The Effect of Income During Infancy:

Evidence from the EITC

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Andrew Barr
Texas A&M University

Alexander A. Smith
U.S. Military Academy, West Point

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Abstract

We provide evidence of the effect of income during the first year of life on educational outcomes. We take advantage of the EITC's January 1 birthdate eligibility cutoff, which results in families of otherwise similar children receiving substantially different amounts of income. Using detailed administrative education data from North Carolina, we show that income during the first year of life has meaningful positive effects on grade 3-12 schooling outcomes. Our baseline estimates indicate that a $1,000 increase in income in infancy raises math and reading test scores by 2 to 3 percent of a standard deviation and the likelihood of high school graduation by 1 percentage point. Results undergoing disclosure review suggest that these effects persist through college and into the labor market.

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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. They will have lower educational attainment and earnings and greater involvement with the criminal justice system. Recent evidence suggests that the period of early childhood may be particularly important in determining these outcomes (Duncan et al. 2010). Indeed, correlational evidence suggests that once income in early childhood is controlled for, the intergenerational relationship in income disappears. However, the inherent difficulties in separating the effects of income from other aspects of a child's environment 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, but it isn't clear to what extent these results generalize to pure increases in income in recent cohorts (Hoynes et al. 2016; Barr and Smith 2018; Anders et al. 2018; Heckman et al. 2010; Olds et al. 1998). 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).

Perhaps most closely related to our paper is Dahl and Lochner (2012), which estimates the effect of contemporaneous income on test scores using changes in the Earned Income Tax Credit (EITC) schedule. The variation in the EITC schedule resulted in shocks to income generated by both the generosity of the EITC credit and changes in the working decisions of mothers. They use this variation to produce instrumental variable estimates of the effect of current family income on test scores, finding that each $1,000 in income results in a 0.04 standard deviation increase in math and reading test scores. While their study provides compelling evidence of the contemporaneous effect of income generated by both the EITC transfer and changes in work decisions, their empirical strategy (and results) limit their ability to say anything about the effect of income received in prior years, particularly early childhood.

We make three contributions. First, we provide the only causal estimates of the long-term effects of income during the first year of life. Second, we provide the only modern causal estimates of the long-term effects of resources during early childhood. Third, we provide the first estimates of the effects on children of a pure income transfer (separate of changes in the incentive to work) generated by the EITC.

To do this, we take advantage of a discontinuity in income provision for the largest cash transfer program to poor families in the United States, the Earned Income Tax Credit (EITC). In 2017, 27 million families received over 65 billion dollars and the EITC lifted 5 million children out of poverty. We employ a regression discontinuity design that leverages the EITC's January 1 birthdate eligibility cutoff. This cutoff results in families of otherwise similar children receiving substantially different amounts of income in the following year. In recent years, low-income families with a single child born before January 1 can receive a credit worth up to 34 percent (roughly $3,400) of their income. Thus families with children born before January 1 will experience a significant increase in income during the first year of their child's life.

We estimate the effect of income during the first year of a child's life on test scores, behavior in school, and educational attainment using detailed education data from North Carolina. These administrative data are particularly well suited to our estimation strategy as they contain each student's exact date of birth, allowing us to precisely take advantage of the January 1 discontinuity. Given that eligibility for the EITC depends on income and family size, we focus most of our analysis on children eligible for free and reduced price lunch (FRL), a proxy for likely EITC eligibility.[1]

[1] We estimate that 55 to 75 percent of free and reduced price lunch (FRL) recipients are 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 find that the intent to treat effect of being born prior to January 1 is a 0.025 standard deviation increase in an index of outcomes that includes math and reading test scores, suspension, and high school graduation. Scaled by the rate of EITC eligibility among families eligible for FRL (55 to 75 percent) and the overall take up rate of the EITC (80 percent), we estimate that the additional income provided by EITC receipt results in a 0.04 to 0.06 standard deviation increase in our combined index.

