Effects of Family Income in Infancy on Child and Adult Outcomes: New Evidence Using Census Data and Tax Discontinuities

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Abstract

Eligibility for child-related tax benefits depends on the calendar year in which a child is born. Families with children born in December are eligible for tax benefits a year earlier than families with children born a few days later in January. These differences create a discontinuity in after-tax income in infancy worth on average approximately $2,000 for families in tax year 2016. This paper uses regression discontinuity techniques to calculate the effect of this change in after-tax income on outcomes for children and young adults in Census data. Evidence show that a $1,000 increase in after-tax income in infancy results in a 1.2 percentage point increase in the probability of a student being grade-for-age by high school, a basic indicator of academic achievement and social maturity. Effects of this income shock are larger for children from families that are more likely disadvantaged at a child's birth, including Black families, and families with low education attainment. After high school, small differences in labor force attachment, earnings and education attainment persist for the adults who experienced the income increase as children. These effects are again pronounced for Black adults and adults born in counties with low average education attainment.

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1 Introduction

Researchers are finding growing evidence of sustained relationships between family economic resources in childhood and later life outcomes. Descriptive research from the U.S. shows that children from lower-income families are at higher risk of poor physical health as children (Case, Lubotsky and Paxson, 2002; Currie, 2009), more likely to perform worse in school (Michelmore and Dynarski, 2017; Reardon, 2011), and less likely to graduate high school (Stark, Noel and McFarland, 2012; Autor et al., 2019). These differences persist into adulthood, as disadvantaged children are less likely to earn college degrees (Bailey and Dynarski, 2011), more likely to have experiences in the criminal justice system, including incarceration (Chetty et al., 2019), more likely to have lower earnings (Chetty et al., 2014) and more likely to have reduced longevity (Ferrie and Rolf, 2011).

The causal mechanisms underlying these relationships are an active field of study, as family income is correlated with unobservable determinants of outcomes for children. Research show that changes in permanent family income can have pronounced impacts on children from lower-income families (Akee et al., 2010; Loken, Mogstad and Wiswall, 2012; Shea, 2000; Chevalier et al., 2013; Bastian and Michelmore, 2018), although permanent income changes produced by specific transfer programs may have smaller effects (Jacob, Kapustin and Ludwig, 2014). In comparison, research on the effects of transitory changes in family income offers more mixed conclusions. Some papers find that changes in transitory family income have short-term impacts on performance of school students (Dahl and Lochner, 2012; Chetty, Friedman and Rockoff, 2011), some papers find long-term impacts (Black et al., 2014), and some papers find neither short nor long-term impacts (Cesarini et al., 2016). One critical topic left largely unaddressed in this evidence is the long-term effect of modest changes in temporary income in infancy on outcomes for children. Research suggests that conditions in infancy and early childhood may be consequential for long-term patterns of child development, so it is possible that effects could be strong at these early ages (Cunha et al., 2006; Duncan, Ludwig and Magnuson, 2011; Currie and Almond, 2011). If impacts are stronger at different ages, such a finding has consequences for transfer policy design. Most transfer policies in the U.S. are not child age-specific, and differences in impacts by age would suggest that increasing benefits at certain ages and decreasing them in others may be a low cost reform that improves outcomes for children.

This paper addresses this gap in the literature by analyzing the effect of a shock to family income that happens in the first year of a child's life. If a child is born before New Year's Day, that child's family is eligible for tax benefits for that child one year earlier than if a child is born after New Year's. This discontinuity in tax policy means that the parents of children born one day earlier have larger after-tax income in the first year of a child's life. The increase in income is modest but non-trivial, worth about $2,000 on average in tax year 2016, and resulting in an average 5% increase in after-tax income. Furthermore, this increase is experienced by a broad share of families, so its effects may be analyzed and compared for families with different income levels. Note that this increase is a speeding up of the tax credit and deduction process for a child, as the families with children born in December, several years later, will be eligible for tax benefits for one year less than families of children born in January. Thus, the cost to the government of this increase in after-tax income comes from just altering the timing of the tax benefits and moving them from a child's later adolescence to infancy.

This research setting is closest to the work in Black et al. (2014) and Bastian and Michelmore (2018). Both of these papers analyze the long-term effects of income shocks from tax policy that happen early in life. Black et al. (2014) find that a $1,700 tax credit income transfer to a child's family at age 5 has effects on student achievement 10 years later. Bastian and Michelmore (2018) use implementation of state Earned Income Tax Credit programs, and conclude that increases in income in ages 0-4 have no detectable effects on high school graduation status and earnings in adulthood. This paper builds on these results with new evidence from a different research setting. Compared to Black et al. (2014), this paper looks at the effects of an income shock that happens even earlier in life, and extends analysis to effects on later life outcomes after school. Compared to Bastian and Michelmore (2018), this research looks at changes in income that reflect transitory income alone, and has more power to distinguish heterogeneous effects at different income levels.[1]

This paper calculates the effect of the shock in after-tax income around the New Year using a regression discontinuity design with date of birth as a running variable. Endogenous birth timing around the New Year is a threat to identification, and this paper accounts for this issue by omitting from the estimation process a region of observations around the New Year. This omitted region is identified using bunching estimation techniques (Chetty et al., 2011; Kleven and Waseem, 2013; Saez, 2010). Three assumptions are sufficient for this strategy to identify the causal effect of this boost in after-tax income on later life outcomes. First, no other treatments must coincide with the passing of the New Year. Second, the region affected by endogenous birth timing must be consistently identified using the omitted region estimation technique. Third, the evolution of an outcome must be consistently estimated using extrapolation through the omitted region.

Results show that this change in income in infancy has impacts on a child being grade-for-age by high school. Students are grade-for-age if they are in the school grade they would be in had they entered Kindergarten or first grade on or before the year they were eligible to enter those grades, and if they progressed

[1]The introduction of a state Earned Income Tax Credit program would impact earnings of families for years into the future and may change labor supply incentives. Hence, the results in Bastian and Michelmore (2018) are best interpreted as a mixture of changes in transitory income and permanent income.

through school without ever repeating a grade.[2] Being grade-for-age is an indication that a student has met academic standards and shown social maturity in school (Xia and Kirby, 2009), so improvements in the share of students grade-for-age indicate multi-dimensional improvements in student development. Consistent with validity of the research design, there is no discontinuity in pre-school attendance and Kindergarten entrance around the New Year. Children born before the New Year, who experience the increase in after-tax income, enter pre-school and Kindergarten at roughly the same rate as the children born after the New Year, who do not experience it.[3] However, by the time students reach high school, students born before the New Year who experienced the increase in family income are approximately 1.1 percentage points more likely to be grade-for-age than students born after the New Year who did not. This finding is robust to a variety of checks, including restricting to students who live in their birth state, and dividing up the sample by birth cohort to use differences in after-tax income by birth cohort to look at effects. Reinterpreting this reduced form effect as a direct effect of income, this evidence shows that an extra $1,000 in the first year of life increases the probability of the average student being grade-for-age by high school by 1.2 percentage points.

These effects of an extra $1,000 on grade-for-age status by high school are largest for groups that had lower family income at birth, including children whose mothers have a high school degree or less, and Black children. These results are consistent with the finding in Loken, Mogstad and Wiswall (2012) that the relationship between income and child outcomes is non-linear. Similarly-sized increases in income have larger effects on lower-income families and smaller effects on higher-income families.

The effects of this increase in income in infancy persist after high school. Following Kling, Liebman and Katz (2007), this paper combines income, participation in the labor force, high school degree attainment and Supplemental Nutrition Assistance Program (SNAP) receipt into a single measure of economic self-sufficiency. In the years after young adults turn 19, there are suggestive but not statistically significant discontinuities in this measure in the full sample between adults who did and did not experience the income increase as infants. However, there are larger and significant discontinuities for young Black adults and adults born in counties with comparatively lower education attainment.[4] These discontinuities in outcomes last until young adults reach their mid-20s, with the discontinuities driven by differences in high school education attainment and earned income. However, these effects fade somewhat at later ages. This evidence

[2]Most school systems define grade-for-age status starting from the first year a child entered Kindergarten or 1st grade. As these entrance dates are not observable in Census data, this definition is the closest analogue.

[3]The claim that this result is consistent with the validity of the research design will be described in more detail later. Technically, there could be gaps that open up in this measure early on either because the grade-for-age status calculation is incorrect (which would suggest that the research set-up is flawed), or because parents want to hold back their children early on before they enter school (which would still be valid with the research design, but is more difficult to interpret). Since there is no detectable gap either way, it suggests that both possibilities have not happened.

[4]Note that looking at adults born in counties with comparably low education attainment is a slightly different subgroup than what was looked at before, children with mothers who have education attainment of a high school degree or less. A large fraction of children move away from home in their 20s, so parent education attainment cannot be defined for them. This subgroup is an imprecise proxy necessitated by data limitations.

is consistent with income in infancy having a small effect on adult outcomes that attenuates with age as young adults gather experience in the labor force.

These results suggest that family income in infancy has effects on child development with ramifications stretching into adulthood, especially for families more likely disadvantaged at a child's birth. Furthermore, compared to some of the previous literature looking at similarly-sized income shocks at later ages, the effects on adult outcomes here are relatively large. This finding may suggest that effects of income in infancy are larger than effects from income at later ages. Overall, these findings fit within and expand on two directions of research: research into the gaps in the development of children that open up before children enter formal schooling, and research focusing on early childhood as a critical period for development. The relatively large effects measured here suggest that transfer policies aimed at families with young children may have substantial long-term benefits. As these effects come from altering the timing of tax benefits from adolescence to infancy, refocusing transfer benefits on earlier periods of life may offer a low-cost way of increasing such transfers to improve outcomes for children.

2 Data

The data used in this paper come from three sources: the Current Population Survey (CPS), the long form sample of the 2000 Census, and the 2001-2016 American Community Survey (ACS). The CPS is a monthly sample of households in the U.S.. Although sizes of samples differ by year, the current CPS samples approximately 60,000 households per month (Bureau of Labor Statistics, 2018). This paper uses the detailed income information in the March CPS to estimate the discontinuity in after-tax income for having a child born before the New Year. Details of this calculation are in the next section and in Appendix A. This paper also uses the information on grade enrollment and grade repetition in the October CPS to analyze general patterns of grade repetition by grade.

The long form of the 2000 Census was a survey mailed to one-sixth of all U.S. households, covering 17% of the U.S. population, or approximately 22 million U.S. households (U.S. Census Bureau, 2009). This survey contained questions on a wide variety of demographic and economic data not otherwise collected in the 100-percent Census, including data on levels and sources of income, household structure, labor force participation and education attainment for respondents ages three and up. The ACS is an annual survey of households. The number of households sampled varies from year to year, but since 2011 the Census Bureau has targeted approximated 3.5 million households (U.S. Census Bureau, 2014). The ACS covers many questions similar to those in the 2000 Census long form, but some question definitions are different. Appendix A covers some of the differences in definitions in more detail and describes how this paper combines the questions into single measures that can be used across years. Both the ACS and the 2000 Census long form were matched to the Numident file of the Social Security Administration using a Protected Identification Key from the Census Bureau. The Numident file offers a listed place of birth for each individual, which was coded into a country of birth by researchers at the University of Michigan. This research uses the 2000 Census and 2001-2016 ACS for all of the regression discontinuity analyses.

One of the key outcomes this paper looks at is whether or not a student is grade-for-age. This research assigns grade-for-age status to students based on four pieces of information: highest grade completed (or most recent grade enrolled), the state of birth of the child, the date of birth of the child and the day on which households respond to the survey. Many states set explicit Kindergarten and 1st grade age entrance requirements that require students to be a specific age by a certain date before being eligible to enter either Kindergarten or 1st grade. Comprehensive data on these state policies for Kindergarten entrance were collected by Bedard and Dhuey (2012), and they generously provided their most recent data covering 1955 to 2015. This data was compiled directly from state statutes and legislative history on school entry policies, and cross-checked against a variety of other data sources. This research assigns expected completed grades to students assuming that they entered Kindergarten or 1st grade in the first year that they were eligible for those grades and then progressed through all other grades sequentially without repeating a grade. A student is grade-for-age if they have completed the most recent grade that this measure lists.

Three complications are worth noting about this measure. First, some states do not specify statewide Kindergarten entrance rules and allow local school districts to set their own cutoffs. As no clear expected grade can be assigned to these individuals without more detailed data on individual school district practices, this paper drops any individuals born in these states from any further calculation. Second, some states make the eligibility cutoff January 1st or December 31st. In the years that such cutoffs are present, children born before and after the New Year would, in addition to the difference in after-tax income, also experience the treatment of different grade eligibility rules. This paper also drops these individuals from any further calculation. Lastly, there are only a handful of grades where grade-for-age status can be reliably assigned due to the nature of the grade attainment and enrollment questions in the 2000 long form Census and 2001-2007 ACS. This issue is described more in Appendix A. The consequence of this limitation is that grade-for-age status can only be consistently calculated in pre-school, Kindergarten, 1st grade, 5th grade, 7th grade, and 9th through 11th grades.

Since this paper analyzes grade-for-age status at different grades using data from 2000 to 2016, the distribution of birth cohorts included in each calculation will differ. For example, the high school grade-for-age calculations include individuals born from 1982 to 2001, but the Kindergarten enrollment calculations involve individuals born 1996 to 2011. In all, results looking at grade-for-age status include children who were born from 1982 to 2011, with the exact birth cohort of children analyzed depending on the grades looked at. To ensure that analyses of outcomes for adults continue to follow these same cohorts, this paper restricts analysis to adults who were born in 1980 and later. Further complications with the use of different cohorts are described later.

Thus, the sample of data varies by outcome analyzed. However, the sample for analysis could broadly be described as adults and children born 1980 and later in states that had statewide Kindergarten entrance cutoffs away from the New Year in the year that the student would have entered Kindergarten in that state.

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3 Overview of Tax Policy Relating to Children

The variation that drives this paper is the discontinuity in after-tax income for families in the first year of an infant's life depending on the birth timing of the child. There are four main child-related tax benefits that parents are eligible for: a personal exemption for a dependent, the Earned Income Tax Credit (EITC), the Child Tax Credit (CTC) and the Child and Dependent Care Credit. Parents are eligible for these tax benefits for a child starting in the tax year that a child is born. So, as Figure 1 shows, parents with children born in December are eligible to claim child-related tax benefits in their child's first year in life. In comparison, parents of children born a few days later in January can only claim them on tax forms starting with the next year.

Figure 2 estimates the average discontinuity in after-tax income for having a child born before the New Year produced by these four benefits. Without access to administrative data on tax records, it is difficult to precisely calculate the value of this discontinuity, but Figure 2 offers the best approximation to this calculation possible with survey data from the March CPS.[5] These estimates are in line with calculations from administrative data. For example, this paper estimates that the average tax benefit of having a child before the New Year was $2,150 for tax filers from 2000 to 2010. LaLumia, Sallee and Turner (2015) estimate with administrative data that the same benefit over the same time period was $2,100.

Figure 2 shows that this discontinuity has been steadily increasing over time, rising from about $800 in 1980 to a little over $2,000 in 2016. A more thorough discussion of the history of these four tax benefits is

[5]This paper calculates this after-tax income discontinuity by using data from the March CPS in a four year radius of a given tax year, and restricting the sample to families with at least one child three years old or younger. It then assigns the family the total income from their household of residence, and treats one of those children three years old and younger as an "infant." Finally, it computes the after-tax return for the family both with and without the "infant" three years old and younger, and the difference between the two tax returns identifies the discontinuity. Ideally, this comparison would only include parents with infants born around December and January given the fact that seasonality in the patterns of birth ensure that the characteristics of parents evolve over time (Buckles and Hungerman, 2013). However, the CPS data do not identify month or quarter of birth. The use of children three years old and younger as "infants" and the use of additional years of CPS data ensure more precision and have minimal effects on point estimates. More details and robustness checks for the choices in this calculation are in Appendix A.

in Appendix B, but in general, the rise in the discontinuity reflects increased generosity of the EITC and CTC. Furthermore, the discontinuity is non-zero and positive for the vast majority of families. The share of parents with no change in their tax liabilities in this calculation is around 10% prior to 1994 and falls to about 6% thereafter. These parents have zero change in tax liabilities for three reasons: either they have very low income, they have already received the maximum of relevant tax credits, or they have high incomes and high deductions. Thus, the vast majority of families experience a modest increase in after-tax income.[6]

Figure 2 also shows average changes in after-tax income for having a child born before the New Year for two subgroups: families where a child's mother has education attainment of a high school degree or less and Black families. These are subgroups this paper will look at later, as they have lower average income at birth than families with higher education attainment and White families. As is clear, the average increases in after-tax income for these groups are similar to or slightly less than the average for all families in early years. However, they gradually increase and become equal to or larger than the average over time. The fact that these discontinuities in income are relatively large for these groups reflects the fact that the EITC and to a lesser extent the CTC are aimed at lower income families. Critical to the size of these tax benefits for these families is the fact that the EITC is a refundable tax credit and the CTC is partially refundable, meaning that individuals who have low tax obligations can actually see a positive tax return from the government.[7]

Figure 3 presents these changes in after-tax income as percentage increases in after-tax income. The average percent increase in after-tax income is generally larger for families where the mother has a high school degree or less and for Black families than it is for all families on average.[8] In particular, the lines rapidly diverge as the generosity of the CTC and EITC ramp up in the 1990s. These patterns demonstrate how these two programs create especially large percentage jumps in income for likely disadvantaged households.

As is clear in Figure 1, the discontinuity in after-tax income described here in infancy does not persist into the next year.[9] In the next tax filing year parents of infants born before and after the New Year will be eligible for the same tax credits and deductions. Furthermore, parents are only eligible for these tax credits

[6]This paper, like many papers in the EITC literature that do not have access to administrative tax data, assumes 100% take-up of tax benefits to calculate the change in after-tax income produced by these tax policies (Hoynes, Miller and Simon, 2015). Take-up rates lower than 100% would mean that the true discontinuity would be lower than the discontinuity in Figure 2, so Figure 2 is best interpreted as an upper bound. While take-up is not 100%, it is still likely high. LaLumia, Sallee and Turner (2015) find that 85% to 90% of newborns born in late December are claimed on a tax return in the 2000s. To understand how different take-up patterns might affect the discontinuity in after-tax income, Appendix A describes an exercise that adjusts Figure 2 for a lower bound on the estimated discontinuity. This analysis suggests that the lower bound on the discontinuity in after-tax income is at most 10% to 20% lower than the upper bound recorded in Figure 2. The effect of this potentially lower discontinuity in after-tax income on later results is also discussed in further detail later and in Appendix A.

[7]The CTC was not partially refundable until tax year 2001. The CTC is partially refundable because it becomes refundable for tax filers with income over a certain threshold (Crandall-Hollick, 2016).

[8]A small share of households each year report no income, less than 5% across all years. These observations are included as a 0 percent change in after-tax income.

[9]This claim assumes that the permanent income of households is unaffected by the income shock. However, researchers have found examples where temporary income shocks result in long-term increases in earned income, presumably from parents seeking out better paying work (Black et al., 2014). This paper discusses this possibility later in the discussion section, and in Appendix B.

and deductions for a set number of years for a given child. Since parents of newborns born in December are eligible for tax credits and deductions a year earlier, then the parents of newborns born in January will be eligible for tax credits and deductions for one year later. For example, when children born in January turn 19, their parents are still eligible for the EITC for the previous tax year. Conversely, when children born in December turn 19, their parents will not be eligible for the EITC for that tax year.[10] So, the effect of having a child born in December as opposed to January of the next year is a speeding up of the tax credit and deduction process for that child.[11]

4 Birth Timing Patterns

Causal analysis of the effect of this change in after-tax income needs to account for the fact that parents and doctors have some degree of control over birth timing. Doctors may deliver children using Cesarian section (C-section) surgery (32% of all births in 2017) or by inducing labor through a variety of methods, including the use of drugs (26% of all births in 2017) (Martin et al., 2018). These delivery methods can be used to alter timing of birth.

There is clear evidence of this control over birth timing in the well-known fact that fewer births happen on weekends. As is clear in Figure 4, there are large dips in counts of births on Saturday and Sunday. This fall on the weekends reflects a decrease in C-section surgeries, but there is a smaller but still noticeable fall in vaginal births as well (Martin et al., 2010). Figure 4 also shows that mothers who give birth on the weekend have slightly lower education attainment. This data alone suggest that some parents, especially parents with slightly higher education attainment, exercise some degree of control over birth timing and have specific preferences over birth timing.

After regression adjusting for day of week in Figure 5 and taking an average of birth counts over 5 years, the distributions of births and the characteristics of births are much smoother.[12] However, there are

[10]Parents with full-time students living at home are able to claim their children for the EITC until their children turn 24, and parents with "permanently and totally disabled" children can claim the EITC at any age.

[11]If families have perfect foresight and perfect liquidity, then knowledge of this future change in after-tax income should attenuate the size of this discontinuity in current family income after accounting for discounting. Assuming a rate of return of 5%, then ability to borrow against future tax benefits may attenuate the current discontinuity by slightly over 40%. However, many of the lower income families with the largest after-tax increases in income are likely liquidity-constrained and hence less able to borrow against future income (Gross and Souleles, 2002). Additionally, evidence suggests that some share of families do not understand timing of how eligibility for tax benefits expires as children age (Feldman, Katuscak and Kawano, 2016). These complications likely mean that attenuation from discounting in the estimated discontinuity in family income is limited.

[12]For this regression adjustment, this paper estimates the following model:

$$Y^{birthcount} = \sum_{i=1}^{6} \beta_i \mathbb{1}[d = i] + \sum_{H} \sum_{i=-5}^{5} \beta_{i_H} \mathbb{1}[d_H = i] + \epsilon \tag{1}$$

where the first set of indicator variables $\mathbb{1}[d = i]$ are a set of six dummy variables (excluding Monday), and the second set of indicator variables $\mathbb{1}[d_H = i]$ are 11 dummy variables for each day within 5 days of each major holiday (indexed by H). The second set of dummy variables exclude from the estimation process all days around holidays, and the first set of dummy variables indicate the average births that are observed on a given day that differ from the births observed on Monday (the clear disruptions in the distribution of births, especially around major holidays (including New Year's Day, Christmas and July 4th).[13] Around these days, there are always fewer births on the holidays alone, and more births on the days around them. Similar to mothers who give birth on weekends, mothers with births that occur on holidays have slightly lower average years of education than mothers with births that do not occur on holidays. However, the average years of education return to previous levels quickly in the days around a holiday. Focusing in particular around New Year's, there is a drop in births on New Year's Day, and a slightly larger drop on Christmas Day, with larger counts of births occurring before and after these holidays. Interestingly, there are relatively few births after New Year's Day compared to before, suggesting that parents and their doctors with some level of control over birth timing are more likely to move births before the New Year compared to after. This pattern may be indicative of strategic timing of births to take advantage of tax benefits, but it also may reflect other preferences over birth timing, including concerns about hospital staffing. LaLumia, Sallee and Turner (2015) find limited evidence of specifically tax-related shifting in birth-timing around the New Year, with most tax-correlated shifting concentrated in a narrow window around the New Year.[14]

5 Methods

Evidence in the previous section suggests that the treatment of being born before New Year's Day is not random for some children, at least within a window of New Year's Day. However, the distribution of births outside of days around New Year's appears relatively smooth, save for other holidays. Intuitively, while parents can shift births in a specific region, they may have limited desire to do so further away, either because the costs of shifting are too high, or the benefits to shifting are too low. Appendix C develops microeconomic theory foundations to justify such a way of thinking, but this general intuition inspires a regression discontinuity strategy with an omitted region (sometimes referred to as a "doughnut regression discontinuity").

