The Effect of Income During Infancy: Evidence from a Discontinuity in Tax Benefits

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 evidence of the long-run effect of income provided during the first year of life. 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 amounts of income. Using the universe of administrative tax data in selected years, we show that an additional $1,000 of income during the first year of life increases young adult earnings by at least 1-2%, with larger effects for males. These effects show up earlier in terms of improved math and reading test scores and a higher likelihood of high school graduation. Estimates of parental behavior in the years after birth suggest an important role for liquidity during the year after birth.

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1 Introduction

One in five children in the United States grows up in poverty. These children are less likely to obtain benchmarks of lifetime economic or social success. 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. Recent evidence suggests that the period of early childhood may be particularly important in determining these socio-economic divides (Duncan et al. 2010). Indeed, correlational evidence suggests that once family income in early childhood is controlled for, the intergenerational relationship in income disappears. However, the inherent difficulties in separating the effects of family income 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. In this paper, we explore whether additional income during the first year of life generates improvements in child outcomes for those born into poverty.

Evidence from the mid-20th century suggests large long-term effects of in-kind transfers to poor families during early childhood (Hoynes et al. 2016; Barr and Smith (forthcoming); Anders et al. (forthcoming); Heckman et al. 2010; Olds et al. 1998). In particular, Hoynes et al. (2016) indicate that availability of the Food Stamp program in early childhood (which arguably amounted to a resource transfer for many families) led to improvements in health and well-being in adulthood. Aizer et al. (2016) similarly indicate positive effects of resources provided through the Mother's Pension program. It isn't clear to what extent these results generalize to pure increases in income in recent cohorts. 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). In contrast, a recent study using variation in wealth generated by lotteries in Sweden suggests little effect of resources on a host of adult and child outcomes (Cesarini et al. 2016). This study suggests a more modest role for family resources, at least in affluent countries with extensive social safety nets, and is more consistent with earlier adoption studies that suggest a weak relationship between family income and child outcomes (Sacerdote 2007).

We bring new evidence to this question by focusing on the effect of income provided during very early childhood. First, we provide the only causal estimates of which we are aware of the long-term effects of income during the first year of life.[1] Second, we provide rigorous causal estimates of the long-term effects of resources provided during early childhood for relatively recent cohorts; using variation in income affecting individuals born in the 1980s and 1990s, we estimate effects across an array of educational, behavioral, and labor market outcomes. Third, we identify the effects of a pure income transfer (separate from changes in the incentive to work) generated by the eligibility rules for child-related tax benefits received by tens of millions of households each year.[2] Because we are using tax-based variation and the tax data, we are able to infer with a relatively high degree of confidence the differences in income experienced. Finally, our use of multiple panel data sources, including the universe of tax data for selected years, allows for a deeper investigation of how additional resources 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 amounts of income in the following year. During our primary sample period in the 1980s and early 1990s, low-

[1] This is distinct from changes that affect children via more permanent changes in resources that begin at similar ages but persist, such as those occurring via adoption (Sacerdote 2007).

[2] Eligibility for the additional income is as good as randomly assigned at the cutoff, so there is no differential incentive to work in the year prior to birth to maximize the benefits received.

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).[3] Thus families with children born before January 1 experience a significant increase in income during the first year of their child's life. Evidence 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. To further circumvent concerns related to the manipulation of birth timing, we focus on a donut specification, excluding 8 days around the January 1 cutoff.

Using the universe of administrative tax data in selected years, we zero in 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. Consistent with this discontinuity reflecting the causal effect of family income, we find that samples of children whose families faced larger increases in tax benefits at the birthdate cutoff exhibit larger improvements in adult earnings at the cutoff.

Overall, we find that an increase in family income of $1,000 in infancy results in children earning 1-2% more during their twenties. Given that our estimates of the increase in income in infancy are over estimates, we view this as a conservative lower bound. Translated to the maximum EITC and child tax credit available today for a first child (between $5,000 and $6,000), these effects are equivalent to an additional year or more of schooling or increasing the value-added of a child's teacher by one standard deviation for five to eight years (Heckman et al. 2006; Chetty et al. 2014). The estimated effects on child earnings percentile ranks imply a rank-rank relationship of family income *in infancy* to earnings in young adulthood of roughly 0.12, approximately one third of the intergenerational correlation in income (Chetty

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

et al. 2014). This is striking given the transitory nature of the policy shock versus the observed persistence of family income.

The major assumption underlying the RD design is that treatment assignment is "as good as random" at the threshold for treatment. 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). We find no evidence of manipulation of birth timing outside of an 8 day donut around the birthdate eligibility threshold. After removing this donut, there is no remaining bunching of the distribution of births and the baseline covariates (including parents' lagged earnings and potential tax benefit increase) are balanced on either side of the threshold. 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 family demographic and lagged income controls.

The size of the discontinuity in outcomes appears to track the size of the income increase across time and subgroups, with the largest effects for the cohorts for which the income increase was the greatest. We observe larger effects for subgroups with larger expected increases in income (for example, those with a history of filing taxes). We similarly see non-zero effects for children who were eligible for substantial additional income and null effects for those who were not (e.g., non-first-born children when the EITC only increased for first-born children). These patterns of effects across subgroups are consistent with effects generated by a difference in income rather than other differences around the eligibility threshold.

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 income during infancy on adult earnings at the tax unit-level. We therefore place some additional emphasis on estimates for males when discussing our results. Consistent with a stronger signal for males, the effects are substantially larger for males, at least 2.4% per $1,000 of income during infancy. 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 income provided during infancy on adult earnings are consistent with a growing body of work on the long-run effects of the early childhood environment. However, 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 increase in the net present value of lifetime family income is tiny (at most 0.2%), the increase in annual family income in the year following birth is substantial (more than 10% for many recipients and up to 20% for some).[4] The results suggest that these transfers may provide increased liquidity for families during a critical window for both parents and children. 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. The window may be critical from the perspective of the child due to the importance of this period for cognitive, physical, and socio-emotional development. Increased liquidity may provide a cushion for families that allows them to avoid adverse events or lead to more general reductions in stress; the effects of these changes may be magnified for young children.[5] While we cannot observe effects on adverse events or household stress directly, we can estimate effects on parental labor supply 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 results in small and suggestive increases in the likelihood that a child's parents are married and larger and persistent increases in family income. In addition to suggesting an important role for liquidity in the year after birth, these sustained improvements in the early childhood environment may directly contribute to the positive effects of income provided in infancy on children's long-run outcomes.

To better understand how income provided in infancy generates improved adult earnings, we turn to detailed administrative education data from North Carolina to trace the effect

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

[5] 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).

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.[6] We find that the intent to treat 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 of income provided during infancy. 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. Taken together, the observed human capital effects can entirely explain our estimated effects on earnings.

Average math and reading test scores increase by 0.03 standard deviations per $1,000. These effects represent roughly 4 percent of the overall gap between those eligible for free lunch and those who are not. Put another way, the estimated effects of $1,000 of income in infancy on tests taken between ages 8 and 14 are of a similar magnitude as the *contemporaneous* effects of $1,000 of income (coupled with changes to parental work incentives) on tests taken at the same ages (Dahl and Lochner 2017). This is true even though the contemporaneous increase is a permanent increase to annual income, implying a much larger effect on permanent income and further suggesting the importance of increased liquidity during the critical period of early childhood. There are larger effects (1.4 percentage points per $1,000) on the likelihood of suspension. The academic and behavioral effects result in a sizable effect on high school graduation of 1.4 percentage points per $1,000. This represents 9 percent of the gap between those eligible for free and reduced price lunch and those who are not, suggesting an incredibly important role for an average increase in income of as little as a thousand dollars during the first year of life. Put another way, the results suggest that

[6] 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.

the 2020 maximum EITC and childcare tax credit benefit for a first child could close the gap across these outcomes by a quarter to a half.

We find evidence that is broadly consistent with other recent work suggesting the importance of interventions targeted at early childhood. In particular, our results suggest that resource transfers to families may be especially impactful after the birth of a child. This conclusion may have important implications for recent and widespread discussions regarding the permanent expansion of child tax benefits.

2 The Importance of Resources

The effects of poverty are pernicious and persistent across generations. Children born to parents in the bottom income quintile are twice as likely as children born to middle-income parents to find themselves in the bottom income quintile as adults. Recent estimates of the intergenerational correlation in income are similarly dramatic, between 0.3 and 0.6 (Black and Devereux 2011, Chetty, Hendren, Kline, Saez and Turner 2014b, Mazumder 2005, Solon 1999). The outcome gaps leading to this low rate of social mobility begin early in life (Sawhill 2012), suggesting that early childhood may be particularly important in explaining the link between family income and child outcomes. Indeed, correlational evidence suggests that once family income in early childhood is controlled for, the intergenerational relationship in income disappears (Duncan et al. 2010).

While these persistent effects of childhood inequality are well established, the extent to which the relationship between family income and adult outcomes is causal, a critical question for policymakers, is not well understood. Until recently, studies that linked childhood income to outcomes did little to address the endogeneity of income. Many of these studies simply regressed an outcome variable on a measure of family income during childhood, controlling for other observable characteristics of the family and environment. As Mayer (1997), Brooks-Gunn (1997) and others have pointed out, these studies merely presented correlations between childhood family income and outcomes. To the extent that children in

poorer families had worse unobservable characteristics or environments, it is possible that these conditions drive the income-outcome relationship instead of income itself.

Blau (1999) and Levy and Duncan (1999) improve upon the earlier correlational studies by conditioning on family fixed effects, identifying the impact of family income by leveraging the differences in income levels across siblings. While these studies remove fixed family factors, they use potentially endogenous shocks to income (e.g., family income may fall when a parent stays home to care for a sick child).

Recently, researchers have begun to address these issues. For example, a handful of welfare-to-work experiments demonstrate the positive effect of cash assistance (Gennetian and Miller 2002; Morris and Gennetian 2003; Hill et al. 2001; Clark-Kauffman et al. 2003). While these studies made some progress in addressing the endogeneity concerns of earlier work, they focus on the short-term behavior and schooling outcomes of children and are unable to isolate the effect of income from changes in the incentive to work.