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. Eligibility increases average math and reading test scores by 0.02 to 0.03 standard deviations. While small, these effects represent roughly 3 to 5 percent of the overall gap between those eligible for free lunch and those who are not. There are larger effects (1 percentage point) on the likelihood of suspension. The academic and behavioral effects result in sizable effects on high-school graduation of roughly one percentage point. This represents 10 percent of the gap between those eligible for free and reduced price lunch and those who are not, suggesting an incredibly important role for even a thousand dollar increase in income during the first year of life.[2]

Thought about another way, our estimated effects of $1,000 of income in infancy on tests taken between 8 and 14 are half to three-quarters of the *contemporaneous* effects of $1,000 of income on tests taken between 8 and 14 (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.

In the next section we briefly review what is known about the relationship between family income and child outcomes, providing context for our study. In Section 3, we describe our data and the construction of key variables and motivate the decision to focus our analyses on those eligible for free and reduced price lunch (FRL). In Section 4, we present estimates of the effect of income on math and reading test scores, high-school graduation, and behavioral

[2] Preliminary estimates from the ASEC supplement to the CPS indicate roughly a thousand dollar increase in income across the January 1 threshold for similar samples.

issues in school. We conclude the paper with a brief discussion of the implications of our findings.

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). However, these studies focus only 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.

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). However, these studies rely on relatively strong assumptions and 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.

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. 2018; Heckman et al. 2010; Olds et al. 1998). While not explicitly income transfers, these programs all provide additional resources to families, resources that are 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 therefore unclear how much these studies inform our understanding of the impacts of current resource transfers, such as those provided by the

EITC.

2.1 Effects of the EITC

The Earned Income Tax Credit (EITC) was first enacted in 1975. While initially intended to be a modest tax credit that provided assistance to low-income working families with children, it has grown into one of the federal government's largest antipoverty program. The EITC originated as a way of encouraging poor individuals to enter the labor force, thereby reducing reliance on Aid to Families with Dependent Children (i.e., "welfare"). In the 1970s, the maximum credit was $400, which was phased out between incomes of $4,000 and $8,000. The credit was expanded substantially under President Reagan, who called it "the best anti-poverty, the best pro-family, the best job creation measure to come out of Congress" (Snyder 1995).

Since Reagan, the generosity of the credit has been expanded and conditions for eligibility have been loosened. Today, the EITC is the largest cash transfer program to poor families in the United States. Last year 27 million families received over 65 billion dollars and the EITC lifted 5 million children out of poverty. Low-income first-time parents can receive a credit of up to 34 percent of their income, while those with two or more children can receive a credit of up to 40 percent.

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 (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 adolescent family income, finding that each $1,000 in income results in a 0.04 standard deviation increase in contemporaneous math and reading test scores, a 0.2% increase in the likelihood of completing high school, and a 0.2% increase in annual earnings between age 22 and 27.

While these studies provide evidence of the impact of EITC changes in adolescence, they have a number of limitations for assessing the causal effect of childhood family income on adult outcomes. First, the nature of the variation that they leverage makes it difficult to distinguish whether the effects are due to the change in generosity of the income transfer or the change in work incentives. Indeed, Bastion and Michelmore (2018) suggest that changes in work incentives are the primary channel for the effects that they observe. Second, this variation makes the identifying assumptions difficult to convincingly test and the results difficult to interpret. For example, Bastian and Michelmore (2018) rely on changes in the average maximum EITC available to an individual's family across different ages based on their year of birth, state of residence, and the number of children in their household; in addition to most of these measures being potentially endogenous, it is difficult to disentangle how changes in the maximum EITC (which isn't relevant for most families in the sample) over several years translate into downstream effects. Third, due to data limitations they cannot estimate the impact of family income at earlier ages. For example, Dahl and Lochner (2012) use only contemporaneous test scores and 92 percent of their sample is between ages 8 and 14. Finally, the data sources used in these studies do not provide sufficient sample sizes to support substantial investigations of heterogeneous effects across baseline income, credit size, family background, or local area characteristics, or to estimate non-linearities in the effect of additional income. Bastian and Michelmore (2018) use a sample of 3,495 observations from the Panel Study of Income Dynamics and Dahl and Lochner (2012) use a sample of 9,796 observations from the Children of the National Longitudinal Survey of Youth.[3] In contrast, we take advantage of a discontinuity in EITC eligibility that generates a sharp and clear discontinuity in family income.[4] Our study departs from the existing

[3]These surveys also suffer from relatively high rates of attrition that are not an issue in the administrative data.

