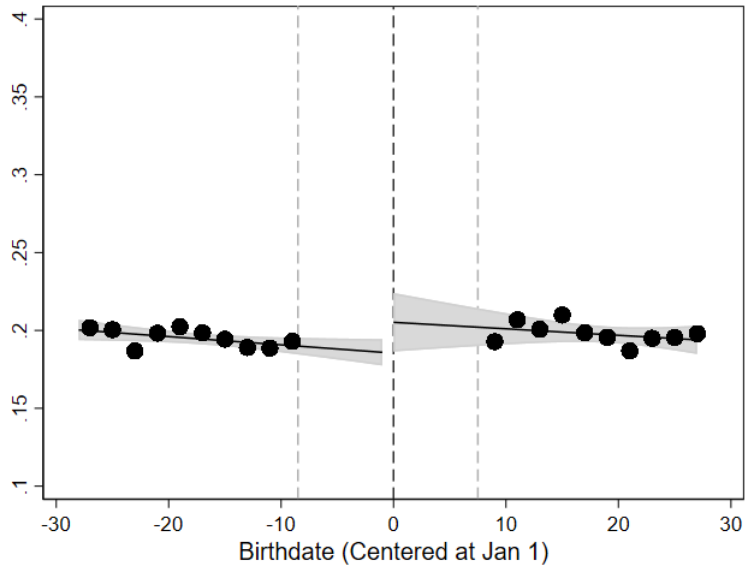
Note: The figure displays the mean of student test score index residuals (after accounting for recentered birth year fixed effects) by 2-day birthdate bin for FRL-eligible students born within 28 days of January 1 in years 1993 to 1998 who entered a North Carolina public school by grade 5. Test score index constructed as the mean of normalized (mean zero, standard deviation one) math and verbal test scores in grades 3 through 8. The horizonal 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 a boost in income in the following year. The shaded area shows the 95% confidence interval.

Figure A17: Effect of Cash Transfer Eligibility on HS Graduation (North Carolina)



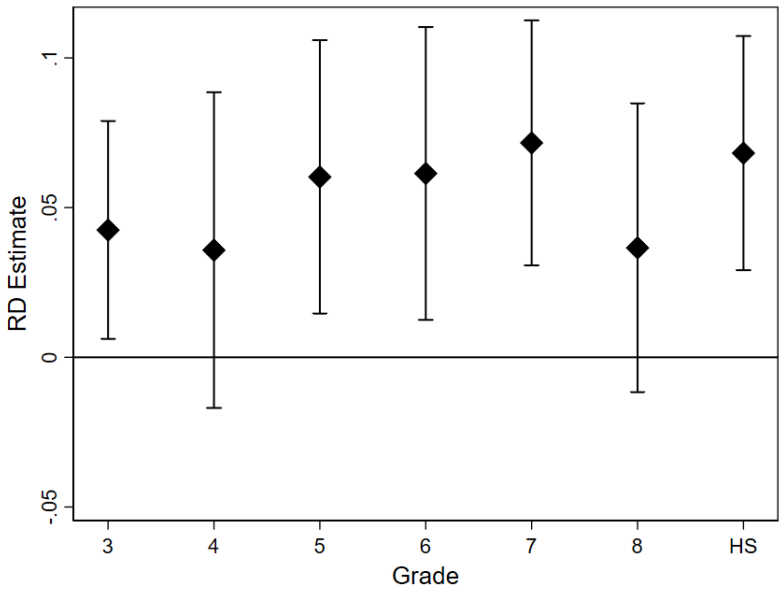
Note: The figure displays the mean of HS graduation rate residuals (after accounting for re-centered birth year fixed effects) by 2-day birthdate bin for FRL-eligible students born within 28 days of January 1 in years 1993 to 1998 who entered a North Carolina public school by grade 5. The horizonal axis 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 a boost in income in the following year. The shaded area shows the 95% confidence interval.

Figure A18: Effect of Cash Transfer Eligibility on Ever Being Suspended (North Carolina)



Note: The figure displays the mean of HS graduation rate residuals (after accounting for re-centered birth year fixed effects) by 2-day birthdate bin for FRL-eligible students born within 28 days of January 1 in years 1993 to 1998 who entered a North Carolina public school by grade 5. The horizonal 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 a boost in income in the following year The shaded area shows the 95% confidence interval.