This is also true of studies that leverage variation in the EITC maximum. Previous studies of the EITC predominately leverage changes in the tax schedule to demonstrate positive effects of tax credits during pregnancy on short-term maternal and infant outcomes (Hoynes et al. 2015; Evans and Garthwaite 2014; Strully et al. 2010) as well as positive effects during adolescence on contemporaneous academic outcomes and longer-term adult outcomes (Dahl and Lochner 2012; Bastian and Michelmore 2018). The variation in the EITC schedule resulted in shocks to income generated by both the generosity of the EITC credit and changes in parental working decisions. Dahl and Lochner (2012) and Bastian and Michelmore (2018) use this variation to produce instrumental variable estimates of the effect of family income. Dahl and Lochner find that each $1,000 in additional income results in a 0.03 standard deviation increase in contemporaneous math and reading test scores. Bastian and Michelmore leverage differences in EITC exposure and find large effects of greater exposure during adolescence on the likelihood of completing high school and annual earnings between age 22 and 27. The nature of the variation that they leverage yields

estimates of the effects of changes in the generosity of the income transfer bundled along with changes in parental work incentives. Indeed, Bastion and Michelmore (2018) suggest that changes in work incentives are the primary channel for the effects that they observe.

Other researchers have explored effects on somewhat longer-term outcomes using natural experiments that generate variation in resources (such as oil booms or the distribution of casino profits). These studies suggest mixed, but mostly positive, effects of resources on educational attainment and criminal behavior (Loken 2010; Loken et al. 2012; Akee et al. 2010). While compelling, the estimated effects are generated by income changes in very unique populations; it is unclear the extent to which these effects generalize to other populations. Furthermore, they tend to focus on income received during middle school or later, overlooking the period of early childhood that recent evidence suggests may be particularly important in explaining the link between family income and child outcomes.

In contrast with these positive estimates, a clever study using random assignment of adopted Korean children to American families finds that, among this unique sample, family income is only weakly related to the child's income and education in adulthood (Sacerdote 2007). This is consistent with a recent study using variation in wealth generated by lotteries in Sweden that suggests little effect of wealth on a host of adult and child outcomes (Cesarini et al. 2016). The lack of effects is largely invariant to the size of the award or the baseline level of income of the family. This impressive study suggests a more modest role for family resources, at least in affluent countries with extensive social safety nets. That said, the study does not present results separately for the period of early childhood, which recent evidence suggests may be most critical, particularly for low-income families who lack access to extensive social safety nets.

Perhaps closest in spirit to addressing the question of the long-run impact of family income in early childhood is a set of studies that indicate large effects from the provision of in-kind benefits in early childhood on a host of adult outcomes. These studies suggest important roles for Food Stamps, early childhood education, and home visitation for first-

time mothers and their children (Hoynes et al. 2016; Barr and Smith 2018; Anders et al. (forthcoming); Heckman et al. 2010; Olds et al. 1998). While not explicitly income transfers, these programs all provide additional resources to families, resources that are often targeted at young children. Of course, one limitation of these studies is their reliance on variation and cohorts from over 50 years ago. It is unclear how much these studies inform our understanding of the impacts of current cash resource transfers, such as those provided by the EITC.

2.1 Liquidity and Early Childhood

While some recent evidence suggests that increased income during the period of early childhood can generate outsized gains, it is not immediately clear why. The increases in income considered are often modest relative to the stream of lifetime income, suggesting that these transfers may provide liquidity increases during a critical window. Increased liquidity may provide a cushion for families that allows them to avoid adverse events such as bankruptcy, eviction, or food insecurity. It may also 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.[7]

The importance of liquidity may be magnified by the sensitivity of child development during this very early window. A growing body of evidence demonstrates the potential importance of the early childhood environment.[8] Resources provided during this early window may have a more pronounced effect than those provided later. Indeed, the most closely

[7]Descriptive evidence from the Survey of Household Economics and Decisionmaking is consistent with this conclusion, showing 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 A1b and A1a).

[8]See for example, recent studies on the long-run effects of Food Stamp availability (Hoynes et al. 2016; 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), 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.

related studies, those evaluating the effects of Food Stamp availability during childhood, leverage the relative importance of the early childhood period as part of their identification strategy (Hoynes et al. 2016; Bailey, Hoynes, Rossin-Slater, and Walker 2020). Hoynes et al. (2016) estimate the effects of the percent of time between conception and age five that a child has the Food Stamp program available in their county to identify the effect of the program. The corresponding dynamic estimates for the economic self-sufficiency index support the conclusion that this period of childhood is the most important, with the estimates implying that the period between conception and age 2 to 3 is most critical.[9] This result is consistent with the dynamic event study estimates for the human capital index (but not the economic self-sufficiency index) produced using much larger samples in Bailey et al. (2020). Where it shows up, the larger gradients between conception and age 2 to 3 suggest that the period that we study may be exactly the period of greatest potential for additional resources to improve child outcomes.

Of course, Food Stamps are not a cash transfer; for some families 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). It is not clear to what extent the average effect of 1960s Food Stamp availability during early childhood (and throughout the rest of one's life) generalizes to pure increases in income in more recent cohorts.

Context aside, we often know little about how resources provided during early childhood translate into observed long-run effects. Our approach and data allow for a deeper investigation of how the additional income provided influences the childhood environment, potentially by providing a liquidity injection that provides new families with a financial cushion during a critical window. While we cannot observe effects on adverse events or household stress directly, we can observe effects on family formation and family income that would likely be affected by these types of changes.

[9] Interestingly, while the estimates for the economic self-sufficiency index suggest that this early window is most critical, the estimates for metabolic syndrome suggest a constant dosage effect between conception and 5.

3 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 family income 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.

3.1 Linked Tax Data

We use the administrative tax data housed at the U.S. Census to explore the long-term effects of income 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.[10] 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 income 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 the linked parents backwards to the closest pre-birth year in which we have the universe of 1040 tax information.[11,12] We use this information (including whether or not an individual's

[10] 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.

[11] 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.

[12] We identify a child as first-born if the 1040 in which they are linked to their parent contains no older siblings

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).[13]

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.[14] 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.[15]

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 increased family income in infancy on adult earnings outcomes. Our key earnings outcome measure is 3-year average earnings, including missing earnings (i.e., non-filers) as zeroes.[16] 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

and the closest pre-birth year tax return contains no other children.

[13]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.

[14]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.

[15]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.

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

on individuals and their spouses if present. We place some emphasis on effects at ages 23-25, at which point we have a clearer measure of effects on individuals due to the higher rate of individual filing.[17] We also present effects at ages 26-28. 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 income during infancy. As a result, we place some emphasis on estimated effects for them in our discussion of the results.

3.2 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.[18] We focus our analysis on children eligible for free and reduced price lunch (FRL), a proxy for likely EITC eligibility.[19] After restricting our sample to those FRL students born within 28 days of January 1 (the threshold date in our RD design), but excluding 8 days on either side of January 1, there are 44,992 students in our analytical sample.

We construct our key measures of aptitude using mean normalized math and 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,

[17] The rate of joint filing rises from 18% to 39% between age 23 and 28.

[18] 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).

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

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 income, 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.[20]

We focus our analysis on students eligible for free or reduced price lunch (FRL). We use this as a proxy for likely-EITC eligibility as the income thresholds for the programs are similar.[21] Among those *eligible* for FRL based on family income, the rate of EITC eligibility is roughly 75 percent.

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).

4 Empirical Strategy

To obtain an estimate of the causal effect of additional family income in early childhood, we take advantage of a natural experiment that resulted in the families of otherwise similar

[20] 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 A7 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.

[21] 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).

children receiving substantially different child-related tax benefits in their child's first year of life. The families of children who are born on December 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.[22] This source of variation allows us to examine the effect of a pure income transfer rather than one that is coupled with changes to work incentives, and it directs our focus at changes in family income 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.

4.1 Increases in Income During Infancy

During our sample period, these changes in family income come primarily via the EITC and, to a lesser extent, the dependent exemption.[23] 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.[24] To obtain an understanding

[22]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.

[23]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.

[24]This income tends to arrive in February.

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.[25] 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 increase in income during infancy as we move across dates of birth until we reach the January 1 threshold, when it drops to $0.[26] This creates an in increase in family income 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 income 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 income 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.[27] Second, we are using

[25]We use a similar strategy to estimate the implied discontinuity in income 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 income 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 family income during infancy using the average of the simulated benefit difference at the January 1 cutoff for those born before the cutoff.

[26]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.

[27]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

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 income during infancy in our main sample of children that we classify as first-born and EITC income-eligible. 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 income 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 income 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 family income experienced during infancy. Correspondingly, our estimated effects on outcomes per $1,000 should be viewed as conservative lower bounds. Second, due to greater rates 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

1985 and 1986 tax years.

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 income 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 income 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.

4.2 Main Specification

Our primary empirical model is a regression discontinuity (RD) design that leverages this sudden increase in family income in the first year of life at the January 1st birthdate cutoff (unrelated to family characteristics) to identify the causal effect of early childhood family income 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.[28] This is an intent to treat parameter (ITT). While we produce estimates of the associated "first-stage" increase in income during infancy, there is some uncertainty in these estimates. We revisit the discussion of these

[28] 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.

complications and the associated scaling of our ITT parameter in the results section.

4.3 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 income 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.[29] 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).[30] Nevertheless, we also follow an approach common in the literature and estimate donut hole RDs, dropping the observations around the January 1 threshold (shaded in gray), to address this concern.

Another conventional "test" of the RD identifying assumption 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

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

[30] 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.

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).[31]

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 births in states and years with a school age cutoff date between November and February from our analyses. To further address concerns about other treatments changing discontinuously across the threshold, we take advantage of variation in the size of the income discontinuity across subgroups. First, the generosity of dependent tax benefits and EITC take up rates increased significantly between 1980 and 1990. We compare outcome effect estimates across birth cohorts, with the expectation that the 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 income across the January 1 threshold due to differences in baseline income or eligibility.

