

Effect of Family Income in Early Childhood on Child and Adult Outcomes: New Evidence Using Census Data and Tax Discontinuities

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The U.S. tax code offers sizable tax credits to families with children, and eligibility for these credits depends on a child's birthday. If the child is born before the New Year, that family is eligible for tax benefits related to the child for the previous tax year, but if the child is born after the New Year, the family will be eligible for the benefits starting with the next tax year. These eligibility rules create differences in after-tax income in the first year of a child's life, worth on average approximately \$2,000 for families in tax year 2016. This paper uses 'doughnut' regression discontinuity techniques to calculate the effect of this discontinuity in after-tax family income on outcomes for children and young adults using restricted access Census and ACS data from 2000 to 2016. This paper finds that a \$1,000 discontinuity in after-tax income results in an estimated 0.94 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. This result is largely driven by children from families that are more disadvantaged at a child's birth, including families with low education attainment and Black families. Moving forward to post-schooling outcomes, small differences in labor-force attachment, earnings and education attainment persist for years after the adults leave high school, and are especially large for Black young adults and adults born in counties with lower education attainment, but appear to attenuate with age.

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1 Introduction

A growing literature in social science suggests a sustained relationship between family economic resources in infancy and later life outcomes. Descriptive research from the U.S. shows that children from families that are poorer and less educated are more likely to do poorly in school (Micheltmore and Dynarski 2017, Reardon 2011), less likely to graduate high school (Stark and McFarland 2012, Autor et al. 2019), less likely to earn a college degree (Bailey and Dynarski 2011), more likely to have poorer physical health (Case et al. 2002, Currie 2008), more likely to have experiences in the criminal justice system, including incarceration (Chetty et al. 2018), more likely to earn less as adults (Chetty et al. 2014) and more likely to have lower longevity (Ferrie and Rolf 2011).

However, the causal mechanisms underlying these relationships remains an open question. Outcomes for children likely reflect the resources available to their families, but they also may reflect the preferences of their families over how to raise children and the real and perceived costs of choices they can make in raising children. Disentangling these relationships and estimating the causal effects of family resources has critical implications for policy. For example, if the differences between the school experiences of poorer children and richer children simply reflects economic resources at home, then more robust transfer programs to poorer families could have sizable impacts on later life outcomes (Aizer et al. 2016). However, if the difference reflects decisions families make in raise children regardless of resources, then interventions aimed at better preparing adults for their responsibilities as parents may more effectively address the disparity (e.g. Michalopoulos et al. 2019, Lavy et al. 2016, Gubbels et al. 2016). Lastly, if there are complementarities in the two, then interventions that combine increased resources with incentives for specific behavior may accomplish more conjointly (Miller et al. 2016).

This paper adds to this literature by offering novel evidence of a long-term effect of family resources in infancy on a variety of adolescent and early adulthood outcomes. Specifically, it exploits the discontinuity in after-tax income for families that occurs in the first year of a child’s life depending on the child’s birth timing. If a child is born before New Year’s Day, that child’s family is eligible for tax benefits that result from having a child for that entire tax year, whereas if a child is born after New Year’s, then that child’s family will only be eligible for the tax-related child benefits for that newborn in the next tax year. This discontinuity in tax policy resulted in a \$2,000 average discontinuity in after tax income for families in tax year 2016, with the discontinuity being larger for lower-income families. To estimate the causal effect of this discontinuity, this paper address concerns over endogenous birth timing around the New Year by estimating a regression discontinuity design with an omitted region, where an omitted region is estimated and identified using bunching estimation techniques (Chetty 2011, Kleven and Wassem 2013, Saez 2010). If there are no

other treatments that coincide with the passing of the New Year, and if the evolution of an outcome can be approximated using extrapolation through the omitted region, a discontinuity estimated around New Year's Day identifies the causal effect of the boost in after-tax income on later life outcomes.

The results show evidence of a long-term effect of income in early childhood 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 through school without ever repeating a grade. Being grade-for-age is an indicator that a student has met academic standards and shown social maturity (Xia and Kirby 2009), so improvements in the share grade-for-age indicate multi-dimensional improvements in student development. There is no discontinuity in Kindergarten entrance around the New Year, as children born before and after the New Year enter Kindergarten on-time at similar rates. By the 7th grade, a discontinuity in the share of students being grade-for-age appears, and by high school there is approximately a one percentage point increase in the probability of a student being grade-for-age if a child is born before the New Year. The increase in the share of students who are grade-for-age is primarily concentrated among children whose mothers have a high school degree or less, a population that likely has lower income at the time of the child's birth and sees a larger proportional jump in after-tax income than children with mothers who have higher education attainment. The increase in the share of students grade-for-age is also larger for Black children, and children whose families who are currently in poverty. Reinterpreting this reduced form effect as a direct effect of income, this evidence suggests that an extra \$1,000 in the first year of life increases the probability of the average student being grade-for-age in high school by one percentage point.

This finding is robust to a variety of checks, including alternate records of Kindergarten and first grade eligibility ages and restrictions to students who live in their birth state.

After demonstrating these impacts on outcomes in school, this paper then shows that the effects of this income difference in early childhood persist after high school. In the spirit of Kling et al. (2007), this paper combines income, participation in the labor force, high school degree attainment and usage of SNAP into a single unitary measure of economic self-sufficiency. In the years after young adults turn 19, there are no detectable discontinuities in this measure that exist in the population at large, but there are small discontinuities that exist for young Black adults and adults born in counties with comparably low education attainment. These discontinuities last until young adults reach their early to mid-20s, and slowly fade thereafter, and are largely driven by differences in high school education attainment and earned income. This evidence is consistent with income in early childhood having a small but persistent effect on adult outcomes that gradually attenuates with age as young adults gather more experience in the labor force.

The paper concludes by situating these estimated effects within the existing literature and offering di-

rections for future work. These results suggest a stronger relationship between family income in infancy and later life outcomes than has been previously estimated, but the magnitude of these results fits in line with preexisting correlational results. Furthermore, comparing these estimated relationships to estimated effects of income in other papers shows that, while the estimated effects here are larger, they are still near to other estimates of the effect of income on children in later grades on other metrics of student performance and adult outcomes. Overall, these findings fit within and expand on two interrelated directions of research: research into the origins of the gaps in the development of children that open up before children enter formal schooling and research focusing on early childhood as being a 'critical period' for development.

2 Literature Review and Contribution

This paper draws together two distinct strands of the literature. First, it fits within a growing literature that directly estimates relationships between family economic resources and outcomes for children. Second, it draws from research in public economics on the tax policies that lead to this discontinuity, and research that examines this particular discontinuity.

Empirical research into the relationship between family economic resources and outcomes for children encompasses three distinct branches - the correlational studies described in the introduction, research into the causal effects of government program expansions either through in-kind or cash transfers that alter family economic resources (e.g. Rossin 2011, Carneiro 2011, Miller and Wherry 2017, Bailey et al. 2018, Heckman et al. 2010, Baker 2005, Almond et al. 2011), and research that directly estimate causal relationships between family resources, like income and wealth, and outcomes for children. This paper fits most closely into the third branch of this literature, but the results from this research help inform the other branches as well, as estimates of the direct relationship between family economic resources and child outcomes help identify income effects on child development and allow researchers to separate the substitution and income effects of program expansions.

Much of the research into the direct relationship between family resources and child outcomes relies on estimating instrumental variables models that instrument for family income. Some of these papers use instruments based around occupation characteristics. For example, Shea (2000), using industry instruments and data on characteristics of fathers' jobs to instrument average father's income, concludes that permanent income has a positive impact on children's long-term wages and earnings in low-income families, but not among all families at large. Chevalier (2013), using similar job and policy-based instruments with data from the U.K., concludes that there is a limited effect of contemporaneous income on continuation into post-compulsory schooling, with the effect especially present among lower income families, but the relationship is

weak after using proxy variables controlling for permanent income. However, the instruments used in this research may be suspect, as they may reflect endogenous decision-making of parents that would also impact outcomes for children outside of family income alone.

Other papers have also instrumented for income by using variation in after-tax income. Dahl and Lochner (2012) use the National Longitudinal Survey of Youth (NLSY) and instrument changes in income with changes in the benefit generosity of the Earned Income Tax Credit (EITC) given a family's lagged income, and find a positive relationship between contemporaneous income and student achievement in school where \$1,000 results in an estimated 0.06 standard deviation increase in test scores. In a similar vein, Bastian and Miccheli (2018) use the Panel Study of Income Dynamics PSID and instrument income with changes in cross-state generosity of EITC programs and find that a \$1,000 increase in family income between ages 13 and 18 increases the likelihood of completing high school by 0.2 percentage points, and increases annual earnings in adulthood in the same population by \$57. They also find slightly larger impacts of a \$1,000 increase in family income at ages 0-5 on annual earnings in adulthood. Manoli and Turner (2018) use tax records linked to college enrollment data and instrument income with changes in EITC benefits in a regression kink design, and they find that \$1,000 dollars in contemporaneous family income at high school graduation results in a 1.3 percentage point increase in the probability of enrolling in college. Chetty et al. (2011) use tax records linked to New York school performance data and use the non-linear structure of the EITC with an added polynomial in income (as Rothstein and Nichols (2015) note, this estimation procedure is close in spirit to a regression kink design). Chetty and coauthors estimate that a \$1,000 increase in contemporaneous EITC income increases performance on test scores by 0.06 standard deviations, and they extrapolate that the benefits from this improved performance in school could translate a \$1,000 increase in contemporaneous EITC income into a 0.54 percentage point increase in lifetime earnings. The variation these papers rely on is clearer than the previous papers reviewed, but the reliance on the comparatively small samples in the PSID or NLSY in the first two papers means that these papers are likely somewhat underpowered for analyzing some questions, and the focus on the effects of contemporaneous income makes it difficult to investigate long-term impacts.

A smaller subset of papers use other increases in income or wealth that do not come from changes in after-tax income but are more plausibly exogenous than the earlier occupation-based instrument literature. One strategy is to use changes in income due to regional development that would impact residents of one region more than another. Akee et al. (2010) use the construction of a casino as a reduced form source of variation in permanent income for families eligible for the profits of the casino. The authors find that annual income for treated families increased by about \$4,000 a year from the casino's profits, and conclude that four years of the benefit resulted in an estimated additional year of education and a 40 percentage point

increase in the probability of graduating high school among families with Native American parents that were ever previously in poverty, and a 16 percentage point increase in the probability of graduating high school in the sample at large. Løken et al. (2010, 2012) use regional shocks to income from oil development, and find a non-linear, concave relationship between income and child outcomes, with substantial marginal effects of additional income in the lower part of the income distribution. Bulman et al. (2017) use variation in state lottery earnings in the U.S. to analyze college going decisions, and find a modest relationship between winnings and college attendance, with most winnings less than \$100,000 resulting in no detectable change in college attendance. Similarly, Cessarini et al. (2016) analyze the effects of lottery winnings in Sweden, and find virtually no causal relationship between lottery winnings and a variety of children’s outcomes, except for a slight reduction in obesity risk in adolescence.

This paper adds to this literature by offering evidence from a clearly identifiable shock to after-tax income that has specific properties that complement the existing evidence. First, while researchers often find a relationship between large changes in permanent income and child outcomes, the relationship between changes in transitory income and child outcomes is less clear. Chevalier (2013) argues that the relationship is small, but Dahl and Lochner (2012) and Chetty et al. (2011) find effects on contemporaneous school performance and on standardized test scores, and Manoli and Turner (2018) find effects on the probability of enrolling in college. This paper adds to these estimates by finding both short and long-term effects of a temporary shock in income on children. Second, while the other studies described here are either underpowered when looking at effects of shocks to family income early in life or focus on variation that occurs later in a child’s life, the shock to income that this paper looks at occurs in the first year of an infant’s life. A large literature in psychology and neuroscience suggests that child experiences early in life can have long-term ramifications for development of cognitive and non-cognitive skills (Duncan et al. 2010, Almond and Currie 2010), so focusing on changes in income that happen early in life may reveal more pronounced long-term impacts, especially since the source of variation looked at here is most pronounced for lower income families.

A second related literature to this paper is the research on the EITC and the tax benefits of the particular discontinuity being analyzed here. As will be discussed later, a substantial share of the discontinuity in after-tax income analyzed here comes from the EITC, especially for lower-income families. Compared to the papers discussed earlier that instrument for income using changes in EITC benefits, much of the rest of the EITC research looks at the impact of the implementation of the program, either from the introduction of the program in 1975, or later expansions that increased benefit sizes for different groups. While this literature has largely focused on labor supply responses (Eissa and Hoynes 2004, Bastian 2017, Meyer and Rosenbaum 2001) and outcomes for parents (Evans and Garwaithe 2014), a number of papers have also looked at impacts

on children, finding improvements in infant health and birth weight (Hoynes et al. 2015) and improvements in reported health status of children (Baughman and Duchovny 2016). While the findings in these papers reflect the effects of the income transfer in the EITC, they also reflect the incentives that come along with changes in marginal tax rates under the EITC, so observed responses are best understood as incorporating both income and substitution effects. This paper adds to this literature by examining a source of variation where, as will be argued, the incentives to alter real economic activity to take advantage of the tax code do not change discontinuously. So, the results in this paper show a clear income effect local to the economic decisions made around the New Year.