Figure A19: Effect of Cash Transfer Eligibility on Student Test Score Index Across Grades (North Carolina)



Note: The figure displays the regression discontinuity estimates across grades. The sample contains FRL-eligible students born within 28 days of January 1 in years 1993 to 1998 who entered a North Carolina public school by grade 5. Test score constructed as the mean of normalized (mean zero, standard deviation one) math and verbal test scores.. The vertical bars illustrate the 95% confidence intervals.

Table A1: Balance on County of Birth Characteristics (from 1980 Census)

	Population (1)	Median Family Income (2)	% Poverty (3)	% Age 5 (4)	% Age 65 (5)	% HS Dropout (6)	% BA+ (7)	% Black (8)	% Hispanic (9)	LFP (10)
Born Before Jan 1	1.67e+04	20.1200	-0.0005	0.0000	0.0004*	0.0005	-0.0007	-0.0005	-0.0003	-0.0001
	(2.30e+04)	(40.2500)	(0.0006)	(0.0001)	(0.0002)	(0.0009)	(0.0006)	(0.0013)	(0.0014)	(0.0004)
Obs	*599,000*	*599,000*	*599,000*	*599,000*	*599,000*	*599,000*	*599,000*	*599,000*	*599,000*	*599,000*
Mean	*1.19e+06*	*19670.00*	*0.13*	*0.09*	*0.14*	*0.33*	*0.17*	*0.13*	*0.09*	*0.62*

Note: Each cell shows the β_1 coefficient (RD) estimate from a separate regression where the row denotes the 1980 county characteristic (based on county of birth) serving as the dependent variable. The sample is restricted to individuals born within 28 days of January 1 in years 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 8 day donut of the January 1 cutoff. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A2: Comparison of Average Family Characteristics in December versus January

	Married (1)	Teen Mom (2)	White (3)
Buckles and Hungerman (2013)			
December (vs. January)	0.0056***	-0.0011***	0.0021***
	(0.0001)	(0.0000)	(0.0001)
Obs	*52,041,052*	*52,041,052*	*52,041,052*
Mean	*0.69*	*0.13*	*0.79*
Full Sample			
December (vs. January)	0.0056***	-0.0018***	-0.0007
	(0.0005)	(0.0004)	(0.0006)
Obs	*19,310,000*	*16,880,000*	*15,900,000*
Mean	*0.44*	*0.12*	*0.63*
First-Born EITC Eligible Sample			
December (vs. January)	0.0007	0.0000	-0.0004
	(0.0004)	(0.0011)	(0.0011)
Obs	*5,767,000*	*4,777,000*	*4,587,000*
Mean	*0.05*	*0.35*	*0.63*

Note: Each cell shows the coefficient for December births (relative to January births). Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A3: Balance on Baseline Student Demographic Characteristics (North Carolina)

	Black	White	Hispanic	LEP	Male
	(1)	(2)	(3)	(4)	(5)
Born Before Jan 1	-0.017	0.022	-0.008	-0.007	-0.011
	(0.016)	(0.017)	(0.008)	(0.008)	(0.011)
Obs	*45,010*	*45,010*	*45,010*	*44,948*	*45,010*
Mean	*0.406*	*0.399*	*0.147*	*0.091*	*0.518*

Note: Each cell shows the β_1 coefficient estimate from a separate regression where the column denotes the demographic characteristic used as the dependent variable. The sample contains FRL-eligible students born within 28 days of January 1 in years 1993 to 1998 who entered a North Carolina public school by grade 5. All regressions exclude observations within an 8 day window of the January 1 cutoff. LEP indicates whether a student has been designated to have limited English proficiency. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A4: Placebo Discontinuities for Earnings

	Age 23-25 (1)	Age 26-28 (2)
Born Before February 1	176.0	-76.5
	(175.8)	(395.5)
Born Before March 1	-237.0**	-237.8
	(112.9)	(339.9)
Born Before April 1	-127.7	-35.6
	(163.2)	(290.8)
Born Before May 1	-64.9	63.6
	(186.4)	(270.1)
Born Before June 1	93.2	-16.6
	(158.7)	(326.4)
Born Before August 1	-49.0	106.2
	(153.5)	(292.7)
Born Before October 1	127.5	-214.0
	(126.3)	(215.5)
Born Before November 1	-82.5	-271.9
	(131.5)	(226.0)