5 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 income during the first year of life generates a $319 increase in average annual earnings between age 23 and 25 and a $456 increase between

[31] We conduct similar exercises using the North Carolina data and find no significant differences in race, gender, or LEP status in the donuted sample of FRL students (Appendix Table A1).

ages 26 and 28.[32] These level effects correspond to around a 1.6-1.7% increase in average earnings. The average estimated increase in income 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 of income provided during infancy. As noted previously, given that the estimated increases in actual income 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 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.[33,34]

Appendix Figure A5 shows how the basic regression discontinuity estimate (β_1) varies by donut size. The estimates are similar across donut size. 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.[35] Appendix Figure A7 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 income at the birthdate threshold. The largest effects are for individuals born in the 1991 and 1992 recentered birth years, when the additional income 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

[32] Appendix Table A4 demonstrates no effect of additional income in infancy on the likelihood of being married as an adult, implying that these results are not driven by changes in household formation.

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

[34] 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 A2 and Appendix Figure A9).

[35] 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.

income actually received during infancy, this difference in the size of the income discontinuity across birth cohorts underestimates the true difference. Specifically, increases in estimated EITC take-up over this time period range as high as 72%.[36] 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 income 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 income discontinuity estimates for the differential take up across cohorts tends to bring these scaled estimates closer together.

Appendix Table A3 provides additional evidence that the size of the discontinuity tracks the size of the income increase 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 income for the families of non-first-born children, they are much smaller than among first-born children.[37] For the full set of cohorts, the estimated additional income during infancy is $1,291 for first-born children, but only $306 for non-first-born children.[38] As a result, if our estimates for the first-born children are driven by increases in childhood family income 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.[39]

[36] Scholz (1990) estimates EITC take-up in the mid-1980s of as low as 50% when using the 1984 SIPP along with non-compliance estimates from 1985. Scholz (1994) estimates take-up of 80.5-86.4% in 1990.

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

[38] 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 income 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.

[39] 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

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 A4 illustrates the difference in mean marriage rates by age and gender and demonstrates that there is no effect of additional income in infancy on the likelihood that an individual is married as an adult. In Table 5, we explore how the results vary by child gender. While we cannot rule out substantial effects for females, the effects of income 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 income implies an increase in earnings of 2.3% per $1,000 of income in infancy. Graphical evidence of these effects is provided in Figure 4.[40] 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 income by gender. However, we see no such evidence 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 income boost 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.[41] The point estimates are too imprecise to draw strong conclusions, but the effects appear to be driven by the children

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.

[40] We also demonstrate the robustness of the male estimates to donut size and bandwidth choice in Appendix Figures A6 and A8. Regression discontinuity plots by gender for percentile wages are in Appendix Figure A10.

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

of single parents at baseline. This pattern is again consistent with the larger predicted income increase for single parents.

Appendix Table A5 breaks out the estimates by cohort and gender. The effects are 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 income 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 increase in income was 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.

5.1 Putting the Effect Sizes in Context

Overall, we find that an increase in family income of $1,000 in infancy results in children earning at least 1-2% more during their twenties. Translated to the maximum EITC and child tax credit available today for a first child ($5,000-$6,000), these effects are equivalent to an additional year or more of schooling or increasing the valued-added of a child's teacher by one standard deviation for five to eight years (Heckman et al. 2006; Chetty et al. 2014). The estimated effects on child wage percentile ranks imply a rank-rank relationship of family income *in infancy* to earnings in young adulthood of roughly 0.12, approximately one third of the intergenerational correlation in income (Chetty et al. 2014). Even these estimates are a conservative lower bound for the effect of income in infancy, because they do not account

for incomplete filing or EITC take-up. 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 cohorts born in the 1960s and early 1970s. For example, Hoynes et al. (2016) report sizable impacts of 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 effects on earnings of $1,460 and on family income of 10 percent per year of Food Stamp participation prior to age 5.[42] Dividing by the average annual Food Stamp benefit among recipients implies an effect on earnings of around $330-430 per $1,000. 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 (Bailey, Hoynes, Rossin-Slater, and Walker 2020).[43] Bitler and Figinski (2019) use administrative data from the Social Security Administration to estimate similar long-run effects of 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.

[42] 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.

[43] 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.

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 "resources" provided across a range of ages (or during the 1960s) should be compared to cash provided to a family just after the birth of a first child.

While distinct in the nature of treatment, evaluations of early childhood programs may provide alternative benchmarks. Evaluations of the Perry Preschool program estimate earnings 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 of income 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, but limited information on child ages and program specifics from

this time period make it difficult to scale their estimates into comparable figures. That said, 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.

6 Mechanisms Along the Life Course

While there is compelling evidence that increases in income 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 income 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 income provided at this critical point in allowing families to avoid these negative shocks. We then attempt to trace the effects of this income 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 increased income 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.

6.1 Effects on Family Structure and Parental Earnings Trajectory in Early Childhood

While the maximum increase in single-year income 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. This suggests that the large impact of this additional income 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, 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 additional income 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 one or two years prior to and three to four years after the recentered year of birth. 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 additional family income. Figure 5 plots these estimates. Across outcomes, 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.[44] We also see suggestive increases in the likelihood that a child has married parents, although the point estimates are small. In combination, the estimates suggest that the income boost following the birth of a first child also had positive and enduring effects on the parents themselves. These effects are consistent with the income boost allowing families to better weather negative shocks or other stressors. 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.

To the extent that the families of children born on or after January 1 are still income-eligible for child-related benefits when the child turns 18 (or cease to be eligible to be claimed as a dependent), the results may further speak to the relative importance of early childhood.[45] At least in our context, income provided just after birth appears to be significantly more impactful than that provided at the point of entry into young adulthood. As a result, our conclusions are more consistent with studies that suggest the relative importance of early childhood relative to adolescence in determining adult outcomes.[46]

[44] The standard regression discontinuity plots are presented in Figure A11.

[45] As noted previously, the families of children born on or after January 1 will potentially have an additional year of tax-related benefits on the back end as these children age out of dependency one tax year later.

[46] While much work suggests the importance of early childhood, some more recent work suggests the opposite (Jones, Akee, and Simeonova et al. 2020).

6.2 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. 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 income 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.[47] Figure 6 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 A12) and bandwidths (Appendix Figure A13).[48]

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 income contained in the bottom row of Table 7 of roughly $1,595.[49] This implies effects of 0.03 standard deviations per $1,000 of additional family income. 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 A8 provides the same estimates from column 1 of Table 7 for FRL-eligible students separately by subgroup. Unlike in the tax data, there is little difference in the estimated effect for males and females. This further

[47] Appendix Table A6 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 A7 shows robustness to alternate index constructions.

[48] 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.

[49] As mentioned previously, we use the tax data to estimate the implied discontinuity in income 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 family income during infancy using the average of the simulated benefit difference at the January 1 cutoff for those born before the cutoff.

suggests that the differences observed in the tax data are a result of the greater noise to signal ratio in our measure of female earnings.

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. 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 income by race, particularly on the test score margin (e.g., if these effects are mediated by school quality, which differs by race). We see an opposite pattern of results when examining effects on earnings nationwide, although the confidence intervals are again too wide to draw strong conclusions.

In Table 8, we present estimates separately by adolescent schooling outcome.[50] Additional income 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 1.3 percentage points on having ever been suspended per $1,000 of additional income during the first year of life. Put in terms of today's maximum EITC and child tax credit for a first child, these results suggest that providing this credit just during the first year of life would close the gap between those eligible for FRL and those who are not by a quarter to a half across these measures.

[50] These estimates are presented graphically in Appendix Figure A14, A15, and A16.

6.3 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 of income 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 additional income during infancy on test scores (Appendix Figure A17).

7 Discussion and Conclusion

Recent evidence suggests the importance of early childhood resources in determining lifetime success. We contribute to this growing literature by taking advantage of a discontinuity in income provision generated by the EITC, the largest cash transfer program to poor families in the United States, and other child-related tax benefits. Combining the universe of 1040 tax data with parent-child linkages spanning four decades and detailed education data from North Carolina, we provide the first estimates of the causal effect of family income during the critical window following childbirth.

We find a substantial effect of resources provided during the first year of life. For an additional $1,000 of income in early childhood, earnings at age 23 to 28 are 1-2% higher. We view this as a conservative lower bound, given that our estimates of the increase in income during infancy are overestimates. The effects of income during infancy are larger for males, for whom we have a stronger signal of individual earnings, and 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. The resulting sustained increases in family income also likely contribute to the observed long-term effects on child outcomes.

These results may have important implications for recent and widespread discussion regarding the expansion of child tax benefits. 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 income 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 reductions in the observed gaps in outcomes.