Lastly, a handful of papers that have looked specifically at parents' responses to this discontinuity. Some of these papers have looked at the degree to which parents may alter the timing of births to take advantage of the tax benefits. Since there are substantial tax-related benefits that tax filers may gain from having a child in December as opposed to January, some tax filers who might otherwise have children in early January have an incentive to alter the timing of births to be just before the New Year. Dickert-Conlin and Chandra (1999) use data from the Panel Study on Income Dynamics and conclude that parents with large potential tax benefits had a high probability of altering the timing of births to take advantage of the tax benefits of being born before the New Year. More recent work from LaLumia et al. (2015) using data from universe of tax filers from 2001 to 2010 concludes that the size of potential tax benefits has a small but positive effect on altering the timing within a narrow window around the New Year. Specifically, restricting to a time period of one week before and after a new year, LaLumia et al. (2015) find that an increase in \$1,000 in potential tax benefits increases the probability of a late December birth by only one percentage point, and this relationship is insignificant in a wider band. This paper fits into this literature by examining how the characteristics of families evolve over the discontinuity and provides demographic evidence complementary to the tax record evidence in LaLumia et al. (2015) that the characteristics of parents evolve smoothly outside of a narrow window around the New Year.

Only one other paper has used this specific discontinuity to look at its effects on parents and children. LaLumia and Wingender (2017) analyze the labor supply response of mothers for having a birth in December compared to January of the next year using the Survey of Income and Program Participation, and find that mothers who receive the benefit have a lower probability of working in some of the months after the child's birth. This paper adds to this research by showing how these tax benefits affect outcomes for children in the medium and long run, and validates a research design that accounts for possible responses.

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. Without access to administrative data on tax records, it is difficult to precisely calculate the discontinuity in income. Figure 1 represents the best approximation to this calculation, which uses March Current Population Survey (CPS) data to estimate the tax benefit.^[1] These estimates are in line with calculations from administrative data; LaLumia et al. (2015) use administrative tax data to estimate that the average tax benefit of having a child in December compared to January from 2000 to 2010 was approximately \$2,100 while this calculation using the CPS data estimates that the benefit over the same period was \$2,150.

Figure 1 clearly shows that this discontinuity is persistent and has been steadily increasing over time. While there is variation in the size of the discontinuity by year, it is still potentially large for many households across many years, and the share of parents with either no change in their tax liabilities or an increase in their tax liabilities is low at around 10 percent prior to 1994 and falling to about 6 percent thereafter. These parents either have zero change in tax liabilities for three reasons: either they have very low income, they have received the maximum of relevant tax credits, or they have very high incomes and high deductions.^[2]

Figures 1 also shows the same averages for families where a child’s mother has a high school degree or less and Black families, as households with parents with lower education and Black families are subgroups this paper will look at later. As is clear, the averages are similar in the early years are similar but they start to diverge slightly as time goes on, reflecting the increased generosity of the tax benefits for children.^[3]

Figure 2 presents these changes in after-tax income as being percentage increases in after-tax income,

¹Specifically, this paper takes all parents of a child two years old or younger, and computes the after-tax return for the family both with and without the child, assigning the family the total income from their household of residence. Ideally, this comparison would only include parents with children 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), but the CPS data do not identify month of birth.

²Inconsistent take-up of benefits in the first year of life is a concern for estimating the size of the discontinuity, but is difficult to adjust for in the specific calculation used here. As documented in LaLumia et al. (2015), approximately 12 to 15% of newborns born in December are not claimed on a tax return. This lack of coverage reflects the fact that not all parents file tax returns, and 5 percentage points of that 12 to 15% 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. LaLumia and coauthors suggest that this lack of coverage may reflect confusion about eligibility for the benefit and timing of receipt of Social Security cards. The average tax return calculations offered here in Figures 1 through 3 include families that see no change in their after-tax income for all the above reasons except for the 5 percent of newborns not claimed. To offer a bound on this potential source of bias, a separate analysis available on request drops the 5 percent of newborns in the simulated data each year whose families have the largest change in tax refunds. As the 5 percent of newborns is likely more broadly distributed through the tax return distribution, this exercise is likely an upper bound. Dropping these returns moves the inflation-adjusted estimated discontinuity in after-tax income by less than 100 dollars for all tax years years up to 2010. Thus, this particular source of bias in the estimated discontinuity in after-tax income is likely small.

³Notably as well, the 25th and 75th percentiles of jumps in after-tax income for these two groups are lower and higher than the 25th and 75th percentiles for the population as a whole, reflecting the fact that more mothers with a high school degree or less and Black families come from households with either very low income such that that family might not file a tax return, or moderately low income such that the family is eligible for a large EITC benefit.

with the lines depicting the average changes in after tax income across the three groups. As is clear, the average increase in after-tax income is generally for families where the mother has a high school degree or less and Black families than it is for all families on average.

The discontinuity depicted in Figures 1 and 2 effects three separate features of how the tax system treats infants: personal exemptions for dependents, the EITC and the Child Tax Credit (CTC). For all years in the data in Figure 1, 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 that parent has an infant born in December 2017, that parent could reduce their taxable income by up to \$4,050. However, this benefit is not refundable, meaning that the additional benefit of the deduction can only reduce a parent's tax obligations to 0. As this was one of the few tax benefits related to children in the years before 1975, the proportional change in after-tax income from having a child born in December compared to January of the next year was comparatively small.

Starting in 1975, the EITC, was added to the tax system and substantively increased the discontinuity in after-tax income from claiming an infant. The EITC increases after-tax income by offering households with earned income above 0 but less than some maximum limit a benefit that gradually increases as income increases 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. As is clear, this change increased the lower bound of the discontinuity in after-tax income. Following its enactment for tax year 1975, the real value of the EITC declined from 1975 to 1986 as the credit was not adjusted annually for inflation (Crandall-Hollick 2018A), but legislative changes since 1987 have gradually made the size of the EITC credit more generous in terms of both an increased maximum benefit in real dollars, and in terms of increasing the number of children for whom tax filers can claim an EITC benefit. ^[4]

Third, 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.^[5] gradually phases out for tax filers with sufficiently high incomes, and has become more generous over time.

Technically, there is a fourth infant-related fourth credit that parents are eligible for if they have an infant before December 31st of a tax year: the Child and Dependent Care Credit. However, given the lack

⁴One 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 as parents with children born after the New Year may be claiming them inappropriately. There is no way to accommodate this fact in this data.

⁵It was not partially refundable until tax year 2001. The CTC is 'partially refundable' because it becomes refundable only for tax filers with income over a certain threshold (Crandall-Hollick 2016).

of information on child care expenses in the CPS, it is omitted from consideration here, although it would on average increase the size of the discontinuity.⁶

However, it should be noted that the discontinuity in after-tax income described here does not persist into the next year, as in the next tax filing year parents of infants born in both December and January will be eligible for the same tax credits and deductions. Furthermore, since parents are only eligible for these tax credits and deductions for a set number of years for a given child, the fact that parents of newborns born in December are eligible for the tax credits and deductions for a year before the parents of newborns born in January means that, several years later, the parents of newborns born in January will be eligible for the tax credits and deductions for one year later than the parents of newborns born in December. So, for example, since children over the age of 19 at the end of the tax filing year are not eligible to be claimed for the EITC (unless they are full-time students under 24), then in the tax year where a child born in December turns 20, the family that could previously claim that child for the EITC benefit will not longer be able to do so. Conversely, the family with a child born slightly later in January of the next year would be able to claim the tax benefit for the child for that tax year. Similarly, parents are no longer able to claim children for the Child Tax Credit after they turn 17, and are no longer able to claim children as dependents after they turn 19 (unless they are full-time students under 24). Thus, the effect of having a child born in December as opposed to January of the next year is largely a speeding up of the tax credit and deduction process for that child.

Intuitively, the sharp discontinuity allows for a comparison of children born before and after December to identify the effect of the change in after-tax income that happens in the first year of a newborn's life. However, such a calculation will depend on how comparable children born before and after the New Year are. Before investigating this question, it is useful to first discuss in greater detail the data for this project.

4 Data

The data in this paper come from three sources - the Current Population Survey, the long form sample of the 2000 Census (otherwise known as the 1-in-6 of 17 percent sample), and the 2001-2016 American Community Survey (ACS). This paper uses CPS data to estimate the size of the discontinuity in after-tax income associated with birth timing as well as patterns in grade repetition, and uses pooled data from the 2000 Census and the 2001-2016 ACS for all other results.

The CPS is a monthly sample of the non-institutional civilian adult population of the U.S. The detailed

⁶The average size of this credit is smaller than credits from the EITC and CTC as it 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, but it would on average increase the size of the discontinuity (Crandall-Hollick 2018B)

information on income in the March CPS provides data for estimating tax obligations using the National Bureau of Economic Research’s TAXSIM calculator. The information on grade enrollment and grade repetition in the October CPS provides the basis for the basis for analyzing 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 approximately 17 percent of the U.S. population (U.S. Census Bureau 2009) or approximately 22 million U.S. households. 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, importantly for this project, 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 (Census 2011). Like the U.S. Census, the ACS covers a variety of demographic and economic data. Many of the questions asked in the long form sample of the 2000 Census and the 2001-2016 ACS are similar, but some question definitions are slightly different. Appendix A covers some of the differences in definitions in more detail and how this paper combines the questions into single measures that can be used across years. As mentioned in the introduction, one of the key outcomes this paper looks at is whether or not a student is grade-for-age. Thus, it is worthwhile to examine in more detail how these surveys ask about education attainment and how grade-for-age status is assigned.

This paper assigns grade-for-age status to students based on three pieces of information: 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 first grade age entrance requirements that require students to have either reached age 5 (for Kindergarten) or age 6 (for first grade) by a specific date before being eligible to enter that grade in that state. Comprehensive data on these state policies for Kindergarten entrance from 1964 to 2005 were collected by Bedard and Duhey (2007), and they generously provided their 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.

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 if they have completed the most recent grade that this measure records a student as having completed.

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 entrance rules. 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. Appendix B describes the sample of states included in calculations by year.

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. To ensure a cleaner comparison so that known treatments do not coincide with birth timing, this paper also omits from consideration states that have these rules by different years.

Third, the response day of a household will affect the grade a student may have completed. In both the Census and the ACS, the education attainment question asks for the highest grade completed by a respondent, or the grade attended within a specific time period. Thus, the date of response to an individual survey matters for determining which grade a student has completed or enrolled in. For example, if a student is in fifth grade in March 2001, then that if that family were responding to the ACS in that month, that family would list that student as having completed the fourth grade. However, if the student progressed to the next grade and the school year ended in May, then if the family responded to the ACS in June, that family would list that student as having completed the fifth grade. To account for this issue, this paper assumes that households that respond to surveys between January 1st and April 10th will still have their children enrolled in the grade that that student would have enrolled in at the beginning of the school year, and households that respond to surveys between July 1st and December 31st will either have their child enrolled in the next grade (if the student passed and is grade-for-age) or the same grade (if the student was retained and is not grade-for age). Potential issues with this assignment process are discussed further in Appendix A.

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. Although the 2008-2016 ACS allow respondents to mark grade completion and grade attendance in all primary and secondary grades, the 2000 Census and 2001-2007 ACS only allow respondents to list whether respondents have completed Nursery School 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 they have recently attended Nursery School, 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 at would be grades where students would be expected to have completed 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. In comparison, if a student is expected to have

completed 6th grade, then whether or not that student has completed 5th or 6th cannot be distinguished from that student’s information in the 2000 Census and the 2001-2007 ACS. More detail on these measures is discussed in Appendix A.

4.1 Birth Timing Patterns

All of this paper’s basic results rely on comparing individuals born before and after the New Year. If the timing of birth were completely exogenously determined, then a simple comparison of outcomes for children born before and after the New Year would suffice for measuring the impact of the boost in after-tax family income depending on birth timing. However, it is well established that parents and doctors have some degree of control over birth timing. Doctors may deliver children using C-section surgery (32 percent of all births in 2017) or by inducing labor through a variety of methods, including the use of drugs (26 percent of all births in 2017) (Martin et al. 2018). Thus, selecting a methodology for estimating outcomes using this discontinuity in policy hinges on understanding patterns of birth timing and accommodating for the fact that some parents exercise some degree of control over birth timing.

Figure 4 shows the distribution of birthdates from July 1st to June 30th in 1994 using the 2000 Census. As is clear, there is substantial variation in date of birth, with the large dips regularly noticeable in counts of births reflecting a fall in births on the weekends. The fall in births on the weekend largely reflects a substantial decrease in C-section surgeries on the weekend, but there is a smaller but still noticeable fall in vaginal births as well (Martin et al. 2010). Also included in the graph is the average number of years of a mother’s education, and as is clear mothers with births 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 while others parents do not to the same degree, or do not have the same preferences over birth timing.

After regression adjusting for day of week in Figure 5, however, the distribution of births and the characteristics of births are much smoother.⁷ However, there are clear disruptions in the distribution of births,

⁷For this regression adjustment, this paper estimates the following model:

$$Y^{birthcount} = \sum_{i=1}^6 \beta_i \mathbb{1}[d = i] + \sum_H \sum_{i=-5}^5 \beta_{iH} \mathbb{1}[d_H = i] + \epsilon \quad (1)$$

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

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

especially around major holidays (including New Year’s Day, Christmas, Labor Day, and Memorial Day). Around these days, there are always fewer births on the holiday alone, and more births on the days around them. Similarly, like births 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, but the average years of education returns to trend quickly after in the days around a holiday.

Focusing in particular on births around New Year’s Day, Figure 6 shows the distribution of births on average from years 1989 to 1994, regression adjusting for day of week. These results focus on an average across years mostly for exposition; results are substantively similar when looking at individual years. As is clear, 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 physicians with some level of control over birth timing moved births to 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 on birth timing, including concerns about hospital staffing. As reported earlier, LaLumia et al. (2015) find limited evidence of shifting in birth-timing around the New Year that correlates with increased after-tax income, concentrated in a window around the New Year⁸

Thus, while there is some scale of birth-timing alteration around holidays, as suggested earlier in the theory section and as demonstrated here in looking at raw patterns, there is a limit on the specific control over birth timing outside of a specific range around holidays. Appendix C offers similar evidence of birth shifting around other holidays.