Specifically, this paper estimates the following model:

omitted category variable). Then, the regression adjusted counts of births would be:

$$\hat{Y}^{birthcount}_{adj} = Y^{birthcount} - \sum_{i=1}^{6} \hat{\beta}_i \mathbb{1}[d = i] \tag{2}$$

[13] Within individual years there are also spikes on Memorial Day, Thanksgiving Day, and Labor Day, but those spikes are not visible in this graph as this graph averages birth counts over 5 years. While New Year's Day, Christmas and July 4th are anchored to specific days in the calendar, Memorial Day, Thanksgiving Day, and Labor Day are not, so the disruptions that happen on these days are not visible when taking an average of birth counts.

[14] Furthermore, LaLumia, Sallee and Turner (2015) show compelling evidence that the correlation of after-tax income and birth timing may largely reflect income tax reporting responses rather than tax-motivated shifting. Note that this result differs from Dickert-Conlin and Chandra (1999), who use data from the PSID and conclude that parents with large potential tax benefits had a high probability of altering the timing of childbirth. LaLumia, Sallee and Turner (2015) show evidence that these patterns happen primarily in a narrow window around the New Year.

$$Y = \beta \mathbb{1}[d < 0] + \sum_{i=1}^{c} \gamma_i^1 d^i + \sum_{i=1}^{c} \Gamma_i d^i \mathbb{1}[d < 0] + \theta \boldsymbol{X} + \epsilon \tag{3}$$

Where Y is some outcome, d is the distance in days to the New Year's, c is the scale of polynomial in d, $\boldsymbol{X}$ is a list of additional covariates (specifically, state fixed effects and day of week fixed effects), and the estimation process includes days in some range $[\underline{D}, \bar{D}]$ but excludes observations in an omitted range of $[\underline{d}, \bar{d}]$. Note that β is the regression discontinuity estimate that reflects the estimated drop in outcome Y on New Year's Day, as on that day d is 0. We can conceptualize this estimate of β as the limit of the estimated means at either side of $d = 0$, even when some region of observations is omitted in the estimation process:

$$\beta = \lim_{\epsilon_1 \uparrow 0} \mathbb{E}[Y|d = 0 + \epsilon_1, X] - \lim_{\epsilon_2 \downarrow 0} \mathbb{E}[Y|d = 0 + \epsilon_2, X] \tag{4}$$

Following the recommendations in the theoretical and applied literatures regarding regression discontinuity estimation, this paper adds three more features to the estimation procedure. First, it uses local linear regressions where $c = 1$ (Hahn, Todd and der Klaauw, 2001). Second, it uses a triangle kernel that weighs observations more in the regression if they are closer to the discontinuity (Fan et al., 1996). Third, it uses a variety of bandwidth choices of $[\underline{D}, \bar{D}]$ to demonstrate sensitivity of the results to the region of observations included. Demonstrating how bandwidth affects these estimates more continuously pushes the limits of disclosure of restricted data from the Census Bureau.[15]

Before discussing the sufficient conditions this paper builds up to estimate β and the validation strategies suggested by those conditions, it is useful to first review the typical sufficient conditions that would apply in this setting if there were no omitted region. First, there must be no other treatment that coincides with the passing of the New Year. Second, as described by Lee and Lemieux (2010), the joint probability of observing various values of d conditional on X and ϵ, or $f(d|X, \epsilon)$, must be continuous in d. That is, for some given values of X and ϵ, the treatment as determined by the birthdate of a child is randomly determined.

To argue that this condition holds in normal settings without an omitted region, many researchers perform two tests to argue validity of the research design:

1. Test the null hypothesis that $f(X|d)$ is continuous by testing for discontinuous changes in variables at New Year's that should not be impacted by treatment.[16]

2. Test the null hypothesis that $f(d|X)$ is smooth at the threshold. A rejection of smoothness at the treat-

[15] There is a robust literature on optimal bandwidth selection in regression discontinuity designs (e.g. Imbens and Kalyanaraman, 2011) with the goal of minimizing expected mean squared error in estimated regression discontinuities. This paper splits the difference between the practical demands of disclosure and the theoretical recommendations by showing robustness to different choices of bandwidths.

[16] This test comes from the fact that applying Bayes' rule shows that $f(X, \epsilon|d) = f(d|X, \epsilon)\frac{f(X,\epsilon)}{f(d)}$.

ment threshold arguably indicates precise and hence non-random control over assignment to treatment (McCrary, 2008).

Without an omitted region, both of these traditional tests are violated in this paper. Figure 6 shows graphical evidence of a discontinuous change in average levels of mothers' education attainment from December 31st to January 1st. Average mother's education attainment is an untreated covariate that should evolve smoothly if the first test were met. Furthermore, there is clear strategic timing of births, with more births occurring around New Year's than on New Year's. If the second test were met, this distribution would be smooth.

With an omitted region, the treatment effect can be consistently estimated under four sufficient conditions. The first two are the same as before but the third and fourth are new. First, there must be no other treatment that coincides with the passing of the New Year. Second, $f(d|X,\epsilon)$ must be continuous in d. Third, the region of manipulated birth timing must be consistently identified and dropped from analysis. Fourth, the remaining data must be sufficient to consistently estimate and extrapolate means into the omitted region. Note that the fourth condition is stronger than the conditions from Lee and Lemieux (2010) discussed above. To see why this addition requirement is necessary, suppose that $f(d|X,\epsilon)$ is continuous, but the evolution of an outcome cannot be consistently extrapolated. Then, the evolution of the outcome may behave unpredictably in the omitted region, and the estimated discontinuity may be inconsistent.

To validate this set-up, note that, if the four conditions above are met, then the first test regarding covariate smoothness described before should still be applicable. Assuming the regression discontinuity specification is valid, there should be no discontinuities in variables that are not impacted by treatment. However, the second test is no longer applicable as a substantial share of the data is omitted, and extrapolating an estimated density into an omitted region rapidly loses power.

Using this estimation strategy depends on properly identifying the region of manipulated birth timing around the New Year. Currently, there is no standardized procedure researchers use to estimate this region. Many papers use ad hoc visual analyses of the size of the manipulated region (Barreca et al., 2011; Gauriot and Page, 2019; Almond and Doyle, 2011), but some papers suggest more regularized methods that are not applicable in this setting.[17]

This paper estimates an omitted region by applying data-driven techniques from a method widespread in the public economics bunching estimation literature (Chetty et al., 2011; Saez, 2010; Kleven and Waseem,

[17] Dahl, Loken and Mogstad (2014) are able to use other years where a treatment does not exist as a counterfactual to estimate the extent of the regions that are not manipulated. Hoxby and Bulman (2016) suggest a method of estimating the region using locally estimated density functions that estimate a counterfactual density. They then estimate the size of the bias in outcomes present due to sorting. In this setting, there is no counterfactual year for comparison as this discontinuity in after-tax income is always present at the New Year, and the nature of the selection process into treatment is not as clear as in Hoxby and Bulman (2016) for estimating bias.

2013). Bunching estimation papers look at situations similar to this paper where individuals alter a running variable to take advantage of some benefit tied to that running variable. The first steps of their technique estimate the length of the running variable affected by bunching. In this setting, those observations would be equivalent to the section of observations that see birth timing shifting. Thus, using this first step offers an estimate of the region of observations that should be omitted.

To apply this method, this paper uses the regression-adjusted counts of births by day from the 2000 Census for August 1989 to July 1994 graphed in Figure 6.[18] This paper follows a three step process to estimate the region of manipulated observations:

1. Choose an upper bound on the days that demonstrate shifted births ($\bar{d}$) and a lower bound ($\underline{d}$) and estimate:

$$Y_d^{birthcount} = \sum_i^c \gamma_i \cdot d^i + \sum_{i=\underline{d}}^{\bar{d}} \psi_i \cdot \mathbb{1}[d = i] + \epsilon \tag{5}$$

   Where the first term is a flexible polynomial of order c. Similar to Kleven and Waseem (2013), this paper uses $c = 5$, although the results are unchanged with higher order polynomials. The second term omits from the estimation process observations that fall between $\underline{d}$ and $\bar{d}$.

2. Calculate the counterfactual distribution of births implied by the estimates in step one for the days that were omitted from the estimation process in the region, $[\underline{d}, \bar{d}]$:

$$\hat{Y}_d^{birthcount} = \sum_i^c \hat{\gamma}_i \cdot d^i \tag{6}$$

   This counterfactual distribution of births represents the distribution of births that would be believed to exist in the absence of strategic timing of births.

3. Calculate the absolute value of the gap between the counterfactual distribution and the observed distribution of birth counts:

$$Gap_{\underline{d},\bar{d}} = \left| \sum_{\underline{d}}^{\bar{d}} \left[\hat{Y}_d^{birthcount} - Y_d^{birthcount} \right] \right| \tag{7}$$

4. Repeat this procedure over values of $\underline{d}$. Choose $\bar{d}$ visually (Kleven and Waseem, 2013), and choose the value of $\underline{d}$ that minimizes the gap.

[18] The process described here could be run for birth counts separately by year of birth, creating different omitted regions for different years of birth. This strategy would likely make the most sense with full count natality data, but given the need to weight population estimates in the Census, it seems less obvious how meaningful slight differences in birth counts are. Averaging over a number of years offers a simpler and less error-prone measure of birth counts by day.

Note that this choice ensures that the surplus births observed for the days before New Year's roughly equals the lost births that occur in the days on and after New Year's.[19]

Because the omitted region needs to be estimated, calculating proper standard errors for this setting means accounting for error introduced by the first step of estimating an omitted region. To do so, this paper bootstraps the estimation procedure in 2,000 replications, using a bootstrapped set of estimated cutoffs, and then applying these estimated cutoffs to bootstrapped data.

Under the four sufficient conditions described before, a regression discontinuity estimate would identify the reduced form effect of the income boost in infancy from being born before the New Year. However, researchers may be interested in converting this reduced form estimate into a direct estimated effect of income. One way to convert these estimated effects into a direct estimated effect of \$1,000 of income in infancy is to divide the reduced-form effect by the estimated change in income in Figure 2, and then multiply by 1,000. Letting α be the estimated increase in after-tax income, this Wald estimator would be:

$$\hat{W} = \frac{\hat{\beta}}{\hat{\alpha}} \tag{8}$$

This strategy is not as efficient as the two-sample two-stage least squares estimator, but that estimation procedure is not readily applicable as the first stage was not estimated using the same regression discontinuity design (Inoue and Solon, 2010).

The delta method shows that the variation of this estimate is approximately:

$$V(\hat{W}) \approx \frac{1}{\hat{\alpha}^2}\Big[V\big(\hat{\beta}\big) + \hat{W}^2 V\big(\hat{\alpha}\big) - 2\hat{W}Cov\big(\hat{\alpha}, \hat{\beta}\big)\Big] \tag{9}$$

Following Angrist and Krueger (1992), this paper assumes that $\hat{\beta}$ and $\hat{\alpha}$ are independent and hence the covariance term is 0.

These instrumental variables estimates should be interpreted with caution given that the increase in after-tax income, α, may be imprecisely estimated. As described in Section 3 above, the calculation in Figure 2 is not done with administrative tax data, and its estimation process is fundamentally different than the regression discontinuity estimation procedure for the reduced-form treatment effects.[20] Nevertheless, if

[19] In some respects, this estimation process ensures that the remaining data meet a smoothness condition similar to the second validity test described above. Omitting dates that demonstrate shifted births isolates attention to births that can be modeled with the counterfactual polynomial. This process effectively finds a region of births where the density of the running variable is smooth. Of course, the density estimation process here ensures that, by design, any estimated density created with this data is smooth, but the estimation process drops observations from the analysis would not fit that smoothness.

[20] Of particular concern is the fact that this figure assumes take-up of benefits is 100%. As described in Section 3 and Appendix A, take-up is likely less than 100% but still high, which means that Figure 2 is best interpreted as an upper bound on the size of the discontinuity in after-tax income. Appendix A describes an exercise that tries to account for these differences in take-up, and concludes that the lower bound on the estimated discontinuity in after-tax income is likely 10% to 20% lower than the upper bound. Thus, with better data to estimate the first stage, the instrumental variables estimate recorded in this

both α and β are consistently estimated, then W is also consistently estimated

5.1 Estimating the Omitted Region

Figure 6 shows results from the density estimation procedures described in equations 5, 6 and 7. The horizontal lines indicate the endpoints of the region of days the procedure suggests should be omitted. Following Kleven and Waseem (2013), 9 days after the New Year appears a good endpoint for the upper region of birth dates demonstrating manipulation in birth timing. The estimation process then calculates that the lower endpoint for the omitted region is 20 days before the New Year. More days are dropped in December than January due to disruptions in birth timing around Christmas. As births shifted away from the New Year cannot be distinguished from births shifted away from Christmas, the calculation process drops all days affected by birth shifting around both holidays. This magnitude of shifting, on the order of between one to two weeks before or after a major holiday (either New Year's or Christmas), is comparable with the birth timing shifting documented elsewhere. Other papers that look at changes in birth timing to qualify for either cash or program benefits tied to birth timing of children have found similar responses (Gans and Leigh, 2009; Neugart and Ohlsson, 2013; Dahl, Loken and Mogstad, 2014). As is clear visually, the density of births appears to return to a smooth distribution outside of these dates.[21]

6 Results

Having estimated the omitted region, the next step is to validate the research design. As mentioned in Section 5, one test for the validity of this design with this omitted region is to look for discontinuous differences in pre-treatment and untreated covariates. If the research design is valid, there should be no detectable differences except those observed at random. Table 1 shows the results from regression discontinuity estimates testing whether these untreated covariates for infants' families vary discontinuously.[22] All of these regression discontinuity estimates include state fixed effects, and day-of-week fixed effects. The variables analyzed include household and parent income, intensive and extensive parent labor force participation in the previous year, education attainment of parents, race of child, marital status of parents and household size.

paper may be up to 11% to 25% higher.

[21]A period of five days before and four days after Thanksgiving are also omitted from these density calculations. This omitted region was calculated using a similar process as the calculation around New Year's. This omission does not translate to a change in the average density depicted in Figure 6, as the timing of Thanksgiving (falling on the fourth Thursday in November) varies from year to year. The results estimating this estimated region are available on request.

[22]Although the results regarding outcomes for children below use pooled data from the 2001-2016 ACS and the 2000 Census, this section uses only the data from the 2000 Census for infants born 1999-2000. The Census data are better suited for looking at these questions than the ACS primarily because the 2000 Census asks for data about income types and levels in 1999 specifically, while the ACS ask about income in the "previous 12 months." This phrasing in the ACS means that, depending on the month in which families respond, they may post responses that reflect common changes in income and labor supply after birth of the newborn (Wingender and LaLumia, 2017). Hence, restricting attention to the cohort of children born 1999-2000 in the 2000 Census long form offers the cleanest test of whether characteristics differ for children born across the New Year.

11 out of 114 tests show significant discontinuities at the 5 percent level. This rejection rate is within the levels that would be expected with random sampling variation and independent tests if the null hypothesis of no discontinuous changes in characteristics were true. Additionally, as these tests are likely positively correlated, rates of rejection expected under this null hypothesis may be even higher. Lastly, it should be noted that most of the rejections take place within the smallest bandwidth, as when bandwidths of two months or more are used, three out of 76 tests are significant. All of the point estimates discussed below will use the two month bandwidth, although other results with different bandwidths will be discussed when relevant. Hence, these results with this omitted region meet the validation test implied by the research design.

6.1 Effect of Family Income in Infancy on Grade-for-Age Status in School

The next step is to use this after-tax income discontinuity to examine the impact of the income discontinuity on school outcomes. The primary school outcome observable in the Census and ACS data is grade-for-age status. A student being grade-for-age is often interpreted as a basic indication of that student achieving academic and social maturity in earlier grades. Table 2 reports all basic results for discontinuities in grade-for-age status by grade. Figures 8A through 8C and Figures 9A through 9D show graphical depiction of these regression discontinuities. As a reminder, all of these regression discontinuity estimates include state fixed effects and day-of-week fixed effects.

In the year that students are eligible for Kindergarten, Table 2 and Figure 8A show that enrollment in Kindergarten or a higher grade in the year of Kindergarten eligibility shows no discontinuity across the threshold. This result suggests that there is no detectable difference in parents delaying their child's entrance into Kindergarten across the New Year. These delays are often referred to as "red-shirting."

This lack of a discontinuity in Kindergarten attendance is important for contextualizing later results. This finding suggests that any subsequent detected discontinuities in grade-for-age status reflect students being retained in a grade and not Kindergarten red-shirting. It is difficult to interpret the meaningfulness of changes in grade-for-age status from red-shirting. The population of students who are red-shirted do not on average have lower cognitive skills and social maturity before they enter school than children who are not red-shirted (Bassok and Reardon, 2013).[23] In contrast, repeating a grade after entering school is usually interpreted as a negative signal about a student's social, emotional or academic readiness for the next grade. Students who are retained in a grade are more likely to have poorer academic performance prior to retention, lower social skills and poorer emotional adjustment. They also are more likely to display problem

[23] Researchers often interpret parents who red-shirt children as looking to gain an advantage for their child in school by having their child enter school slightly older than the rest of the children in their grade (Deming and Dynarski, 2008).

behaviors in class, including inattention and absenteeism (Xia and Kirby, 2009).[24] Thus, any subsequent detected changes in grade-for-age status in this setting are an indication of changes in the conditions that make students more likely to be retained within a grade.[25]

Figure 8A also shows an important pattern in the omitted region that is worth noting for all subsequent graphs in Figures 8 and 9. The students born right after the New Year appear to be slightly less likely to have entered Kindergarten on time than the students born right before. These data were excluded from the regression discontinuity estimation process for the reasons discussed earlier regarding strategic birth timing. This drop that happens right after the New Year likely reflects both the fact that students born after the New Year did not get the income boost, and the fact that these children are negatively selected compared to the children born before the New Year. As was discussed previously regarding Figure 5, these children born right after the New Year come from households where mothers have, on average, slightly lower education attainment.

As children enter first grade, Table 2 and Figure 8B show that a small gap opens up in the probability of a child being grade-for-age around the New Year, with students who experience the income shock being slightly more likely to be grade-for-age than students who do not. This gap is relatively small, at around half a percentage point, and not statistically distinguishable from 0. As Figure 7 shows, Kindergarten is one of the grades students are most likely to repeat, so a change in grade-for-age status around the New Year by this grade would not be surprising. It is worth noting that this result, unlike the other results discussed here, is relatively sensitive to the size of the omitted region. With a smaller omitted region, the gap is larger and statistically distinguishable from 0 (results available on request). These results offer suggestive evidence that a discontinuity has opened up in the share of students grade-for-age, but that discontinuity is relatively modest.[26]

These results are confirmed when looking at the share of students grade-for-age in 5th grade in Table 2 and Figure 8C. As before, there is a drop in the share of students grade-for-age among the students born right after the New Year, but the estimated discontinuity reported in Table 2 is close to 0. This small

[24]Note also that students who repeat grades are more likely to be children of color from less educated and less better-off households (Xia and Kirby, 2009) while red-shirted children tend to come from families with higher incomes and are more likely to be White (Bassok and Reardon, 2013).

[25]Retention policies differ across states, districts and schools, and the students that are retained in one location may not have been retained in another. As of 2018, 16 states have 3rd grade retention policies that require students to repeat a grade if those students have not reached some minimum threshold of achievement (Education Commission of the States, June 2018*b*). Even across school districts in the same state, rates of retention can vary French (2013), as do district policies and implementation of standards (Schwager et al., 1992). Thus, the meaningfulness of this outcome may differ from location to location, with some teachers in some states much more willing to use it as a tool than others.

[26]While repetition of Kindergarten may represent a type of red-shirting (Deming and Dynarski, 2008), it is worth noting that the characteristics of children who repeat Kindergarten are on average different than those of students who delay entrance into Kindergarten. As mentioned above, children who delay entrance into Kindergarten tend to be White and come from better-educated families with higher incomes than their peers who do not. The characteristics of children who repeat Kindergarten tend to be similar to the characteristics of students who are held back in grades; compared to their peers they are more likely to repeat later grades, have below-average school work, and be described by their teachers as having behavioral issues (National Center for Education Statistics, 2000).

discontinuity, coupled with the somewhat larger but still statistically insignificant discontinuity from first grade, suggest that there is at most only a modest change in the share of students grade-for-age across the New Year by this point.

Moving forward to 7th grade in Table 2 and Figure 9A, a larger detectable discontinuity has opened up in the share of students grade-for-age. The regression discontinuity estimate shows that students born before the New Year see a 1.05 percentage point increase in the probability of being grade-for-age. The increase in the discontinuity here makes sense, given that Figure 7 shows that there is a gradual increase in retention rates from 5th grade to 7th grade. As is clear from visual inspection of Figure 9A, this result appears somewhat sensitive to the upper bound of dates excluded, but this result is suggestive evidence of an eventual shift in grade-for-age status taking place. Table 2 also converts this reduced form impact into an instrumental variables estimate of the effect of $1,000 of income in infancy. These results show that $1,000 more in family income in infancy results in an 0.88 percentage point increase in the probability of a student being grade-for-age by 7th grade.

Lastly, looking at 9th, 10th and 11th grades in Table 2 and Figures 9B through 9C, the discontinuity in the share grade-for-age appears to eventually grow in magnitude. Although there is some variation in the estimated discontinuity in grade-for-age status, the estimated discontinuity is consistently positively signed and generally significant at the 5 percent level. Furthermore, the results depicted in Figures 9B through 9C appear to become less sensitive to the upper bound on dates omitted, unlike Figure 9A. Table 2 and Figure 9D show the average discontinuity in grade-for-age status using all high school years together. These results show that children born just before the New Year are approximately 1.13 percentage points more likely to be grade-for-age in high school. As the control mean for the share of students grade-for-age by high school is 87%, this is a meaningful shift in grade-for-age status.[27] Table 2 converts these reduced form results into a direct estimate of the effect of income, and shows that a $1,000 increase in income in the first year of life results in a 1.2 percentage point increase in the probability of a student being grade-for-age by high school.