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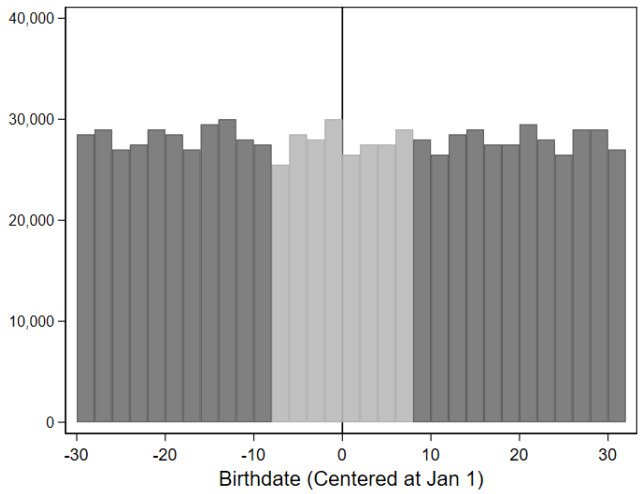
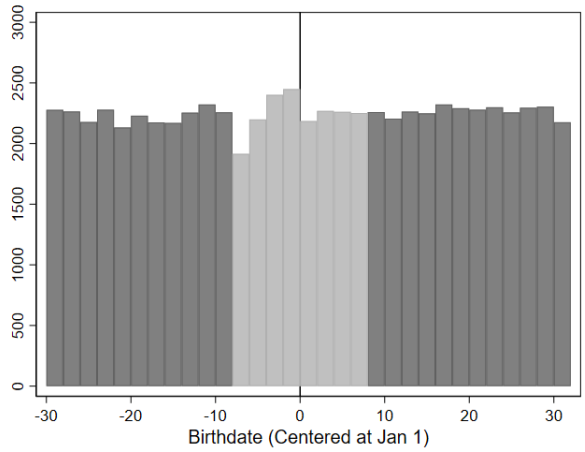
Figure 1: Distribution of Birthdates by Sample



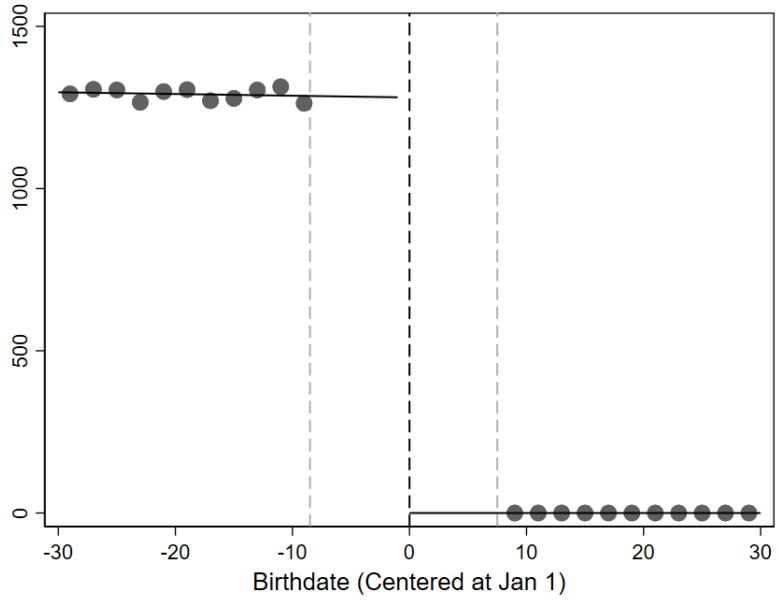
(A) Tax Data (EITC-Eligible Families)



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

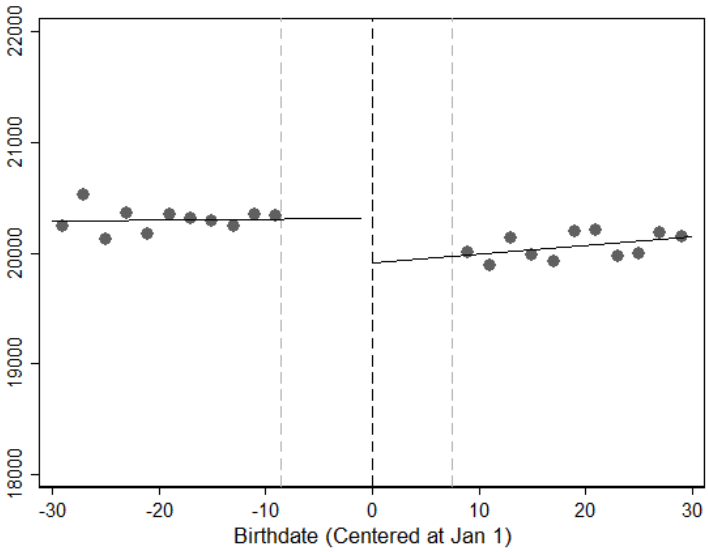
Note: Panel A displays the distribution of birthdates (relative to January 1) for all individuals who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. Panel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

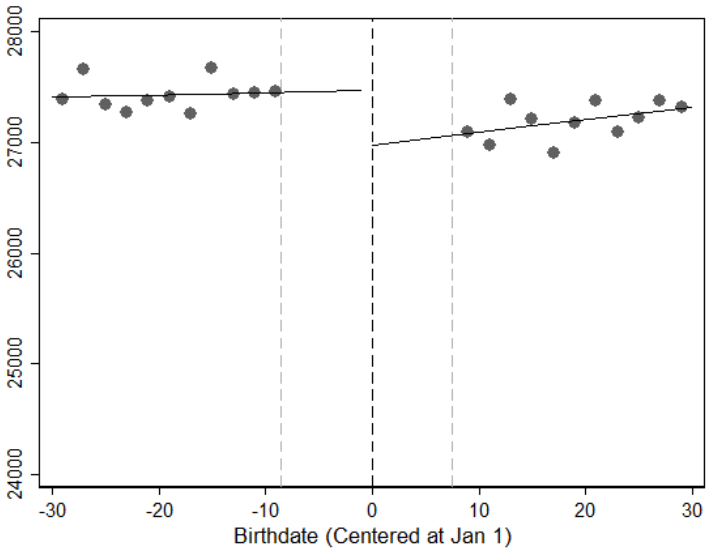


Note: The figure displays mean additional income during infancy by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. Additional income from child-related tax benefits is calculated using information from prior tax filings combined with NBER's TAXSIM program. 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 Eligibility on Adult Earnings



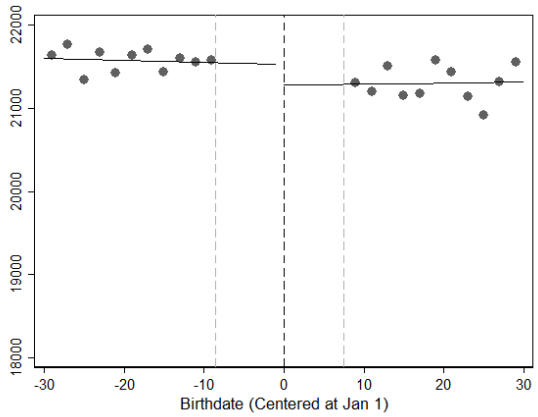
(a) Earnings (23 to 25)



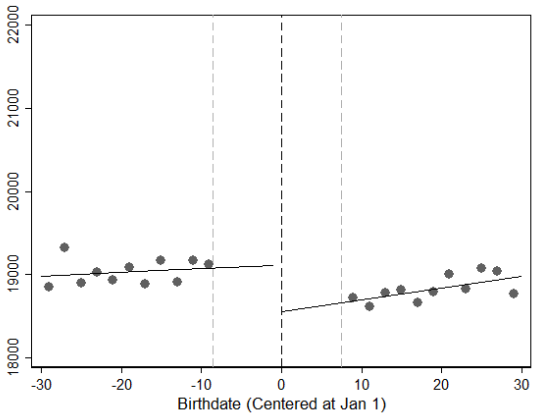
(b) Earnings (26 to 28)

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.

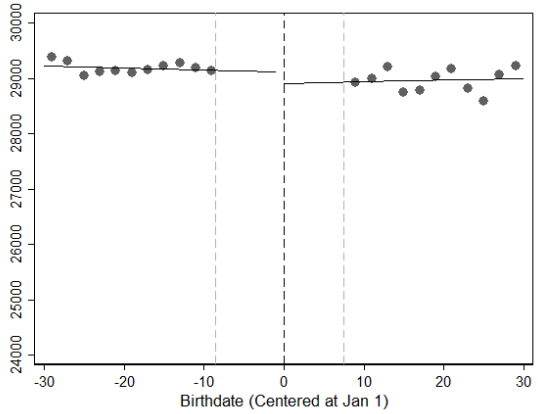
Figure 4: Heterogeneity by Sex in the Effect of Eligibility on Adult Earnings



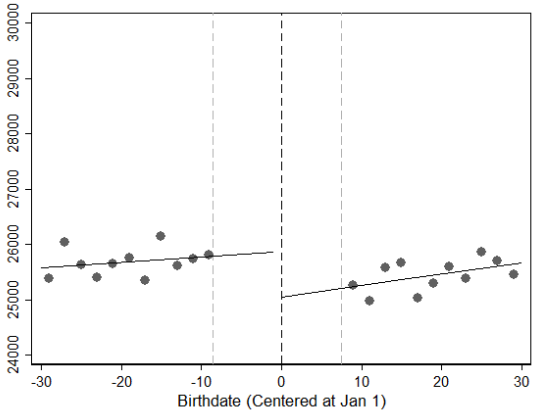
(a) Female Earnings (23 to 25)



(b) Male Earnings (23 to 25)



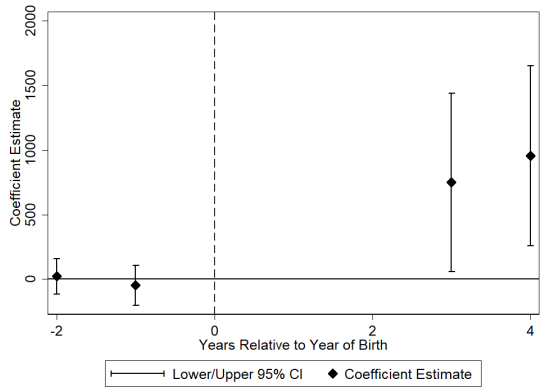
(c) Female Earnings (26 to 28)



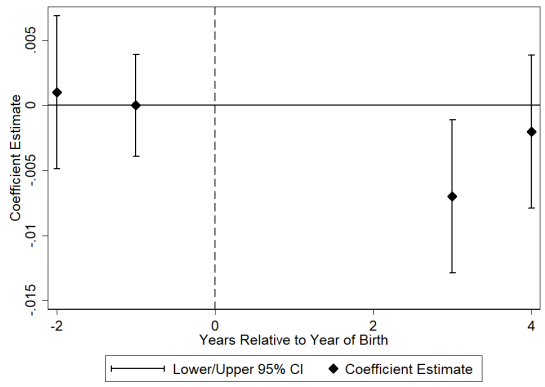
(d) Male Earnings (26 to 28)

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.