[4]This source of variation has been used previously by Schulkind and Shapiro (2014) to examine effects of EITC on C-section birth timings and health consequences for infants, LaLumia et al. (2015) to examine effects of birth timing

EITC literature in terms of our focus on early childhood, our ability to convincingly study the effects of family income during this period on long-term outcomes, and the impressive array of large administrative data sources we bring to bear.[5] Our access to multiple data sources with detailed information on individuals in childhood through adulthood allows us to accurately measure family income and environment across the life course and explore how the effects of income vary by socioeconomic background and childhood environment.

3 Data and Descriptive Statistics

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. Another advantage of these data is the large analytical sample size that it yields. After restricting our sample to those born within one month of January 1 (the threshold date in our RD design) and observed in North Carolina schools in 3rd grade, there are 95,844 students in our analytical sample.

We construct our key measures of aptitude using mean normalized math and verbal test scores from grades 3 through 8. These scores are normalized to have a mean of zero and a standard deviation of one within grades. We also construct a measure of behavioral issues, an indicator variable equal to one if an individual is ever observed as suspended. 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.

and tax reporting, Meckel (2015) to examine effects of EITC on birth spacing, Wingender and LaLumia (2016) to examine effects of EITC on maternal labor supply, and Jones (2013) to examine effects of EITC on number of hours worked by single mothers already in the labor market.

[5] We anticipate adding in our Census and tax-based estimates in the coming month.

We construct our index using a weighted average of z-scores of its components, with the sign of each measure oriented such that the beneficial outcomes (math and reading test scores and high-school graduation) have higher scores than the negative outcome (suspension). The z-scores are generated by subtracting off the control group mean and dividing by the control group standard deviation.[6]

We focus much of 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.[7] In practice, roughly 55 percent of families of students receiving FRL are also eligible for the EITC. Among those *eligible* for FRL based on family income, the rate of EITC eligibility is even higher, roughly 75 percent. In contrast, less than 10 percent of those ineligible for FPL are eligible for the EITC.

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 (Table 1). These differences are driven by large differences in math and verbal test scores, rates of suspension, and rates of high school graduation.

4 Effects of Income on K-12 Outcomes

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 children receiving substantially different tax credit amounts 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 the Earned Income Tax Credit (EITC) in the following year, while the families of

[6]We impute missing index component values using the random assignment group mean. This results in differences between treatment and control means of an index being the same as the average of treatment and control means of the components of that index (when the components are divided by their control 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 (Kling et al. 2007).

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

children who are born on January 1st are not eligible for these credits for an additional year.[8] Therefore, some children whose families look the same on average experience additional family income for one year based entirely on the luck of being born one day earlier. 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, earnings, and criminal behavior. Our basic model is as follows:

$$Y_{ibt} = \beta_0 + \beta_1 D_b + \beta_2 z_b + \beta_3 (D_b * z_b) + \epsilon_{ibt}, \tag{1}$$

Where Y_{ibt} is an outcome of interest (such as our standardized index of outcomes) in year t for child i born on date b. D_b is a "treatment" indicator equal to one if birthdate b is prior to January 1st. The "assignment" variable z_b is the difference between birthdate b and January 1st (on January 1, z_b is zero).[9] The primary coefficient of interest is β_1, which identifies the effect of changes in likely eligibility for the EITC among poor families (i.e., FRL), rather than the effect of changes in EITC receipt or income. We discuss the scaling of this ITT parameter below.

4.1 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

[8] Where relevant for scaling our effects into dollar terms, we also take into account variation in additional kid-based benefits such as those provided through dependent exemptions and the childcare tax credit. The child tax credit, beginning in 1998, isn't available for most of our cohorts and outcomes.