Note: Each cell shows the β_1 coefficient estimate for being born before the 1st of a month from a separate regression where the row title denotes the month pair comparison. The sample is restricted to individuals born within 28 days of each 1st of the month in the given re-centered 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 8 day donut of the 1st cutoff. 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 indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A5: Effect of Cash Transfer Eligibility on Adult Earnings Percentile

	(1)	(2)	(3)
Earnings Percentile (23 to 25)	0.471**	0.433*	0.434*
	(0.224)	(0.226)	(0.223)
Mean	*47.22*	*47.22*	*47.22*
Earnings Percentile (26 to 28)	0.337	0.305	0.337
	(0.214)	(0.215)	(0.213)
Mean	*45.90*	*45.90*	*45.90*
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
Lagged Income Control			X

Note: Each cell shows the β_1 coefficient (RD) estimate from a separate regression where the row denotes the outcome variable. Each column indicates the set of birth years included. The sample is restricted to individuals born within 28 days of January 1 in the given re-centered 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 8 day donut of the January 1 cutoff. Adult earnings percentile is constructed by taking the 3-year average of earnings at the filing unit level (counting non-filing as zero) and then finding the percentile of that average within a child's cohort. 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 indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A6: Effect of Cash Transfer Eligibility on Adult Earnings of Non-First-Born By Cohort

	1981-82	1986-87	1991-92	All
	(1)	(2)	(3)	(4)
Earnings (23 to 25)	187.4	-194.8	-215.7	-110.0
	(507.6)	(664.8)	(449.0)	(251.6)
Mean	*21,520*	*17,860*	*18,590*	*19,170*
Earnings (26 to 28)	-624.3	-4.99	-217.3	-271.1
	(624.6)	(801.7)	(555.2)	(366.9)
Mean	*27,020*	*24,350*	*24,070*	*24,860*
Cash Transfer in Infancy	291.3	219.9	345.7	306.2
Observations	53,000	44,000	116,000	213,000

Note: Each cell shows the β_1 coefficient (RD) estimate from a separate regression where the row denotes the outcome variable. Each column indicates the set of birth years included. The earnings outcome is constructed as the 3-year average of the earnings (including non-filers as zeroes) at the filing unit level. The sample is restricted to non-first-born children born within 28 days of January 1 in the given re-centered 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 8 day donut of the January 1 cutoff. 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 indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A7: Effect of Cash Transfer Eligibility on Marital Status

	All (1)	Female (2)	Male (3)
Married (23 to 25)	0.002	0.004	0.001
	(0.003)	(0.004)	(0.004)
Mean	*0.16*	*0.19*	*0.12*
Married (26 to 28)	0.001	-0.001	0.004
	(0.003)	(0.004)	(0.004)
Mean	*0.24*	*0.27*	*0.20*
Cash Transfer in Infancy	1,295	1,295	1,295
Observations	625,000	312,000	313,000

Note: Each cell shows the β_1 coefficient (RD) estimate from a separate regression where the row denotes the outcome variable. Each column indicates the subsample by sex. The sample is restricted to first-born children born within 28 days of January 1 in the years 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 8 day donut of the January 1 cutoff. 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 indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A8: Effect of Cash Transfer Eligibility on Adult Earnings by Sex and Cohort

	1981-82		1986-87		1991-92		All	
	Female	Male	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Earnings (23 to 25)	-259.3	555.2	-17.79	296.4	519.0	782.2**	110.6	559.6***
	(321.8)	(386.3)	(334.5)	(375.8)	(331.7)	(345.5)	(165.4)	(209.7)
Mean	*23,150*	*19,930*	*20,140*	*17,690*	*20,730*	*18,970*	*21,280*	*18,830*
Earnings (26 to 28)	-148.4	521.4	545.0	499.5	105.8	1208.0**	168.3	781.9***
	(454.3)	(561.5)	(408.1)	(568.8)	(449.9)	(481.2)	(210.9)	(293.7)
Mean	*29,830*	*25,560*	*28,800*	*25,440*	*28,320*	*25,340*	*28,940*	*25,440*
Observations	94,000	89,500	100,000	101,000	117,000	122,000	312,000	313,000