While estimates of specific discontinuities are often noisy, the pattern of the evolution of the discontinuity across grades is worth noting. By 1st grade, a slight discontinuity that is statistically insignificant opens up, and by 5th grade the discontinuity is still indistinguishable from 0. While it is difficult to read much into this early pattern, it may be weak evidence of a small if undetectable gap beginning. The estimated

[27] Changes in grade-for-age status that occur in high school are harder to interpret than changes that happen in earlier grades. Retention in high school may reflect students failing to accumulate enough credits to advance their academic standing. Hence rather than being required to repeat an entire grade, as might be the case in earlier grades, such retention may reflect students being only required to repeat one specific course (West, 2012). However, two features are worth noting of this discontinuity. First, this sort of retention, while not necessitating an additional year of schooling, indicates that a student has not met certain benchmarks, and is hence meaningful in its own right. Second, the previous results show the discontinuity in grade-for-age status evolving over time, suggesting that the discontinuity in grade-for-age status in high school reflects changes that occur both in high school and in the grades beforehand.

discontinuity in grade-for-age status in 7th and 9th grade is larger, and in high school, it continues to grow. While these estimates are imprecise, they suggest a gradual increase over time in the size of the discontinuity, with perhaps the largest increases happening in grades where students are most likely to be retained.[28]

Heterogeneous Effects for Subgroups in Grade-for-Age Status Results

Tables 3 through 5 and Figure 10 break these results down further by showing how these results vary among subgroups. Here, for concision, the only grades analyzed are grades 5, 7 and then 9, 10 and 11 conjointly.[29]

Much of the previous research looking at the effects of income on outcomes for children has found non-linear impacts. Similarly sized increases in income in this research have often had larger effects for lower income families than higher income families. Ideally, to test for that non-linearity here, data would be available on the characteristics of families at birth so that families could be identified that have lower income at time of child's birth. However, without such information, identifying high impact samples depends on choosing information that retroactively could indicate high-impact groups. This paper uses two possible signifiers of high impact groups: Black students, and students with mothers who have a high school degree or less. Both of these groups likely have lower income at time of child's birth because they have lower average income throughout childhood (Tamborini, Kim and Sakamoto, 2015).

When comparing Black children with White children in Table 3 and Figure 10B, both White and Black children have virtually no detectable discontinuity in grade-for-age status in 5th grade. For the subsequent grades, both groups show some discontinuity in the share grade-for-age around the New Year. However, in 7th grade and high school, the estimated discontinuity shows a larger point estimate for Black children. By high school, for example, the estimated discontinuity in the share grade-for-age for Black children is 1.3 percentage points, while the estimated discontinuity for White children is one percentage point. Converting these reduced form estimates into a direct effect of income shows that a $1,000 increase in family income in infancy results in a one percentage point increase for White children in the probability of being grade-for-age by high school. For Black children, the same income shock results in a 1.6 percentage point increase in grade-for-age status. It should be noted, though, that the difference between the two is significant at the 10 percent level in 7th grade and insignificant in high school. However, these tests for differences in discontinuities between White and Black children are likely imprecise given the size of the omitted region and the smaller number of Black children compared to White children. In all, these results suggest that the

[28] The reasons that students are retained may differ by grade. In early grades, students are often retained on the basis of social and emotional immaturity (Xia and Kirby, 2009; Byrd and Weitzman, 1994), while in later grades retention is additionally correlated with other risk factors and grade-specific metrics of academic achievement (Peixoto et al., 2016).

[29] The use of data from high school grades conjointly is for precision. Results for individual grades are similar.

discontinuity is larger for Black children than White children, although the magnitude of the difference is unclear.

There are even stronger differences when comparing children born to mothers with different education attainment levels. The results in Table 4 and Figure 10C show that a large share of the estimated discontinuity in grade-for-age status in high school comes from effects on children with mothers who have lower education attainment. The discontinuity is a statistically insignificant 0.19 percentage points for children from mothers who have more than a high school degree, and 1.73 percentage points for children with mothers who have earned a high school degree or less. Furthermore, the difference between the two groups is significant at the 10 percent level among children in high school. Converting these results into a direct effect of income in Table 2 shows that $1,000 of income in infancy results in a 0.17 percentage point increase in grade-for-age status for children of more educated mothers. Among children of less educated mothers, the same increase of income in infancy results in a 2.05 percentage point increase in grade-for-age status in high school.

In general, these results show that the effect of $1,000 of income in infancy is larger for groups that are more likely to be disadvantaged at a child's birth. This result suggests that the impacts of this additional income are nonlinear, in that the benefits of increased income are stronger for families with comparatively lower incomes.

6.2 Robustness Checks on Grade-for-Age Status Results

Conditioning on State of Birth

This paper assigns Kindergarten age eligibility cutoffs to children depending on the state in which they were born, and these cutoffs determine what the grade-for-age status of a student is. However, the appropriate state eligibility rules that children face when entering Kindergarten would be those for the state the child lived in when the child was first eligible to enter Kindergarten at age 5. As information on state of residence at age 5 is not available retrospectively in this data, state of birth is an imperfect proxy, and some students may have misaligned grade-for-age status.

Students will have misaligned grade-for-age status if the grade they are expected to have completed to be grade-for-age is not correct.[30] For example, if this paper's metric of grade-for-age says that a student should

[30] Misalignment will only happen if the child's birthdate is between both the correctly and incorrectly assigned Kindergarten birthdate cutoffs. If the birthdate is after both of the cutoffs, or before, then the student would need to be in the same grade to be grade-for-age under both cutoffs, and grade-for-age status would be the same in both. Assuming that the child's birthdate is between both the correct and incorrect birthdate cutoffs, grade-for-age status is biased upwards if the incorrectly assigned birthdate cutoff is before the correct cutoff. For example, say a child is born in November in a state that had a Kindergarten age-eligibility cutoff of October 1st, and moved to a state at age 5 that had an age-eligibility cutoff of December 1st. The incorrectly assigned birthdate cutoff suggests that a student should be in a grade to be grade-for-age that is lower than the grade a student would actually need to be in if that student were grade-for-age. Thus, even if this student were retained once, this measure will mistakenly record that student as being grade-for-age. Conversely, grade-for-age status would be biased downwards if the incorrectly assigned birthdate cutoff is after the correct cutoff. For example, suppose a child is born in

have completed 8th grade to be grade-for-age, but the true grade that a student should have completed to be grade-for-age is 9th grade, then that misalignment may result in a student being improperly marked as being grade-for-age. In this setting, misaligned grade-for-age status will only bias the estimated discontinuity in grade-for-age status upwards.[31] Particularly concerning is the possibility that students may have moved from birth states to states or districts that have age-eligibility cutoffs for Kindergarten that coincide with January 1st or December 31st, as this misalignment could especially bias the estimated effect upward.

One test for bias is to further restrict the sample to children who are currently residing in the same state as their state of birth. Under the assumption that students living in their state of birth did not live in another state with different age eligibility rules at age 5, these students would have correctly assigned grade-for-age status. Table 5 shows that effects observed among this subsample are even larger than those observed in the full sample. Notably, the control mean of students who are grade-for-age here is lower than the full sample. This pattern makes sense, as the population of students who continue to reside in their state of birth is negatively selected. Families that do not engage in interstate migration are more likely to be less educated than families who do (Molloy, Smith and Wozniak, 2011), and previous results have already shown that effects of income on grade-for-age status are larger for less-educated families.

Thus, the findings discussed before are robust to whatever error is added from the misassignment of state of residence at age 5.

Separating Data by Birth Cohort

All of the preceding results have pooled together data across years for additional precision. However, as is clear in Figure 2, the size of the discontinuity in after-tax income has increased over time, so later birth cohorts see a larger discontinuity in after-tax income than earlier birth cohorts. Hence, an alternate way to use the data to explore the relationship between family income and outcomes for children is to compare the estimated discontinuity across different birth cohorts. If the relationship between after-tax income and grade-for-age status by high school is positive, then there should be increases in this discontinuity for later cohorts that saw a larger change in after-tax income for being born before the New Year.

November in a state that had a Kindergarten age-eligibility cutoff of December 1st, and moved to a state at age 5 that had an age-eligibility cutoff of October 1st. The incorrectly assigned birthdate cutoff suggests that a student should be in a grade to be grade-for-age that is higher than the grade a student would actually need to be in if that student were grade-for-age. Thus, even if this student never skipped a grade and was never retained, this measure will mistakenly that student as not-being grade-for-age.

[31] Data in the regression discontinuities is organized by school cohort. Consider the first example in the previous footnote, where the true Kindergarten eligibility age cutoff a child experienced was after the one assigned via birth state. This observation would be included in a cohort born before the New Year. As discussed in the previous footnote, that child's recorded grade-for-age status is likely biased upwards. However, the other child, who experienced a true age cutoff that was before the one assigned from the child's birth state, would not be included in a cohort before the New Year, as the first observations in that cohort would begin with the children born after the assigned birth state cutoff. Thus, misaligned grade-for-age status can only bias the estimated discontinuity upward.

Table 6 separates the sample of students in grades 9 through 11 into three different groups depending on year of birth: students born 1982-1986, 1987-1993, and 1994-2001. This combination of cohorts into years of birth reflects different eras of the EITC and CTC programs. As is clear in Figure 2, the average value of the discontinuity in after-tax income for having a child born before the New Year actually falls in real terms from 1982 to 1986, then begins rising from 1987 to 1993 following changes to the EITC, and then lastly increases substantially from 1994 to the early 2000s following further changes to the EITC and the introduction of the CTC.

Table 6 shows that an increase in the discontinuity in after-tax income by birth cohort happens alongside an increase in the estimated discontinuity in grade-for-age status by high school. Notably, the estimated discontinuity in grade-for-age status for being born before the New Year for the cohort born 1994-2001 is 60% larger than the estimated discontinuity for the cohort born 1982-1986. Since the only statistically significant change in grade-for-age status comes from the cohort of students born 1994-2001, the previous results that group all cohorts together are largely driven by children who were born in this later cohort when the EITC and CTC were most generous.

Note that this way of analyzing the data allows a check on the identifying assumption that no other treatments coincide with the passing of the New Year. If the previously observed results reflected some other treatment that occurred with the passing of the New Year, and if that other treatment remained constant, then the reduced form discontinuities in grade-for-age status across these birth cohorts should be constant. The stark differences across years is evidence that the previous results do not just reflect a constant New Year-specific treatment.

Interestingly, the direct effect of $1,000 on grade-for-age status by high school is relatively stable over time. Receiving $1,000 in infancy results in a 1.14 percentage point increase in grade-for-age status by high school for the 1982-1986 cohort, a 0.78 percentage point increase in the 1987-1993 cohort, and a 0.90 percentage point increase for the 1994-2001 cohort. As all these estimates have substantial standard errors on them, they are not distinguishable from each other. Hence, it is difficult to read too much into the specific pattern of results over time, but the similarity of results is suggestive.

6.3 Effect of Income in Infancy on Outcomes in Early Adulthood

When extending analysis beyond grade-for-age status in school, the context of the treatment changes. First, there is a second discontinuity in after-tax income that happens as a child ages into adulthood. As is clear in Figure 1, parents of children born in December see various tax benefits expire one tax year before parents of children born in January. Research shows that the size of those tax benefits at those ages has consequences for behavior of their families, including enrollment of children in college (Manoli and Turner, 2018) and parent labor force participation (Lippold, 2019).[32]

Second, when looking at outcomes other than grade-for-age status, it is important to remember that being retained in grade is both a potential indicator of that child's progression through school but also a form of mediation that may have long-term repercussions. Research suggests that the cumulative effects of not being grade-for-age are unclear and may differ depending on the age at which retention occurs. Researchers looking at red-shirting and retention in the early grades have found that these changes may result in short-term improvements in school achievement (Datar, 2006).

Researchers have analyzed policies where states retain students in grades depending on test scores. Some researchers have found no impacts or negative impacts of retention on short-term achievement in early grades (Roderick and Nagaoka, 2005) and increases in high school dropout rates that vary by grade of retention (Jacob and Lefgren, 2009). Other researchers have found positive short-term impacts of retention on achievement and no impact on eventual high school graduation (Schwerdt, West and Winters, 2017).[33] Thus while the initial income shock treatment in infancy is clear, other compensating responses happen subsequently that may complicate interpretation of effects in adulthood.

As the discontinuity in grade-for-age status was concentrated among more likely disadvantaged households, discontinuities in outcomes in early adulthood are likely concentrated in these groups as well. However, as children age into young adulthood, many move away from their parents. Consequently, it is harder to identify children who grew up in likely disadvantaged households as they get older. This paper uses two strategies to identify these groups. First, this paper looks at outcomes among Black children. While Black children did not display consistently statistically different results in grade-for-age discontinuities than White children, Black children had larger point estimates of changes in grade-for-age status. Second, this paper looks at outcomes for children born in counties that have average mother's education attainment in the bottom quarter of the education distribution (weighted by population). Mother's education levels were a strong predictor of the discontinuity described previously, but no parent education attainment variables are observable for young adults no longer living at home. Hence, conditioning on education attainment levels in county of birth is a proxy for this group of individuals.

For relevant later life outcomes, this paper looks at high school completion rates, earned income, labor force participation, and SNAP receipt from ages 19 to 32 for children born in 1980 forward.[34] Additionally,

[32] These later discontinuities in after-tax income are likely small, as the share of families that claim EITC benefits for newborns is much larger than the share of families that claim EITC benefits for older children. Appendix B discusses these patterns in more detail.

[33] The difference in these results highlights the fact that the effects of retention likely depend on other interventions related to retention.

[34] Age 18 is excluded here. Given the way the sample is constructed, young adults aged 18 are expected to have completed high school if they graduated on time. By definition, the previously estimated discontinuities in grade-for-age status ensure

as these outcomes have more variation than the previous analysis of grade-for-age status, this paper follows Kling, Liebman and Katz (2007) in combining these four measures of outcomes into a single unitary measure of economic sufficiency. This single measure allows more power in measuring effects that move in the same positive direction. To compute this measure, this paper normalizes each outcome into a z-score and adds the four z-scores with signs reflecting whether the outcome is beneficial (positive for labor force participation, earned income, and high school attainment, and negative for SNAP receipt). The normalizing mean and standard deviation for each of the z-scores come from outcomes for adults born in the month and a half after the New Year, excluding the omitted region.

Figures 11A through 11C show some of the basic variation in post-high school outcomes by age of adults. These figures show average outcomes for children born in December and January, excluding children born in the region around the New Year who are omitted in this paper. As such, they only demonstrate the underlying variation in outcomes and are not meant to be interpreted as causal impacts. As is clear, there is little detectable difference in high school graduation rates, nor in labor force attachment in the population as a whole between people born in January and December. However, there is a slightly more persistent gap in earnings, with adults born right before the New Year often earning slightly more than adults born right after the New Year. While these gaps are within the margin of error for most years, the gap varies from about \$50 to \$500 depending on the year. Importantly, the gap seems to attenuate or disappear in later years.

Figure 12A combines all four measures into a unitary measure of economic self-sufficiency for all adults. Note that, by construction, this measure has average value 0 for people born in January, but there is still a standard error on the estimate as it is an average and has sampling variation. Figure 12A shows that, while there is a gap of 0.04 to 0.01 standard deviations in the self-sufficiency measure in the early years, the gap disappears over time. Figures 12B and 12C show similar graphs for Black young adults and adults born in counties with comparatively low education attainment. The composite measure is recalibrated for these samples such that the measure again has average value 0 for people born in January within this subgroup. Here, the patterns are much noisier given the smaller sample sizes, but similarly the gap varies from 0.09 to 0.01 standard deviations, and attenuates over time to low numbers by the time adults reach their late 20s and early 30s.

To formalize these comparisons, Table 7 computes regression discontinuities over the conjoint measure of economic self-sufficiency and each of the four outcomes separately for the full sample. Figure 13A shows

that high school graduation rates at age 18 would be different. Young adults aged 19, on the other hand would be expected to have completed high school if they graduated either on time or one year later. The results look at individuals born 1980 and later for reasons discussed earlier in the data section. This sample restriction ensures that outcomes for adults are analyzed for cohorts for which there is data from the previous section showing changes in grade-for-age status. Age 32 is an arbitrary ending age reflecting the fact that data get sparse for later ages in the 2001 to 2016 ACS when looking at adults born 1980 and later.

results for discontinuities in the self-sufficiency measure. Given the small differences observed in Figures 11 and 12A, it is useful to compile different ages into bins to increase precision. While the exact grouping of the bins can be somewhat arbitrary, this paper computes discontinuities for adults aged 19-22, 23-27 and 28-32 to demonstrate how patterns evolve over time. As is clear in Figure 12A, however, there are individual outliers within these age groups that can be important for driving measured effects, so it is worthwhile to be cautious in interpreting any one given result.

Table 7 and Figure 13A show that adults aged 19-22 who experience the higher income in infancy see an estimated increase in their self-sufficiency measure of approximately 0.02 standard deviations. Converting this discontinuity into a direct effect of income, $1,000 in infancy results in a 0.03 standard deviation increase in the self-sufficiency measure. However, this gap has a wide standard error, so it is not statistically distinguishable from 0 at the 10 percent confidence level. Looking at the individual components, Table 7 shows that adults who experienced the income boost as children are an estimated 0.1 percentage points more likely to have completed high school off a baseline rate of 90%, and earn an estimated $8 more annually. Neither of these effects are distinguishable from 0 at the 10 percent level.

Moving to ages 23-27, young adults who experience the higher income in infancy see an estimated drop in their self-sufficiency measure of 0.02 standard deviations, again not statistically distinguishable from 0 at the 10 percent level. Converting to a direct effect of income, $1,000 in infancy results in a 0.02 standard deviation drop in the sufficiency measure. Table 7 shows that adults who experienced the higher income are an estimated 0.002 percentage points more likely to have completed high school, and earn an estimated $280 less annually, but again neither of these effects are distinguishable from 0.

Lastly, looking at ages 28-32, the estimated fall in the self-sufficiency measure for adults who experience the income boost is still -0.02 standard deviations, again not distinguishable from 0 at the 10 percent confidence level. Similarly, the direct effect of $1,000 of income is -0.03 standard deviations. The adults who experienced the income shock are an estimated 0.4 percentage points less likely to have completed high school, and estimated to earn $2 less annually than adults who did not experience the income increase as infants, but again neither of these effects are distinguishable from 0.

Taking these point estimates at face value, like Figure 12, they suggest a weak treatment effect in early adulthood that falls over time as young adults age into their mid to late 20s, although strictly speaking no effects are distinguishable from 0.

Heterogeneous Effects by Subgroups on Outcomes in Early Adulthood

Table 8 computes regression discontinuities for White and Black young adults separately. The table only reports discontinuities in the conjoint measure of self-sufficiency for concision. As most of these individual discontinuities are noisy, they should be interpreted with caution, but the high school graduation status and earned income discontinuities are referenced here for context.

White young adults who experienced the income boost as infants display a small estimated treatment in their self-sufficiency measure in ages 19-22 of 0.009 standard deviations. However, Black young adults display a much larger estimated treatment effect of 0.134 standard deviations. Both estimates are not distinguishable from 0 at the 10 percent level, but they are distinguishable from each other at the 10 percent level. Converting these reduced form results into a direct effect of income suggests that White young adults see a 0.02 standard deviation increase in their economic self-sufficiency score from $1,000 in infancy. Black young adults see a 0.18 standard deviation increase from the same sized shock. This increase in the composite score for Black young adults comes from increases in high school graduation rates. Black young adults who experienced the income boost are 2 percentage points more likely to have completed high school off a baseline high school graduation rate of 81%. While this is a large effect and distinguishable from 0 at the 1 percent level, it still has a wide standard error on it, and the effect is not sustained into later ages, so it should be interpreted with caution. Black young adults also earn $18 more annually off a mean of $6,007, but again this effect is not distinguishable from 0 at the 10 percent level.

Moving to young adults aged 23-27, White young adults who experienced the income shock display a treatment effect of -0.03 standard deviations in their self-sufficiency measure while Black young adults display a treatment effect of 0.11 standard deviations. Both estimates are not distinguishable from 0, and they are not distinguishable from each other at the 10 percent level. Converting these results into a causal effect of income suggests that a $1,000 increase in income in infancy for White children results in a decrease in their self sufficiency score of 0.05 standard deviations. For Black young adults, the same sized income shock increases their self-sufficiency score of 0.18 standard deviations. These effects among Black adults come from changes in labor force participation and earnings. Black young adults who experience the income boost are 0.5 percentage points more likely to have graduated high school off a baseline rate of 83.8%, 2 percentage points more likely to be in the labor force off a baseline rate of 69%, and earn $700 more annually off a baseline mean of $13,200. However, again, none of these effects are distinguishable from 0 at the 10 percent level.

Note that when combining all young adults aged 19-27, the estimated treatment effect for White young adults is -0.005 standard deviations in their self-sufficiency measure. However, the estimated treatment effect for Black young adults is 0.12 standard deviations. The increase for Black young adults is statistically distinguishable from 0 at the 10 percent confidence level, and distinguishable from the treatment effect for Whites at the 5 percent level. Converting these reduced form results into a direct effect of income shows that White young adults who experienced $1,000 in after-tax income in infancy see a 0.01 standard deviation drop in their self-sufficiency score. Black young adults who experienced the same income shock see a 0.19 standard deviation increase in their self-sufficiency score.

Lastly, looking at young adults aged 28-32, the treatment effect for Whites is -0.03 standard deviations in their self-sufficiency score, and the treatment effect for Black young adults is to 0.03 standard deviations. These effects are not statistically distinguishable from 0, or distinguishable from each other at the 10 percent level. Converting to direct effects, these estimates say that for a $1,000 shock in income in infancy, White adults see a 0.02 standard deviation drop in outcomes, but Black young adults see a 0.07 standard deviation increase. Black young adults who experience the income boost are 0.6 percentage points less likely to have graduated high school off a baseline rate of 86.1%, and earn $1,227 less annually off a baseline mean of $20,500. None of these effects are distinguishable from 0 at the 10 percent level.