5 Methods

Evidence in the previous section suggests that the treatment of being 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 Day appears relatively smooth, save for other holidays. Intuitively, while parents can shift births closer to holidays, they may have limited ability to do so further away, either because the costs of shifting are too high, or the benefits to shifting are too low. Appendix D develops microeconomic foundations to justify such a way of thinking, but this intuition inspires a regression discontinuity strategy with an omitted region (often referred to as a ‘doughnut regression discontinuity’).

Specifically, this paper estimates the following model:

⁸Furthermore, LaLumia et al. (2010) show compelling evidence that a large share of the correlation of after-tax income and birth timing may reflect income tax reporting responses rather than tax-motivated shifting.

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

Where Y is some outcome, d is the distance in days to the New Year's, c is the scale of polynomial in d , \mathbf{X} is a list of covariates, and the estimation process includes days in some range $[D_1, D_2]$ but excludes observations in an omitted range of $[\bar{d}_1, \bar{d}_2]$. 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:

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

Following the recommendations in the theoretical literature regarding regression discontinuity estimation, this paper restricts attention to local linear regressions where $c = 1$ (Hahn, Todd, and van der Klaauw 2001) where the estimation process is weighted using a triangle kernel that weighs observations more in the regression process the closer they are to the discontinuity (Fan and Gijbels 1996). To demonstrate the sensitivity of these results, this paper uses a variety of bandwidth choices that restrict attention to smaller regions of d around the cutoff. Demonstrating how these estimates vary more continuously pushes the limits of disclosure of restricted data from the Census Bureau⁹

Before discussing the sufficient conditions this paper builds up and the validation strategies suggested by such conditions, it is useful to first review the typical assumptions for regression discontinuity analyses without omitted regions. As described by Lee and Lemieux (2009) a sufficient condition for a regression discontinuity strategy to consistently estimate β , or the treatment effect of the change in after-tax income, would be that the joint probability of observing various values of d conditional on X and ϵ , or $f(d|X, \epsilon)$ is 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. Furthermore, if this probability distribution is continuous, then Bayes' Rule suggests that the joint distribution of observable covariates X and ϵ should also evolve smoothly:

⁹There is a robust literature on optimal bandwidth selection in regression discontinuity designs (e.g. Imbens and Kalyanaraman 2009) with the goal of minimizing mean squared error in estimated regression discontinuities. This paper splits the difference by showing robustness to different choices of bandwidths.

$$f(d|X, \epsilon) = \frac{f(d, X, \epsilon)}{f(X, \epsilon)} \tag{5}$$

$$f(X, \epsilon|d) = f(d|X, \epsilon) \frac{f(X, \epsilon)}{f(d)}$$

To test whether or not this condition holds in normal regression discontinuity estimation settings without an omitted region, many researcher test whether or not $f(X|d)$ is continuous by, for example, testing whether or not there is a discontinuous change in means at the threshold. For this sufficient criterion to hold, it must be the case that the distributions and means of covariates X evolve continuously around the threshold and do not display detectable jumps¹⁰. As a second related test, some researchers analyze the distribution of the running variable, with the assumption being that lack of smoothness and discontinuities in the density of the running variable that determines treatment indicate control over assignment to treatment, and hence non-random assignment to treatment (McCrary 2008).

In this setting, these traditional assumptions are clearly violated, as there is clear strategic timing of births, and graphical evidence of a change in the education levels of mothers from December 31st to January 1st in Figure 6. However, as long as the region of manipulated birth timing can be identified and dropped from analysis, and as long as the unmanipulated birthdates meet the conditions described before, then estimating the conditional means functions in equation 9 at the New Year should identify the effect of the change in treatment. Effectively, this strategy hinges on restricting attention to observations that do not show manipulation in the running variable, and then assuming that extrapolation of the estimated conditional mean function into the region of the manipulated observations shows the conditional mean function that would be estimated absent manipulation of the running variable. While the method of estimating this region will be discussed, as before, a necessary condition for these assumptions to hold is that the distributions and means of covariates X do not display detectable jumps. The test of the density of the running variable, however, is not operable if the omitted region is large. Instead, this paper argues that the choice of the omitted region can function as a type of density test.

There is no standardized way that researchers identify an omitted region for this form of regression discontinuity estimation. Many papers that use this strategy use ad hoc visual analyses of the size of the manipulated region (Almond and Doyle 2008, Barecca et al. 2011), but some papers suggest more regularized methods. Dahl et al. (2014) are able to use other years where a treatment does not exist as a counterfactual

¹⁰Another sufficient condition for consistent estimation would be that the conditional mean functions on either side of the cutoff $E[Y|d, X]$ are continuous, but there is no way to directly test whether this assumption is true.

to estimate the extent of the regions that are not manipulated. Hoxby and Bulman (2015) suggest a method of estimating the region that should be omitted using locally estimated density functions that estimate a counterfactual density and 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 and outcomes is not as clear as in Hoxby and Bulman for estimating bias. This paper proposes a data-driven method widespread in the public economics bunching estimation literature (Chetty et al. 2011, Saez 2010, Kleven and Wassem 2013). Specifically, this method sets an upper bound on the region of manipulation (after January) and then uses density estimation techniques from those papers to estimate the region of observations that appear to show birth shifting.

To apply this method, this paper takes the regression-adjusted counts of births by day from the 2000 Census for January 1996 to January 1999 and averages births regression-adjusted for day of week (see equations 1 and 2) by day away from the New Year. This step creates a single distribution of births by day for analysis, depicted in Figure 7^[11]

Next, this paper follows a two step process to estimate the scope of manipulated observations:

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

$$Y_d^{birthcount} = \sum_i^c \gamma_i \cdot d^i + \sum_{i=\underline{d}}^{\bar{d}} \psi_i \cdot \mathbb{1}[d = i] + \epsilon \quad (6)$$

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

2. Calculate the counterfactual distribution of births for the days that were omitted from the estimation process:

$$\hat{Y}_d^{birthcount} = \sum_i^c \hat{\gamma}_i \cdot d^i \quad (7)$$

This counterfactual distribution of births here represents the distribution of births that would be

¹¹The 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.

believed to exist in the absence of strategic timing of births.

3. Compare the absolute value of the gaps between the counterfactual distribution and the observed distribution of birth counts:

$$Gap_{\underline{d}, \bar{d}} = \left| \sum_{\underline{d}}^{\bar{d}} (\hat{Y}_d^{birthcount} - Y_d^{birthcount}) \right| \quad (8)$$

Note that $Gap_{\underline{d}, \bar{d}}$ shows the gap between the counterfactual births and the observed births. Kleven and Wassem (2013) recommend that one of the cutoffs be chosen by visual selection, and the other cutoff be chosen that would minimize this gap, as this choice would ensure that the surplus births observed for the days before New Year’s must roughly equal the lost births that occur in the days after New Year’s. Omitting dates that demonstrate shifted births in this manner and isolating attention to births that can be modeled with the counterfactual polynomial can be thought of as finding a region of births where the density of the running variable is smooth.

This paper uses an upper cutoff of the 9th of January. ¹²

5.1 Estimating an Omitted Region

Figure 7 depicts the results from the density estimation procedures described in equations 5, 6 and 7. Following Kleven and Wassem (2013), 9 days after the New Year appears an effective endpoint to the alterations of birth timing. The horizontal lines indicate the limits of the region of days that this procedure suggests should be omitted. As is clear, the estimation process leads to an omitted region of 20 days before the New Year and 9 days after the New Year. The larger estimate of days dropped in December reflects the effect of birth shifting away from Christmas, which also contributes to the bunching of births away from the New Year. As births shifted away from the New Year cannot be distinguished from births shifted away from Christmas, this omitted region corresponds to omitting the entire region of births 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 roughly comparable with the birth timing shifting documented by other papers that look at changes in birth timing to qualify for either cash or program benefits tied to birth timing of children (Gans and Leigh 2009, Neugart and Ohlsson 2013, Dahl et al. 2014)

¹²Note that, because the omitted region needs to be estimated, calculating proper standard errors for any regression means accounting for error introduced by the first step of estimating an omitted region. To do so, this paper will eventually bootstrap the estimation procedure, using a bootstrapped set of estimated cutoffs, and then applying these estimated cutoffs to bootstrapped data, clustering the sampling at the day of year level to accommodate clustering on running variable, as is common in the applied regression discontinuity literature to account for potential model misspecification (Lee and Card 2008). Current standard errors are parametric standard errors clustered on the running variable, but results with bootstrapping are pending disclosure.

and the birth timing patterns observed around major holidays discussed in Appendix C. As is clear visually, the density of births appears to return to a relatively smooth distribution outside of these dates.¹³

5.2 Validating Omitted Regions

Before using the estimated omitted regions for inference, however, it is important to first verify that the characteristics of births before and after the New Year evolve smoothly to validate that this procedure meets the suffice. Table 1 shows the results from regression discontinuity estimates testing whether the characteristics of children’s parents and their households vary discontinuously using the omitted region and three separate bandwidths.¹⁴

As is clear, 11 out of 114 tests are significant at the 5 percent level. These rejections are within the levels that would be expected with tests that have random sampling variation and are independent. Additionally, as these test are likely positively correlated, it is likely the case that the rates of rejection that would be expected at random would be even higher than they are here, further demonstrating the fact that these data fail to reject the null hypothesis that pre-treatment characteristics of parents and children do not display discontinuities. Lastly, it should be noted that most of the rejections take place when using relatively small bandwidths, as when bandwidths of two months or more are used, three out of 76 tests are significant. Hence, these results with this omitted region seem to meet the sufficient covariate smoothness condition implied by equation 5, and the estimation procedure seems valid.

6 Results

6.1 Outcomes in School

Having validated the basics of the empirical strategy, the next step is to use this discontinuity to examine the impact of the tax discontinuity on school outcomes. Given the relative lack of data in the Census and ACS about the well-being of children, the best measure of a child’s well-being recorded in this data is that child’s progression through school. In particular, this paper focuses on grade-for-age status.

¹³A period of 5 days before and after Thanksgiving are omitted from these density calculations, an omission calculated using a similar process as the calculation around Christmas. This omission does not translate to a change in the average density depicted as the timing of Thanksgiving (falling on the fourth Thursday in November) varies from year to year.

¹⁴ Although the results regarding outcomes for children below use pooled data from the ACS and Census, this section uses only the data from the 2000 Census and looks at the characteristics of children and their families for children born in 1999-2000 reported in the Census. The Census data is better suited for looking at these questions than the ACS primarily because the Census data ask for data about income types and levels in 1999 specifically, while the ACS data ask about income in the ‘previous 12 months,’ meaning that parents of newborns born in the previous year, if the parents respond at different months, may post responses that reflect the effects of the treatment. Notably, LaLumia and Wingender (2017) find evidence of a labor supply response. Furthermore, as the sampling structure of the ACS results in responses at different months, the coverage of the total population of children born will be complete for the months before the survey is sent out, but will be incomplete for all months thereafter. Hence, restricting attention to the cohort of children born 1999-2000 in the 2000 Census long form offers the clearest test of whether characteristics differ across for children born across the New Year.

As mentioned in the introduction, a student being grade-for-age is often interpreted as a basic indication of a student achieving academic and social maturity in earlier grades. However, the meaningfulness of any changes in grade-for-age status depends in part on timing of the change. Students will not be grade-for-age if either they delayed entry into Kindergarten, or if they entered Kindergarten when first eligible and were later retained in a grade as they progressed through school. The populations that experience these two treatments are different. Parents who delay their child’s entrance into Kindergarten, referred to as ‘red-shirting,’ tend to have higher incomes and are more likely to be white (Bassock and Reardon 2013). Furthermore, the children who are red-shirted are not more likely to have lower cognitive skills and social maturity before they enter school than children who are not red-shirted (Bassock and Reardon 2013). These trends are often interpreted as showing that the parents who red-shirt children are looking to gain an advantage in school by having their children enter school slightly older than the rest of the children in their grade (Deming and Dynarski 2008). In contrast, the students who repeat grades are more likely to be children of color from less educated and less better-off households (Xia and Kirby 2009).¹⁵ Repetition of a grade is usually interpreted as a negative signal about a student’s social, emotional or academic readiness for the next grade, with students who are retained in grade being more likely to have poorer academic performance prior to retention, lower social skills and poorer emotional adjustment, and more problem behaviors in class, including inattention and absenteeism (Xia and Kirby 2009).¹⁵ Thus, if an increase in the share of students who are grade-for-age reflects a decrease in red-shirting, it is not clear what meaningfulness this change has about students’ preparation for and performance in school, but if an increase in the share of students grade-for-age reflects a decrease in retention, it may be a sign that the treatment has positive impacts on students’ academic performance and gradual social maturity. These questions can be resolved empirically by looking at the grades at which the apparent change in grade-for-age status occur, and the populations that experience retention.

Grade-for-Age Results

Table 2 reports all basic results testing whether children are grade-for-age by grade, with Figures 8 through 15 showing graphical depiction of these regression discontinuities.