Note: Each cell shows the β_1 coefficient (RD) estimate from a separate regression where the row denotes the outcome variable. Each column indicates the set of birth years included and the gender of the individual. The sample is restricted to individuals meeting the given subsample criteria who were born within 28 days of January 1 in years 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 8 day donut of the January 1 cutoff. 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 indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A9: Effect of Cash Transfer Eligibility on Parent Outcomes

	Control Mean (1)	RD Estimate (2)
Cumulative Earnings	625,600	11550**
		(4903)
Mean Earnings	58,250	903.7**
		(458.7)
Mean Poverty	0.21	-0.001
		(0.002)
Always in Poverty	0.04	0.001
		(0.002)
Always Filer	0.46	0.007
		(0.003)
Ever Married	0.71	-0.002
		(0.005)
Ever Single	0.84	-0.005*
		(0.003)

Note: Each cell in column (1) contains control means. Each cell in column (2) shows the β_1 coefficient (RD) estimate from a separate regression where the row denotes the outcome variable. The sample is restricted to individuals meeting the given subsample criteria who were born within 28 days of January 1 in years 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 8 day donut of the January 1 cutoff. 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 indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A10: Effect of Cash Transfer Eligibility on Adult Earnings (North Carolina only)

	(1)	(2)	(3)
Earnings (23 to 25)	1611**	1289*	1375*
	(797.9)	(731.3)	(730.3)
Mean	*18,370*	*18,370*	*18,370*
Earnings (26 to 28)	1997*	1447	1524
	(1177.0)	(1087.0)	(1064.0)
Mean	*24,140*	*24,140*	*24,140*
Observations	18,500	18,500	18,500
Recentered Birth Year Fixed Effects	X	X	X
Demographic Controls		X	X
Lagged Income Control			X

Note: Each cell shows the β_1 coefficient (RD) estimate from a separate regression where the row denotes the outcome variable. The earnings outcome is constructed as the 3-year average of the earnings (including non-filers as zeroes) at the filing unit level. The sample is restricted to individuals born in North Carolina within 28 days of January 1 in years 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 8 day donut of the January 1 cutoff. 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 indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A11: Effect of Cash Transfer Eligibility on Student Outcome Index: Robustness to Inclusion of Various Fixed Effects

	(1)	(2)	(3)	(4)	(5)
Born Before Jan 1	0.051***	0.043**	0.039**	0.040**	0.039**
	(0.016)	(0.019)	(0.019)	(0.019)	(0.019)
Obs	*44,992*	*39,322*	*39,319*	*39,804*	*39,757*
Mean	*-0.059*	*-0.052*	*-0.052*	*-0.051*	*-0.051*
Cash Transfer in Infancy	1,595	1,595	1,595	1,595	1,595
Recentered Birth Year FEs	X	X	X	X	X
Day-of-Week FEs	X	X	X	X	X
District FEs		X			
District X Recentered Birth Year FEs			X		
School FEs				X	
School X Recentered Birth Year FEs					X

Note: Each cell shows the β_1 coefficient estimate from a separate regression where the the column denotes the inclusion of different sets of fixed effect controls: school district, school, district by recentered birth year, and school by recentered birth year. All regressions exclude observations within an 8 day window of the January 1 cutoff. The sample consists of FRL-eligible students born within 28 days of January 1 in years 1993 to 1998 who entered a North Carolina public school by grade 5. See the text for additional details on variable construction and sample restrictions.Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A12: Effect of Cash Transfer Eligibility on Student Outcome Index: Robustness to Alternative Index Construction

	(1)	(2)	(3)
Primary Index	0.051***	0.051***	0.047***
	(0.016)	(0.016)	(0.016)
Obs	*44,992*	*44,992*	*44,992*
Mean	*-0.059*	*-0.059*	*-0.059*
Only observed components are included	0.046***	0.046**	0.041**
	(0.018)	(0.018)	(0.017)
Obs	*44,992*	*44,992*	*44,992*
Mean	*-0.063*	*-0.063*	*-0.063*
Students with any missing components dropped	0.051**	0.050**	0.049**
	(0.020)	(0.020)	(0.019)
Obs	*29,191*	*29,191*	*29,191*
Mean	*0.009*	*0.009*	*0.009*
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

Note: Each cell shows the β_1 coefficient estimate from a separate regression where the row denotes different index constructions and the column denotes the inclusion of various controls. All regressions exclude observations within an 8 day window of the January 1 cutoff. The sample consists of FRL-eligible students born within 28 days of January 1 in years 1993 to 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 eligible for FRL based on their reported 1040 AGI at the relevant ages. See the text for additional details on variable construction and sample restrictions. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A13: Heterogeneity of Effect of Cash Transfer Eligibility on Student Outcome Index by Student Demographics