Overall, the treatment effects are larger for Black young adults than White young adults. Furthermore, observed treatment effects for Black young adults follow the pattern established earlier in the sample as a whole, where estimated treatment effects are largest in earlier years and appear to attenuate with time. The pattern of results here is likely more suggestive than the previous results looking at grade-for-age status. The previous results showed that White children saw an increase in the probability of being grade-for-age if they experienced the income shock as children. Taken at face value, however, some of these estimated coefficients on post-schooling outcomes for Whites suggest negative treatment effects, which would be odd given the positive effects seen on grade-for-age status earlier. The noisiness of these estimates likely reflects the fact that there is more variation in these outcomes than in the previous grade-for-age analysis. Also, the sample sizes become much smaller when looking at older adults. Ultimately, what seems more instructive is that Black adults display consistently larger estimated treatment effects, and some of these treatment effects are statistically distinguishable from 0 and distinguishable from estimated treatment effects for Whites.

Table 9 offers a similar exercise for young adults born in counties with average mothers' education attainment above and below the lowest quartile. Again, most of these individual discontinuities are noisy, but the high school graduation status and earned income discontinuities are referenced for context.

When looking at young adults aged 19-22, the estimated discontinuity in the self-sufficiency score for young adults born in counties with high average mothers' education attainment is 0.02 standard deviations. The estimated discontinuity for young adults born in counties with low education attainment is 0.05 standard deviations. Converting these discontinuities into a direct effect of income, young adults from counties with higher education attainment see an 0.02 standard deviation increase in their self-sufficiency score from a $1,000 shock to income in infancy. Young adults from counties with lower education attainment see an 0.06 standard deviation increase in the score from the same shock. These estimates are not statistically distinguishable from 0, or from each other at the 10 percent level. Young adults in counties with low education attainment who experience the income increase see an increase in $68 in earned income off a baseline mean of $9,074 and an 0.3 percentage point increase in the probability of having graduated high school off a baseline mean of 87.9%. None of these effects are distinguishable from 0 at the 10 percent level.

Larger effects appear when looking at young adults aged 23-27. The estimated treatment effect for young adults born in counties with high mothers' education attainment is -0.02 standard deviations, but the estimated treatment effect for young adults born in counties with low mothers' education attainment is 0.09 standard deviations. Note that these treatment effects are statistically distinguishable at the 10 percent level in the widest bandwidth. Converting these estimates into a direct effect, adults born in counties with high education attainment see a 0.04 standard deviation increase in their self-sufficiency score from a $1,000 income shock, but adults from counties with high education attainment saw a 0.12 standard deviation decrease. The young adults from counties with low education attainment who experience the income increase see a 1.0 percentage point increase in the probability of graduating high school off a baseline mean of 88.7%, and an increase of annual earned income in $679 off a baseline mean of $19,280, although again none of these effects are distinguishable from 0 at the 10 percent level.

When combining all young adults aged 19-27, the estimated treatment effect is -0.003 standard deviations for adults born in counties with higher average mothers' education attainment, and 0.07 standard deviations for adults born in counties with lower average mother's education attainment. Converting to a direct effect, adults born in counties with higher education attainment see an 0.016 standard deviation increase in their self-sufficiency score from $1,000 in income in infancy. adults born in counties with lower education attainment saw an 0.098 standard deviation decrease from the same shock.

Finally, looking at adults aged 28-32, the estimated treatment effect is -0.01 standard deviations for adults born in counties with higher average mothers' education attainment and -0.12 standard deviations for adults born in counties with lower average mothers' education attainment. Converting both in to direct effects, adults born in counties with higher average mothers' education attainment saw an 0.012 standard deviation increase in their self-sufficiency score from a $1,000 shock in infancy, but adults born in counties with lower education attainment saw a 0.20 standard deviation decrease. Young adults from counties with low education attainment who experience the income increase see a 1.4 percentage point decrease in the probability of having graduated high school off of a control mean of 90.5% and a $150 decrease in annual earned income off of a control mean of $29,120. Neither of these effects are distinguishable from 0.

These long-term effects tell a consistent story: while effects of the income increase in infancy seem to persist in terms of impacts on education attainment and earnings after turning 19, these impacts apparently attenuate with time as students age into their late 20s and early 30s. Again, as before, estimated effects are largest for groups that likely had lower average income at birth, specifically Black adults and adults born in counties with lower average education attainment. It is possible that the lower effects measured here at later ages reflect the fact that the cohorts analyzed in these regressions would have been born in the early 1980s when take-up of tax benefits may have been lower, and the size of the first stage jump in after-tax income in infancy more inconsistent. Future research will need to follow the current cohorts of graduates to see if their effects are similar to the effects measured here.

7 Discussion

The effects found in this research show a relationship between income in infancy and educational outcomes while in school. These estimated effects appear to persist as differences in income, education attainment and labor force attachment into early adulthood for at least some subgroups. It is difficult to directly relate these findings to other estimates. Few other papers have used such a specific, sharply defined, and relatively modest change of income in the first year of a child's life. However, some comparisons are possible to other research on the effect of family income on child outcomes.

First, the results here suggest a non-linear relationship between family income and student achievement that has been found in other settings from changes in permanent income. The effect of an additional $1,000 in infancy on outcomes is largest for groups that likely had lower average earnings in the first year of a child's life, including Black children and children with mothers with lower education attainment. Similarly, Loken, Mogstad and Wiswall (2012) and Akee et al. (2010) find that changes in permanent family income for lower-income families have the largest impacts on outcomes for children in school and in early adulthood.

Second, this paper suggests that a $1,000 change in family income in infancy results in changes in school performance, and other papers find that similarly-sized income shocks later in a child's life also have effects on school performance. Both Chetty, Friedman and Rockoff (2011) and Dahl and Lochner (2012) find that $1,000 of contemporaneous income results in a 0.06 to 0.09 standard deviation rise in contemporaneous test scores. Black et al. (2014) find that a $1,000 income shock at age 5 results in a 0.1 to 0.6 standard deviation increase in test scores at age 15. These papers do not consider grade-for-age status, likely because there is less year-to-year variation in that measure compared to test scores. However, such changes in tests scores, especially if they happen in the lower part of the test score distribution, may have non-trivial impacts on retention. Data from Florida on test scores and retention patterns suggest that a 0.06 to 0.09 standard deviation change in test scores correlates to a reduction in the probability of students being retained in grade 4 by 0.6 to 0.8 percentage points.[35] While this relationship from the Florida data is not causal, it is

[35] This estimate comes from the evidence reported in Schwerdt, West and Winters (2017). In Figure 2A of their paper, the authors offer average retention rates by test scores. In Appendix Figure A-2 the authors show the distribution of test scores. Shifting the distribution of test scores in the lower regions up by 0.06 to 0.09 standard deviations produces the 0.6 to 0.8

suggestive that changes in test scores from a $1,000 change in after-tax income may result in similar effects on retention as those measured in this paper.

Third, this paper finds that a $1,000 change in income in infancy results in modest long-term changes in outcomes in adulthood, and other papers show a similar relationship. Chetty, Friedman and Rockoff (2011) provide a method of linking changes in test scores to changes in future earnings. They then use these estimates to convert the impact of $1,000 in after-tax income in childhood on test scores into the impact of the income shock on later life earnings of adults. Using this method, they conclude that a $1,000 increase in after-tax income when children are in later primary and high school grades results in a 0.38 to 0.57 percentage point increase in earnings as adults. Similar sized effects are present in this paper from an income shock in infancy for some subgroups. The point estimates in this paper show that a $1,000 increase in income in infancy results in a 0.56 percentage point increase in earned income for Black young adults from ages 20-30. The same income shock results in a 0.60 percentage point increase in earned income for young adults born in counties with low average education attainment. Both estimates, it should be noted, are not distinguishable from 0 at the 10 percent level, and there are minimal effects in the population at large. However, the fact that these point estimates are within similar ranges as Chetty, Friedman and Rockoff (2011) is suggestive.

However, while the pattern of results in this paper fit within the pre-existing literature, the magnitudes of these estimated effects are often near or above the upper bound of previous estimates of impacts. Arguably, the larger relationships found here reflect the fact that this paper looks at the effect of family income in infancy, while other papers primarily focus on shocks to income that happen later in a child's life. To think about the context for this difference, it is necessary to look more broadly at the literature on experiences in childhood and later life outcomes.

A wide array of research in social science suggests that family conditions in infancy and early childhood are particularly consequential for patterns of long-term development for children. First, gaps in measured cognitive and non-cognitive abilities between children open up at early ages and are observable clearly before students enter school (Loeb and Bassok, 2007; Cunha and Heckman, 2007). Similar gaps open up in many measures of child health (Figlio et al., 2014; Case, Lubotsky and Paxson, 2002; Currie and Almond, 2011). These gaps are highly correlated with family economic resources. Second, a literature in biology suggests the existence of critical periods for development where inputs are especially important for later life outcomes (Reviewed in Cunha et al. (2006)). Lastly, research shows that some policy interventions that affect the resources available to low-income families can have both short-term consequences (Hoynes, Miller and Simon, 2015; Almond, Hoynes and Schanzenbach, 2011; Rossin-Slater, 2013) and long-term consequences for outcomes for children (Black et al., 2014; Hoynes, Schanzenbach and Almond, 2016; Aizer et al., 2016;

percentage point reduction in retention. Baseline retention rate in this data among all students is 1.87%.

Milligan and Stabile, 2009). Those papers find effects across health, cognitive skills, non-cognitive skills, and other metrics of child development. Thus, it would not be surprising that an income shock in infancy would relate to multi-faceted improvements in outcomes for children that may have different long-term effects than income shocks later in life.

The literature on the effects of family conditions in infancy and early childhood on later life outcomes offers a few clues as to potential mechanisms. Disadvantaged families with infants are likely to be income constrained. Over the sample period included here, around 50% of black newborns and 35% of newborns in families where the mother has a high school degree or less are in poverty. By the time those children turn 15, the shares of those families in poverty drop to 40% and 23% respectively. Releasing that constraint may have two effects of families.

First, changes in income of these families in infancy might have significant impacts on consumption patterns. Differences in income between families correlate to differences in spending patterns on children (Caucutt, Lochner and Park, 2017). Research shows that changes in income from tax credits result in changes in spending on resources that might affect child development (McGranahan and Schanzenbach, 2013). Even if parents do not spend the money directly on their children, they may spend it on goods that increase the family's earnings over time. For example, research suggests that EITC recipients use the increase in their after-tax income from the EITC to pay down debt and spend on transportation (Goodman-Bacon and McGranahan, 2008; Mendenhall et al., 2012).[36] To the degree that these spending patterns might enable slightly higher labor force attachment in subsequent years, such patterns may increase the family's permanent income (Ramnath and Tong, 2017; Black et al., 2014).

Second, even if consumption patterns on children and permanent income are unaffected, the simple act of loosening the family's budget constraint may have impacts on how parents interact with their children. Research has found that parental stress, parental depression, and martial conflict are all highly correlated with family income, and in turn correlated with adverse outcomes for children (Wadsworth et al., 2005; Conger et al., 1994; Gershoff et al., 2007). Thus, even small changes in the economic resources of families can have consequences for important early life experiences of children, either through changes in consumption patterns, changes in permanent income, or changes in the family environment.

Finally, note that the experiment created by the income variation in this paper has interesting consequences for policy. First, the results suggest that shifting the eligibility for child-related deductions and credits a year earlier would improve students' achievement in school. Second, the results also suggest that shifting eligibility for these tax benefits forward while removing eligibility for an additional year in adoles-

[36] This research looks at spending of these recipients on average and does not specifically look at spending of parents with newborns.

cence may improve some outcomes in adulthood. Families with children born in January are eligible for an additional year of tax benefits after children born in December are no longer eligible. But, adults born in December, especially from groups that were more likely disadvantaged at birth, still see an increase in the self-sufficiency score as adults from the income shock in infancy. Thus, the benefits that children born in January receive from that additional year of eligibility do not undo the benefits that children born in December received from that year of eligibility as infants. The cost of implementing such a policy would simply come from altering children's age of eligibility.[37]

A full cost-benefit analysis of the effects of shifting the eligibility timeline forward is beyond the scope of this paper. Such a calculation would require taking into account all the benefits that researchers have from that additional year of eligibility (e.g. including increased college enrollment (Manoli and Turner, 2018). However, these results are suggestive that benefits geared towards families with younger children may have lasting repercussions in ways that benefits aimed at families with older children do not. Most transfer programs, including SNAP and the tax credits analyzed in this paper, do not change benefit levels in ways that relate to a child's age.[38] But, the natural experiment created by this setting suggests that increasing these transfers to families with young children may offer a cost-effective reform that would improve outcomes for children and adults.

8 Conclusion

This paper demonstrates compelling effects of family income in infancy on outcomes in childhood and early adulthood. Specifically, this paper shows that a \$1,000 change in family income in infancy results in a 1.2 percentage point increase in the probability of a student being grade-for-age in high school. These results are driven by large treatment effects for children likely disadvantaged in infancy, specifically Black children and children from families with low education attainment. Small but suggestive effects on adult outcomes in earnings, labor force attachment, high school graduation status and SNAP usage persist into early adulthood, in particular among Black young adults, and adults from counties with low education attainment. As the effects of an additional \$1,000 in infancy are largest for children from these likely disadvantaged groups, they suggest a non-linear relationship between changes in income and changes in child outcomes.

These results are on the upper end of estimated relationships between family income and outcomes for children. However, they fit in line with a broad literature suggesting that changes in family economic

[37] As discussed in Appendix B, the share of families that receive EITC benefits for older children is substantially lower than the share of families who receive them for newborns. So, altering the age of eligibility would also result in an increase in receipt of EITC benefits, and hence additional costs. For more on these points, turn to Appendix B.

[38] A clear exception is the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) Program which is aimed at parents with infants and children up to age 5.

resources in infancy may have substantial long-term impacts on outcomes for children. This increase in income could affect children's outcomes through changing family spending patterns, improving future family earnings, or changing the home life circumstances that young children face early in life.

Furthermore, it is notable that these results come from altering timing of receipt of tax benefits from adolescence to infancy. These results may indicate that transfer programs focused on families with very young children may result in larger effects on child and adult outcomes than transfer programs aimed at families with older children. More broadly, these results suggest that altering transfer programs to be more child age-specific may a fruitful and low-cost avenue for policy reform.

In all, these results suggest that changing the resources available to low-income families can result in long-term improvements for their children. Directions for future research in this project include examining effects on siblings, and investigation into mechanisms of effects in consumption data.

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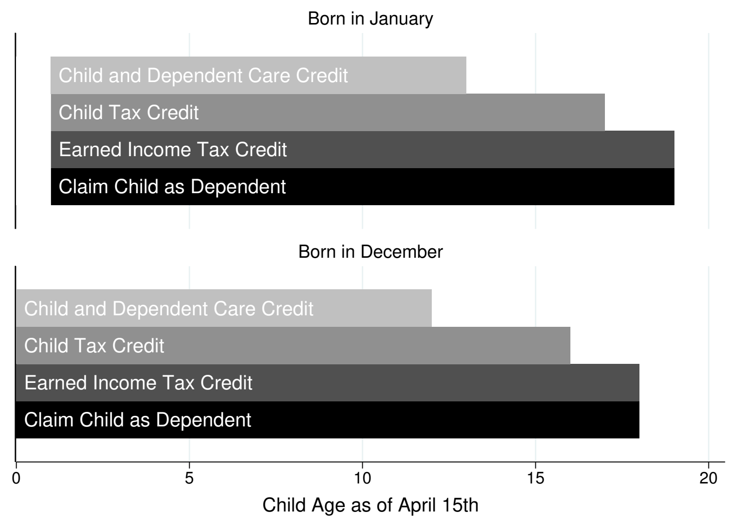
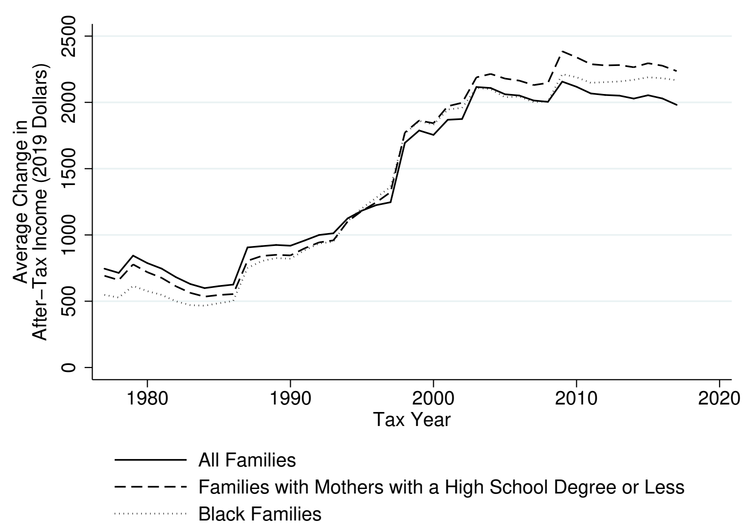
9 Figures and Tables

Figure 1: Family Eligibility for Child Tax Benefits for Children Born in December and January by Age



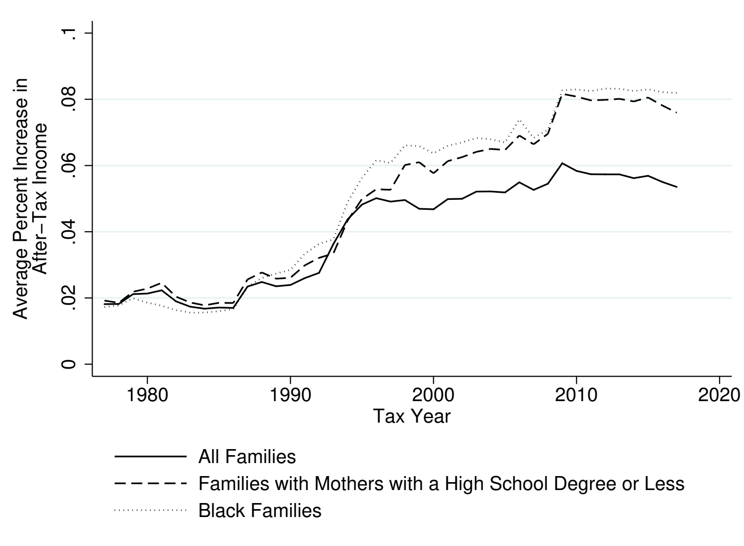
Notes: Figure depicts eligibility for tax benefits by child age and birth month. The age variable on the horizontal axis lists age as would be recorded by a family on April 15th. For example, newborns in their first year of life born in January and December would be age 0 by April 15th.

Figure 2: Change in Family After-Tax Income in Infant's First Year of Life from Birth in December Compared to January



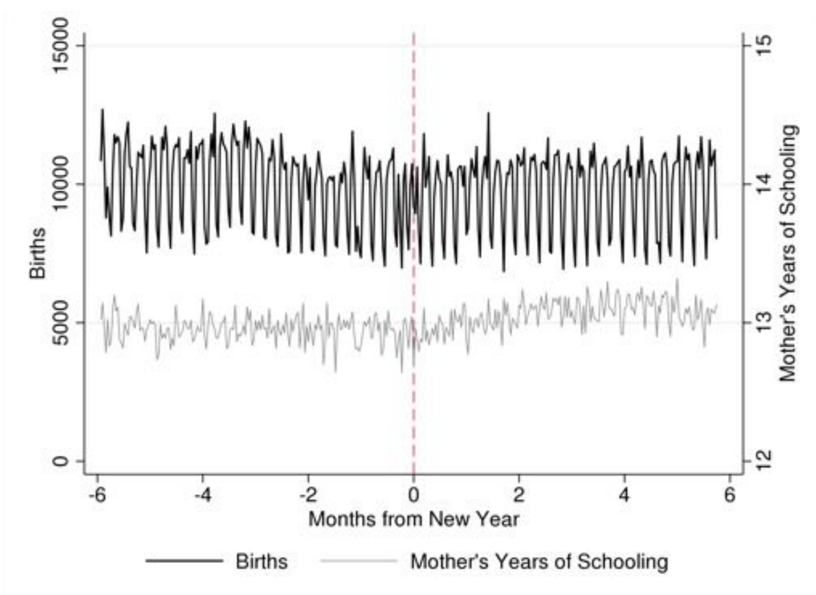
Notes: Figure depicts average estimated difference in family after-tax income in the first year of a child's life for families that have a child born in December compared to January of the next year. Incomes measured in 2019 dollars. Year variable on horizontal axis records tax year of birth. For example, the difference reported for tax year 1986 measures the difference in after-tax income in tax year 1986 for having a child born in December 1986 compared to January 1987. Estimation process draws inspiration from Hoynes, Miller and Simon (2015) and uses the March CPS. Additional details on estimation are in the text and in Appendix A. Standard error bars omitted here for clarity.

Figure 3: Percent Increase in Family After-Tax Income in Infant's First Year of Life from Birth in December Compared to January



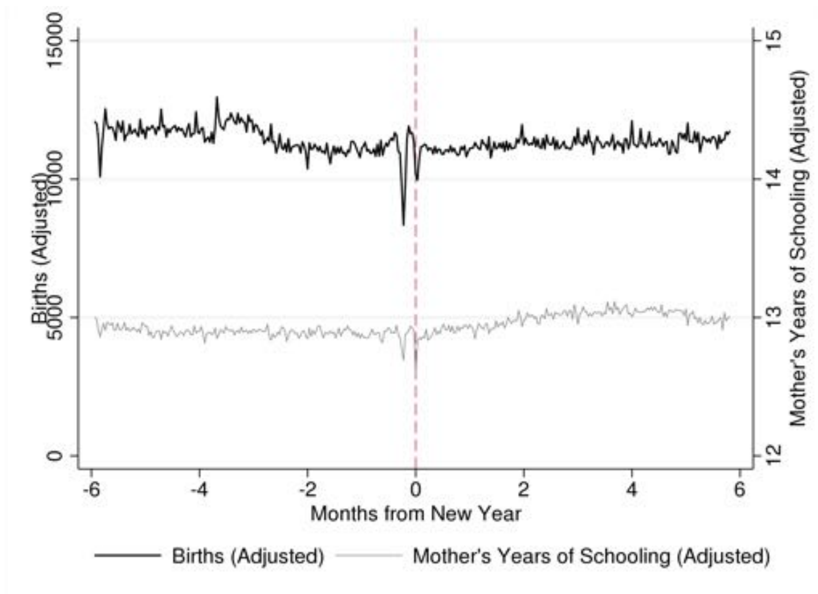
Notes: Figure depicts average percent increase in after-tax family income in the first year of a child's life for families that have a child born in December compared to January of the next year. Year variable on horizontal axis records tax year of birth. For example, the difference reported for tax year 1986 measures the difference in after-tax income in tax year 1986 for having a child born in December 1986 compared to January 1987. Same estimation process as described in Figure 2, the main text, and Appendix A. Standard error bars omitted for clarity, but standard errors are less than 0.2 percentage points for all groups and all years.