[9] We also explore a number of alternative specifications including (1) using a non-linear functional form for the relationship between z_b and Y_{ibt} as well as using a non-parametric estimation strategy, (2) allowing the relationship between z_b and Y_{ibt} to vary by year, and (3) including other covariates such as individual demographics.

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 those eligible for FRPL and those who are not. As seen in the figures, the distribution of birth dates among those eligible for the EITC is largely smooth. This is 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; Shulkind and Shapiro 2014).[10] Nevertheless, we also follow an approach common in the literature and estimate donut hole RDs, dropping the observations around the January 1 threshold, to address this concern.

Another conventional "test" of the RD identifying assumption which we employ is to explore whether predetermined characteristics are balanced across the threshold for treatment, analogous to a balancing test in the context of a randomized control trial. The intuition here is that if the observable predetermined characteristics appear to be balanced across the threshold then we can be reasonably confident that the unobservable characteristics are as well. While we find some evidence of a small imbalance in race for FRL students, this imbalance disappears when we drop observations immediately on either side of the cutoff (Table 2).[11] We find no significant differences in gender or LEP status.

Another potential concern is that our treatment is confounded by other treatments that change discontinuously across the January 1 threshold.[12] To address this concern, we estimate similar specifications among individuals who are ineligible for FRL and thus unlikely

[10]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 .54 percentage-point rise in the likelihood of a December birth.

[11]We revisit concerns about racial imbalance at the cutoff below, arguing that for the imbalance to account for the difference in outcomes at the cutoff the racial gap in outcomes among students from low income families would need to be 7.5 times what is observed in the data.

[12]The only treatments that we are aware of currently are school starting ages in some states and years (not North Carolina). Currently, our plan is to exclude births in these states and years from our analyses, but we could also investigate complementarities between childhood income and starting age.

to have been eligible for the EITC. We use this group to test for (and potentially net out) the effects of other treatments that change discontinuously cross the January 1 threshold.

4.2 Results

Our baseline estimates in Table 3 indicate that likely eligibility for additional income during the first year of life generates a 0.028 standard deviation increase in our index of behavioral and academic outcomes. Figure 2 illustrates these results graphically, suggesting minimal relationship between the assignment variable and the index, with a clear jump down as we move across the eligibility threshold. Moving across the columns of Table 3, we demonstrate the robustness of the results to excluding individuals born just around the January 1 cutoff (and thus susceptible to manipulation of birth timing). These donut RD estimates are quite similar to the standard RD estimate and are stable across bandwidths (Figure 3). The estimates are similarly robust to the inclusion of covariates or controls for the day of the week on which an individual was born (Table 4). The estimates consistently indicate a 0.021 to 0.028 standard deviation increase in our index of behavioral and academic outcomes. This represents 4 to 6 percent of the gap between those eligible for the FRL and those who are not.

In Table 5, we present estimates separately by outcome.[13] Additional income increases math and reading test scores by 0.02 to 0.03 standard deviations. While small, these effects represent roughly 3 to 5 percent of the overall gap between those eligible for the EITC and those who are not. There are larger effects (1 percentage points) on the likelihood of suspension. These effects result in sizable increases in the likelihood of high-school graduation. As a result of the additional income in early childhood, individuals are 1.1 to 1.2 percentage points more likely to graduate from high school. This represents 10 percent of the gap between those eligible for the FRL and those who are not, suggesting an important role for even small increases in family income during early childhood.

[13] These estimates are presented graphically in Figure **??**.

4.3 Magnitudes

The estimates presented thus far are all intent-to-treat effects of being born prior to January 1 (and thus more likely to receive additional income). However, only 55 to 75 percent of families deemed eligible for FRL are also eligible for the EITC. Therefore, effects on EITC eligibility (frequently reported in the literature) should be scaled by 1.3 to 1.8. In other words, family EITC eligibility during the first year of life increases an individual's combined index of outcomes by 0.03 to 0.05 standard deviations. To put this in terms of EITC receipt, we would scale these again by 1.25 (due to the 80

Alternatively, we could scale the effects by the size of the implied increase in income. Preliminary estimates indicate a roughly $1,000 difference in EITC receipt across the threshold in a similar population. In other words, the ITT effects can be conveniently viewed as the effect of an additional $1,000 in income during the first year of life.