	Black (1)	White (2)	Hispanic (3)	LEP (4)	Male (5)	Female (6)
Born Before Jan 1	0.011	0.098***	0.014	-0.028	0.049**	0.051**
	(0.026)	(0.025)	(0.039)	(0.047)	(0.024)	(0.023)
Obs	*18,348*	*17,811*	*6,607*	*4,002*	*23,302*	*21,690*
Mean	*-0.163*	*0.045*	*-0.093*	*-0.253*	*-0.130*	*0.017*
Cash Transfer in Infancy	1,657	1,595	1,554	NA	1,595	1,595

Note: Each cell shows the β_1 coefficient estimate from a separate regression where the column denotes the subsample. All regressions exclude observations within an 8 day window of the January 1 cutoff (i.e., Donut Size = 8). The sample consists of FRL-eligible students born within 28 days of January 1 in years 1993 to 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 eligible for FRL based on their reported 1040 AGI at the relevant ages. See the text for additional details on variable construction and sample restrictions. LEP indicates whether a student has been designated to have limited English proficiency. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A14: Effect of Cash Transfer Eligibility on Adolescent Outcomes

	(1)	(2)	(3)
HS Algebra Test Score	0.036*	0.035*	0.032
	(0.021)	(0.021)	(0.020)
Obs	*32,527*	*32,527*	*32,526*
Mean	*0.064*	*0.064*	*0.064*
HS English Test Score	0.073***	0.072***	0.064**
	(0.026)	(0.026)	(0.026)
Obs	*34,842*	*34,842*	*34,842*
Mean	*-0.142*	*-0.142*	*-0.142*
Graduate HS	0.022**	0.022**	0.023**
	(0.011)	(0.011)	(0.011)
Obs	*36,519*	*36,519*	*36,517*
Mean	*0.748*	*0.748*	*0.748*
Ever Suspended in HS	-0.006	-0.006	-0.006
	(0.006)	(0.006)	(0.006)
Obs	*36,991*	*36,991*	*36,989*
Mean	*0.050*	*0.050*	*0.050*
Cutoff Year Fixed Effects	X	X	X
Day-of-Week Fixed Effects		X	X
Demographic Controls			X

Note: Each cell shows the β_1 coefficient estimate from a separate regression. All regressions exclude observations within an 8 day window of the January 1 cutoff (i.e., Donut Size = 8). The sample consists of FRL-eligible students born within 28 days of January 1 in years 1993 to 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 eligible for FRL based on their reported 1040 AGI at the relevant ages. See the text for additional details on variable construction and sample restrictions. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Appendix B: AGI Prediction and Simulated Cash Transfer

As discussed in the text, we use predicted Adjusted Gross Income (AGI) in multiple places in our analyses (e.g., to restrict our sample to EITC-eligible individuals, to simulate the magnitude of the cash transfer in infancy, to conduct balance checks, etc.). To fit our prediction model, we use data from the late 90s and early 2000s when we have a more complete panel of tax data. Specifically, we use data from 1994-1995, and 1998-2005. We impose the same sample restrictions (i.e., parents with a child born around the January 1 cutoff) as we do for earlier birth cohorts. We then regress AGI at time t (AGI_t) on indicators for parent (maximum) age interacted with flexible set of AGI bins at time $t-k$ (AGI_{t-k}), where $k \in \{1, 2\}$.[76] More specifically, the procedure was as follows:

1. Adjust observed AGI for inflation (to $2015) and count missing as zero AGI
2. Create AGI bins:
   - $1000 increments from $0-$40,000
   - $5,000 increments from $40,000-$80,000
   - $10,000 from $80,000-$150,000
   - Above $150,000
3. For each $k \in \{1, 2\}$, regress current year AGI (AGI_t) on parent (maximum) age indicators interacted with lagged (AGI_{t-k}) bins (using tax data from 1994-1995, 1998-2005)
4. Use the coefficients fit in the prior step to predict unobserved AGI 1 and 2 years after an observed year
   - Use the 1979 tax data to predict AGI for 1980 and 1981 tax years (corresponding to 1981 and 1982 recentered birth years)

[76] We also estimated this relationship for $k \in \{3, 4, 5\}$, but the predictions were significantly worse over these longer time spans (e.g. the rate of actual EITC income-eligible families predicted to be non-eligible increases by more than 23% from $k \in \{1, 2\}$ to $k \in \{3, 4, 5\}$ in our sample of first-born children).