In the year that students are eligible for Kindergarten, Table 2 and Figure 8 show that enrollment in Kindergarten or a higher grade in the year of Kindergarten eligibility shows no discontinuity. This result shows that there is no discontinuity in the likelihood of school entrance into Kindergarten or a higher grade

¹⁵Retention policies differ substantially across states, districts and schools, and the students that are retained in one location may not have been retained in another. As of 2014, 6 states have test-based retention policies that require students to repeat a grade if those students have not reached some minimum threshold of achievement (Workman 2014). Even across school districts in the same state, rates of retention can vary substantially (French 2013), as do district policies and implementation of standards (Schwager et al. 1992).

across the threshold, and suggest that any subsequent gap in the share of students grade-for-age reflects differential rates of students repeating a grade. Furthermore, Figure 8 shows an important pattern in the omitted region that is worth noting for subsequent graphs. The students who are born right after the New Year appear to not have entered Kindergarten on time in ways that are discontinuous with the students born right before the New Year. This gap likely reflects the fact that the children born right after the New Year, as demonstrated in Figure 6, appear negatively selected compared to the students born right before the New Year.

Table 2 shows that a gap opens up in the probability of a child being grade-for-age as children enter first grade, but this gap is relatively small, at around half a percentage point, and not statistically distinguishable from 0. As Figure 3 shows, Kindergarten is one of the grades students are most likely to repeat, a decrease in the share of students who are grade-for-age is unsurprising. It is worth noting that this result, unlike the other results discussed here, is relatively sensitive to the size of the omitted region, as with a smaller omitted region the gap is larger and statistically distinguishable from 0 (see Appendix E). These results suggest that a discontinuity has opened up in the share of students grade-for-age, but that discontinuity is likely relatively modest and inconsistent. These results are confirmed when looking at the share of students grade-for-age in 5th grade. 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 discontinuity is depicted in Figure 10. Thus, by 5th grade, the cumulative effects of retention and redshirting patterns for students born before and after the New Year seem to result in only a modest change in the share of students grade-for-age.¹⁶

Moving forward to 7th grade in Table 2 and Figure 11, a larger discontinuity has opened up in the share of students grade-for-age. The regression discontinuity estimate is an 1.02 percentage point increase in the probability of being grade-for-age for students born before the New Year. Again, similar to the transition from Kindergarten into first grade, the increase in the discontinuity here makes sense, given that Figure 3 again shows that there is a gradual increase in retention rates by grade from 5th grade to 7th grade. As is clear in Figure 11, this result is likely somewhat sensitive to the upper bound of dates excluded, but is suggestive evidence of an eventual shift in grade-for-age status taking place.

Lastly, looking at 9th, 10th and 11th grades in Table 2 and Figures 12-14, the discontinuity in the share grade-for-age here appears relatively stable, and slightly larger in magnitude than the discontinuities observed

¹⁶While repetition of Kindergarten may represent a type of red-shirting (e.g. Dynarski and Demming 2009) it is worth noting that the characteristics of children who repeat Kindergarten are on average substantially 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 other grades, have below-average school work, and be described by their teachers as having behavioral issues (NCES 2000).

earlier in the share of students grade for age from the 7th grade. Although there is some variation in the estimated discontinuity in the share of individuals grade-for-age across grades, it is consistently positively signed and generally significant. Furthermore, the results depicted in Figures 12 through 14 become less and less sensitive to the upper bound on dates omitted. As a final measure, Table 2 and Figures 15 show the average discontinuity using all high school year together. These results show that children born just before the New Year are approximately 1.14 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 percent, this is a meaningful shift in grade-for-age status.

Converting these reduced form results into a direct estimate of the effect of the income boost on grade for age status by high school, a \$1,000 increase in income in the first year of life results in a 0.94 percentage point increase in the probability of being grade-for-age by high school.

Heterogeneous Effects for Subgroups

Tables 3 through 5 break these results down further by showing how these results vary by the conditions of children and their families. Here, for concision, the only grades analyzed are grades 5, 7 and then 9, 10 and 11 conjointly. Ideally, data would be available on the characteristics of families at birth so that families could be identified that see larger proportional jumps in after-tax income. However, without such information, identifying high impact samples necessarily depends on choosing information that retroactively could indicate high-impact groups. This paper uses three possible signifiers of a high impact group: Black students, students with current family income below the poverty threshold (given the family size and age of household members), and students with mothers who have a high school degree or less.

When comparing Black children with White children in Table 3, both White and Black children have virtually no detectable discontinuity in 5th grade. For all subsequent grades, both groups show some discontinuity in the share grade-for-age, however in all of these comparisons 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 1.7 percent, while the estimated discontinuity for white children is 1.0 percent. It should be noted, though, that empirical tests for a statistical difference are only occasionally rejected at conventional significance levels, and as Appendix X shows, these results appear sensitive to the size of the omitted region. However, these tests for a difference in discontinuities between White and Black children are likely underpowered given the size of the omitted region and the comparatively smaller number of Black children compared to White children. In all, these results suggest that the discontinuity is larger for Black children than White children, although the magnitude of the difference is unclear.

Table 4 compares children in families with income below the poverty threshold to children in families with income above the poverty threshold. Children in families with income below the poverty threshold see a slightly larger discontinuity in the share of children grade-for-age around the New Year’s in nearly all specifications, but none of the differences are significant. However, the gaps are similar in magnitude to the gaps observed previously when comparing Black and White children, and the discontinuity in grade-for-age status for children in families with incomes below the poverty threshold in high school is significant at the 5 percent confidence level when using the largest bandwidth. Again, as previously, these tests are likely underpowered for two reasons. First, there are fewer children living in households with income below the poverty line than above, as is reflected in the large difference in standard errors. Second, whether or not the family’s current income is below the poverty threshold is likely an imprecise proxy for poverty in childhood. 39 percent of children under 17 experience at least one year with their family where their family’s income falls below the relevant poverty threshold, but slightly less than a third of those children experience “persistent poverty” of at least 8 years of living in poverty (Ratcliffe 2015). Thus, being below the poverty threshold in one year is an imprecise indicator of having been a low income household at the child’s birth.

A more revealing way to divide the sample into subgroups is to separate children by the education level of their mothers. Given the strong lifetime relationship between earnings and education attainment (Tamborini et al. 2015), parent education attainment is likely a stronger correlate of lifetime family resources. Comparing children born to mothers with an education attainment of a high school degree or less to children born to mothers with more than a high school degree in Table 5 shows that a large share of the estimated change in the probability of a child being grade-for-age in high school comes from children with mothers who have comparatively lower education attainment. The discontinuity is a statistically insignificant 0.34 percent for children from families with more than a high school degree, and 1.73 percent for children from families with mothers with a high school degree or less. Furthermore, the difference between the two groups is consistently significant at conventional levels with children in high school. As children with mothers that have a high school degree or less were shown before to see a larger percentage increase in after-tax income, it makes sense that the majority of the increase in the probability of being grade for age would be driven by these students.

6.2 Robustness Checks on Grade-for-Age Results

6.2.1 Conditioning on State of Birth

As noted previously, this paper assigns Kindergarten age eligibility cutoffs to the state in which a child is born, and these cutoffs determine what the appropriate grade-for-age status of a child should be. However, the appropriate state eligibility rules that children face when entering Kindergarten would be those for the

state the child lives in when the child is 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 for state of residence at age 5, and some students may have misaligned grade-for-age status. The danger of the misalignment depends on whether the assigned Kindergarten entrance cutoff is before or after the actual cutoff a student faced. If the actual kindergarten entrance age cutoff a student faced is before the Kindergarten cutoff this paper assigns them (e.g. August instead of September), then it would not bias the grade-for-age status. For example, if a child born in a state that had a Kindergarten age-eligibility cutoff of September 1st moved to a state at age 5 that had an age-eligibility cutoff of December 1st, and the child was born in December, that child would still be expected to be in the same grade to be grade-for-age as if the child had gone to school in another state. On the other hand, if the actual Kindergarten entrance age cutoff a student faces is after the Kindergarten cutoff this paper assigns them, then that error would likely upwardly bias the share of students who are grade-for-age. In the previous example, if the child was born in November, then if that child were grade-for-age, that child would actually have completed the grade above the grade that the child is currently coded as needing to achieve to be grade-for-age. This misalignment would bias the assigned grade-for-age status upward. 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 would be expected to bias the estimated effect on children being grade-for-age upward. The share of students born in states in the sample (with state Kindergarten entrance eligibility cutoffs earlier than December 31st) who move to states that are excluded from the sample by age 5 (with either entrance eligibility cutoffs of January 1st, December 31st, or that districts may choose) is small at two percent, and students born before and after the New Year show no difference in the probability of moving to these states. Thus, the consequences of this error in assignment could bias estimated effects upward, but the effects are likely modest.

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 be known to be correctly assigned the year of expected Kindergarten entrance. Table 6 shows that effects observed among this subsample are nearly identical to those observed in the full sample, if slightly larger. Notably, the control mean of students who are grade-for-age 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, as families who do not engage in interstate migration are more likely to be less educated than families who do (Molloy et al. 2011). Thus, the findings discussed before are robust to whatever error is added from the misassignment of state of residence at age 5.

6.2.2 Conditioning on Subsample of Dates

One concern with the preceding results is that there may be coding errors in assigning Kindergarten entrance dates. As noted before, this paper uses Kindergarten entrance eligibility rules collected by Bedard and Dhey (2007). As noted before, they compiled their data from legislative histories directly and compared their results to Kindergarten entrance eligibility rules collected by other researchers to document and investigate discrepancies, and these data have been used in other papers. However, there may still be errors in the dates they assign. For example, while the Bedard and Dhuey data list New York State as having a statewide Kindergarten eligibility cutoff of December 1st, the Education Commission of the States (ECS) collects its own data on the same policies, and listed New York State as allowing local school districts to choose their own cutoff dates between the ages of 4 and 6. Further investigation shows that New York City allows students to enroll in Kindergarten if they reach age five on or before December 31st, so clearly the Kindergarten eligibility cutoff of December 1st recorded in Bedard and Dhuey is not enforced statewide.

To examine whether the results listed here reflect errors in cutoff assignment, this paper limits attention to states where the Bedard and Dhuey data list a date in the years 1990 to 2010 that agree with the dates listed in the ECS data within a week. The list of states that agree with the Bedard and Dhuey data is listed in Appendix B. This restriction is conservative in the sense that the possibility for bias would be most concerning if the listed eligibility cutoff for a state was sometime before December 31st but the actual cutoff was on December 31st or January 1st, and this restriction throws out more states' data.

Table 7 shows that restricting attention to these states shows a reduction in power (reflecting a decrease in the available data from the sample restriction) but all the main results remain - there is a smallish and statistically insignificant discontinuity when looking at 5th grade, but the discontinuity increases in 7th grade and high school.

6.3 Outcomes in Early Adulthood

Having investigated the consequences of this discontinuity in early adulthood, the next step is to examine what long-term consequences these discontinuities have for outcomes in early adulthood. When extending analysis beyond grade-for-age status in school, the context of the treatment changes. First, as mentioned earlier, there is a second discontinuity in after-tax income that happens as the child ages into adulthood. Parents of children born in December see various tax benefits expire one tax year before parents of children born in January, and research shows that the size of those tax benefits has consequences at that time in a child's life for behavior of families, including enrollment in college (Manoli and Turner 2018) and parent labor force participation (Lippold 2019). Second, when looking at outcomes other than grade-for-age status,

it is important to remember that not being grade-for-age, either due to red-shirting or retention in grade, is both a potential indicator of that child’s progression through school but also a form of a treatment that may have long-term repercussions. Research suggests that the cumulative effects of not being grade-for-age are unclear and likely vary depending on the age at which they occur. Red-shirting and retention in the early grades can have short-term improvements on school achievement (Datar 2004). However, these benefits are presumably traded off against the fact that children with delayed entrance would either be eligible to drop out of school in earlier grades (Deming and Dynarski 2008) or would graduate and enter the labor force later. The effects of retaining students in grade is also an active field of research, with some studies using test score cutoff-based retention policies and showing either no impacts or negative impacts 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). However, other research using the same types of cutoffs in other states finds positive short-term impacts and no impact on graduation (Schwerdt et al. 2017). Thus while the initial treatment in infancy is clear, other treatments happen subsequently that may complicate analyses of outcomes in adulthood.

As the discontinuity in grade-for-age status was concentrated in less educated and poorer households, changes in outcomes in early adulthood would likely be concentrated in these groups as well. However, as children age into young adulthood, a substantial fraction move away from their parents, and thus it is harder to identify children who grew up in disadvantaged households as they get older. This paper uses two strategies to identify these groups. First, this paper looks at outcomes among Black children who, while they did not display consistently statistically different results in grade-for-age discontinuities than White children, had larger point estimates. Second, this paper looks at outcomes for children born in counties that have lower mother’s education attainment on average. Specifically, this paper restricts attention to children born in counties that have average mother’s education attainment in the bottom quarter of the education distribution (weighted by population). As mother’s education levels were a strong predictor of the discontinuity described previously, but no parent education attainment variables are directly observable for children, conditioning on features of counties of birth is the best available proxy for this group of individuals, and strongly correlates with poverty levels in the county.

For relevant later life outcomes, this paper looks at high school completion rates, earned income, labor force participation, and SNAP receipt for children born in the years considered for grade-for-age results in this study forward. Additionally, as these outcomes have more variation than the previous analysis of grade-for-age status, this paper follows Kling et al. (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

O 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 16, 17 and 18 show some of the basic variation in post-high school outcomes by age of adults in high school graduation rates, labor force participation and earned income, respectively. 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. These means just demonstrate the underlying variation in outcomes. As is clear, there is little detectable difference in high school graduation rates, nor in labor force attachment in the population as a whole. There is a slightly more persistent gap in earnings, with people born right before the New Year often earning slightly more than people 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 19 combines all four measures into a unitary measure of economic self-sufficiency for all adults. Note that, by construction, this measure has value 0 for people born in January. Figure 19 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 20 and 21 show similar graphs to Figure 19 for Black young adults, and adults born in counties with comparatively low education attainment, with the measure recalibrated for these samples such that the measure again has 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 .09 to .01 standard deviations, and attenuates over time to low numbers by the time adults reach their late 20s and early 30s. Among Black adults, the gap in the self-sufficiency measure in the early 20s partly reflects differences in high school degree attainment, but over time, high school degree attainment equalizes, and most of the gap reflects a difference in income that again varies from about \$50 to \$500. Full graphs depicting these outcomes for these subgroups are available in Appendix E.