If we were to assume that these effects extrapolate linearly, raising median income among FRL eligible to the national median (roughly $20,000) would eliminate most of the observed gaps in outcomes between those eligible for FRL and those who are not.[14]

4.4 Putting the Effect Sizes in Context

While there are few causal estimates of the effects of resources in early childhood for recent cohorts, we can benchmark our results against a few estimates generated from cohorts born in the 1960s and 70s. For example, Hoynes et al. (2016) report sizable impacts of access to the Food Stamp program in early childhood (age 0 to 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. 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. We can scale these estimates to generate effect sizes for one year of FSP use by dividing by 0.43 (to get the

[14] We are not suggesting that the effect is likely to extrapolate linearly, just trying to put the effect size in some context.

effect of participation) and dividing by 5.75 (to get the per year effect). This results in estimated effects on high-school graduation of 7.3 and effects on the index of economic self-sufficiency of 0.12 standard deviations.

Our preferred estimates indicate effects on high-school graduation of around 1 percentage points. To put in the context of the literature, these estimates imply effects of roughly 1 percentage points per $1,000 of additional income in early childhood, a quarter to a third of the size of the effects estimated for the early Food Stamp program.

5 Discussion and Conclusion

Recent evidence suggests the importance of early childhood resources in determining lifetime success. We bring new evidence to this question by taking advantage of a discontinuity in income provision generated by the largest cash transfer program to poor families in the United States, the EITC. Combined with detailed education data from North Carolina and five decades of universe 1040 Tax data linked with multiple Census surveys, we provide the only modern estimates of the causal effect of income during the critical first year of life. Across an array of outcomes across the life course, we demonstrate that a substantial portion of the relationship between family resources and child outcomes is causal.

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 with young children would result in substantial reductions in the income gap in outcomes.

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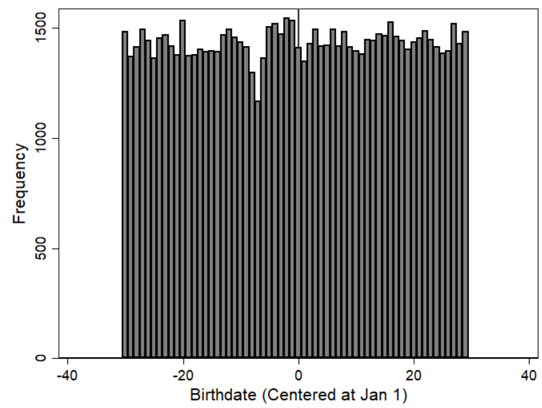
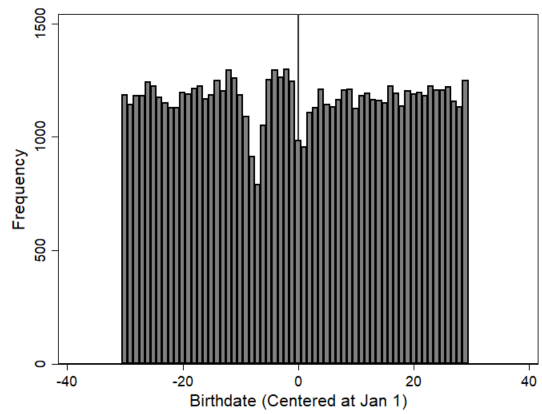
Figure 1: Birthdate Distributions