Figure 4: Births by Day of Year - 1996 to 1997



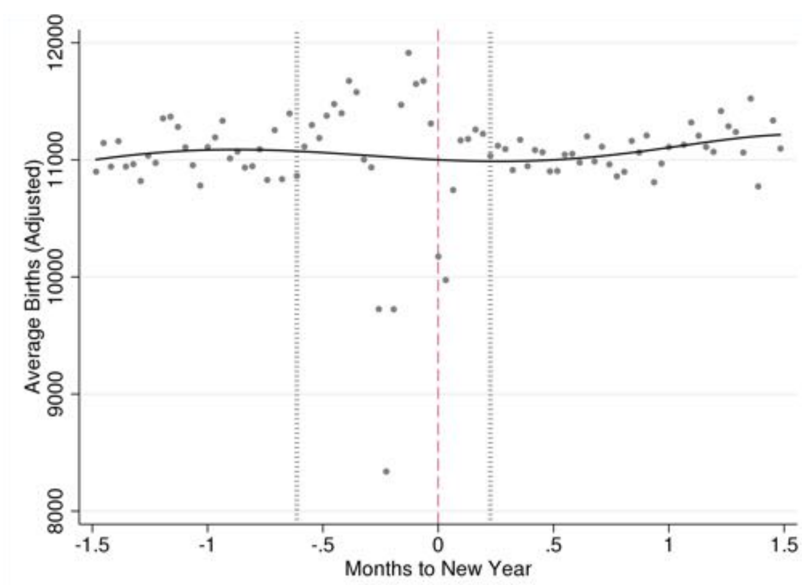
Notes: Figure depicts birth counts by day of year estimated in the 2000 Census from July 1st 1996 to June 30th 1997, centered on the New Year in 1997.

Figure 5: Births by Day of Year Adjusted by Day of Week



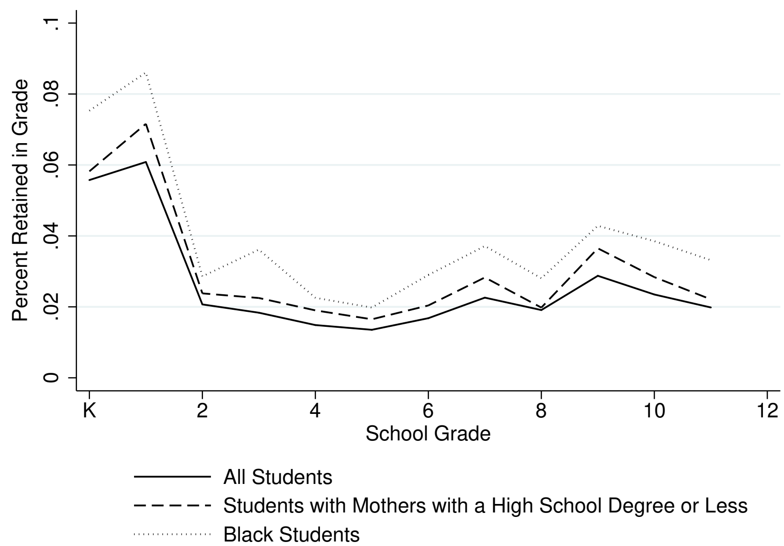
Notes: Figure depicts average births by day of year from 1989-1994 regression-adjusted for day of birth following equations (1) and (2).

Figure 6: Estimated Birth Timing Manipulation



Notes: Figure depicts average births by day of year from 1989-1994 regression-adjusted for day of birth following equations (1) and (2). Vertical bars indicate manipulated region omitted from calculation. Upper bound selected visually at 9 days after the New Year. Lower bound selected through estimation process described in the text.

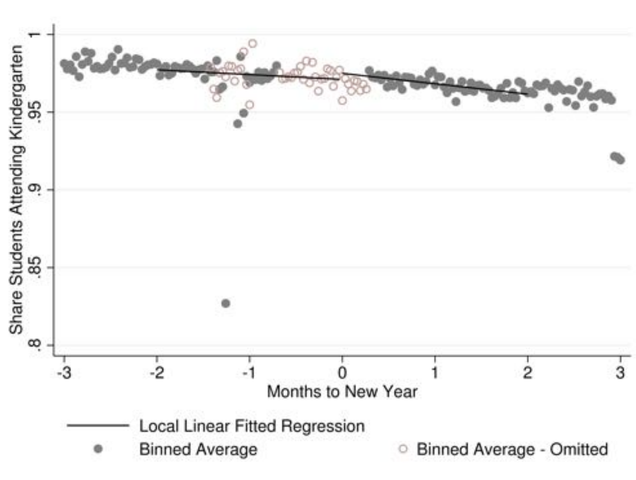
Figure 7: Average Share of Students Retained in Grade - 1990-2005



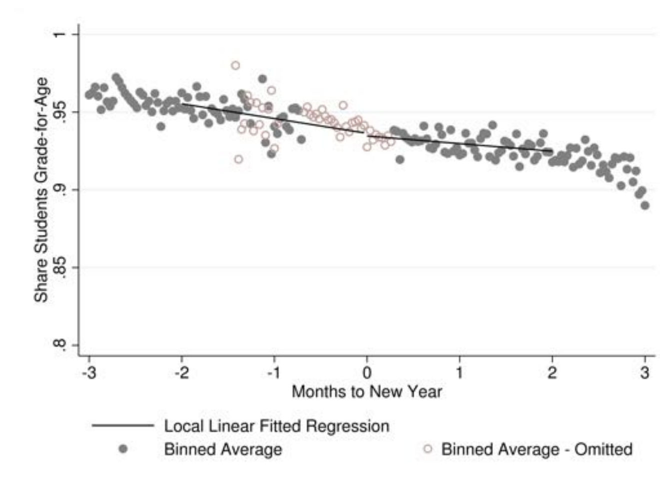
Notes: Figure depicts average share of students retained in each grade over the years 1990 to 2005 estimated in the October CPS. Standard error bars omitted for clarity, but are less than 0.1 percentage points across all groups and years.

Figure 8: Estimated Reduced Form Discontinuities in Grade-for-Age Status - Primary and Pre-Primary School

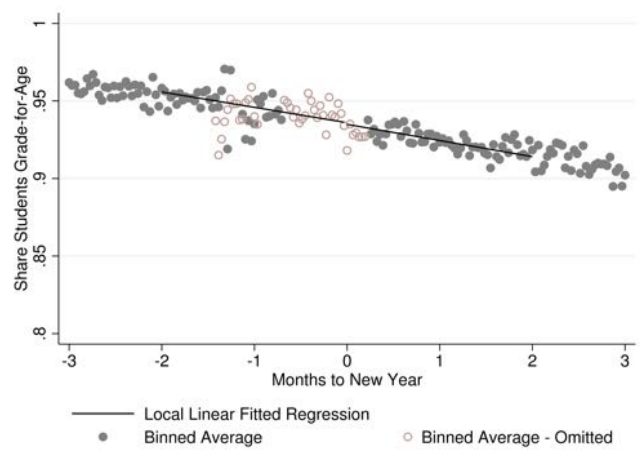
(a) Kindergarten



(b) 1st Grade



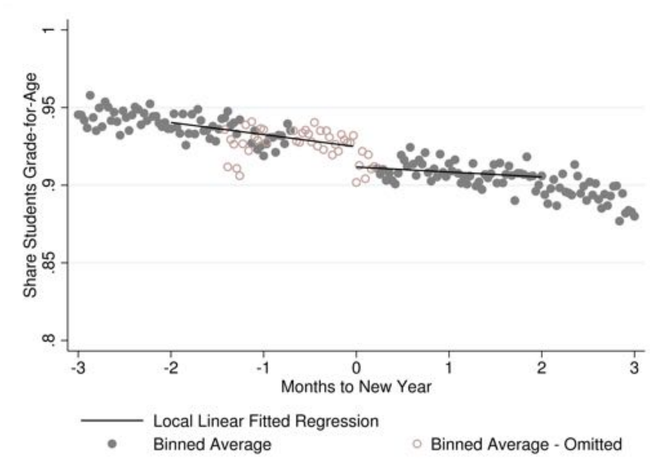
(c) 5th Grade



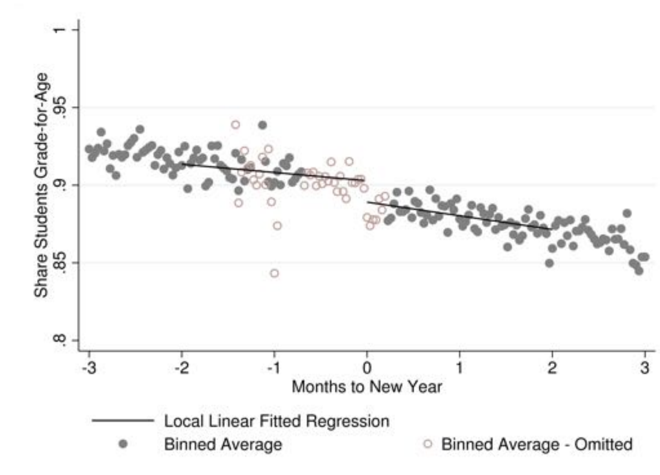
Notes: Figures depicts discontinuity in share of students attending Kindergarten, and share of students grade-for-age in 1st grade and 5th grade around the New Year. Red empty circles are data omitted from estimation process, and grey solid circles are data that could be included. The estimated line uses a bandwidth of two months around the New Year, and the solid grey circles covered by the estimated line represent data included in the estimation process. See Table 2 for point estimates. Regressions include fixed effects by day of week, and state of birth fixed effects. Estimation process detailed in text.

Figure 9: Estimated Reduced Form Discontinuities in Grade-for-Age Status - Middle and Secondary School

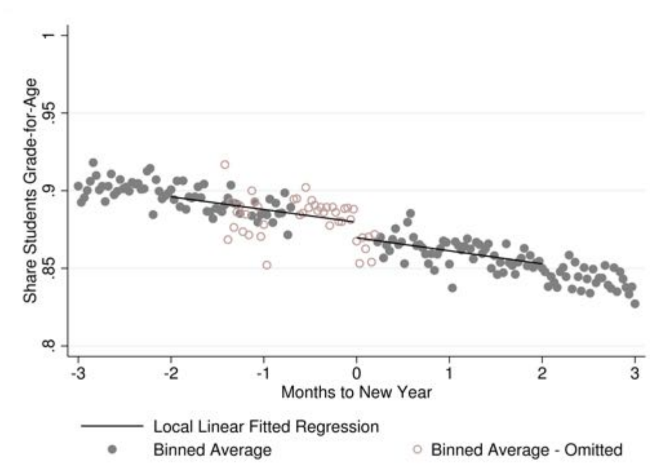
(a) 7th Grade



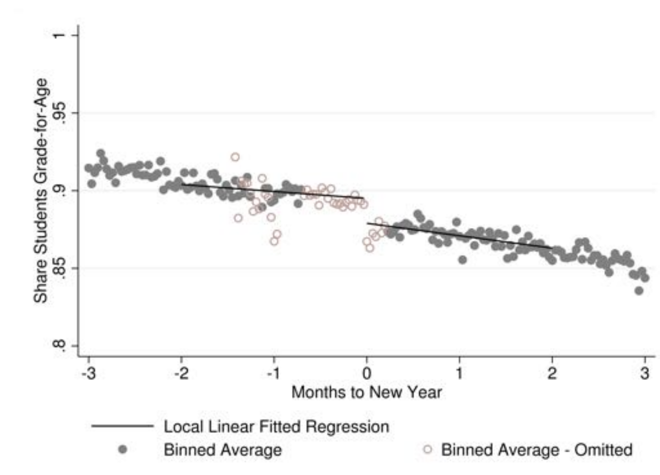
(b) 9th Grade



(c) 10th Grade



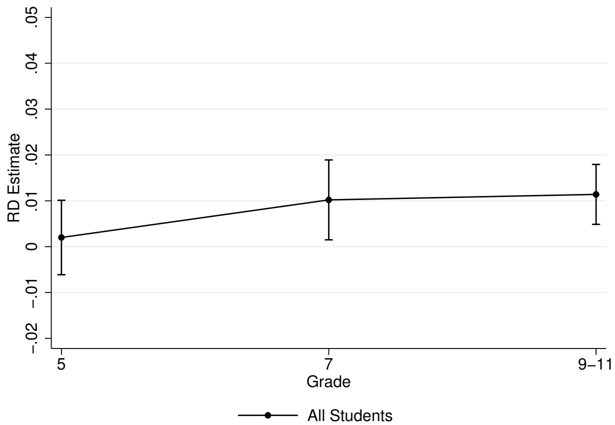
(d) 9th-11th Grade



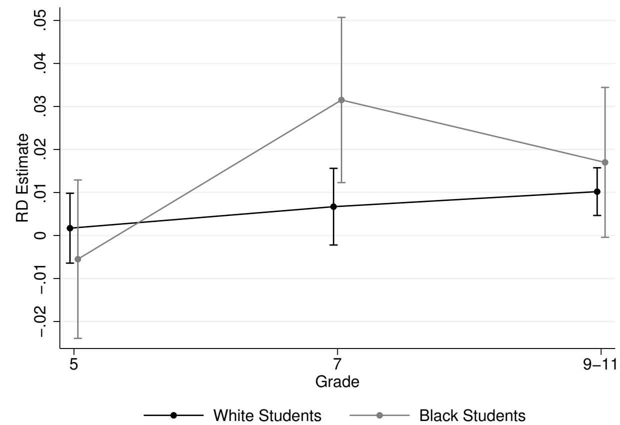
Notes: Figures depicts discontinuity in share of students grade-for-age in 7th grade, 9th grade, 10th grade, and 9th through 11th grade around the New Year. Red empty circles are data omitted from estimation process, and grey solid circles are data that could be included. The estimated line uses a bandwidth of two months around the New Year, and the solid grey circles covered by the estimated line represent data included in the estimation process. See Table 2 for point estimates. Regressions include fixed effects by day of week, and state of birth fixed effects. Estimation process detailed in text.

Figure 10: IV Treatment Effect of $1,000 in Infancy in Grade-for-Age Status by Grade and Subgroup

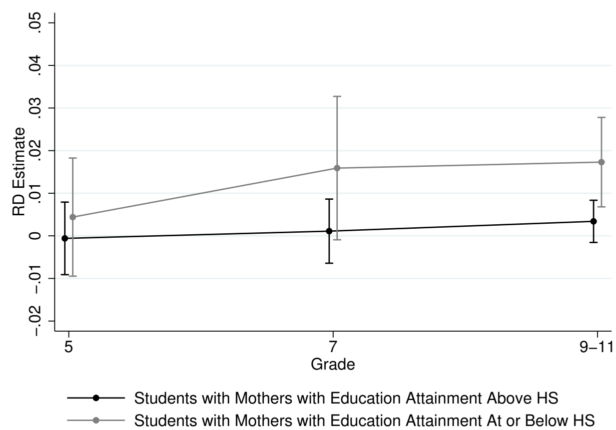
(a) Full Sample



(b) Separated by Race



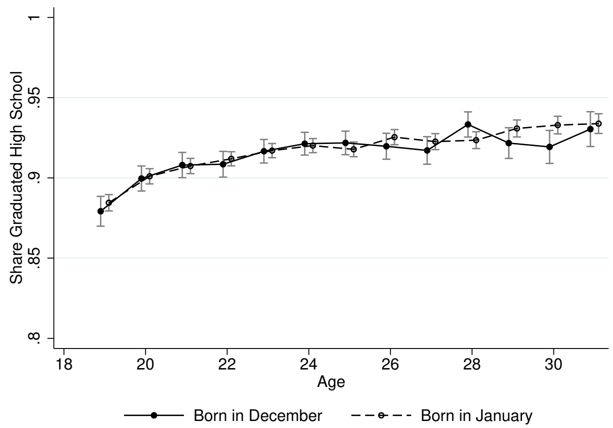
(c) Separated by Mother's Education Attainment



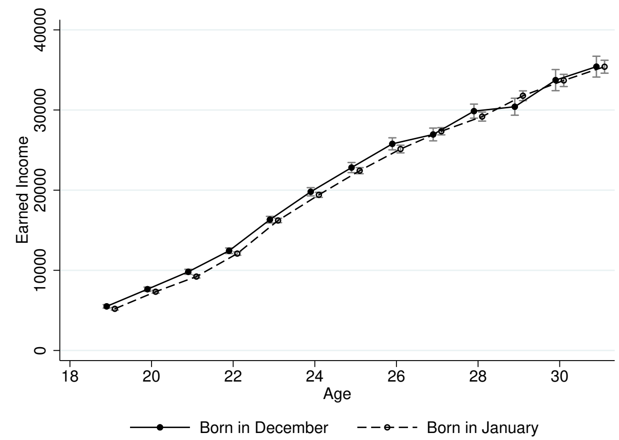
Notes: Figures depicts estimated instrumental variable treatment effect of $1,000 in infancy on grade-for-age status in grades 5, 6 and 9-11 recorded in Tables 2, 3, and 4 with a bandwidth of two months. Regressions include fixed effects by day of week, and state of birth fixed effects. Standard errors calculated with 2,000 bootstrap replications. Estimation process detailed in text.

Figure 11: Average Adult Outcomes by Age Group

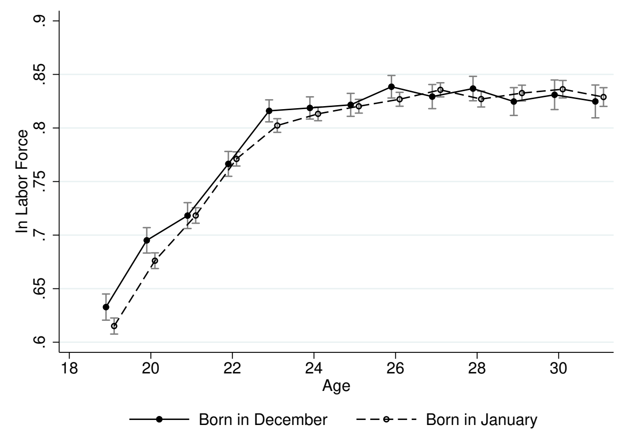
(a) Share Graduated High School



(b) Average Earned Income



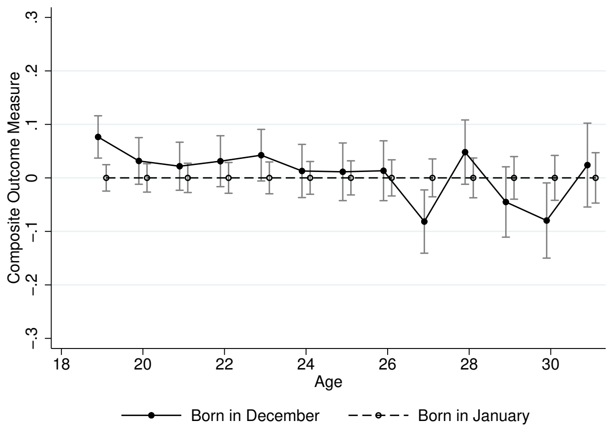
(c) Share in Labor Force



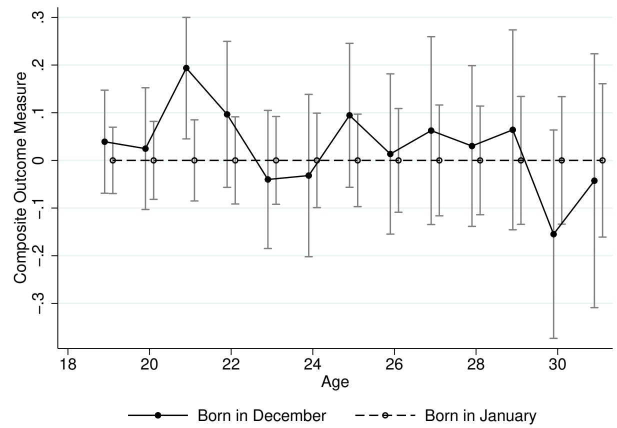
Notes: Figure depicts trends in the full sample of averages by month omitting adults born December 11th through January 9th. "December" births are children born from November 15th to December 10th, and "January" births are children born from January 10th to February 15th.

Figure 12: Average Composite Measure of Outcomes by Age Group and Subgroup

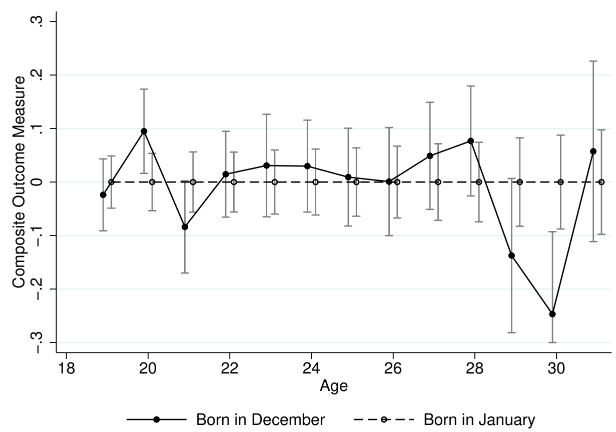
(a) Full Sample



(b) Black Adults



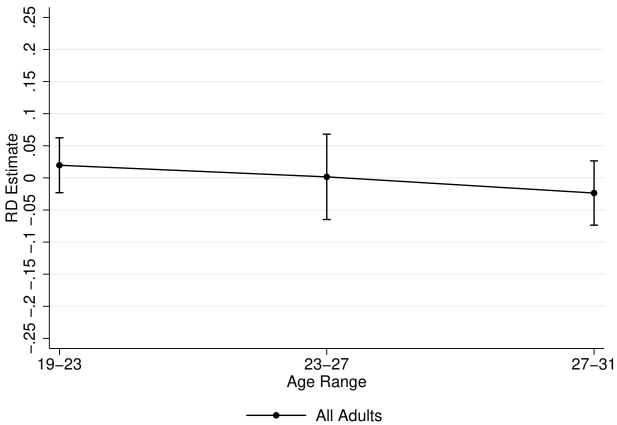
(c) Adults Born in Counties with Average Mothers' Education Attainment in Lowest Quartile



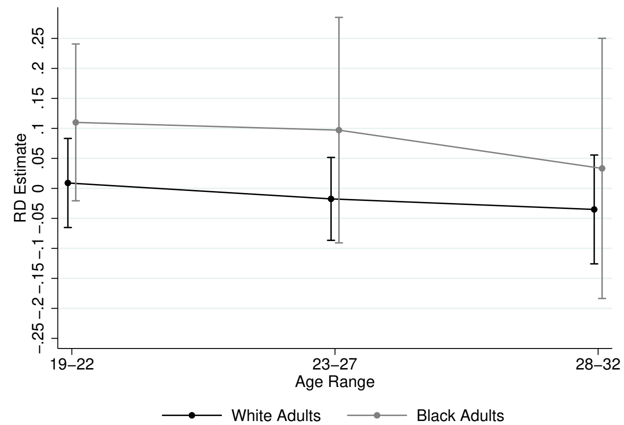
Notes: Figure depicts average trends in a composite measure of adults' outcomes by age, omitting adults born December 11th through January 9th. The composite measure reflects labor force participation, earned income, SNAP receipt and high school graduation status. The process that creates this composite measure described in text. Note that the measure takes on average value 0 for individuals born after the New Year by construction, but there is a standard error present due to sampling variation.