6.3.1 Full Sample

To formalize these comparisons, Table 9 computes regression discontinuities over the conjoint measure of economic self-sufficiency, and each of the four outcomes separately for the full sample. Given the small nature of the effects observed in Figures 16 through 21, it is useful to compile different ages into bins to increase power. While the exact grouping of the bins can be somewhat arbitrary, this paper bins ages into adults aged 19-22, 23-27 and 28-32 just to demonstrate how patterns evolve over time. As is clear in Figures 19 through 21, however, there are individual outliers within age groups that can be important for driving

measured effects. Table 9 shows that the aggregate measure of self-sufficiency shows a small estimated change in the self-sufficiency measure from ages 19-22 of approximately 0.02 standard deviations, but with a side standard error so it is not statistically distinguishable from 0 at the 10 percent confidence level. Moving to ages 23-27, the estimated discontinuity falls to 0.002, again not statistically distinguishable from 0 at the 10 percent level, and then looking at ages 28-32, the estimated discontinuity falls to -0.001, again not distinguishable from 0 at the 10 percent confidence level. These estimated gaps reflect slight differences in labor force attachment and earnings in the early years that fall over time, but again the changes in these specific outcomes are not distinguishable from 0 at the 10 percent level. Taking these point estimates at face value, like Figure 21, they demonstrate a weak treatment effect in early adulthood that falls over time as young adults age into their mid to late 20s.

6.3.2 Black and White Adults

Table 10 computes regression discontinuities for White and Black young adults separately. As is clear, while White young adults display a small estimated treatment effect in ages 19-22 of 0.009 standard deviations, Black young adults display a much larger estimated treatment effect of 0.122 standard deviations. While both estimates are not distinguishable from 0 at the 10 percent level, they are distinguishable from each other at the 10 percent level. This gap for African-Americans is largely driven by gaps in high school graduation rates between young adults born before and after the New Year. However, as was clear in Figure 20, this gap is sensitive to outliers, especially young adults aged 21. Moving to young adults aged 23-27, White young adults again display a treatment effect of -0.017 standard deviations while Black young adults display a treatment effect of 0.099 standard deviations. Both estimates are not distinguishable from 0, and they are not distinguishable from each other at the 10 percent level. The treatment effect for Black young adults is driven by a treatment effect on earned income and labor force participation, both positively signed but not distinguishable from 0 at the 10 percent level. Note that when combining all young adults aged 19-27, the estimated treatment effect for Black young adults is 0.11 standard deviations, a gap statistically distinguishable from 0 at the 10 percent confidence level, and the estimated treatment effect for White young adults is -0.005 standard deviations. Lastly, looking at young adults aged 28-32, the treatment effect for black young adults falls to 0.03 standard deviations, while for whites the treatment effect is -0.03 standard deviations, not distinguishable from 0. These estimated effects for White and Black young adults are distinguishable from each other at the 10 percent confidence level. Overall, then treatment effects are larger for Black young adults than White young adults, and observed treatment effects for Black young adults follow the pattern established earlier, where estimated treatment effects are largest in earlier years and appear to attenuate with time.

6.3.3 Adults Born in Counties With Different Levels of Mothers' Education Attainment

Lastly, Table 11 offers a similar exercise for young adults born in counties with average mothers' education attainment below and above the lowest quartile. When looking at young adults aged 19-22, the estimated discontinuity for young adults born in counties with comparatively low mothers' education attainment is 0.005 standard deviations, and the estimated discontinuity for young adults born in counties with comparatively high mothers' education is 0.02 standard deviations. Again, both estimates are not statistically distinguishable from 0, or from each other at the 10 percent level. These estimated coefficients seem to push against the previous claims that treatment effects are larger for more disadvantaged groups, but they are not distinguishable from 0. Larger effects appear, however, when looking at young adults aged 23-27, as the estimated treatment effect for young adults born in counties with comparatively low mothers' education attainment is 0.09 standard deviations, but the estimated treatment effect for young adults born in counties with comparatively high mothers' education attainment is -.02 standard deviations. Note that these treatment effects are statistically distinguishable at the 10 percent level. The treatment effect for young adults in counties with low mothers' education attainment is driven by differences in earned income and labor force participation. When combining all young adults aged 19-27, the estimated treatment effect is 0.07 standard deviations for adults born in counties with lower average mothers' education, and -0.003 standard deviations for adults born in counties with higher average mother's education. Lastly, looking at adults aged 28-32, estimated treatment effect is -0.12 standard deviations for adults born in counties with lower average mothers' education attainment and -0.01 standard deviations for adults born in counties with higher average mothers' education attainment.

Ultimately, these long-term effects are more suggestive than the previous results, but they appear to tell a consistent story. While effects of the income seem to persist in terms of impacts on education attainment and earnings after turning 18, these impacts appear to attenuate with time as students age into their late 20s and early 30s.

7 Discussion

The effects found in this research show a substantial relationship between income in early childhood and educational outcomes while in school, and these estimated effects appear to persist as differences in income, education attainment and labor force attachment into early adulthood. Few other papers have used such a specific and sharply defined treatment that affects income in the first year of a child's life, so it is difficult to contextualize these findings with similar research, but some comparisons are possible. First, like other papers

that look at relationships between income and later life outcomes, the relationships here using variation in after-tax income are larger than those that are estimated using cross-sectional evidence (Chetty 2011). Some of the difference may reflect a non-linear relationship between family income and student achievement, where changes in income for less well-off families result in larger benefits for children (Løken et al. 2012). Second, while the income shock here is relatively small, other papers find that temporary income shocks of \$1,000 through the same tax benefit mechanism show strong relationships between income and contemporaneous school achievement (Dahl and Lochner 2012, Chetty et al. 2011) that correlate with positive later life outcomes (Chetty et al. 2011). 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, but both Chetty et al. (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 (depending on the specification). It is worth noting that, for a similarly sized shock in childhood, this paper finds a similarly small but in some cases detectable change in the adult outcomes summary index of 0.02 to 0.13 standard deviations.

While test scores are obviously not comparable to the outcomes analyzed here, Chetty et al. (2011) estimate a conversion process from test score increments to future income that allows for a more direct comparison of the effects measured in this paper. Specifically, Chetty et al. (2011) provide estimates of the impact of test scores on later earnings, and then use these estimates to convert the impact of \$1,000 in after tax income on later life earnings. Using their estimates in this manner, their paper predicts that a one standard deviation in contemporaneous test scores raises total earned income from age 20 to age 30 by about 6.3 percent. Hence, Chetty et al. (2011) conclude that a \$1,000 increase in after-tax income results in a 0.38 to 0.57 increase in earnings. Taking this paper’s estimated discontinuities in earned income alone at face value, this paper estimates that \$1,000 increment results in no positive increase in earned income in the sample as a whole, but a 0.56 increase in earned income for Black young adults and a 0.60 increase in earned income for young adults born in counties with comparatively low education attainment. Both estimates, it should be noted, have substantial error bars around them, but nonetheless it is interesting that estimated impacts are within a similar range. , in the two subgroups expected to have the largest impacts in young adulthood, there are

Nonetheless, it would be accurate to say that the estimates in this paper are larger than those found in other research looking at causal variation in family income, especially as those papers tend to look at the impacts of income later in life. For example, Bastian and Micheltore (2018) find no detectable relationship between income in early life (as instrumented by EITC benefits) and education attainment and earnings. However, as mentioned in the introduction, most of the papers that look at causal variation in income have not had substantial power in looking at the effects of income in early life, and the EITC literature is complicated

by potential labor supply responses to EITC benefits. Correlational research into the relationship between income and later life outcomes suggests strong relationships between income in early life and high school graduation. Duncan et al. (1998), for example, estimate logistic models of high school completion and find that a \$10,000 increment in family income in ages 0-5 for low income families (with income below \$20,000) results in a three-fold increase in the probability of graduating high school. Taking their models at face value, their correlational estimate suggests that a \$1,000 boost for these families would increase high school graduation rates by 5 percent, suggesting a large relationship between income early in life for at least some children. This paper finds that a \$1,000 increment in after-tax income results in a 2.1 percent increase in probability of having graduated high school for Black young adults aged 19-27, and an 0.5 percent increase in the probability of having graduated high school for adults who were born in counties with comparably low mothers' education attainment.

As mentioned before, while the results regarding experiences in school are strong, the longer-term results are weaker, but suggestive of benefits for precisely the high impact groups that showed the strongest response to treatment before. As discussed previously, this difference may come from two factors. First, while being retained in a grade is likely an indication of a student's growth and maturation in school, it is also a specific form of a treatment that may have complicated long-term effects on a student's performance later in school and experiences after school. To the degree that the students selected into retention are more likely to benefit from the intervention, retention may actually improve longer-term outcomes.

8 Conclusion

This paper demonstrates compelling effects of family income in early childhood on outcomes in childhood and early adulthood. Specifically, this paper shows that children born before the New Year are on average 0.96 percentage points more likely to be grade-for-age when they reach high school. These results suggest that a \$1,000 increment in income in the first year of life could increase the share of children grade-for-age by the New Year. Outcomes for adults are less clear, but adults in high impact groups that might particularly benefit from the discontinuity in after-tax income show statistically significant impacts on earned income, labor force participation and earned income that appear to persist until their mid-20s. These impacts are within the range of previous estimates

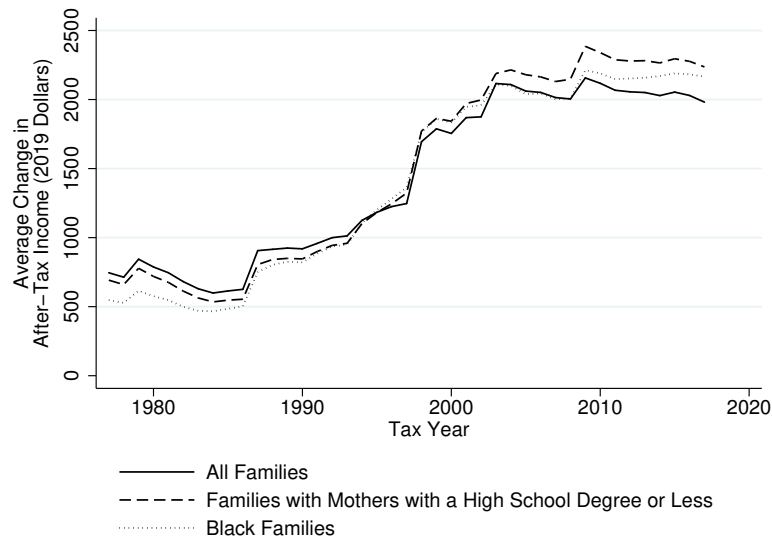
Furthermore, the size of the discontinuity in after-tax income has grown over time, suggesting that future research with other datasets looking at more recent cohorts may find clearer evidence. The results here regarding grade-for-age status reflect mechanisms of school experiences and outcomes that are unobservable in Census data. Future work with large administrative databases may confirm these results and track

students' progression through school and early life experiences to assess the meaningfulness of the change in grade-for-age status documented here. Future work also can help clarify pathways through which this treatment occurs.

In all, these results suggest that changing the resources available to low-income families can result in long-term improvements in the conditions of children as adults, and points the way towards similar interventions specifically aimed at providing resources to parents of young infants. Additional experiments should examine whether the effects observed here might also be expected at other ages.

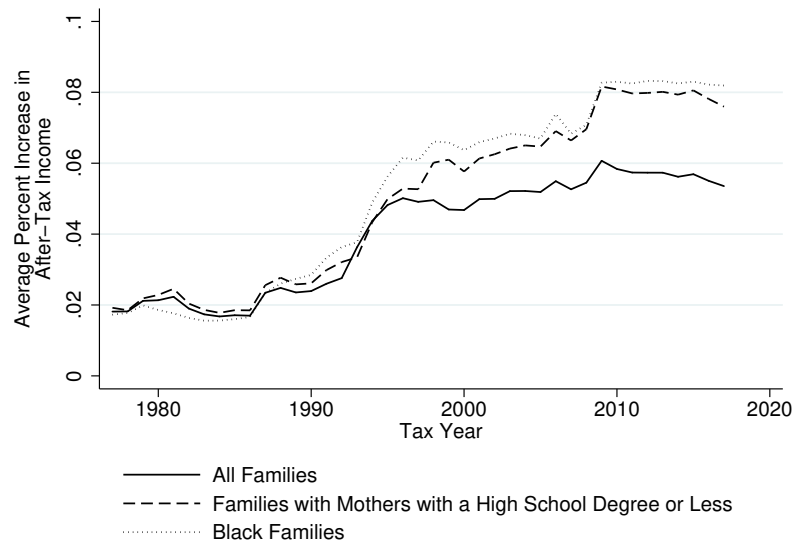
Figures and Tables

Figure 1: Percentiles of Tax Benefit from Having Newborn in December Compared to January



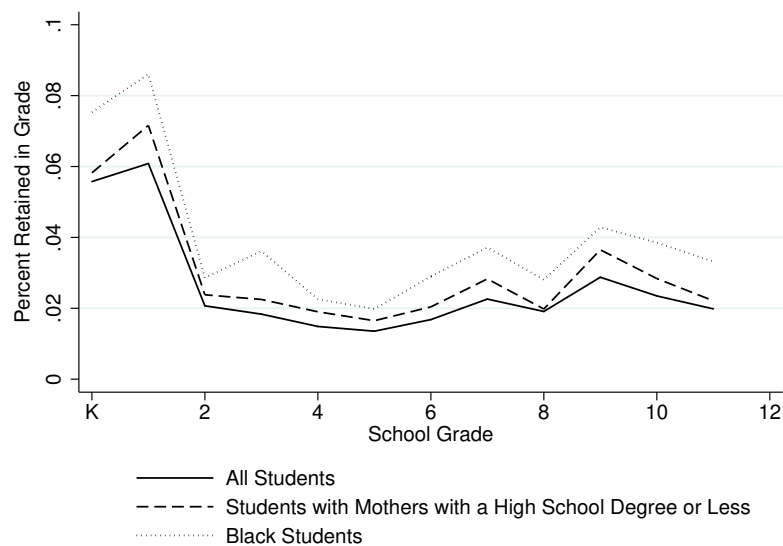
Note: Figure depicts average estimated discontinuity in after-tax income for families for having a child born in December compared to January by tax year of birth in 2018 dollars. The estimation process draws inspiration from Hoynes et al. (2015). The sample of observations used in the calculation are all parents with an infant under three who are in the March Current Population Survey (CPS) in a four year radius for the year after a given tax year (e.g. 1984 - 1990 March CPS files for the 1986 tax year), with all types of income inflation-adjusted to the relevant tax year. Tax obligations for having a child born in December calculated by summing income measures at the household level and calculating the total state and federal tax burden assuming that the family with the infant under 3 are the relevant tax filing unit. Tax obligations for having a child born in January calculated with the same income measures, but reducing the number of dependents under the age of 13 by one (as the infant is born after December). The tax discontinuity is then the difference between the two calculated tax obligations. More issues with the calculation process are described in the text. Standard error bars here omitted for clarity. Figure depicts average increase in after-tax income for all families, for families where the mother has a high school degree or less, and Black families.