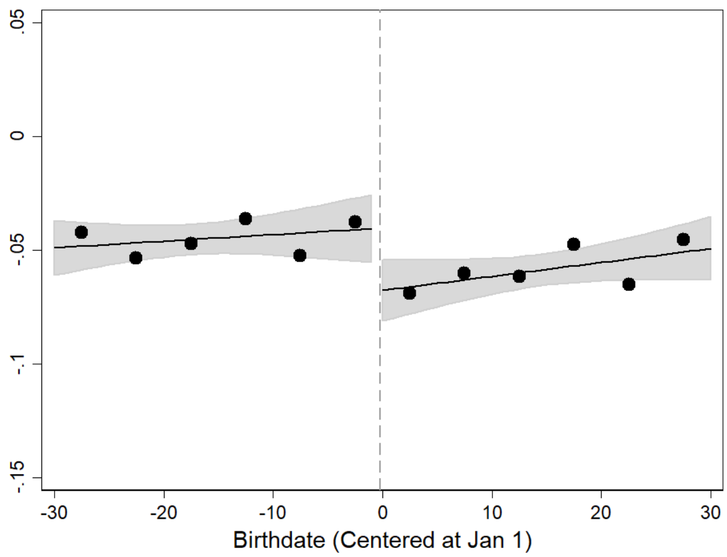
(A) Free and Reduced Price Lunch



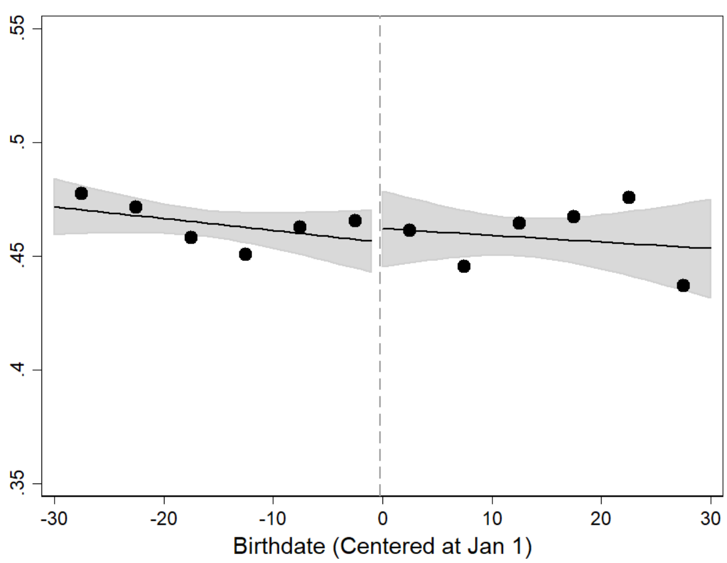
(B) Non-Free and Reduced Price Lunch

Note: The histograms display the distribution of students by birthdate (relative to January 1) for students who enter North Carolina public school by grade 3 and were born within one month of January 1. Sample restricted to 1993 to 1998.