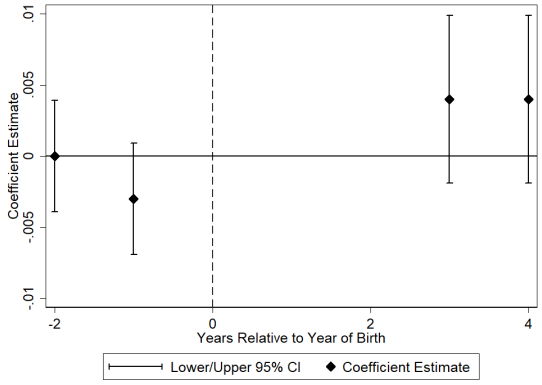
Figure 5: Effects of Eligibility on Early Family Environment Before and After Birth



(a) Parent Earnings



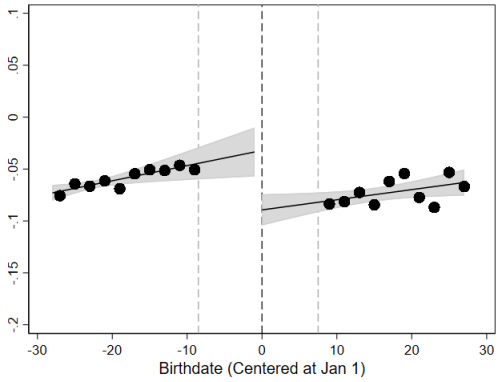
(b) Parent Poverty Status



(c) Parent Marital Status

Note: The figure displays the basic regression discontinuity estimate for parental outcomes 1 and 2 years before the year of the child's birth (i.e., -1 and -2) and 3 and 4 years after the child's birth (i.e, 3 and 4). 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 6: Effect of 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
Additional Income 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. 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 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*
Additional Income 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. 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 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*
Additional Income 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. 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 Eligibility on Adult Earnings

	All (1)	Female Child (2)	Male Child (3)	Single Parent (4)	Married Parent (5)	Filer Parent (6)	Non-filer 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*
Additional Income 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. 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. 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 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*
Additional Income 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. 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 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*
Additional Income 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 increase in income 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 the text for additional details on variable construction and sample restrictions. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

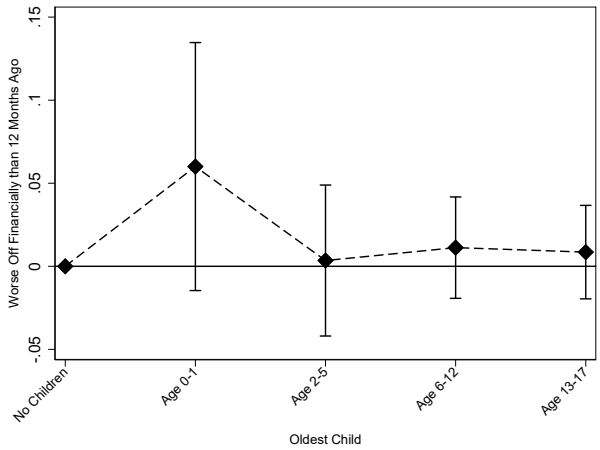
Table 8: Effect of Eligibility on Individual Student Outcomes (North Carolina)

	(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*
Additional Income 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

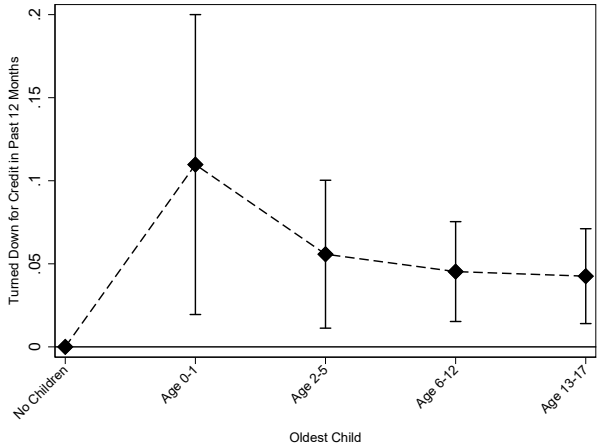
Note: Each cell shows the β_1 coefficient estimate from a separate regression where the row denotes the student outcome and the column denotes the inclusion of different controls. The test score index is constructed as the mean of normalized (mean zero, standard deviation one) math and verbal test scores 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 increase in income 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



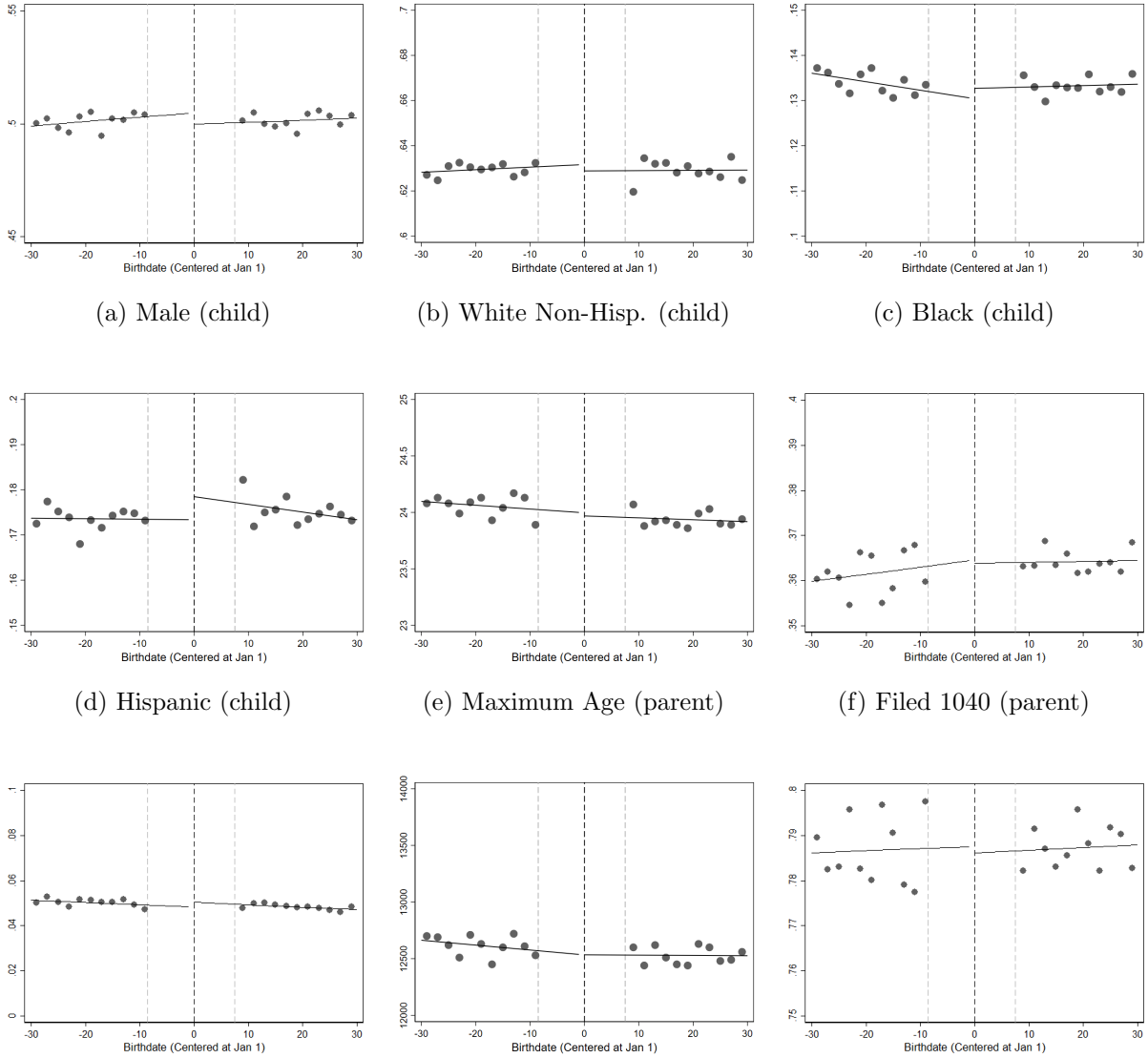
(a) Worse Off Financially



(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



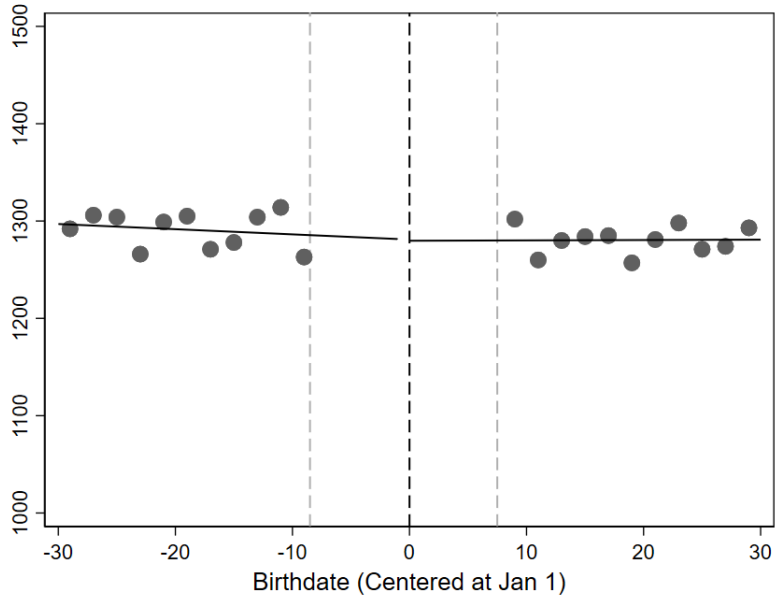
(a) Male (child) (b) White Non-Hisp. (child) (c) Black (child)