Figure 2: Average Percent Increase in After-Tax Income from Having Newborn in December Compared to January (2019 Dollars)



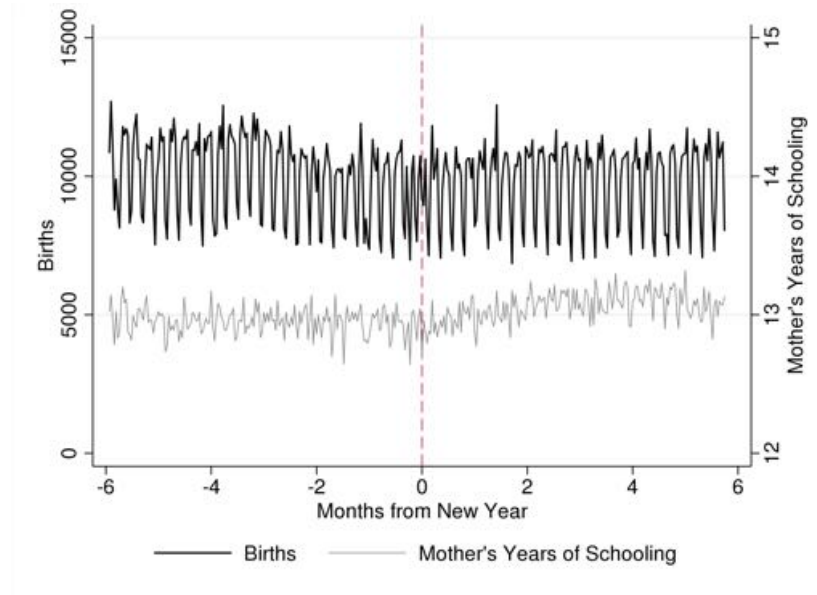
Note: Figure depicts percent increase in after-tax income for all families, for families where the mother has a high school degree or less, and Black families, for having a child born in December compared to January by tax year of birth in 2019 dollars. Same estimation process as described in Figure 1. Standard error bars here omitted for clarity.

Figure 3: Average Share of Students Retained in Grade - 1990-2005



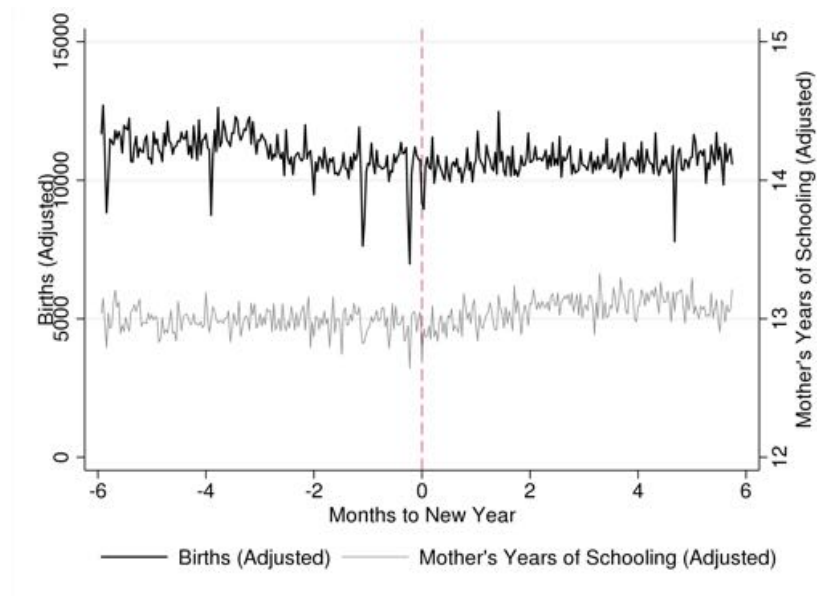
Note: Figure depicts average share of students retained in each grade over the years 1990 to 2005 estimated in the October CPS. Standard error bars here omitted for clarity.

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



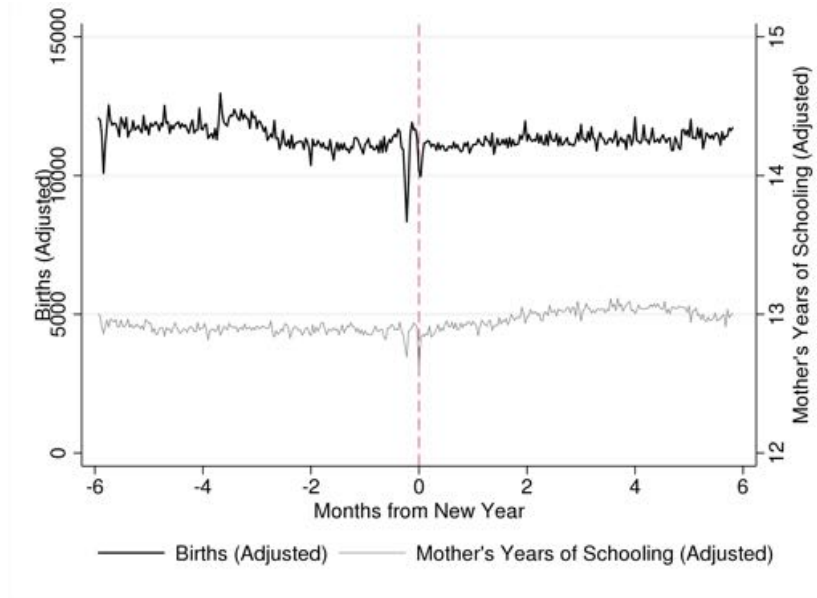
Note: Figure depicts birth counts by day of year estimated in the 2000 Census from July 1st 1996 to June 30th 1997, centered on the New Year in 1997.

Figure 5: Births by Day of Year Adjusted by Day of Week - 1996 to 1997



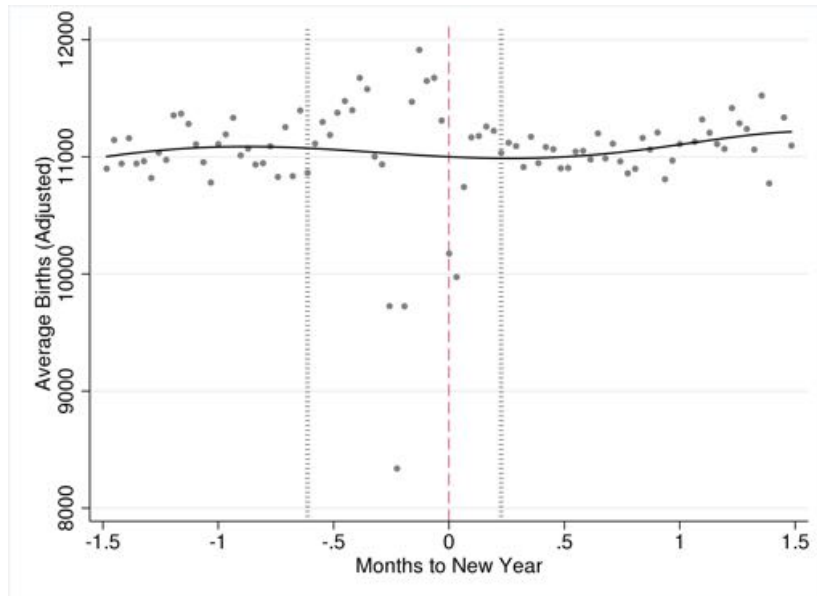
Note: Figure depicts average births by day of year estimated in the 2000 Census from July 1st 1996 to June 30th 1997, centered on the New Year in 1997. Counts of births have been adjusted by day of week.

Figure 6: Births by Day of Year Adjusted by Day of Week



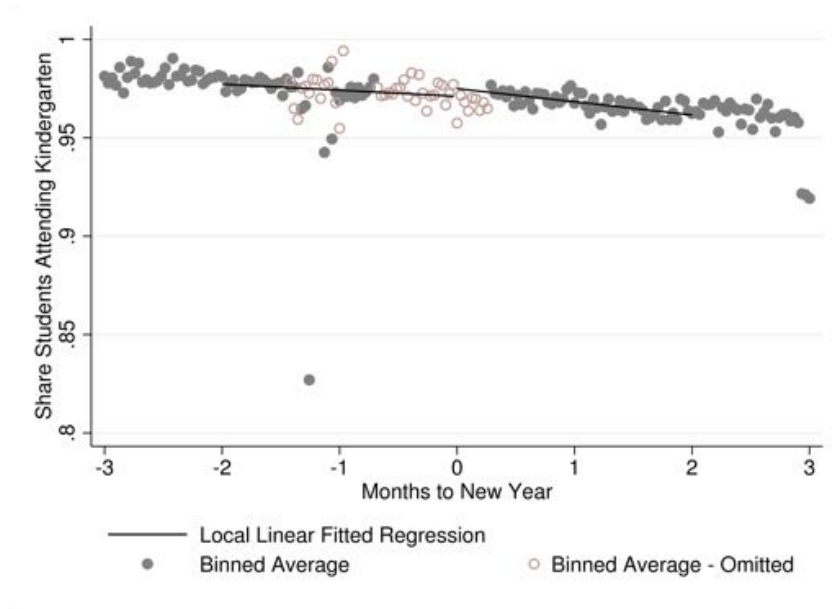
Note: Figure depicts average births by day of year estimated in the 2000 Census from July 1st 1996 to June 30th 1997, centered on the New Year in 1997, and regression-adjusted for day of birth following equations (1) and (2) in the text.

Figure 7: Estimated Birth Timing Manipulation



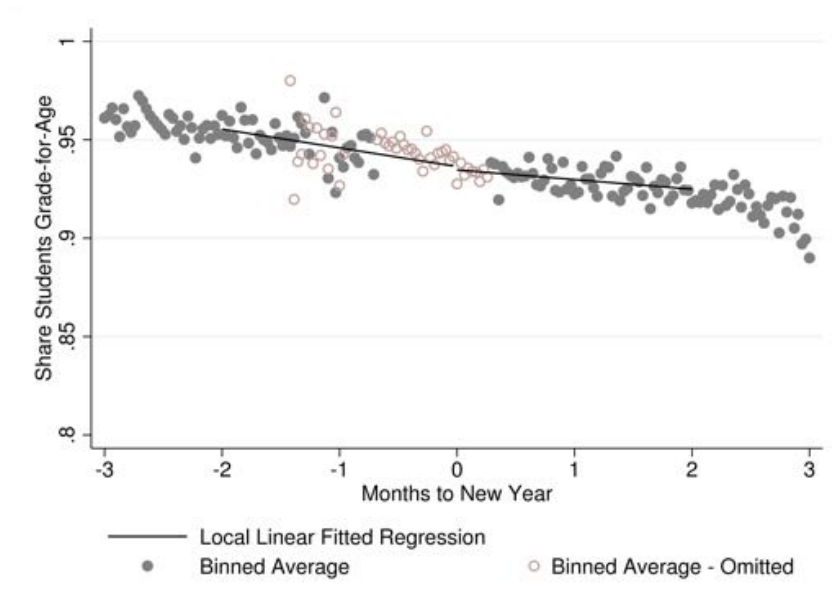
Note: Figure depicts average births by day of year from 1989-1994 regression-adjusted for day of birth following equations (1) and (2). Vertical bars indicate manipulation region omitted from calculation. Upper bound selected visually at 9 days after the New Year. Lower bound selected through estimation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

Figure 8: Estimated Discontinuity in Share of Students Attending Kindergarten



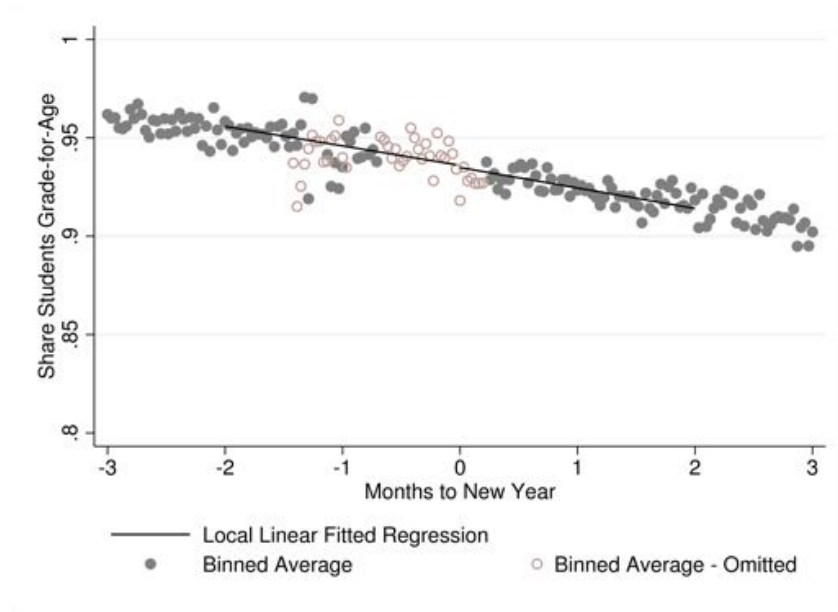
Note: Figure depicts discontinuity in share of students attending Kindergarten. Red empty circles are data omitted from estimation process, and grey solid circles are data that was included. The estimated line uses a bandwidth of two months around the New Year. See Table 2 for point estimates. Estimation process detailed in text.