Figure 13: Estimated IV Treatment Effect of $1,000 in Infancy on Composite Measure of Outcomes by Age Group and Subgroup

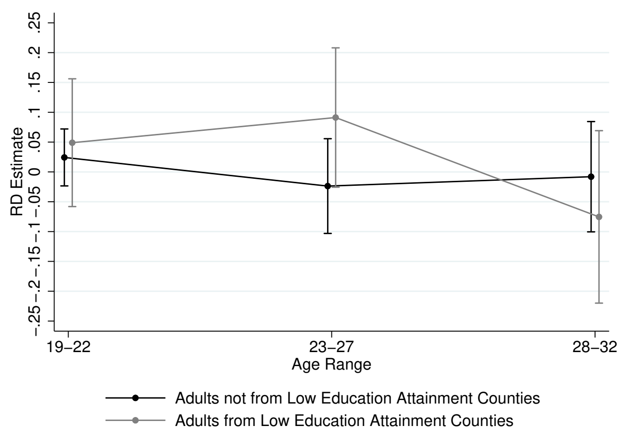
(a) Full Sample



(b) By Race



(c) By County Education Level



Notes: Figures depicts estimated instrumental variable treatment effect of $1,000 in infancy on composite measure of outcomes for adults aged 19-22, 23-27 and 28-32. Results recorded in Tables 6, 7, and 8 with a bandwidth of two months. Regressions include fixed effects by day of week, and state of birth fixed effects. Standard errors calculated with 2,000 bootstrap replications. Estimation process detailed in text.

Table 1: Validating Regression Discontinuity Procedures

Outcome	Control Mean	Reduced Form RD Treatment Effect Estimates by Bandwidth			IV Treatment Effect of $1,000 in Infancy
		1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth	2 month bandwidth
Child is White	0.725	-0.0410	-0.0227*	-0.0172*	-0.0119
	(0.001)	(0.0258)	(0.0119)	(0.0097)	(0.0340)
Child is Black	0.117	0.00140	0.00400	0.00240	0.00210
	(0.001)	(0.0129)	(0.0066)	(0.0055)	(0.0069)
Child is non-White, non-Black	0.159	0.0396**	0.0187**	0.0147*	0.00980
	(0.001)	(0.0193)	(0.0092)	(0.0077)	(0.0279)
Child State of Residence Same as Birth	0.955	-0.00430	-0.00240	-0.00480	-0.00120
	(0.001)	(0.0101)	(0.0053)	(0.0042)	(0.0045)
Total Children in Household	1.937	-0.0480	-0.0295	-0.0299	-0.0155
	(0.001)	(0.0466)	(0.0218)	(0.0197)	(0.0450)
Child Live with Both Parents	0.706	0.00980	-0.00900	-0.00560	-0.00470
	(0.001)	(0.0235)	(0.0122)	(0.0093)	(0.0147)
Child's Household Has Any Earned Income	0.807	0.0457**	0.0144	0.00600	0.00760
	(0.001)	(0.0178)	(0.0092)	(0.0077)	(0.0218)
Child's Household Has Any Other Income	0.112	-0.00510	0.000500	0.000600	0.000300
	(0.001)	(0.0130)	(0.0078)	(0.0061)	(0.0042)
Child's Household Has Any Retirement Income	0.0300	-0.00390	0.00290	0.00440	0.00150
	(0.001)	(0.0066)	(0.0039)	(0.0031)	(0.0048)
Child's Household Has Any Supplemental Income	0.0150	0.00300	0.00330	0.00310	0.00170
	(0.001)	(0.0058)	(0.0035)	(0.0030)	(0.0051)
Child's Household Has Any Welfare Income	0.0600	-0.0138	-0.00200	-0.00340	-0.00110
	(0.001)	(0.0215)	(0.0105)	(0.0081)	(0.0063)
Child's Household's Earned Income	41500	2300	474.8	79.18	249.7
	(71600)	(1700)	(950)	(800)	(859.9)
Child's Household's Other Income	469.8	-8.781	4.689	20.45	2.465
	(182.3)	(85.16)	(53.63)	(42.86)	(29.04)
Child's Household's Suppemental Income	84.83	11.92	13.89	14.39	7.309
	(23.32)	(30.15)	(19.57)	(16.46)	(22.93)
Child's Household's Total Income	42000	1600	1300	814.7	683.6
	(84000)	(1600)	(843.7)	(712.7)	(1966.)
Child's Household's Wage Income	39500	1300	70.91	-137.6	37.28
	(68500)	(1800)	(950.9)	(790.2)	(510.8)
Child's Household's Welfare Income	119.3	-72.69**	-27.29	-18.18	-14.35
	(19.20)	(35.38)	(19.45)	(15.35)	(41.51)
Maximum Age of Parents	30.72	0.142	0.103	0.0206	0.0541
	(0.002)	(0.3087)	(0.1557)	(0.1246)	(0.1724)
Either Parent has Any Wage Income	0.880	0.0174	0.00170	-0.00160	0.000900
	(0.001)	(0.0113)	(0.0069)	(0.0055)	(0.0044)
Either Parent has Any Welfare Income	0.0480	-0.00960	-0.00240	-0.00540	-0.00130
	(0.001)	(0.0132)	(0.0080)	(0.0066)	(0.0055)
Maximum Education Attainment of Parents	13.68	0.136	-0.00610	-0.0222	-0.00320
	(0.001)	(0.1117)	(0.0629)	(0.0489)	(0.0343)
Maximum Wage Income of Parents	33000	999	1000	806	525.8
	(54500)	(1600)	(848.2)	(670.9)	(1539.)
Either Parent is in Labor Force	0.897	0.00260	-0.00300	-0.00250	-0.00160
	(0.001)	(0.0104)	(0.0068)	(0.0049)	(0.0056)
Either Parent is Married	0.808	0.0147	0.00620	0.00640	0.00320
	(0.001)	(0.0169)	(0.0104)	(0.0082)	(0.0106)
Maximum Usual Hours of Work of Parents	41.24	0.248	0.0283	-0.0144	0.0149
	(0.013)	(0.9542)	(0.5250)	(0.4227)	(0.2792)

Notes: Table records estimated discontinuities in child and family covariates for a child being born before the New Year, and an instrumental variables estimate of the effect of a $1,000 increase in family income in infancy. Results estimated using children in the 2000 Census born between 1999 and 2000. Regressions include fixed effects by day of week, and state of birth fixed effects. Standard errors calculated with 2,000 bootstrap replications. Estimation strategy described in text.

Table 1 Continued: Validating Regression Discontinuity Procedures

Outcome	Control Mean	Reduced Form RD Treatment Effect Estimates by Bandwidth			IV Treatment Effect of $1,000 in Infancy
		1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth	2 month bandwidth
Maximum Weeks of Work Last Year of Parents	43.04	0.842	-0.0117	-0.0482	-0.00620
	(0.013)	(0.9000)	(0.4911)	(0.4152)	(0.2588)
Either Parent Worked Last Year	0.936	0.00840	0.00260	0.00340	0.00130
	(0.001)	(0.0081)	(0.0052)	(0.0040)	(0.0046)
Age of Mother	28.44	0.428	0.0868	0.0229	0.0457
	(0.002)	(0.3302)	(0.1772)	(0.1443)	(0.1583)
Mother Has Any Wage Income	0.681	0.0548**	0.0192	0.0111	0.0101
	(0.001)	(0.0236)	(0.0133)	(0.0109)	(0.0291)
Mother Has Any Welfare Income	0.0480	-0.00810	-0.00660	-0.00860	-0.00350
	(0.001)	(0.0122)	(0.0072)	(0.0060)	(0.0104)
Mother's Education Attainment	13.27	0.3927***	0.0721	0.0196	0.0379
	(0.001)	(0.1312)	(0.0841)	(0.0676)	(0.1152)
Mother's Wage Income	15000	2900***	1300**	850.0*	683.6
	(26500)	(1000)	(567.4)	(466.8)	(1939.)
Mother is in Labor Force	0.554	0.0302	0.00210	0.00100	0.00110
	(0.001)	(0.0295)	(0.0156)	(0.0123)	(0.0088)
Mother is Married	0.836	0.00420	0.00560	0.00840	0.00300
	(0.001)	(0.0175)	(0.0107)	(0.0079)	(0.0100)
Mother is Single Household Head	0.0770	0.00570	0.0127**	0.00730	0.00670
	(0.001)	(0.0106)	(0.0061)	(0.0052)	(0.0190)
Mother's Usual Hours of Work	25.86	1.949**	0.8732*	0.653	0.459
	(0.022)	(0.8465)	(0.4650)	(0.3950)	(1.310)
Mother's Weeks of Work Last Year	29.36	2.157**	0.664	0.456	0.349
	(0.031)	(1.039)	(0.5903)	(0.5056)	(1.027)
Mother Worked Last Year	0.711	0.0454*	0.0179	0.0137	0.00940
	(0.001)	(0.0239)	(0.0132)	(0.0110)	(0.0272)

Notes: Table records estimated discontinuities in child and family covariates for a child being born before the New Year, and an instrumental variables estimate of the effect of a $1,000 increase in family income in infancy. Results estimated using children in the 2000 Census born between 1999 and 2000. Regressions include fixed effects by day of week, and state of birth fixed effects. Standard errors calculated with 2,000 bootstrap replications. Estimation strategy described in text.

Table 2: Baseline Results for Regression Discontinuity Estimate of Treatment Effect on Grade-For-Age Status in School

Grade	Control Mean	Reduced Form RD Treatment Effect Estimates by Bandwidth			IV Treatment Effect of $1,000 in Infancy
		1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth	2 month bandwidth
Pre-K	0.758	-0.00253	0.00401	0.00418	0.00238
	(0.001)	(0.01336)	(0.0081)	(0.0068)	(0.0052)
K	0.970	0.00610	-0.00230	-0.00220	-0.00150
	(0.001)	(0.0055)	(0.0025)	(0.0020)	(0.0016)
1st	0.931	0.00280	0.00520	0.00610	0.00350
	(0.001)	(0.0121)	(0.0059)	(0.0045)	(0.0039)
5th	0.915	-0.00520	-0.00180	0.00200	-0.00140
	(0.001)	(0.0083)	(0.0048)	(0.0041)	(0.0037)
7th	0.903	0.0158	0.0105*	0.0102**	0.0088*
	(0.001)	(0.0102)	(0.0057)	(0.0044)	(0.0048)
9th	0.878	0.0139**	0.0084**	0.0088***	0.0089**
	(0.001)	(0.0059)	(0.0042)	(0.0032)	(0.0044)
10th	0.864	0.00200	0.00560	0.00500	0.00630
	(0.001)	(0.0120)	(0.0066)	(0.0052)	(0.0074)
11th	0.855	0.0245***	0.0205***	0.0211***	0.0221***
	(0.001)	(0.0076)	(0.0043)	(0.0033)	(0.0047)
9th-11th	0.866	0.0123**	0.0113***	0.0114***	0.0120***
	(0.001)	(0.0059)	(0.0032)	(0.0024)	(0.0034)

Notes: Table records estimated discontinuity in grade-for-age status for a child being born before the New Year by expected grade of student for full sample. Table also records an instrumental variables estimate of the effect of a $1,000 increase in family income in infancy on grade-for-age status. Results estimated using children in the 2000 Census and 2001-2016 ACS. Regressions include fixed effects by day of week, and state of birth fixed effects. Standard errors calculated with 2,000 bootstrap replications. Estimation strategy described in text.

Table 3: Regression Discontinuity Estimates of Treatment Effect on Grade-For-Age Status in School by Race

Grade	Race	Control Mean	Reduced Form RD Treatment Effect Estimates by Bandwidth			IV Treatment Effect of $1,000 in Infancy
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth	2 month bandwidth
5th	White	0.922	0.000600	-0.00130	0.00170	-0.00100
		(0.001)	(0.0080)	(0.0049)	(0.0041)	(0.0038)
	Black	0.871	-0.0194	-0.0127	-0.00550	-0.0110
		(0.001)	(0.0188)	(0.0110)	(0.0093)	(0.0095)
	Difference		-0.0200	-0.0114	-0.00720	-0.0100
7th	White	0.912	0.00990	0.00680	0.00670	0.00560
		(0.001)	(0.0105)	(0.0059)	(0.0045)	(0.0049)
	Black	0.845	0.0218	0.0311**	0.0315***	0.0282***
		(0.001)	(0.0223)	(0.0118)	(0.0097)	(0.0107)
	Difference		0.0119	0.0244*	0.0248**	0.0226*
9th-11th	White	0.879	0.00720	0.0102***	0.0102***	0.0106***
		(0.001)	(0.0065)	(0.0036)	(0.0028)	(0.0037)
	Black	0.793	0.0207	0.0132	0.0170*	0.0160
		(0.001)	(0.0207)	(0.0111)	(0.0088)	(0.0134)
	Difference		0.0135	0.00310	0.00690	0.00540

Notes: Table records estimated discontinuity in grade-for-age status for a child being born before the New Year by expected grade of student among White and Black children. Table also records an instrumental variables estimate of the effect of a $1,000 increase in family income in infancy on grade-for-age status. Results estimated using children in the 2000 Census and 2001-2016 ACS. Regressions include fixed effects by day of week, and state of birth fixed effects. Standard errors calculated with 2,000 bootstrap replications. Estimation strategy described in text.

Table 4: Regression Discontinuity Estimates of Treatment Effect on Grade-For-Age Status in School by Mother's Education Level

Grade	Mother's Education Level	Control Mean	Reduced Form RD Treatment Effect Estimates by Bandwidth			IV Treatment Effect of $1,000 in Infancy
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth	2 month bandwidth
5th	Above High School	0.941	-0.00650	-0.00320	-0.000600	-0.00230
		(0.001)	(0.0078)	(0.0052)	(0.0043)	(0.0037)
	High School or Below	0.887	-0.00540	-0.00200	0.00440	-0.00170
		(0.001)	(0.0153)	(0.0072)	(0.0061)	(0.0060)
	Difference		0.00110	0.00130	0.00500	0.000600
7th	Above High School	0.932	-0.00120	-0.000700	0.00110	-0.000600
		(0.001)	(0.0081)	(0.0047)	(0.0038)	(0.0035)
	High School or Below	0.874	0.0207	0.0168	0.0159*	0.0153
		(0.001)	(0.0180)	(0.0107)	(0.0085)	(0.0109)
	Difference		0.0219	0.0175	0.0148	0.0159
9th-11th	Above High School	0.916	0.00350	0.00190	0.00340	0.00170
		(0.001)	(0.0058)	(0.0031)	(0.0025)	(0.0029)
	High School or Below	0.825	0.0105	0.0173**	0.0173***	0.0205**
		(0.001)	(0.0117)	(0.0067)	(0.0053)	(0.0097)
	Difference		0.00700	0.0155**	0.0139**	0.0187*

Notes: Table records estimated discontinuity in grade-for-age status for a child being born before the New Year by expected grade of student among children with different levels of mother education attainment. Table also records an instrumental variables estimate of the effect of a $1,000 increase in family income in infancy on grade-for-age status. Results estimated using children in the 2000 Census and 2001-2016 ACS. Regressions include fixed effects by day of week, and state of birth fixed effects. Standard errors calculated with 2,000 bootstrap replications. Estimation strategy described in text.

Table 5: Regression Discontinuity Estimate of Treatment Effect on Grade-For-Age Status in School - Children Living in Same State as Birth

Grade	Control Mean	Reduced Form RD Treatment Effect Estimates by Bandwidth			IV Treatment Effect of $1,000 in Infancy
		1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth	2 month bandwidth
5th	0.915	0.00150	0.00190	0.00420	0.00150
	(0.001)	(0.0093)	(0.0056)	(0.0047)	(0.0044)
7th	0.904	0.0177	0.0110*	0.0100**	0.0092*
	(0.001)	(0.0114)	(0.0063)	(0.0050)	(0.0053)
9th-11th	0.867	0.0172***	0.0129***	0.0125***	0.0138***
	(0.001)	(0.0055)	(0.0032)	(0.0025)	(0.0034)

Notes: Table records estimated discontinuity in grade-for-age status by grade of student for a child being born before the New Year among children living in the same state as birth. Table also records an instrumental variables estimate of the effect of a $1,000 increase in family income in infancy on grade-for-age status. Results estimated using children in the 2000 Census and 2001-2016 ACS. Regressions include fixed effects by day of week, and state of birth fixed effects. Standard errors calculated with 2,000 bootstrap replications. Estimation strategy described in text.

Table 6: Regression Discontinuity Estimates of Treatment Effect on Grade-For-Age Status in School by Cohort Year of Birth

Grade	Cohort Year of Birth	Control Mean	Reduced Form RD Treatment Effect Estimates by Bandwidth			IV Treatment Effect of $1,000 in Infancy
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth	2 month bandwidth
9th-11th	1982-1986	0.873	0.00770	0.00740	0.00680	0.0114
		(0.001)	(0.0101)	(0.0054)	(0.0043)	(0.0084)
	1987-1993	0.856	0.00720	0.00740	0.0090*	0.00780
		(0.001)	(0.0122)	(0.0065)	(0.0051)	(0.0069)
	1994-2001	0.875	0.0244**	0.0123*	0.0114**	0.0090*
		(0.001)	(0.0121)	(0.0065)	(0.0051)	(0.0047)

Notes: Table records estimated discontinuity in grade-for-age status for a child being born before the New Year by expected grade of student among children with different levels of mother education attainment. Table also records an instrumental variables estimate of the effect of a $1,000 increase in family income in infancy on grade-for-age status. Results estimated using children in the 2000 Census and 2001-2016 ACS. Regressions include fixed effects by day of week, and state of birth fixed effects. Standard errors calculated with 2,000 bootstrap replications. Estimation strategy described in text.

Table 7: Baseline Results for Regression Discontinuity Estimates of Treatment Effects for Young Adults

Outcome	Age Range	Control Mean Mean	Reduced Form RD Treatment Effect Estimates by Bandwidth			IV Treatment Effect of $1,000 in Infancy
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth	2 month bandwidth
Composite Measure	19-27	0.0000	-0.0473	0.0028	0.0101	0.0036
		(1)	(0.0484)	(0.0261)	(0.0216)	(0.0334)
Composite Measure	19-22	0.0000	0.0481	0.0249	0.0197	0.0287
		(1)	(0.0597)	(0.0409)	(0.0336)	(0.0473)
Composite Measure	23-27	0.0000	-0.1204*	-0.0152	0.0016	-0.0201
		(1)	(0.0643)	(0.0301)	(0.0253)	(0.0397)
Composite Measure	28-32	0.0000	-0.0819	-0.0175	-0.0236	-0.0260
		(1)	(0.0748)	(0.0447)	(0.0374)	(0.0667)
Graduated High School	19-27	0.9161		0.0006	0.0012	0.0007
		(0.0006)		(0.0029)	(0.0023)	(0.0037)
Graduated High School	19-22	0.9092		0.0008	0.0011	0.0008
		(0.0009)		(0.0044)	(0.0038)	(0.0051)
Graduated High School	23-27	0.9210		0.0002	0.0011	0.0002
		(0.0007)		(0.0034)	(0.0028)	(0.0046)
Graduated High School	28-32	0.9321		-0.0047	-0.0044*	-0.0070
		(0.0009)		(0.0034)	(0.0026)	(0.0051)
Earned Income	19-27	16780		-143	-111	-182.55
		(42.6)		(182)	(155)	(232.34)
Earned Income	19-22	9582		7.5	14	8.6628
		(43)		(169)	(133)	(195.20)
Earned Income	23-27	21920		-280	-217	-369.77
		(62.7)		(292)	(244)	(385.62)
Earned Income	28-32	33100		-1.76	-376	-2.6259
		(129)		(675)	(569)	(1.0e+0)
In Labor Force	19-27	0.7763		0.0048	0.0048	0.0061
		(0.0009)		(0.0048)	(0.0037)	(0.0061)
In Labor Force	19-22	0.7238		0.0070	0.0041	0.0081
		(0.0014)		(0.0086)	(0.0064)	(0.0099)
In Labor Force	23-27	0.8138		0.0028	0.0051	0.0037
		(0.0010)		(0.0051)	(0.0039)	(0.0067)
In Labor Force	28-32	0.8234		-0.0032	-0.0010	-0.0047
		(0.0014)		(0.0081)	(0.0068)	(0.0120)
SNAP	19-27	0.1528		0.0015	0.0004	0.0018
		(0.0007)		(0.0050)	(0.0040)	(0.0064)
SNAP	19-22	0.1480		-0.0021	-0.0020	-0.0024
		(0.0011)		(0.0091)	(0.0072)	(0.0105)
SNAP	23-27	0.1561		0.0041	0.0022	0.0054
		(0.0010)		(0.0043)	(0.0035)	(0.0057)
SNAP	28-32	0.1566		-0.0036	-0.0026	-0.0053
		(0.0013)		(0.0069)	(0.0055)	(0.0103)

Notes: Table records estimated discontinuity in adult outcomes for an adult being born before the New Year by age group for the full sample. Table also records an instrumental variables estimate of the effect of a $1,000 increase in family income in infancy on adult outcomes. Results estimated using adults in the 2001-2016 ACS. Regressions include fixed effects by day of week, and state of birth fixed effects. Standard errors calculated with 2,000 bootstrap replications. Estimation strategy described in text.