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

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

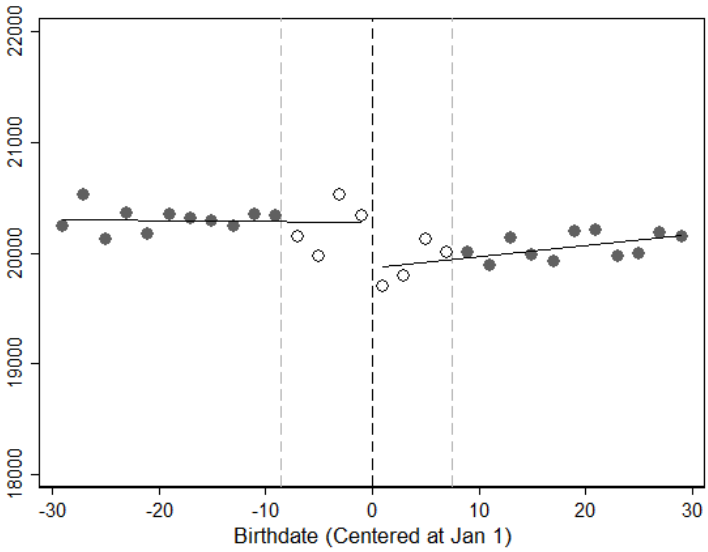
Note: The figure displays the mean of each baseline covariate by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. 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 Income During Infancy had Child been Born Prior to Jan 1

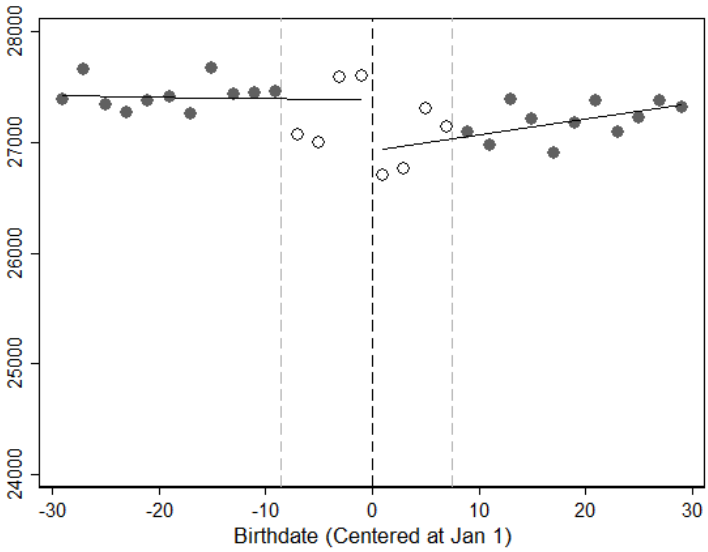


Note: The figure displays the mean potential additional income by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. 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 Eligibility on Adult Earnings (No Donut)



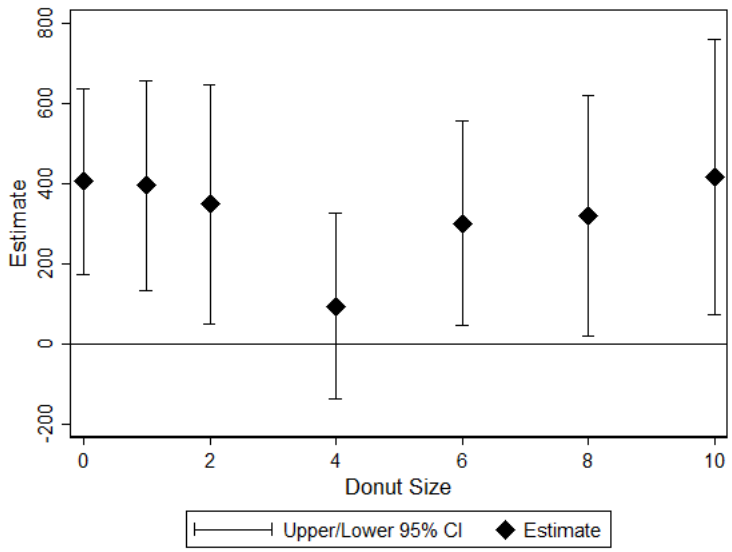
(a) Earnings (23 to 25)



(b) Earnings (26 to 28)

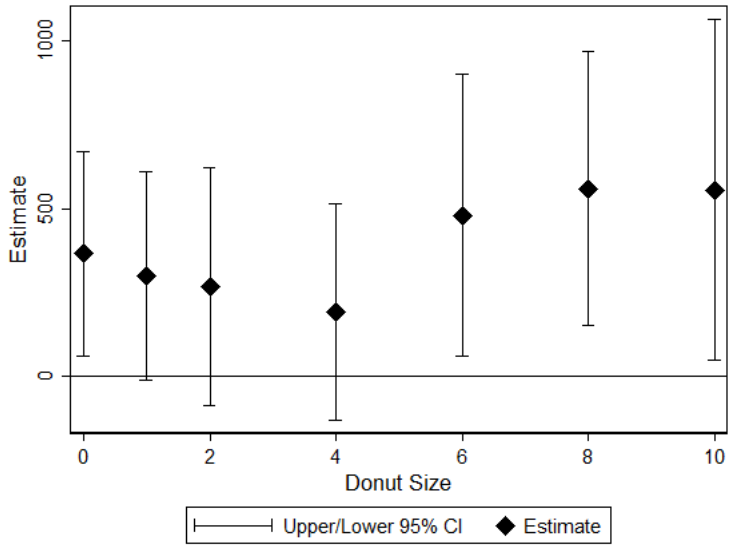
Note: The figure displays mean earnings by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. The earnings outcome 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



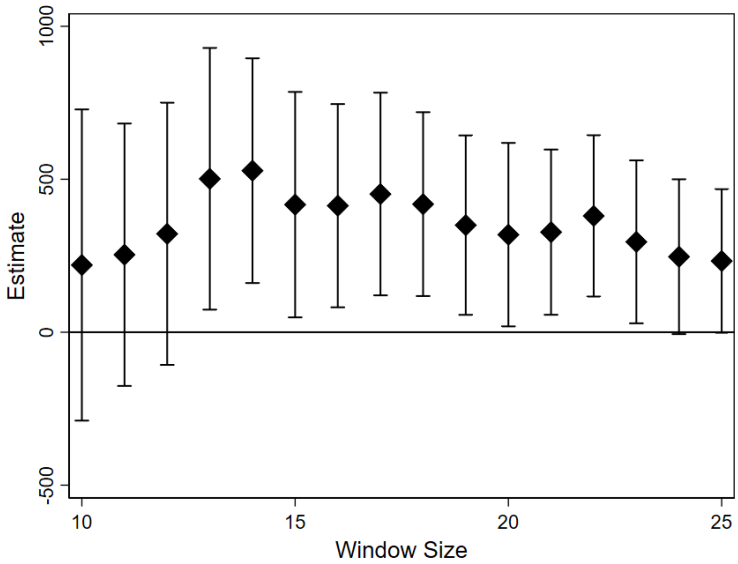
Note: The figure displays an estimate of β_1 (the January 1 RD estimate) of earnings at ages 23-25 for various donut sizes. A bandwidth of 20 days on either side of the donut is used throughout. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003,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

Figure A6: Adult Earnings RD Estimates by Donut Size (Males)



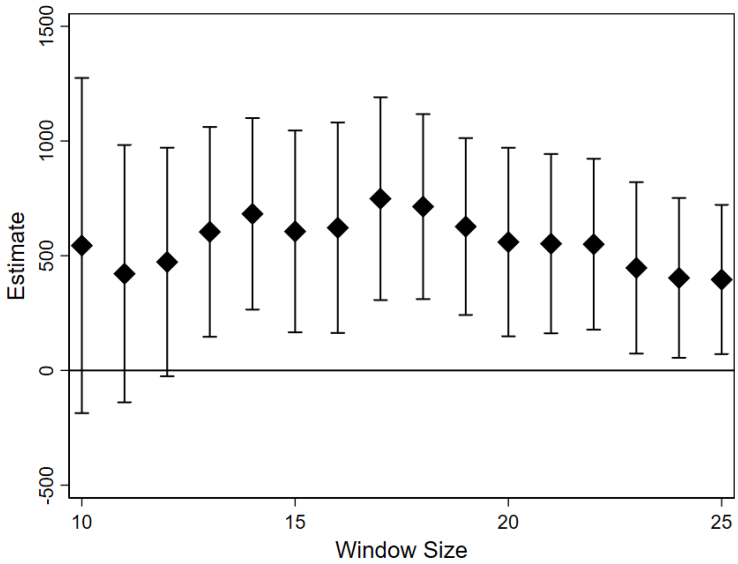
Note: The figure displays an estimate of β_1 (the January 1 RD estimate) of earnings at ages 23-25 for various donut sizes. A bandwidth of 20 days on either side of the donut is used throughout. Sample restricted to males. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002,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

Figure A7: Adult Earnings RD Estimates by Bandwidth



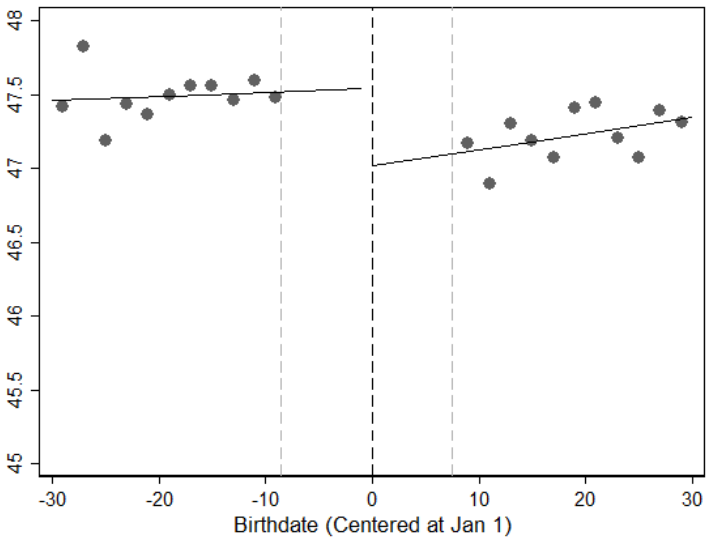
Note: The figure displays an estimate of β_1 (the January 1 RD estimate) of earnings at ages 23-25 for various bandwidth sizes (on either side of the donut). A donut of 8 days on either side of the cutoff is used throughout. Bars indicate 95 percent confidence intervals. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.