Figure 9: Estimated Discontinuity in Share of Students Grade-for-Age - 1st Grade



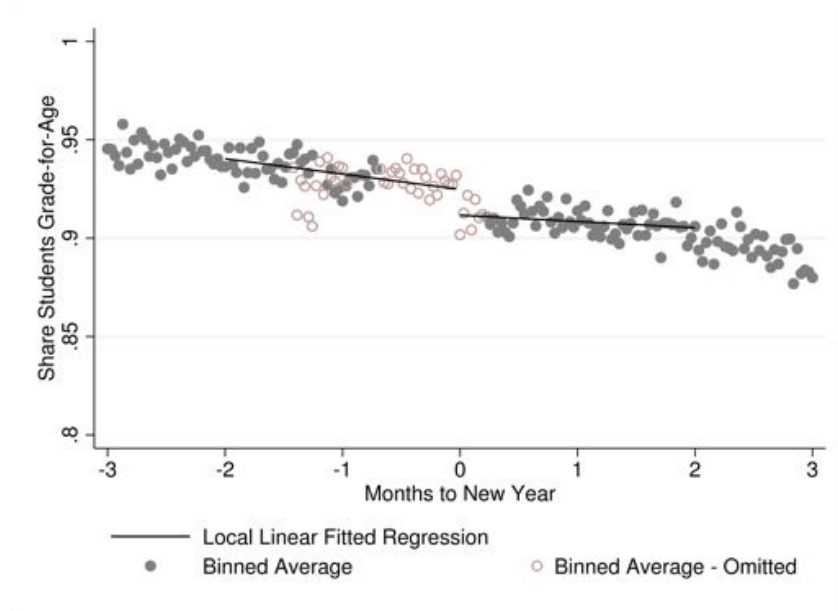
Note: Figure depicts discontinuity in share of students grade-for-age in 1st grade. Red empty circles are data omitted from estimation process, and grey solid circles are data that was included. The estimated line uses a bandwidth of two months around the New Year. See Table 2 for point estimates. Estimation process detailed in text.

Figure 10: Estimated Discontinuity in Share of Students Grade-for-Age - 5th Grade



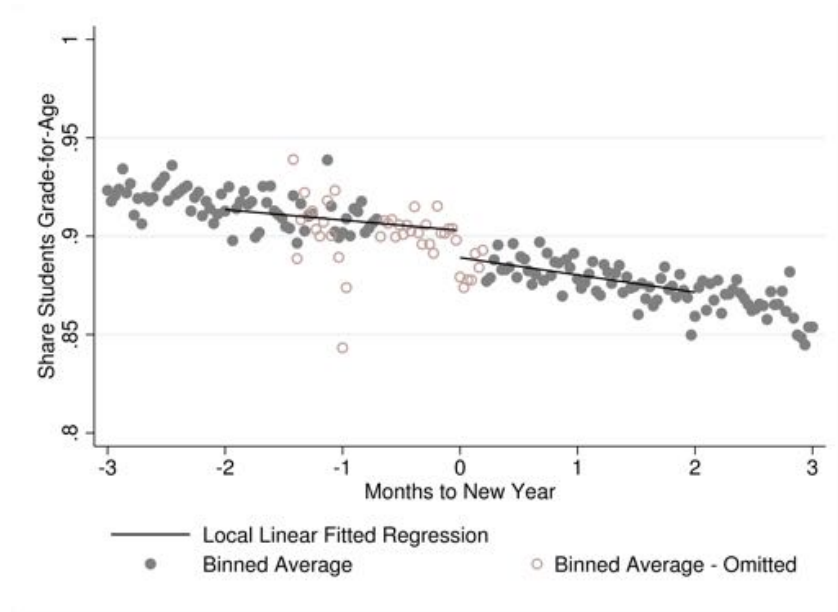
Note: Figure depicts discontinuity in share of students grade-for-age in 5th grade. Red empty circles are data omitted from estimation process, and grey solid circles are data that was included. The estimated line uses a bandwidth of two months around the New Year. See Table 2 for point estimates. Estimation process detailed in text.

Figure 11: Estimated Discontinuity in Share of Students Grade-for-Age - 7th Grade



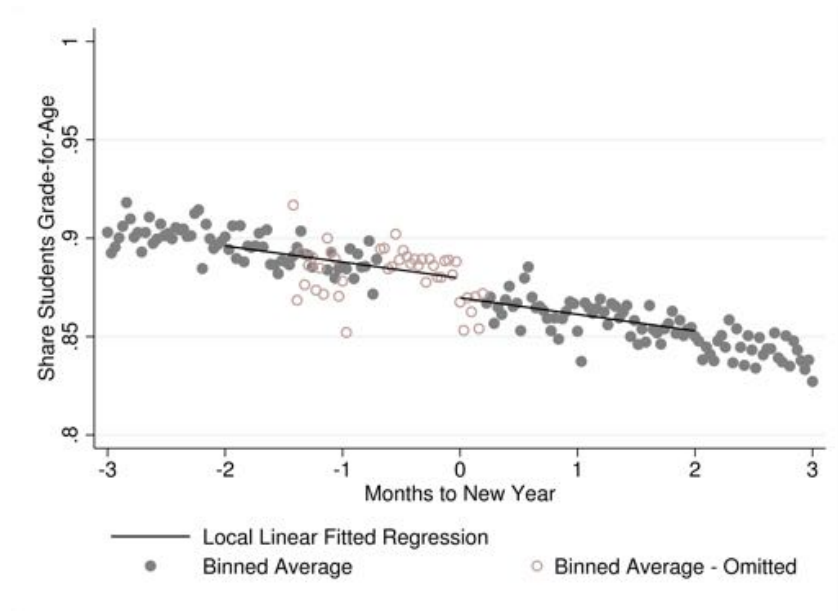
Note: Figure depicts discontinuity in share of students grade-for-age in 7th grade. Red empty circles are data omitted from estimation process, and grey solid circles are data that was included. The estimated line uses a bandwidth of two months around the New Year. See Table 2 for point estimates. Estimation process detailed in text.

Figure 12: Estimated Discontinuity in Share of Students Grade-for-Age - 9th Grade



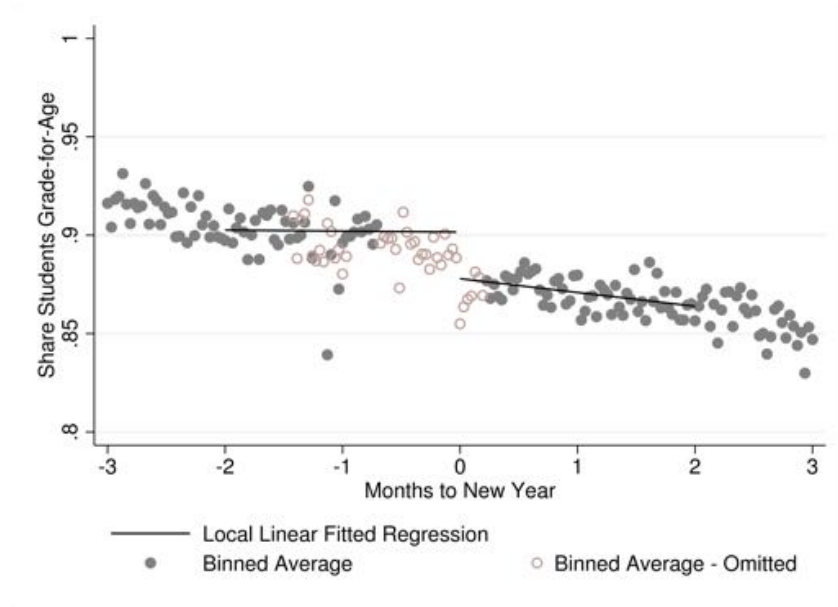
Note: Figure depicts discontinuity in share of students grade for age in 9th grade. Red empty circles are data omitted from estimation process, and grey solid circles are data that was included. The estimated line uses a bandwidth of two months around the New Year. See Table 2 for point estimates. Estimation process detailed in text.

Figure 13: Estimated Discontinuity in Share of Students Grade-for-Age - 10th Grade



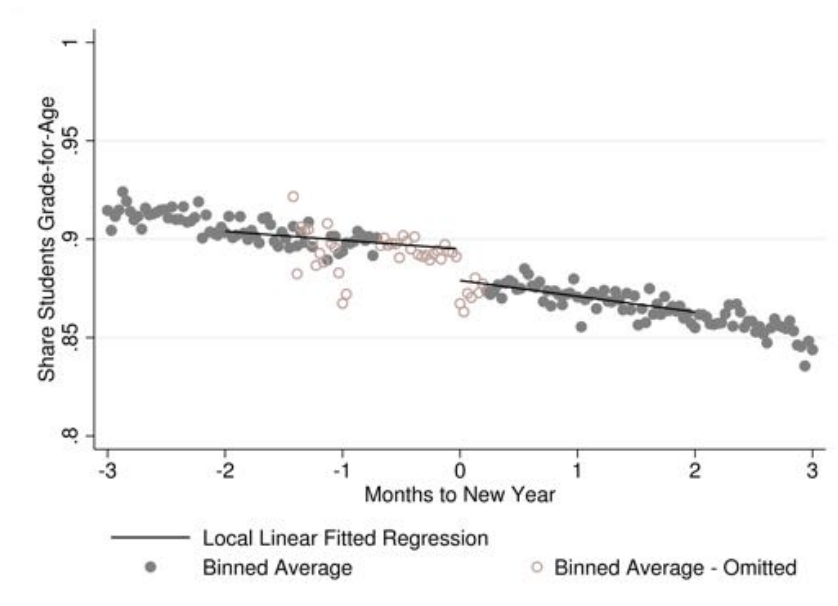
Note: Figure depicts discontinuity in share of students grade-for-age in 10th grade Red empty circles were omitted from estimation process. This figure depicts the regression discontinuity estimated under the larger omitted region with a bandwidth of 2 months. See Table 2 for point estimates. Estimation process detailed in text.

Figure 14: Estimated Discontinuity in Share of Students Grade-for-Age - 11th Grade



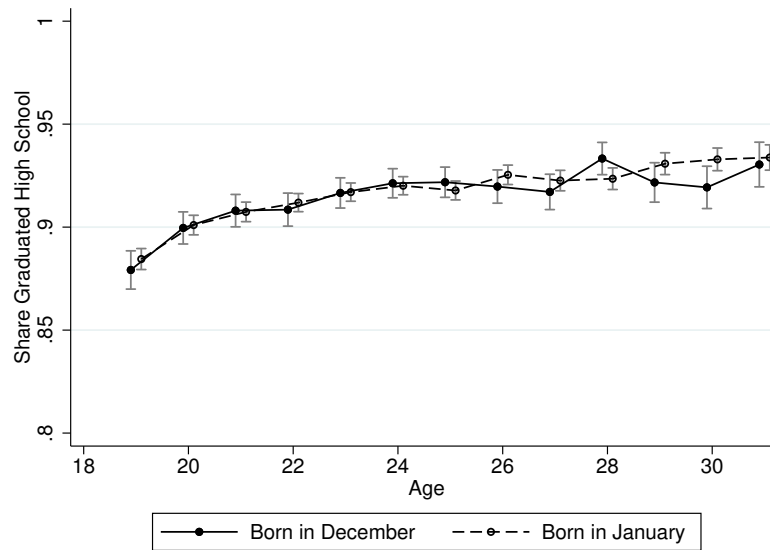
Note: Figure depicts discontinuity in share of students grade for age in 11th grade. Red empty circles are data omitted from estimation process, and grey solid circles are data that was included. The estimated line uses a bandwidth of two months around the New Year. See Table 2 for point estimates. Estimation process detailed in text.