Table 8: Regression Discontinuity Estimate of Treatment Effects on Composite Outcomes for Young Adults by Race and Age

Outcome	Age Range	Race	Reduced Form RD Treatment Effect Estimates by Bandwidth			IV Treatment Effect of $1,000 in Infancy
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth	2 month bandwidth
Composite Measure	19-27	White	-0.0658	-0.0121	-0.0059	-0.0145
			(0.0582)	(0.0334)	(0.0269)	(0.0404)
		Black	0.0775	0.1240*	0.0990*	0.1925*
			(0.1208)	(0.0744)	(0.0550)	(0.1155)
		Difference	0.1433	0.1361*	0.1049**	0.2071*
Composite Measure	19-22	White	0.0382	0.0206	0.0090	0.0228
			(0.0742)	(0.0440)	(0.0375)	(0.0487)
		Black	0.1101	0.1340	0.1100**	0.1794
			(0.1489)	(0.1086)	(0.0660)	(0.1454)
		Difference	0.0719	0.1134*	0.1010**	0.1566
Composite Measure	23-27	White	-0.1434*	-0.0364	-0.0175	-0.0454
			(0.0788)	(0.0432)	(0.0349)	(0.0540)
		Black	-0.0148	0.1124	0.0971	0.1840
			(0.2210)	(0.1162)	(0.0949)	(0.1903)
		Difference	0.1286	0.1488	0.1146	0.2295
Composite Measure	28-32	White	-0.0257	-0.0142	-0.0351	-0.0199
			(0.1019)	(0.0567)	(0.0458)	(0.0795)
		Black	-0.1497	0.0333	0.0672	0.0664
			(0.2525)	(0.1361)	(0.1095)	(0.2714)
		Difference	-0.1240	0.0475	0.1023	0.0864

Notes: Table records estimated discontinuity in composite measure of economic self-sufficiency for an adult being born before the New Year by age group among White and Black adults. Table also records an instrumental variables estimate of the effect of a $1,000 increase in family income in infancy on the self-sufficiency measure. Results estimated using adults in the 2001-2016 ACS. Regressions include fixed effects by day of week, and state of birth fixed effects. Standard errors calculated with 2,000 bootstrap replications. Estimation strategy described in text.

Table 9: Regression Discontinuity Estimate of Treatment Effects on Composite Outcomes for Young Adults by Average County Mothers' Education Attainment and Age

Outcome	Age Range	Average County Education Attainment of Mothers	Reduced Form RD Treatment Effect Estimates by Bandwidth			IV Treatment Effect of $1,000 in Infancy
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth	2 month bandwidth
Composite Measure	19-27	Above Lowest Quartile	-0.0772	-0.0150	-0.0029	-0.0163
			(0.0589)	(0.0287)	(0.0241)	(0.0312)
		Below Lowest Quartile	0.0496	0.0692	0.0555	0.0987
			(0.0888)	(0.0518)	(0.0375)	(0.0739)
		Difference	0.1269	0.0842	0.0584	0.1151
Composite Measure	19-22	Above Lowest Quartile	0.0146	0.0190	0.0243	0.0192
			(0.0773)	(0.0485)	(0.0401)	(0.0492)
		Below Lowest Quartile	0.1527	0.0497	0.0049	0.0632
			(0.1206)	(0.0667)	(0.0541)	(0.0849)
		Difference	0.1381	0.0308	-0.0194	0.0440
Composite Measure	23-27	Above Lowest Quartile	-0.1490**	-0.0415	-0.0237	-0.0464
			(0.0732)	(0.0321)	(0.0277)	(0.0359)
		Below Lowest Quartile	-0.0173	0.0838	0.0912	0.1240
			(0.1144)	(0.0746)	(0.0590)	(0.1103)
		Difference	0.1317	0.1253	0.1149*	0.1705
Composite Measure	28-32	Above Lowest Quartile	-0.0303	0.0102	-0.0080	0.0127
			(0.0860)	(0.0558)	(0.0467)	(0.0695)
		Below Lowest Quartile	-0.2692*	-0.1171	-0.0754	-0.1956
			(0.1490)	(0.0873)	(0.0730)	(0.1459)
		Difference	-0.2389	-0.1273	-0.0674	-0.2084

Notes: Table records estimated discontinuity in composite measure of economic self-sufficiency for an adult being born before the New Year by age group among adults born in counties where average mothers' education is below the lowest quartile and above the lowest quartile. Table also records an instrumental variables estimate of the effect of a $1,000 increase in family income in infancy on the self-sufficiency measure. Results estimated using adults in the 2001-2016 ACS. Regressions include fixed effects by day of week, and state of birth fixed effects. Standard errors calculated with 2,000 bootstrap replications. Estimation strategy described in text.

Appendices

Appendix A Additional Detail on Variables and Data

This paper uses the 2000 long-form Census and the 2001-2016 ACS to estimate causal regression discontinuities. These estimates identify the effect of an increase in family income from being born before the New Year on later-life outcomes for children. It also uses the CPS to estimate the size of the family's discontinuity in after-tax income from having a child born before the New Year. This appendix discusses data quality issues associated with these two data sources sequentially.

A.1 Assigning Grade-for-Age Status in the 2000 Census and 2001-2016 ACS

As described in the text, this paper assigns grade-for-age status to students based on four pieces of information: a child's highest grade completed or current grade enrolled, the state of birth of the child, the year and date of birth of the child, and the day on which households respond to the survey. Many states set explicit Kindergarten and 1st grade age entrance requirements that require students to be a specific age by a certain date before being eligible to enter either Kindergarten or 1st grade in that state. Comprehensive data on these state policies were collected by Bedard and Dhuey (2012) and they generously provided their most recent data covering 1955 to 2015. Using this data, this paper assigns expected completed grades to students assuming that they entered Kindergarten or first grade in the first year that they were eligible for those grades and then progressed through all other grades sequentially without repeating a grade. A student is grade-for-age for the purposes of this research if they have completed the most recent grade that this measure records a student as having completed.[39]

Four complications are worth noting about this measure. First, some states do not specify statewide Kindergarten entrance rules and allow local school districts to specify their own rules. No clear expected grade can be assigned to these individuals without more detailed data on individual school district practices. Consequently, this paper drops any individuals born in these states from any further calculation involving either outcomes for children or outcomes for adults.

Second, some states make the eligibility cutoff January 1st or December 31st. In the years that such cutoffs are present, children born before and after the New Year would, in addition to the treatment described, also experience the treatment of different grade eligibility rules. This paper also drops these individuals from any further calculation.

[39]As noted in the paper, most school systems define grade-for-age status starting from the first year a child enters Kindergarten or 1st grade. As these entrance dates are not observable in Census data, this definition is the closest analogue.

Third, there are only a handful of grades where grade-for-age status can be reliably assigned due to the nature of the grade attainment and enrollment questions in the 2000 Census and 2001-2007 ACS. The 2008-2016 ACS allow respondents to mark grade completion and grade attendance in all primary and secondary grades. However, the 2000 Census and 2001-2007 ACS only allow respondents to list whether their children have completed Nursery School/Preschool through 4th grade, 5th grade through 6th grade, 7th grade through 8th grade, and 9th, 10th, 11th and 12th grades. These same surveys only allow respondents to list whether their children have recently attended Nursery School/Preschool, Kindergarten, 1st through 4th grade, 5th grade through 8th grade, and 9th grade through 12th grade. Therefore, the best grades to measure grade-for-age status would be grades where students would be expected to have completed or be currently attending a grade where the student's family could have listed completion or attendance of a prior grade. These grades would be pre-Kindergarten, Kindergarten, 1st, 5th, 7th, 9th, 10th and 11th grades. To see why, for example, 6th grade cannot be included, note that whether or not a student has completed 5th or 6th grade cannot be distinguished from that student's information in the 2000 Census and the 2001-2007 ACS. Note that the recent grade completed question can be used to determine grade-for-age status for 5th, 7th, 9th, 10th and 11th grades. The recent grade enrolled question can be used to calculate enrollment status for pre-Kindergarten and Kindergarten, and grade-for-age status in 1st grade.

Fourth, the response day of a household will affect the most recent grade a student may have completed or attended. In both the Census and the ACS, the education attainment question asks for the highest grade completed by a respondent and most recent grade enrolled. Thus, the date of response to an individual survey matters for determining the most recent grade a student has completed or recently attended.

The effect of date of response differs between the most recent grade enrolled and the most recent grade completed questions. Consider first how date of response will affect completed grades, which are used to calculate grade-for-age status in 5th grade and up. Suppose a student is in 5th grade in March 2001. If that family were responding to the ACS in that month, that family would list that student as having completed the fourth grade. However, suppose the student progressed to the next grade, the school year ended in May, and the family responded to the ACS in June. Then, that family would list that student as having completed the 5th grade. To account for this issue, this paper assumes that households responding to surveys between January 1st and April 10th will still have their children enrolled in the grade that they would have enrolled in at the beginning of the school year. Thus, these children will be recorded as having finished the previous grade they completed before enrolling in their current grade. This paper also assumes that households that respond to surveys between July 1st and December 31st will either have completed the previous grade (if the student passed and is grade-for-age) or will only have completed the grade before that (if the student was retained and is not grade-for age). As grade-for-age status cannot be ascertained reliably for the intervening months, this paper drops individuals who respond in those months from consideration for all calculations.[40] To ensure that post-schooling outcomes look at similarly structured cohorts as well, this paper also omits responses from these months when looking at outcomes for adults.

Date of response affects the ways families answer the question regarding the most recent grade enrolled in a slightly different manner. The most recent grade enrolled question is used to calculate enrollment status for students in pre-Kindergarten and Kindergarten, and grade-for-age status in 1st grade. Suppose a student is in Kindergarten in March 2001, and the family responded to the ACS in that month. That family would list that student as being enrolled in Kindergarten. Now suppose the student progressed to the next grade and the school year ended in May. If the family responded to the ACS in June, that family would still list that student as having most recently attended Kindergarten. If the households respond by October, however, it is likely that the next school year has begun, and the family would list that student as having been most recently enrolled in 1st grade. To account for this issue, this paper assumes that households responding to surveys between January 1st and April 10th will still have their children enrolled in the grade that they would have enrolled in at the beginning of the school year. This paper also assumes that households that respond to surveys between September 30th and December 31st will either be enrolled in the next grade (if the student passed and is grade-for-age) or will still be enrolled in the same grade (if the student was retained and is not grade-for age). As grade-for-age status cannot be ascertained reliably for the intervening months when using the current grade enrolled question, this paper drops individuals who respond in those months from these calculations. Again, note that this specific adjustment only happens when looking at enrollment in pre-Kindergarten and Kindergarten and grade-for-age status in 1st grade.[41]

These sampling restrictions are necessary to ensure accurate assignment of grade-for-age status, but they may introduce bias related to response dates. If different types of households are more likely to respond to the survey at different times, then restricting attention to individuals who respond in specific months may bias the sample. If these sample restrictions change the sample in ways that do not vary across the New Year, it would mean that the treatment effect measured by the discontinuity is a local treatment effect for the population created by the sampling restrictions. If the sample restrictions change the sample in ways

[40] Since almost all states allow districts to set school calendar start and end dates (Education Commission of the States, April 2018*a*), there is substantial variation in the dates at which the school year ends for students in the U.S.. Ideally, the April 10th date would be the latest possible date before any school district has ended the school year and the July 1st date would be the earliest possible date after any school district has ended the school year. Although national data for all districts is not available on school start and end dates, Florida collects data on these dates for its school districts. In Florida, all school districts start school in August to September, and end the school year in May to June (Florida Department of Education, 2020). A sample of large school districts surveyed by Pew indicates that most school districts start school in August to September as well (Desilver, 2019). Hence, the sampling restrictions by date of response used in this paper fit with the limited data available.

[41] Note that this set-up is similar to the previous adjustment when looking at grade-for-age status by grade completed, but omits slightly more data from the summer months. It is possible to assign families who respond in these summer months to a grade-for-age calculation with the most recent grade enrolled variable. Families who respond in the summer would presumably list their children as having been most recently enrolled in the grade that their student completed in the early spring. However, the previously described restrictions on response dates are used throughout the paper when looking at adults. Hence, omitting these months from the calculation keeps data sampling decisions as similar as possible among all calculations.

that vary across the New Year, it could bias the estimated treatment effect in complex ways that make any treatment effects measured harder to interpret.

The bias introduced in the ACS data by these sampling restrictions by date of response is likely small. As mentioned in the text, the ACS samples households throughout the year, with the vast majority of households assigned a sampling date in the year at random (U.S. Census Bureau, 2019).[42] Hence, children born before and after the New Year are sampled at similar rates at different times across the year, and restricting attention to households sampled in particular months should not bias the composition of the sample of observations. The effect of this sampling restriction on the 2000 U.S. Census data is more complicated. The vast majority of responses to the 2000 Census happened in March through the end of April (Stackhouse and Brady, 2003*a*). Hence, most responses would have been sent in by April 10th. However, the households that respond later are more likely to be harder to reach, and more likely to be larger than households that respond earlier (Stackhouse and Brady, 2003*b*). These factors may correlate with family disadvantage, meaning that dropping responses in the summer months drops observations from families that are more likely disadvantaged.

One check on the potential bias of this sampling feature of the 2000 Census data is to drop this data from calculations. Table 6 offers a version of such a check. This table separates the data by birth cohorts when looking at grade-for-age status by high school. The 2000 Census data would not be included in the regression discontinuity calculations looking at children born 1987-1993 or 1994-2001, as the children born in these cohorts were not in high school in 2000. As is clear, the measured discontinuities in grade-for-age status for the cohorts born after 1987 are in the same range or larger than those for the birth cohort born before. Thus, the bias introduced by this sampling feature of the 2000 Census is likely minor.

One further issue with household response dates worth noting is how date of response affects enrollment rates in nursery school and pre-Kindergarten. Other school grades are nearly always organized by regular school calendars. So, the previously mentioned omissions of households by month of response result in data that reflect the average likelihood of a child being grade-for-age within that grade. However, with children in pre-Kindergarten, there are many different enrollment policies across states, districts and local private care providers. The diversity of programs and program structures ensures that more children tend to be enrolled in pre-Kindergarten programs for months closer to the beginning of the next school year. The 2000 Census responses happen primarily in the later spring months before the lead-up to the next school year. Hence, the children in the 2000 Census are more likely to be enrolled in pre-Kindergarten than if these children were surveyed in the previous fall. While including the 2000 Census data does not impact the significance of discontinuities in enrollment across the New Year, it does increase average enrollment

[42]Exceptions include households in rural Alaska and areas with high concentrations of Native Americans.

levels in pre-Kindergarten. Thus, this paper restricts attention to individuals in the ACS 2001-2016 for this calculation. The average in this data offers a more accurate estimate of average likelihood of being enrolled in nursery school or pre-Kindergarten in the year prior to Kindergarten enrollment.

A.2 Estimating the Discontinuity in After-Tax Income using CPS Data

As described in the text, this paper uses the March CPS to estimate the size of the discontinuity in after-tax income for a family for having a child born in December rather than January. The estimation process draws inspiration from Hoynes, Miller and Simon (2015). The sample for the estimation process are parents with at least one infant under three who are in the March CPS in a four year radius for the year after the tax year. So for example, when calculating the discontinuity for the 1986 tax year, this paper uses all parents with at least one infant under three in a four year radius of the 1987 March CPS (1983 to 1991). Note that the central year in the data included is the year after the relevant tax year. The CPS March income data reflect income from the previous calendar year, which is the relevant year for computing taxes for the tax year. Parents with an infant under three are treated as having at least one infant under one who could have been born in January or December. The inclusion of other survey years and other child ages in the data is only to increase precision when calculating effects for smaller and more likely disadvantaged groups. A later part of this section investigates potential bias introduced by this choice.

Using this sample, this paper calculates tax obligations for having a child born in December by summing income measures at the family level and calculating the total state and federal tax burden using TAXSIM assuming that the family with the infant under three is the relevant tax filing unit.

This paper calculates tax obligations for having a child born in January using the same data with the same income measures but reducing the number of dependents under the age of 13 by one (as if the infant were born after December and hence not claimed on that year's tax return). The tax discontinuity is then the difference between the two calculated tax obligations. The percent change in after-tax income is this change divided by the after-tax income calculated for that family assuming the child was born in January. Families with no reported income are included in all calculations, but they comprise a small share of households over all years, and are included as a 0 increase in income and a 0 percent change in income.

Appendix Figure A.1 shows a check on the potential for bias from including parents with slightly older children and other years of survey data in the calculation. This figure shows the average estimated discontinuity when using only parents with infants under 1 and responses in the current tax year, and compares it to the results in Figure 2. As is clear, the measure is somewhat noisier, reflecting the smaller sample sizes, but the evolution of the discontinuity is similar over time, with the average gap between the two measures being \$44. Note that using just the individuals with newborns who were born during the tax year results in a larger estimated increase in after-tax income. This difference is because families with older children are less likely to be in poverty, and hence usually have smaller CTC and EITC tax credits. However, the bias is relatively small across all years. Thus, it is likely the case that the other estimated discontinuities in Figure 2 are only slightly biased downwards by including families with older children and other tax years of data.

This paper, like many papers in the EITC literature that do not have access to administrative tax data, assumes 100% take-up of tax benefits to calculate the change in after-tax income produced by these tax policies (Hoynes, Miller and Simon, 2015). While take-up is not 100%, it is still likely high. LaLumia, Sallee and Turner (2015) find that 85% to 90% of newborns born in late December are claimed on a tax return in the 2000s. Of the remaining 15% to 10% of children who do not appear on tax returns, 5 percentage points are children whose parents do file tax returns but do not claim their newborn on that year's tax return, a phenomenon driven by low-income parents. Thus, likely 10 to 5 percentage points of the remaining share of newborns not claimed on taxes likely come from parents who are not required to file tax returns.

While the data in LaLumia, Sallee and Turner (2015) do not allow a strict calculation about take-up rates, a separate literature on take-up of the EITC suggests that, conditional on eligibility, take-up of the EITC is substantial. Among eligible families with children, Scholz (1994) estimates EITC take-up in 1990 of 80% to 86%, and U.S. Government Accountability Office (2001) find EITC take-up in 1999 is 86%. A large share of the families who do not claim EITC benefits are families not required to file taxes. For example, Blumenthal, Erard and Ho (2005) suggest that take-up of the eligible population of parents that are required to file taxes is 90% to 95%. Note furthermore that these take-up rates consider families with all ages of children, but the relevant take-up rate of interest for this paper would be take-up among families with newborns. Research shows that take-up of benefits among families with newborns is especially large. For example, twice as many newborns appear in tax returns as 11 year-olds (Dowd and Horowitz, 2011).

Take-up of child-related tax benefits like the EITC is likely high for three reasons. First, the IRS has taken steps to ensure low income households claim EITC benefits. Prior to 1991, the IRS had a policy of offering the EITC to tax filers they deemed eligible even if they failed to claim it (U.S. Government Accountability Office, 1993). After 1991, the IRS switched to mailing tax filers who they concluded might be eligible to remind them of the availability of tax benefits (U.S. Government Accountability Office, 1993). Second, private tax preparers encourage low-income filers to file for the EITC since the tax preparers can claim a fraction of the tax return as compensation (Blumenthal, Erard and Ho, 2005). These arrangements have likely boosted outreach to low income eligible tax payers. Third, as the size of the credit has increased, so has the willingness of families to file to claim it (Blumenthal, Erard and Ho, 2005).

Without administrative data, it is impossible to come up with a precise understanding of how differential take-up might affect the estimated discontinuity in after-tax income used in this paper. Any decrease in take-up would by definition lower the estimated discontinuity. As such, Figure 2 in the paper is best understood as an upper bound on the size of the discontinuity in after-tax income.

A descriptive exercise with the CPS data offers a lower bound. For each year, assume that 10% of newborns are not claimed in tax returns, and assume that these newborns come from families with either zero AGI, or families with the largest possible increases in after-tax income among the families not required to file taxes. Assume an additional 5% of newborns are also not claimed in tax returns, and assume that these newborns come from families who are legally required to file taxes and have the largest possible increases in after-tax income among this population. These percentages follow the results in LaLumia, Sallee and Turner (2015) above, where 10% of newborns were not claimed on taxes because their parents did not file taxes, and an additional 5% were not claimed even through the families filed tax returns. Note that because this adjustment drops observations from the population of filers who see large changes in after-tax income, it maximizes the drop in the estimated discontinuity that comes from this adjustment.

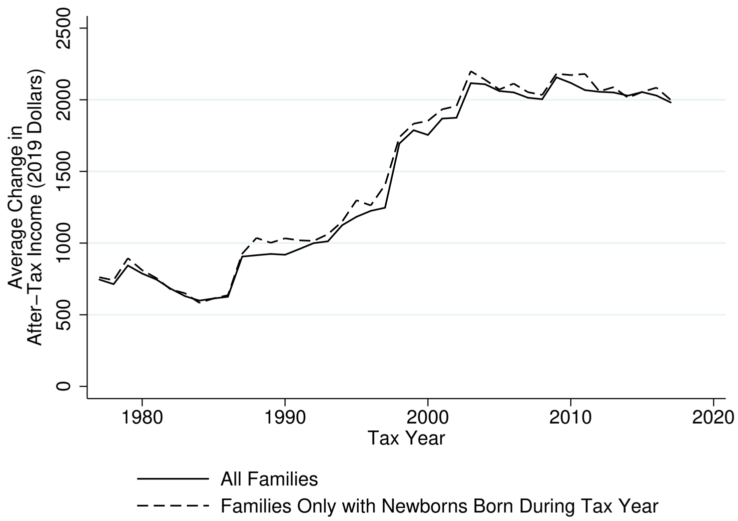
Appendix Figure A.2 below compares the results from this exercise to the estimated discontinuity reported in the paper in Figure 2. As is clear, this process adjusts the estimated discontinuity to be somewhere from 10% to 20% lower depending on the year. The estimated EITC take-up rate in the CPS data after applying these adjustments is 70% to 75%, which is lower than the take-up estimates listed above. Hence, this lower bound is conservative.

This paper does not do similar exercises like Appendix Figure A.2 for the two subgroups analyzed in the paper, children born to families with lower education attainment and Black children. Doing a calculation like Appendix Figure A.2 for these groups would require taking a clear stand on where the newborns not claimed on tax returns come from and their distribution among different demographics. It is not clear how to do such an exercise with available data. It is likely the case that a larger proportional share of these newborns come from families with low education attainment and Black families, as they likely have lower average income at time of a child's birth, and are hence more likely to not be required to file taxes. Hence, the percentage drops could be larger for these groups.