- Use the 1984 tax data to predict AGI for 1985 and 1986 tax years (corresponding to 1986 and 1987 recentered birth years)
- Use the 1989 tax data to predict AGI for 1990 and 1991 tax years (corresponding to 1991 and 1992 recentered birth years)

Appendix Figure B1 illustrates the structure of the available tax data and the corresponding set of births that form our analytical sample. We use the predicted AGI directly to test for balance and to restrict our sample based on (predicted) EITC eligibility for a family under the assumption that the just-born child counted for tax purposes (regardless of whether the child was born before January 1). Because this sample restriction is based on having a predicted AGI below a particular level in each year, changes to the prediction approach only end up shifting relatively small groups of individuals into or out of the analytical sample. As a result, changes to the prediction method have minimal influence on the estimated effect of eligibility for child-related tax benefits. As discussed in the text, we also use the predicted AGI to obtain an understanding of the magnitude of the benefits associated with having a child born before January 1. We use the AGI prediction, combined with information on marital status and number of dependents, to recover the taxes owed and credits due to each family when claiming 1 child compared with no children using NBER's TAXSIM program.[77] We calculate this difference for the family of every child in our sample. The estimated cash transfers in infancy reported in the text do not allow for forecast error in the predicting AGI that determines child tax benefits (i.e., everyone with the same lagged AGI and maximum parent age is assigned the same predicted AGI and child tax benefits). Due to the non-linearity of the relationship between child tax benefits and AGI, this may result in a biased prediction

[77] We use a similar strategy to estimate the implied discontinuity in child-related benefits during infancy within our sample of North Carolina students, using the tax data to select families with similar-aged children and incomes in North Carolina before tracking them back to their pre-birth information to estimate the implied effect on resources during infancy. First, we capture the set of children meeting the following conditions: (1) the child is born in NC in recentered birth years within the same sample period where we observe the universe of tax filings (i.e., 1994, 1995, and 1998), and (2) the child is ever eligible for FRL in grade 3-8 (based on their age and the income information on parent's tax filings). Next, we link these children back to their parent's tax filing prior to their birth year or the prior year (for those born in years where we observe the universe of tax filings). Finally, we estimate the implied discontinuity in child-related tax benefits during infancy using the average of the simulated benefit difference at the January 1 cutoff for those born before the cutoff.

of the size of the cash transfer within groups (and overall). We have re-estimated the size of the cash transfer allowing for forecast error and generally find similar estimates of the magnitudes of the cash transfers within groups. To allow for forecast error, we simply collect the residuals from the prediction regressions we fit using the more complete panel of tax data we have in later years. We then randomly draw (with replacement) a residual within each cell defined by the interaction of AGI bin and parent maximal age and assign it to an observation in the associated cell in our analytic sample. For each observation we then add this residual (i.e., forecast error) to the predicted AGI level to obtain a predicted AGI with forecast error. Finally, we run these predicted AGIs (with forecast error) through the NBER's TAXSIM program. This produces estimates of the size of the cash transfer, under the assumption that forecast errors are relatively similar across years. Under these assumptions, Appendix Table B1 illustrates that the mean cash transfers are somewhat smaller than the estimates produced without forecast error.