Figure A8: Adult Earnings RD Estimates by Bandwidth (Males)

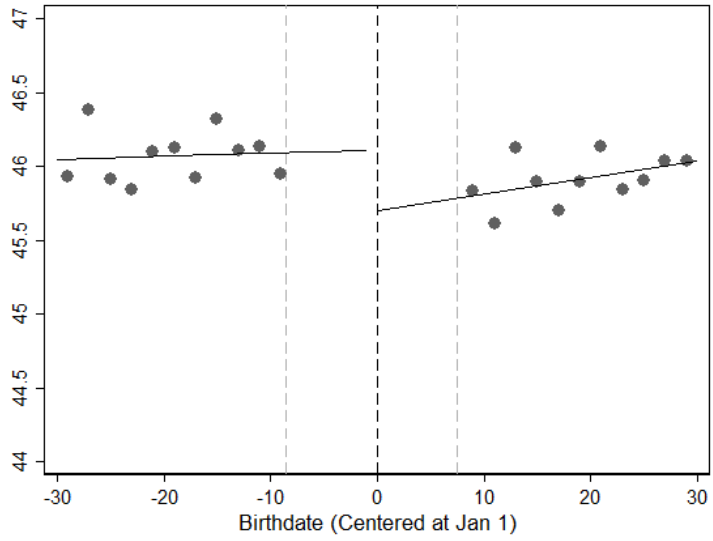


Note: The figure displays an estimate of β_1 (the January 1 RD estimate) of earnings at ages 23-25 for various bandwidth sizes (on either side of the donut). A donut of 8 days on either side of the cutoff is used throughout. Sample restricted to males. Bars indicate 95 percent confidence intervals. Census statistics approved for release under disclosure numbers CBDRB-FY2021-CES010-002, CBDRB-FY2021-CES010-003, and CBDRB-FY2021-CES010-010.

Figure A9: Effect of Eligibility on Adult Earnings Percentile



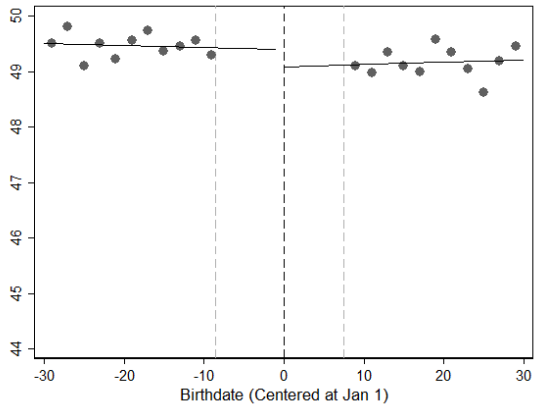
(a) Earnings Percentile (23 to 25)



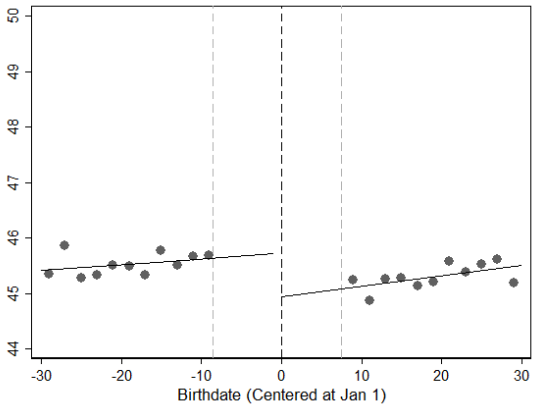
(b) Earnings Percentile (26 to 28)

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.

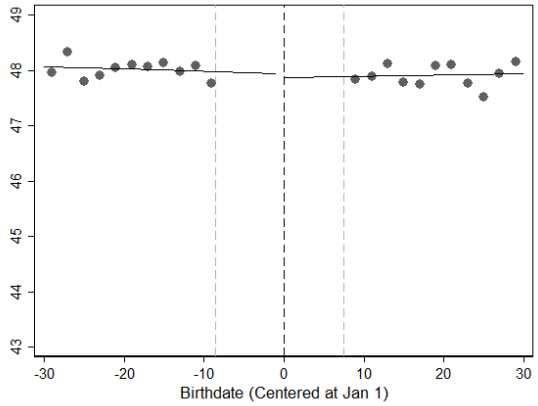
Figure A10: Heterogeneity by Sex in the Effect of Eligibility on Adult Earnings Percentile



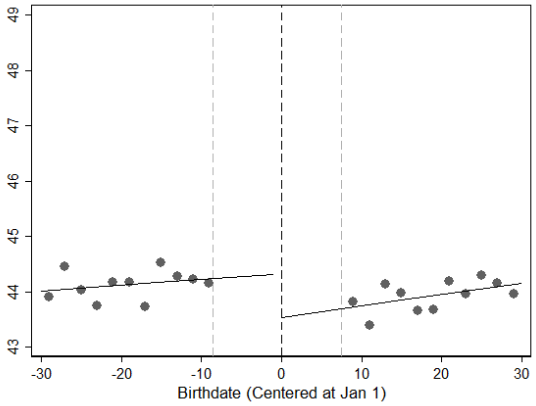
(a) Female Percentile Earnings (23 to 25)



(b) Male Percentile Earnings (23 to 25)



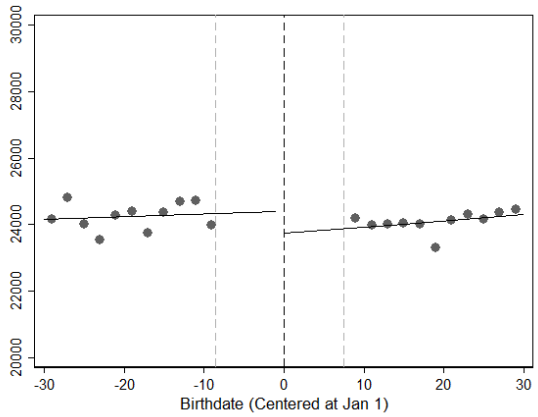
(c) Female Percentile Earnings (26 to 28)



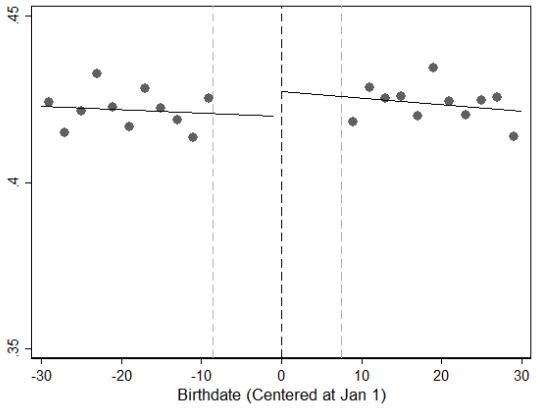
(d) Male Percentile Earnings (26 to 28)

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.

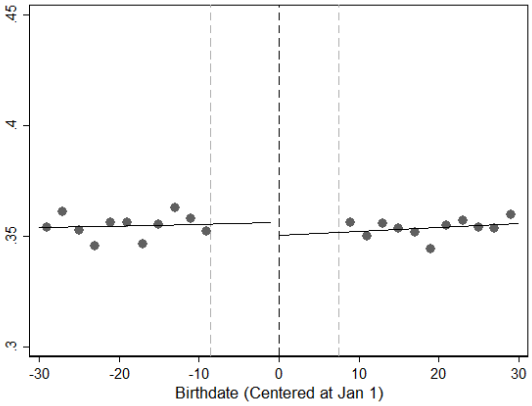
Figure A11: Effects of Eligibility on Early Family Environment (3-4 Years After Birth)