If the true discontinuity in after-tax income across the New Year is lower than was reported in the paper, then that would alter the instrumental variables estimates of the direct effect of income in infancy on later-life outcomes. A lower discontinuity in after-tax income would suggest that the real size of the estimated coefficient in the first stage is smaller, which would suggest that the instrumental variables estimates should be larger (as the denominator α in equation 8, would be lower). The effect of this drop on each instrumental variable estimate would depend on the years included, as the gap in the first stage differs by year. However, as the maximum gap between the upper bound and lower bound in after-tax income in Appendix Figure

A.2 is 20%, that would suggest that instrumental variables estimates in the paper could be at most 25% higher.[43]

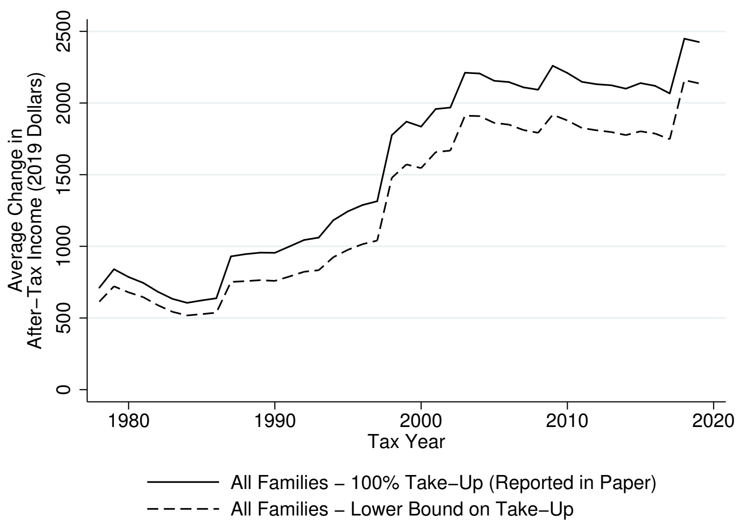
Figure A.1: Robustness of Estimated Average Increase in After-Tax Income from Having Newborn in December Compared to January Under Alternate Samples (2019 Dollars)



Notes: Figure depicts average increase in after-tax income for all families. The solid line is the average increase depicted in the paper. The dotted line uses an alternate subsample of the data, restricting attention to families with children aged 0 in the relevant March CPS year and using only CPS data from the relevant year. Details in the text. Standard error bars omitted for clarity, but standard errors are less than $100 for both lines and for all years.

[43]Note: $\frac{1}{0.8} = 1.25$

Figure A.2: Bounding Exercise for Estimated Average Increase in After-Tax Income from Having Newborn in December Compared to January (2019 Dollars)



Notes: Figure depicts average estimated discontinuity in after-tax income for families for having a child born in December compared to January of the next year by tax year of birth in 2019 dollars. The solid line is the average increase depicted in the paper, and assumes 100% take-up of eligible benefits. The dotted line is a robustness exercise that offers a lower bound on the estimated average increase in family income. Details in the text. Standard error bars omitted for clarity, but standard errors are less than $50 for both lines and for all years.

Appendix B Tax Policies Related to Children

As discussed in the paper, the discontinuity depicted in Figures 2 and 3 reflects four main child-related tax benefits that depend on timing of birth: personal exemptions for a dependent, the EITC, the CTC and the Child and Dependent Care Credit. These four tax benefits have changed substantially over time, but eligibility for them in the first year of a child's life has always been determined by calendar year of birth, with children first eligible for them in the first tax year that they are born.

For all years in the data in Figure 2, parents may claim infant dependents as a personal exemption for a reduction in their taxable income. In tax year 2017, if a parent has a taxable income greater than 0 after applying other deductions, and if that parent has an infant born in December 2017, that parent could reduce their taxable income by up to $4,050. The value of this change in their tax obligations depends on their marginal tax rate. However, it is important to note that this benefit is not refundable, meaning that the additional benefit of the deduction can only reduce a parent's tax obligations to 0. Hence, it provides limited benefits to families that already have low tax obligations.

Starting in 1975, parents were also eligible to claim EITC benefits for infant dependents. This program, over time, has substantially increased the discontinuity in after-tax income from claiming an infant on a tax return. The EITC offers households with earned income above 0 a benefit that gradually increases in income until it reaches a maximum level and eventually phases out to 0. Importantly, this benefit is refundable, meaning that it can both reduce tax obligations and result in a tax refund where a parent receives a refund for the difference between tax obligations and the size of the EITC credit. Following its enactment, the real value of the EITC declined from 1975 to 1986 as the credit was not adjusted annually for inflation (Crandall-Hollick, 2018*b*). Legislative changes since 1987 have gradually made the size of the EITC credit more generous. This increase has happened through both raising the maximum benefit in real dollars, and increasing the number of children for whom tax filers can claim an EITC benefit.[44]

Since 1998, parents with infants who have incomes below a certain level are also eligible for the Child Tax Credit (CTC). Similar to the EITC, the child tax credit is partially refundable, and gradually phases out for tax filers with sufficiently high incomes.

[44] One notable change from 1986 complicating analysis of take-up in this data is the fact that, beginning in tax year 1987, tax filers were required to list the Social Security Number for exemptions for dependents that they claimed. It is well-known that this requirement resulted in a drop of the number of dependents claimed from 77 million in tax year 1986 to 70 million in tax year 1987. Thus, it is possible that there is not as sharp a discontinuity in claiming of dependents around the New Year in years prior to 1987. Parents with children born after the New Year in those earlier years may be claiming them inappropriately regardless of timing of birth. There is no way to accommodate this issue in this data when calculating the increase in after-tax income in Figure 2. This issue would complicate analysis of results because it would suggest that the discontinuity in after-tax income is potentially less sharp in earlier years. However, it should be noted that Table 6 looks at grade-for-age status of high schoolers, and separates the data into children born before and after 1987. As is clear, the measured change in grade-for-age status for being born before the New Year is larger for the cohorts of children born after 1987. Whatever the take-up issues created by this specific policy change in 1987, the same basic causal results are observed for cohorts born afterward.

Technically, there is a fourth infant-related tax credit that parents are eligible for if they have an infant born before December 31st of a tax year: the Child and Dependent Care Credit. Given the lack of information on child care expenses in the CPS, this credit is omitted from consideration here, although it would on average increase the size of the discontinuity in after-tax income.[45]

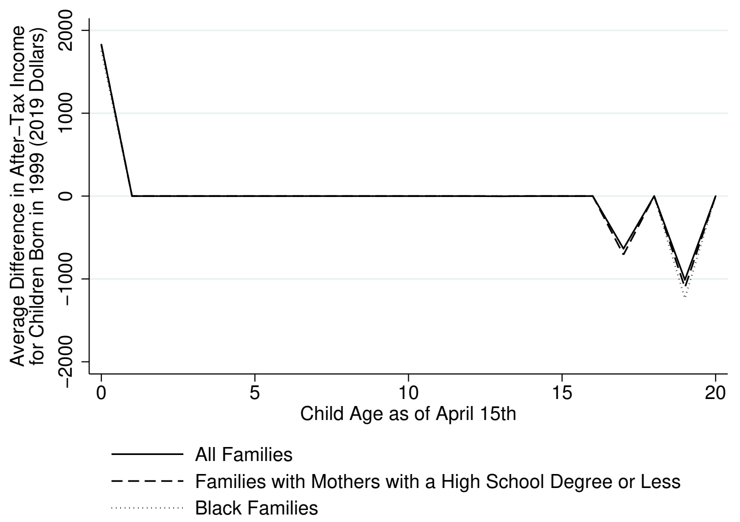
As depicted in Figure 1, eligibility for tax benefits phases out over time as children age. As a result, there are later discontinuities in after-tax income that occur as children reach various ages. For example, as shown in Figure 1, in the calendar year in which children born in December turn 17, their families are no longer eligible to claim the Child Tax Credit for them. However, families with children born in January are still eligible to claim the Child Tax Credit for their children in that tax year.

Appendix Figure B.1 offers an indication of how these changes in eligibility impact after-tax income for families as their children age. This figure looks at the evolution of the gap in after-tax income by child age for the cohort of families with children born in December 1999 or January 2000. This gap is estimated in the March CPS using the procedures discussed earlier in Appendix A. As is clear, when children are infants, families with December births see the increase in after-tax income depicted in Figure 2. In the next year, however, all families are eligible for the tax credits, so the difference disappears.[46] When the children born in December turn 17, however, their families are no longer eligible for the Child Tax Credit for them, so the families with children born in January see slightly larger after-tax incomes. When these children turn 18, there is again no difference in their after-tax income as both groups are eligible for the same tax benefits. However, when these children turn 19, the families with children born in January see slightly larger after-tax incomes, as they are eligible to claim the EITC for these children and the families with children born in December are not.

[45]The average size of this credit among tax filers who claim it is smaller than credits from the EITC and CTC. The average value of the credit is usually $500 to $600 as opposed to over $1,000. It is concentrated among middle and upper-middle income taxpayers, and is claimed by only 13 percent of taxpayers with children. Hence, its impact on after-tax income for the tax discontinuity studied here is likely comparatively small. (Crandall-Hollick, 2018*a*)

[46]This estimation strategy cannot account for changes in income that might happen because of responses to the income shock in infancy. Black et al. (2014) show that a modest shock of a slightly larger size than the shock considered in this work resulted in a long-term change in labor force participation of mothers. If similar dynamics happen here, then there may be a non-zero difference in income in the years after children are infants. This possibility is a direction for future work described in the conclusion.

Figure B.1: Difference in After-Tax Income for December and January Births by Age of Child for Children born in December 1999 compared to January 2000 (2019 Dollars)



Notes: Figure depicts average estimated difference in family after-tax income by child age for families that have a child born in December 1999 compared to January of 2000. Incomes measured in 2019 dollars. Age variable on the horizontal axis lists age as would be recorded by a family on April 15th. For example, newborns in their first year of life born in January and December would be age 0 by April 15th. Estimation process draws inspiration from Hoynes, Miller and Simon (2015) and uses the March CPS. Additional details on estimation are in the text and in Appendix A. Standard error bars here omitted for clarity, but standard errors are less than $10 for all groups and all years.

Appendix C Theoretical Foundations of Birth Shifting

To better understand the choices families make about birth timing and the meaning of the discontinuity described earlier, it is necessary to think about the incentives families face when considering timing births around the New Year. This appendix offers theoretical foundations for two features of the intuition underlying the empirical method. First, there is a limit on how far birth timing is moved by families as, outside of a region around the New Year, there is less incentive to engage in strategic birth-timing. Second, omitting data around the New Year restricts attention to a sample that can identify the theoretical effect of the change in treatment across the threshold.

Consider the following one period family utility optimization problem:

$$\max_{d,C,F,L} \quad V(\delta C, F, L) - f(d - d') - \eta \mathbb{1}[d = 0]$$

$$w.r.t \quad p_C C + p_F F = wL + \mathbb{1}[d < 0]T(wL, d < 0) + \mathbb{1}[d \geq 0]T(wL, d \geq 0) + I$$

Assume that:

$$\frac{\partial V}{\partial C} > 0, \frac{\partial V}{\partial F} > 0, \frac{\partial V}{\partial L} < 0$$
$$\frac{\partial T}{\partial L}_{d<0} > 0, \frac{\partial T}{\partial L}_{d \geq 0} > 0, \frac{\partial^2 T}{\partial L^2} = 0$$
$$V \text{ is concave}$$

In the first equation, C is spending on a newborn, δ is a multiplier on C drawn from a distribution (where higher levels of δ indicate high marginal utility of investments in C), F is spending on the rest of the family, L is a unitary measure of labor for the household, d is the realized date of birth (centered such that $d = 0$ is New Year's day) and d' is the date of birth that would happen without a parent altering the timing of birth, and $f(d - d')$ is a cost function that reaches a minimum when $d = d'$. This term reflects the fact that altering the exact date of birth of a child away from the expected due date, either by Cesearian section or induced labor, is costly to a family in terms of consequences to an infant and a mother's health. Given the relatively smooth distribution of births outside of holidays depicted in Figure 5, assume that d' is randomly assigned. The final term, η is a utility cost to being born on the New Year independent of tax benefits.

$T(wL)$ is an equation representing tax obligations, but the tax schedule differs in this first year depending on whether a child is born before or after New Year's Day. So, there are two separate functions T if d is less than or greater than 0. Assume that, for each level of wL, the after-tax income of having a child before the New Year is greater than having a child after the New Year, or $T(wL, d < 0) > T(wL, d \geq 0)$. Assume that the tax schedule is linear for simplicity. I is a fixed endowment.

Lastly, suppose that the family optimization problem proceeds in the following order:

1. A family chooses L given a certain prior on d', $g(d')$;

2. d' is realized;

3. A family chooses C, F and d to maximize utility with respect to the budget constraint.

Note that the later timing of choices over C, F and d compared to earlier decisions over L reflects the fact that changes in real economic behavior, such as labor supply, are more difficult for births that might happen close to the New Year. Further away from the New Year, there may be more opportunities to alter economic activity after a child's birth.

A critical piece of the family's optimization problem that will determine their decisions is the shape of the cost function for altering birth timing, f. Consider three possibilities:

Case 1: $f(d - d') = \infty$ if $d - d' \neq 0$

Suppose that $f(d - d')$ is infinite for every value except $f(0)$, and keep w, p_C, p_F and $g(d')$ the same for all families. Then, the infinite utility cost associated with altering birth timing means that a family would have no desire to alter birth timing, and families would be randomly assigned on either side of New Year's Day depending on their assignment of d'. In such a scenario, L would be constant for everyone with the same δ, and the additional shock to income given by being bumped into a different tax bracket would be a pure income shock that would both impact investments in C and F. Thus, a simple comparison of people born before and after the New Year will identify the effect of the income boost.

This outcome is depicted in a simulated example in Appendix Figure C.1. Note that the counts of births are relatively smooth, as is average δ. The lack of variation in both variables reflects the fact that no selection across the New Year occurs in this setting.

Case 2: $f'(d - d') = 0$ and $f \geq 0$

Suppose that $f'(d - d') = 0$, and keep w, p_C, p_F and $g(d')$ the same for all families. Then, the lack of a utility cost that varies with d means that families' decisions about birth timing is unaffected by the assignment of d'.

In such a scenario, families' choice of L and d would depend on their value of δ and the value of f. Families that have $d < 0$ would not have any incentive to shift birth timing, as there is no tax benefit to doing so. Among the families that have $d \geq 0$, families with higher δ would be more willing to shift birth timing. They would more highly value the marginal utility of an additional dollar of expenditure on their newborn, and hence would value more highly the value of the tax benefit from being born before the New Year. Importantly, though, families' choices over d would not change depending on d', as the costs to altering birth-timing are constant. Note that the selection here ensures that the families with births before the New Year are different than families with births after the New Year.

This model has important implications for what happens near the discontinuity. First, unlike the infinite cost setting before, actual observed birthdays d will not be randomly distributed, and a larger mass of individuals will move from the days after New Year's Day to the day right before New Year's Day. Second, comparing spending patterns of individuals right before the New Year to spending patterns of individuals born on New Year's day is no longer indicative of the pure income effect of increasing a family's economic resources. The individuals born after the New Year will include people with comparatively low values of δ, indicating that their spending on their infants will be comparatively lower, and the individuals born before the New Year will include people with comparatively higher values of δ, indicating that their spending on their infants will be comparatively higher. Thus, a comparison of their spending will both indicate the pure effect of the increase in after-tax income, but also the difference in the distribution of δ that comes from the people selecting to have births before the New Year having higher marginal utility of spending on children. These differences would mean that a naive comparison of spending on children at the New Year would offer a biased upwards treatment effect.

This outcome is depicted in a simulated example in Appendix Figure C.2. For this graph, assume that each family has a function f that is a constant draw from some distribution. In this situation, there are an abnormally large number of births that happen on the day before the New Year, reflecting shifting of births from families that would have otherwise had births after the New Year. Technically, in this setting, families would be indifferent between scheduling births on the day before New Year's or on any other day before New Year's. As is clear, there are permanently lower births after New Year's, reflecting the fact that families' decisions to alter birth timing is unrelated to d. Furthermore, the average δ of births that happen the day before the New Year is noticeably higher than the days around it, reflecting the fact that the families that move to schedule a birth before New Year's Day have higher δ. Conversely, the children who are born after New Year's have lower average δ.

Case 3: $f(d-d')$ is convex Suppose alternatively that f is convex, and keep w, p_C, p_F and $g(d')$ the same for families. As in case 2, families assigned births d' that are before New Year's Day see no benefit from altering their birth timing as the tax benefits to having a child before the New Year are always larger. So they will continue to select d' as a child's birth date. However, families with $d' \geq 0$ will choose $d = -1$ as long as the utility they achieve from having their birth before the New Year is larger than that they would have if they timed their births after the New Year. That is, as long as:

$$V(\delta C_{-1}, F_{-1}, L) - f(-1 - d') > V(\delta C_{d'}, F_{d'}, L) - \eta \mathbb{1}[d' = 0]$$

Where C_{-1}, F_{-1}, $C_{d'}$, $F_{d'}$ represent consumption choices such that budget sets balance at either $d = -1$ or $d = d'$. As in case 2, families' choice of L and d would depend on their value of δ and the value of f. Taking L and d as given, note that, for any given level of δ , the convex cost in d' means that there is some maximum date past which individuals will not move the timing of their birth. Furthermore, note that for each level of d', the individuals who move the timing of their birth will have larger values of δ, indicating a larger marginal utility of spending on children.

As in case 2, there is selection into birth timing around the New Year. However, for each level of δ, there is some birthdate d' such that no family would move timing of the birth. Thus, dropping birthdates that appear affected by birth shifting and restricting attention to days away from the New Year gives a sample unaffected by the bias created by the uneven distribution of δ. A comparison of spending between these restricted samples would identify, again, the pure income effect of the change in resources on investments in children.

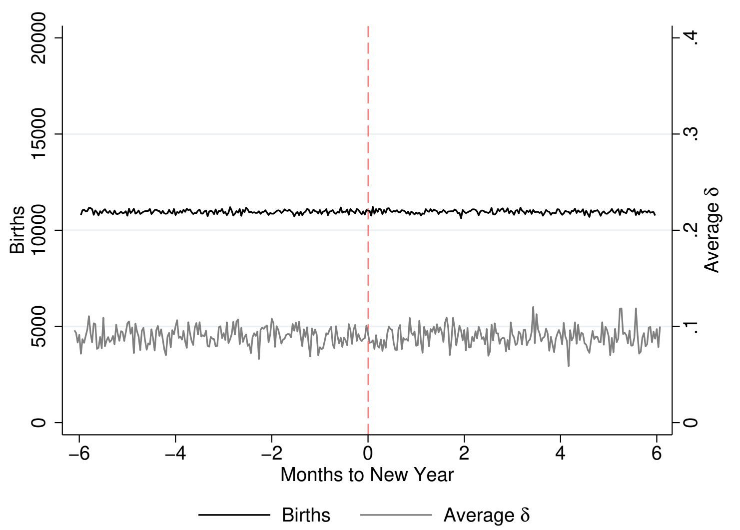
This outcome is depicted in a simulated example in Appendix Figure C.2. Note that there is a massive spike in births on the day before New Year's Day, as this would be the least costly day for families to move timing of birth to.

Some complications of how families perceive the discontinuity are important. First, the analysis in this paper focuses less on immediate spending on children then on intermediate and longer-term outcomes for children, which can be thought of as demonstrating the long-term consequences of that spending. The discussion section at the end touches on how similar income shocks tend to be spent by families in other settings, but there are none directly comparable to the shock in this paper.

Second, the size of the discontinuity in resources will depend on how families understand the tax system. As discussed in the text, this income shock is technically a speeding up of the tax benefits related to children, as families that have children born in December are eligible for the tax benefits one year before families with children born in January, but then their eligibility expires one year earlier as well. If families fully understand this feature of how the system works, then the shock to their spending might be smaller in the short-run, as they could borrow against future earnings (hence increasing I in the model above). As discussed in the text, there is evidence that some share of families misunderstand the timing of how benefits expire in the tax system. Furthermore, the families that benefit from these transfers, especially less educated families, are likely credit constrained, and thus less able to borrow against future income. Both of these features of this setting mean that families with children born in January have limited ability to borrow against future earnings.

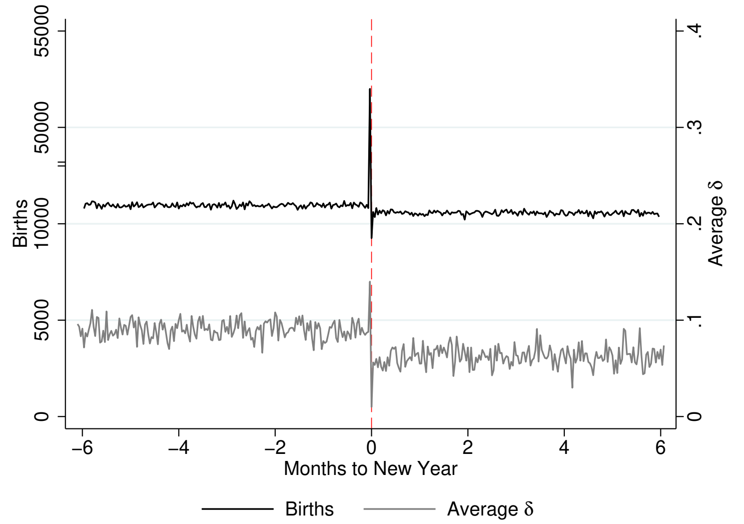
Thus, this setting shows that basic microeconomic theory and simple assumptions about the optimization process can explain the basic intuition motivating the empirical approach in this paper. First, there is limited birth shifting outside of a window around holiday. Second, omitting the data that demonstrate shifting ensures that a comparison of people born after and born before the New Year identifies the effect of the increase in after-tax income.

Figure C.1: Simulation Of Births by Day of Year Under Case 1 for f



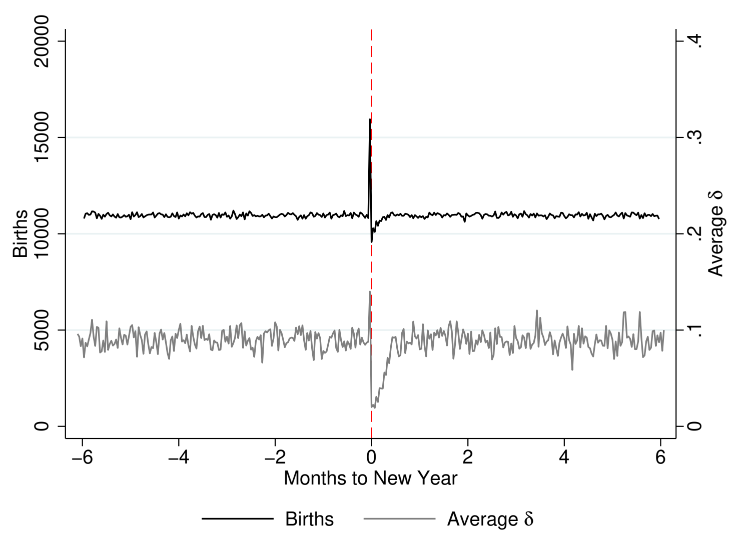
Notes: Graph shows simulated distribution of births by day of year under case 1 for f described above, where $f(d - d') = \infty$ if $d - d' \neq 0$.

Figure C.2: Simulation Of Births by Day of Year Under Case 2 for f



Notes: Graph shows simulated distribution of births by day of year under case 2 for f described above, where $f'(d-d')=0$ and $f \geq 0$.

Figure C.3: Simulation Of Births by Day of Year Under Case 3 for f



Notes: Graph shows simulated distribution of births by day of year under case 3 for f described above, where $f(d-d')$ is convex.