Figure 2: Effect of EITC Eligibility on Student Outcome Index



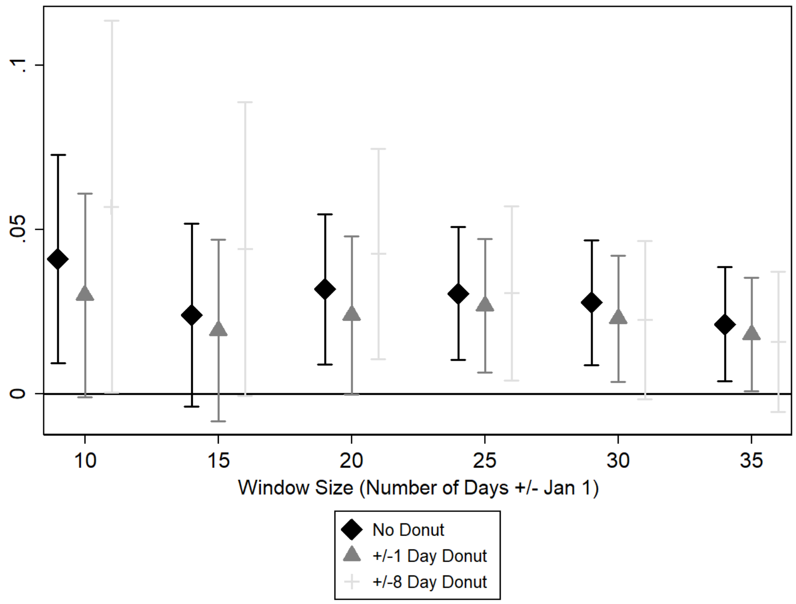
(A) Free and Reduced Price Lunch



(B) Non-Free and Reduced Price Lunch

Note: The figure displays the mean student outcome index by 5-day birthdate bin for students who enter North Carolina public school by grade 3 and were born within one month of January 1. Sample restricted to 1993 to 1998. Student outcome constructed as the mean of normalized test scores, 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 left panel shows the sample of students who are eligible for free or reduced price lunch (and whose families were likely to have been eligible for the EITC). The right panel shows the remaining students (whose families were not likely to have been eligible for the EITC). The shaded area shows the 95% confidence interval.

Figure 3: RD Estimate for Student Outcome Index by Bandwidth/Donut Size



Note: The figure displays an estimate of the January 1 discontinuity in the student outcome index for various bandwidths and donut sizes. Sample restricted to FRL students.

Tables

Table 1: Summary Statistics

	FRL (1)	Non-FRL (2)
Student Outcome Index	-0.05	0.46
Test Score Index	0.04	0.70
HS Graduation	0.76	0.91
Any Suspension	0.20	0.08
Black	0.42	0.11
Limited English Proficiency	0.08	0.01
Observations	59,566	36,278

Note: The sample is restricted to individuals born within one month of January 1 and observed in North Carolina schools in 3rd grade. Sample restricted to years 1993-1998. Free and Reduced Price Lunch (FRPL) status constructed as a binary variable equal to one if an individual is ever observed eligible for FRPL. Test score index constructed as the mean of normalized (mean zero, standard deviation one) math and verbal test scores in grades 3 through 8. Student outcome constructed as the mean of normalized test scores, high school graduation, and any suspension.

Table 2: Covariate Balance

	FRL		Non-FRL	
	(1)	(2)	(3)	(4)
Black	-0.025***	-0.006	0.000	0.003
	(0.010)	(0.013)	(0.007)	(0.011)
Mean	*0.417*	*0.416*	*0.110*	*0.109*
LEP	-0.012	-0.006	0.002	0.001
	(0.010)	(0.006)	(0.002)	(0.003)
Mean	*0.078*	*0.077*	*0.008*	*0.009*
Male	-0.011	-0.005	0.004	0.004
	(0.008)	(0.010)	(0.010)	(0.013)
Mean	*0.515*	*0.513*	*0.513*	*0.513*
Donut Size (Days)	0	+/-8	0	+/-8

Note: This table shows the regression discontinuity estimates for the discontinuity at the January 1 cutoff. Each cell shows the β_1 coefficient estimate from a separate regression where the row denotes the dependent variable and the column denotes the sample and "donut" size of the specification. The sample is restricted to individuals born within one month of January 1 and observed in North Carolina schools in 3rd grade. Sample restricted to years 1993-1998. Column 1 and 2 are limited to Free and Reduced Price Lunch (FRL) students (likely to be from families eligible for the EITC) and column 3 and 4 are limited to Non-Free and Reduced Price Lunch (Non-FRL) students (unlikely to be from families eligible for the EITC). Donut size refers to the number of days on either side of the January 1 cutoff that are dropped to account for the possibility of manipulation around the cutoff. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table 3: Effect of Eligibility on Student Outcome Index

	(1)	(3)	(5)
FRL Student	0.028***	0.023**	0.023*
	(0.010)	(0.010)	(0.012)
Obs	*59,566*	*59,539*	*59,333*
Mean	*-0.052*	*-0.051*	*-0.051*
Non-FRL Student	-0.006	-0.004	-0.007
	(0.010)	(0.009)	(0.012)
Obs	*36,278*	*36,391*	*36,689*
Mean	*0.461*	*0.461*	*0.460*
Cutoff Year Fixed Effect	X	X	X
Day-of-Week Fixed Effect	X	X	X
Demographic Controls			
Donut Size (Days)	0	+/-1	+/-8