(a) Parent Earnings



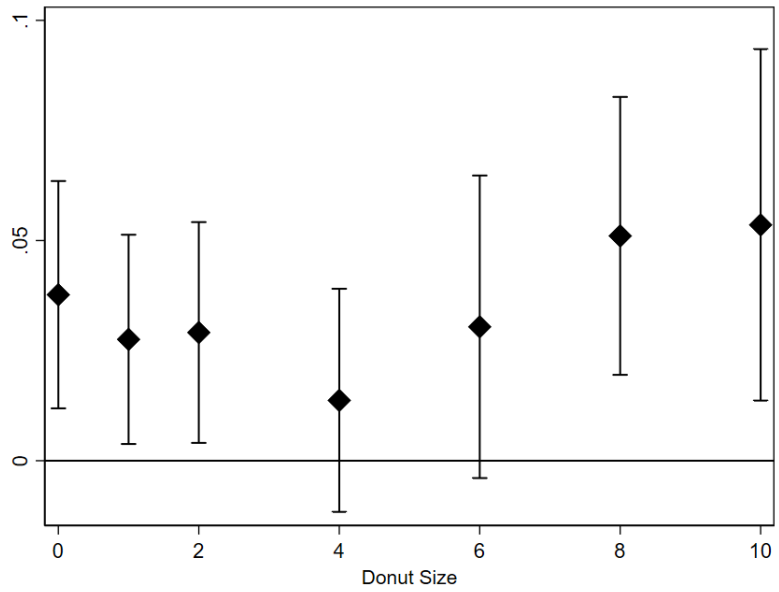
(b) Parent Poverty Status



(c) Parent Marital Status

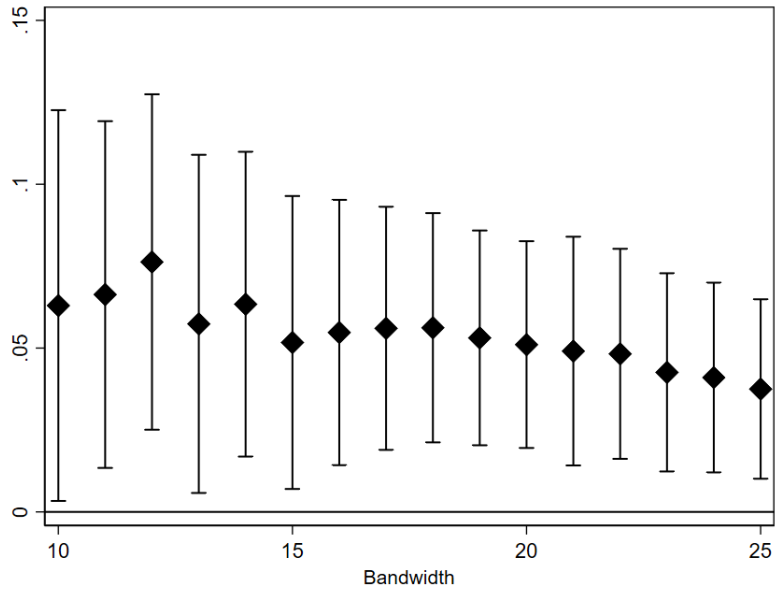
Note: The figure displays each outcome by 2-day birthdate bin for first-born children who were born within 28 days of January 1 in years 1981-82, 1986-87, and 1991-92, and whose families have predicted AGI below the EITC eligibility maximum in the relevant tax year preceding birth. The horizontal axis 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 A12: Student Outcome Index RD Estimates by Donut Size (North Carolina)



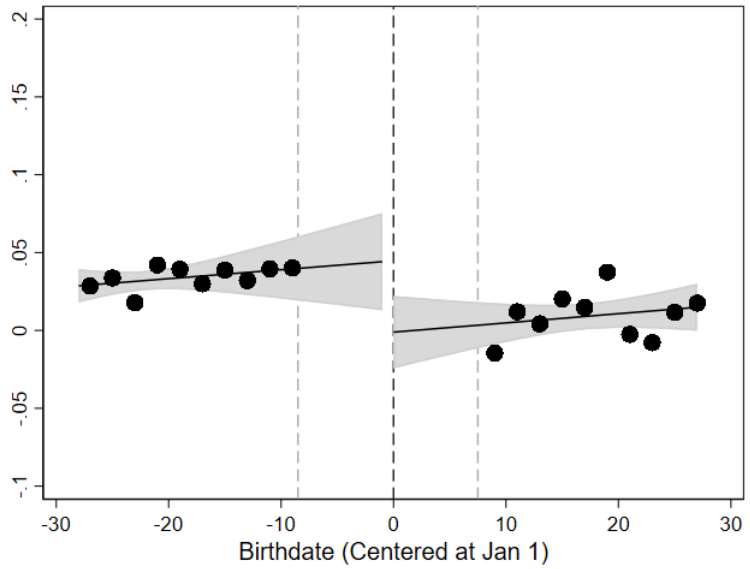
Note: The figure displays an estimate of the January 1 discontinuity in the student outcome index for various donut sizes. The sample consists of FRL-eligible students born within 28 days of January 1 in years 1993 to 1998 who entered a North Carolina public school by grade 5. See the text for additional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

Figure A13: Student Outcome Index RD Estimates by Bandwidth (North Carolina)



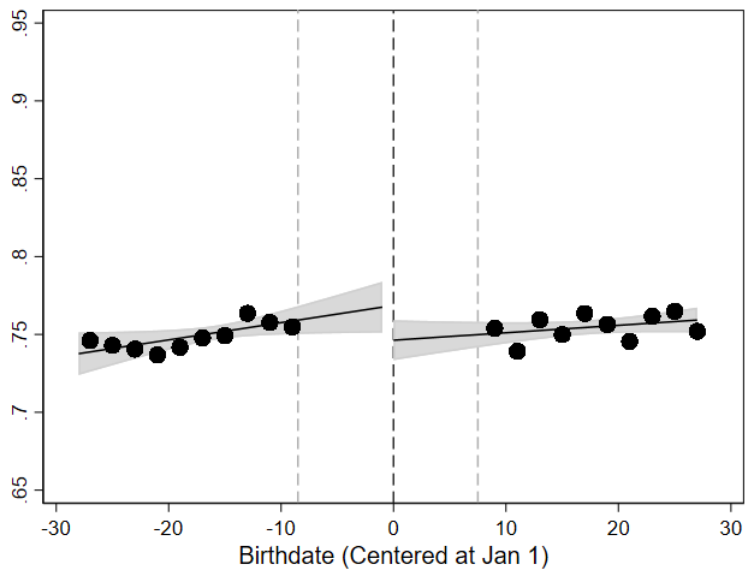
Note: The figure displays an estimate of β_1 (the January 1 RD estimate) of earnings at ages 23-25 for various bandwidth sizes on either side of the 8-day donut. The sample consists of FRL-eligible students born around January 1 in years 1993 to 1998 who entered a North Carolina public school by grade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

Figure A14: Effect of Eligibility on Student Test Score Index (North Carolina)



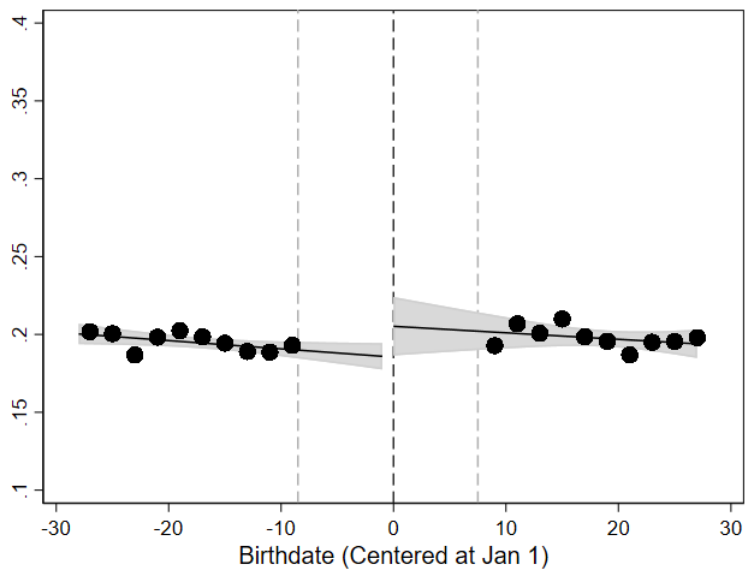
Note: The figure displays the mean of student test score index residuals (after accounting for recentered birth year fixed effects) by 2-day birthdate bin for FRL-eligible students born within 28 days of January 1 in years 1993 to 1998 who entered a North Carolina public school by grade 5. Test score 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 A15: Effect of Eligibility on HS Graduation (North Carolina)



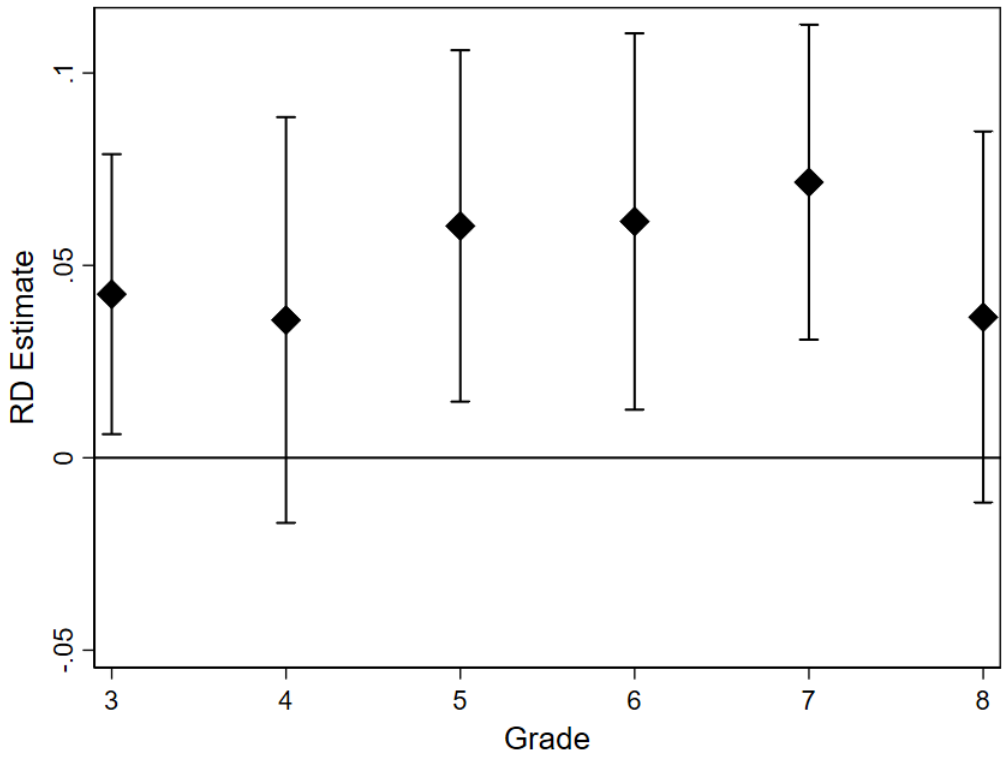
Note: The figure displays the mean of HS graduation rate residuals (after accounting for re-centered birth year fixed effects) by 2-day birthdate bin for FRL-eligible students born within 28 days of January 1 in years 1993 to 1998 who entered a North Carolina public school by grade 5. The horizonal axis 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 A16: Effect of 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 A17: Effect of 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 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. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table A2: Effect of 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*
Additional Income 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 A3: Effect of 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*
Additional Income 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 A4: Effect of 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*
Additional Income During 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 A5: Effect of 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 A6: Effect of 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*
Additional Income 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 A7: Effect of 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*
Additional Income 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 increase in income 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 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 A8: Heterogeneity of Effect of 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*
Additional Income 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 increase in income 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 the text for additional details on variable construction and sample restrictions. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).