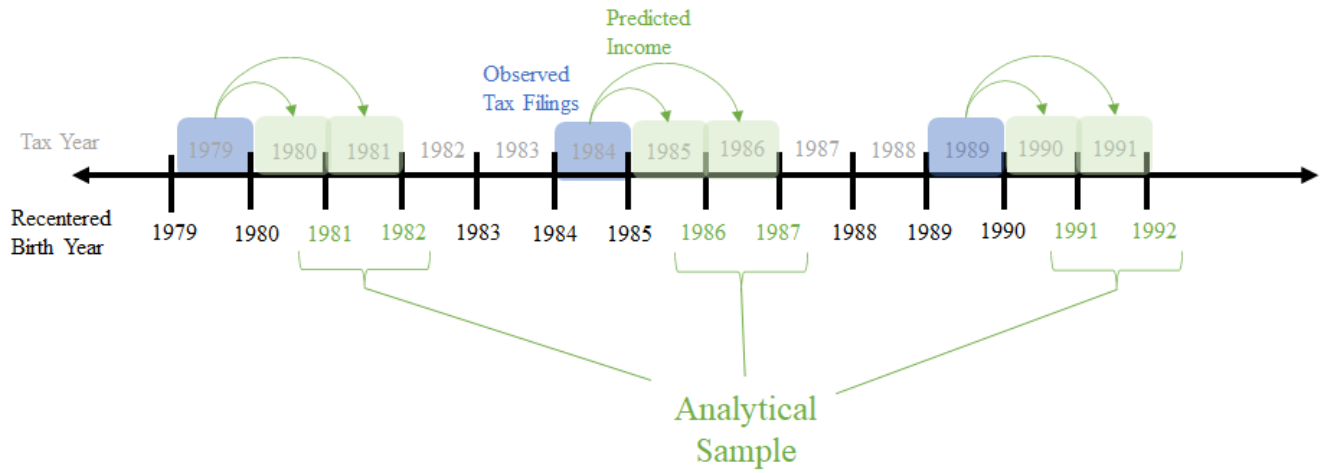
As we discuss in the text, while this measure provides a reasonable indication of the size of the average cash transfer in infancy, significant uncertainty remains. First, there is uncertainty induced by our predicted income measure. Because our predictions are based off of tax filing information from the mid-1990s to early-2000s (when the universe of 1040s is observed in consecutive years), our predictions are likely to bias our estimate of the size of the average increase in resources if the relationship between lagged income and current income changes over time. This may be more problematic in the earlier years of our sample (1981-82 and 1986-87 recentered birth years) due to the labor market fluctuations during this period.[78] Second, we are using AGI and predicted AGI to infer eligibility for the EITC. It is possible that earned income and AGI differ for some individuals (e.g. due to capital gains), which would likely result in an overestimate of the associated credit amount. Third, there is uncertainty induced by our classification of a dependent as a first child. Because the 1040 did not require a child's SSN until 1994, we use these later 1040s to link a child with their parents. While we

[78]Specifically, the labor market weakened significantly between 1979 (the base year used for prediction) and the 1980 and 1981 tax years, and strengthened significantly between 1984 (the base year used for prediction) and the 1985 and 1986 tax years.

use the prior 1040 information on number of dependent children claimed (in the 1979, 1984, and 1989 tax years) to improve our classification, there likely remains some misclassification. The primary concern is that an older sibling may not be claimed as a dependent in 1994, which may result in us incorrectly identifying a child as a first child. Because a non-first-born child generates significantly smaller tax benefits during our sample period, any improper classification of them as a first child would result in an upward bias of the implied increase in resources during infancy in our main sample of children that we classify as first-born and EITC income-eligible.[79] While we think this misclassification is modest, it is likely to be more problematic during the earlier years of our sample (1981-82 and 1986-87 recentered birth years) given the implied age of an older sibling by 1994. Finally, but perhaps most significantly, there is uncertainty generated by incomplete filing and take up of the EITC. Our estimated increase in resources during infancy assumes that eligible individuals are filing taxes and claiming the EITC, but take-up has never been 100%. Some estimates suggest that EITC take-up was roughly 70% in the mid-1980s and rose to 81-86% by 1990 (Scholz 1990; Scholz 1994). This suggests an upward bias in our estimates of the implied increase in resources during infancy that is, again, likely worse for the earlier years of our sample.

[79] In our samples, we estimate the tax benefit as $306 for non-first born children vs. $1,291 for first-born children.

Figure B1: Data Structure and Analytical Sample



Note: The diagram illustrates the structure of the available tax data and the corresponding set of births that form the analytical sample.

Table B1: Alternative Estimates of Cash Transfer Means

	Mean Cash Transfer (1)
All Cohorts	785.2
1981-1982	644.4
1986-1987	504.4
1991-1992	1130.0

Note: Each cell contains the mean estimated cash transfer within the group indicated by the row. See Appendix B for details of construction. The sample is restricted to individuals meeting the given subsample criteria who were born within 28 days of January 1 in years 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 8 day donut of the January 1 cutoff. 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 indicated by: * ($p<0.10$), **($p<0.05$), ***($p<0.01$).