Note: This table shows the regression discontinuity estimates for the discontinuity at the January 1 cutoff. Each cell shows the β_1 coefficient estimate from a separate regression where the row denotes the sample and the column denotes the donut" size of the specification. All regressions include cutoff year and day of week fixed effects. The sample is restricted to individuals born within one month of January 1 and observed in North Carolina schools in 3rd grade. Sample restricted to years 1993-1998. Row 1 is limited to Free and Reduced Price Lunch (FRL) students (likely to be from families eligible for the EITC) and row 2 is limited to Non-Free and Reduced Price Lunch (Non-FRL) students (unlikely to be from families eligible for the EITC). Donut size refers to the number of days on either side of the January 1 cutoff that are dropped to account for the possibility of manipulation around the cutoff. Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table 4: Effect of Eligibility on Student Outcome Index

	(1)	(2)	(3)
FRL Student	0.023*	0.023*	0.021*
	(0.012)	(0.012)	(0.012)
Obs	*59,333*	*59,333*	*59,333*
Mean	*-0.051*	*-0.051*	*-0.051*
Non-FRL Student	-0.008	-0.007	-0.007
	(0.012)	(0.012)	(0.012)
Obs	*36,689*	*36,689*	*36,689*
Mean	*0.460*	*0.460*	*0.460*
Cutoff Year Fixed Effect	X	X	X
Day-of-Week Fixed Effect		X	X
Demographic Controls			X

Note: This table shows the regression discontinuity estimates for the discontinuity at the January 1 cutoff. Each cell shows the β_1 coefficient estimate from a separate regression where the row denotes the sample and the column denotes the inclusion of different controls. All regressions exclude observations within an 8 day window of the January 1 cutoff (i.e., Donut Size = 8). The sample is restricted to individuals born within one month of January 1 and observed in North Carolina schools in 3rd grade. Sample restricted to years 1993-1998. Row 1 is limited to Free and Reduced Price Lunch (FRL) students (likely to be from families eligible for the EITC) and row 2 is limited to Non-Free and Reduced Price Lunch (Non-FRL) students (unlikely to be from families eligible for the EITC). Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).

Table 5: Effect of Eligibility on Component Outcomes

	FRL		Non-FRL	
	(1)	(2)	(3)	(4)
Test Score Index	0.029**	0.021*	-0.008	-0.006
	(0.013)	(0.012)	(0.013)	(0.014)
Obs	*76,685*	*76,685*	*48,187*	*48,187*
Mean	*-0.031*	*-0.031*	*0.630*	*0.630*
HS Graduation	0.011*	0.012*	-0.001	-0.001
	(0.006)	(0.006)	(0.010)	(0.010)
Obs	*68,775*	*68,775*	*46,516*	*46,516*
Mean	*0.726*	*0.726*	*0.846*	*0.846*
Any Suspension	-0.009**	-0.008**	0.002	0.002
	(0.004)	(0.004)	(0.005)	(0.005)
Obs	*79,549*	*79,549*	*52,056*	*52,056*
Mean	*0.173*	*0.173*	*0.069*	*0.069*
Cutoff Year Fixed Effect	X	X	X	X
Day-of-Week Fixed Effect	X	X	X	X
Demographic Controls		X		X

Note: This table shows the regression discontinuity estimates for the discontinuity at the January 1 cutoff. Each cell shows the β_1 coefficient estimate from a separate regression where the row denotes the sample and the column denotes the inclusion of different controls. All regressions exclude observations within an 8 day window of the January 1 cutoff (i.e., Donut Size = 8). The sample is restricted to individuals born within one month of January 1 and observed in North Carolina schools in 3rd grade. Sample restricted to years 1993-1998. Row 1 is limited to Free and Reduced Price Lunch (FRL) students (likely to be from families eligible for the EITC) and row 2 is limited to Non-Free and Reduced Price Lunch (Non-FRL) students (unlikely to be from families eligible for the EITC). Significance levels indicated by: * (p<0.10), **(p<0.05), ***(p<0.01).