Figure 15: Estimated Discontinuity in Share of Students Grade-for-Age - 9th-11th Grade



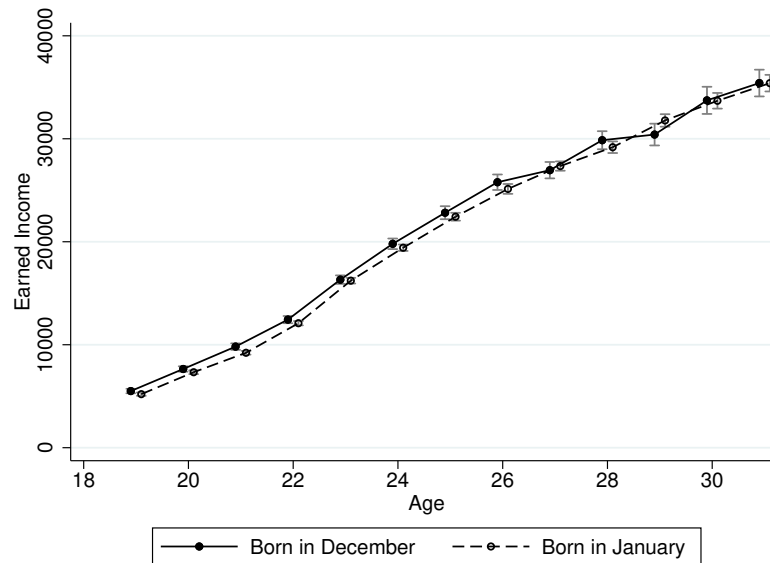
Note: Figure depicts discontinuity in share of students grade for age in 9th through 11th grade. Red empty circles are data omitted from estimation process, and grey solid circles are data that was included. The estimated line uses a bandwidth of two months around the New Year. See Table 2 for point estimates. Estimation process detailed in text.

Figure 16: Share of Sample that have Graduated High School by Age Group and Birth Timing



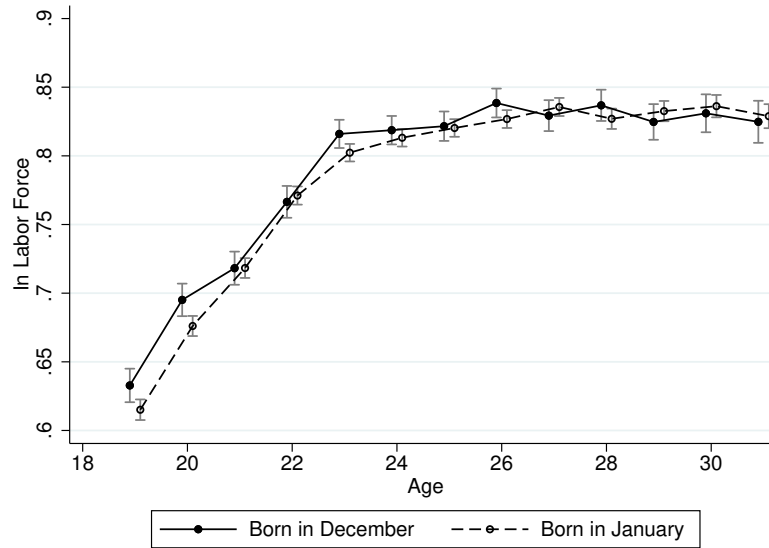
Note: Figure depicts average trends in high school graduation status in the sample by age, omitting adults born December 11th through January 9th.

Figure 17: Average Earned Income by Age Group and Birth Timing



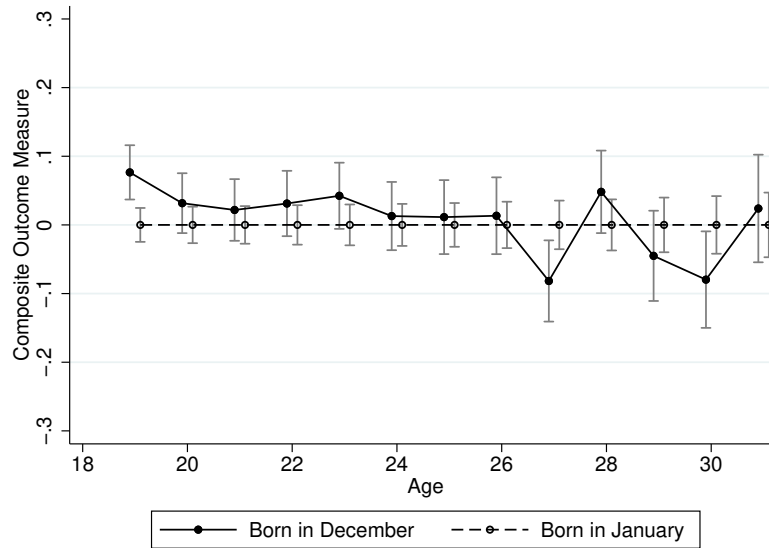
Note: Figure depicts average trends in earned income in the sample by age, omitting adults born December 11th through January 9th.

Figure 18: Labor Force Participation Rates by Age Group and Birth Timing



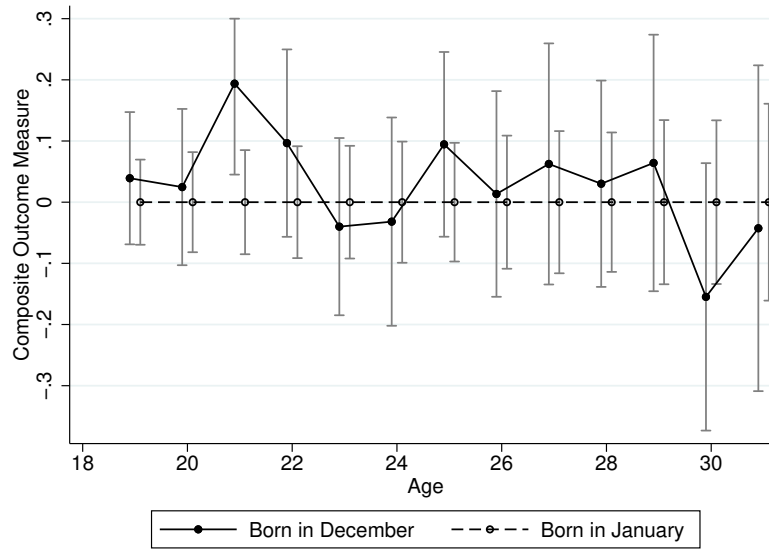
Note: Figure depicts average trends in labor force participation rates in the sample by age, omitting adults born December 11th through January 9th.

Figure 19: Composite Measure of Outcomes by Age Group and Birth Timing



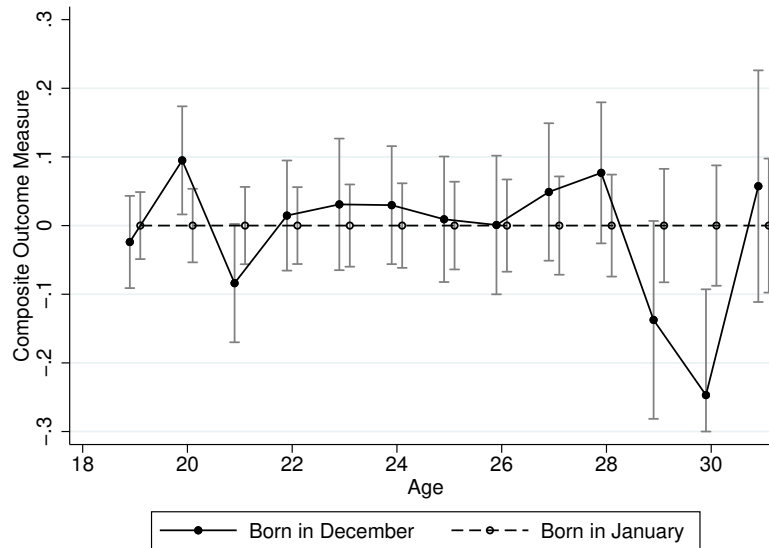
Note: Figure depicts average trends in a composite measure of adult outcomes by age, omitting adults born December 11th through January 9th. The composite measure reflects labor force participation, earned income, SNAP receipt and high school graduation status. The process that creates this measure is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

Figure 20: Composite Measure of Outcomes by Age Group and Birth Timing for Black Adults



Note: Figure depicts average trends in a composite measure of Black 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 measure is described in detail in the text. Note that the measure takes on value 0 for individuals born after the New Year by construction.

Figure 21: Composite Measure of Outcomes by Age Group and Birth Timing for Adults Born in Counties with Average Mothers' Education Attainment in Lowest Quartile



Note: Figure depicts average trends in a composite measure of adult outcomes by age for adults born in counties with average mothers' education attainment in the lowest quartile, 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 measure is described in detail in the text. Note that the measure takes on value 0 for individuals born after the New Year by construction.

Table 1: Validating Regression Discontinuity Procedures

	Outcome	Control Mean	Regression Discontinuity Treatment Effect Estimates by Bandwidth		
			Omit December 11th - January 9th		
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth
	Child is White	0.725 (0.001)	-0.0410 (0.0258)	-0.0227* (0.0119)	-0.0172* (0.0097)
	Child is Black	0.117 (0.001)	0.00140 (0.0129)	0.00400 (0.0066)	0.00240 (0.0055)
	Child is non-White, non-Black	0.159 (0.001)	0.0396** (0.0193)	0.0187** (0.0092)	0.0147* (0.0077)
	Child State of Residence Same as Birth	0.955 (0.001)	-0.00430 (0.0101)	-0.00240 (0.0053)	-0.00480 (0.0042)
	Total Children in Household	1.937 (0.001)	-0.0480 (0.0466)	-0.0295 (0.0218)	-0.0299 (0.0197)
	Child Lives with Both Parents	0.706 (0.001)	0.00980 (0.0235)	-0.00900 (0.0122)	-0.00560 (0.0093)
	Child's Household Has Any Earned Income	0.807 (0.001)	0.0457** (0.0178)	0.0144 (0.0092)	0.00600 (0.0077)
	Child's Household Has Any Other Income	0.112 (0.001)	-0.00510 (0.0130)	0.000500 (0.0078)	0.000600 (0.0061)
	Child's Household Has Any Retirement Income	0.0300 (0.001)	-0.00390 (0.0066)	0.00290 (0.0039)	0.00440 (0.0031)
	Child's Household Has Any Supplemental Income	0.0150 (0.001)	0.00300 (0.0058)	0.00330 (0.0035)	0.00310 (0.0030)
	Child's Household Has Any Welfare Income	0.0600 (0.001)	-0.0138 (0.0215)	-0.00200 (0.0105)	-0.00340 (0.0081)
	Child's Household's Earned Income	41500 (71600)	2300 (1700)	474.8 (700)	79.18 (800)
	Child's Household's Other Income	469.8 (182.3)	-8.781 (85.16)	4.689 (53.63)	20.45 (42.86)
	Child's Household's Supplemental Income	84.83 (23.32)	11.92 (30.15)	13.89 (19.57)	14.39 (16.46)
	Child's Household's Total Income	42000 (84000)	1600 (1600)	1300 (843.7)	814.7 (712.7)
	Child's Household's Wage Income	39500 (68500)	1300 (1800)	70.91 (950.9)	-137.6 (790.2)
	Child's Household's Welfare Income	119.3 (19.20)	-72.69** (35.38)	-27.29 (19.45)	-18.18 (15.35)

Table 1 Continued: Validating Regression Discontinuity Procedures

Outcome	Control Mean	Regression Discontinuity Treatment Effect Estimates by Bandwidth		
		Omit December 11th - January 9th		
		1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth
Maximum Age of Parents	30.72 (0.002)	0.142 (0.3087)	0.103 (0.1557)	0.0206 (0.1246)
Maximum Wage Income of Parents	0.880 (0.001)	0.0174 (0.0113)	0.00170 (0.0069)	-0.00160 (0.0055)
Maximum Welfare Income of Parents	0.0480 (0.001)	-0.00960 (0.0132)	-0.00240 (0.0080)	-0.00540 (0.0066)
Maximum Wage Income of Parents	33000 (54500)	999 (1600)	1000 (848.2)	806 (670.9)
Maximum Education Attainment of Parents	13.68 (0.001)	0.136 (0.1117)	-0.00610 (0.0629)	-0.0222 (0.0489)
Either Parent is in Labor Force	0.897 (0.001)	0.00260 (0.0104)	-0.00300 (0.0068)	-0.00250 (0.0049)
Maximum Usual Hours of Work of Parents	41.24 (0.013)	0.248 (0.9542)	0.0283 (0.5250)	-0.0144 (0.4227)
Maximum Weeks of Work Last Year of Parents	43.04 (0.013)	0.842 (0.9000)	-0.0117 (0.4911)	-0.0482 (0.4152)
Either Parent Worked Last Year	0.936 (0.001)	0.00840 (0.0081)	0.00260 (0.0052)	0.00340 (0.0040)
Age of Mother	28.44 (0.002)	0.428 (0.3302)	0.0868 (0.1772)	0.0229 (0.1443)
Mother Has Any Wage Income	0.681 (0.001)	0.0548** (0.0236)	0.0192 (0.0133)	0.0111 (0.0109)
Mother Has Any Welfare Income	0.0480 (0.001)	-0.00810 (0.0122)	-0.00660 (0.0072)	-0.00860 (0.0060)
Mother's Wage Income	15000 (26500)	2900*** (1000)	1300** (567.4)	850.0* (466.8)
Mother's Education Attainment	13.27 (0.001)	0.3927*** (0.1312)	0.0721 (0.0841)	0.0196 (0.0676)
Mother is in Labor Force	0.554 (0.001)	0.0302 (0.0295)	0.00210 (0.0156)	0.00100 (0.0123)
Mother is Married	0.836 (0.001)	0.00420 (0.0175)	0.00560 (0.0107)	0.00840 (0.0079)
Mother is Single Household Head	0.0770 (0.001)	0.00570 (0.0106)	0.0127** (0.0061)	0.00730 (0.0052)
Mother's Usual Hours of Work	25.86 (0.022)	1.949** (0.8465)	0.8732* (0.4650)	0.653 (0.3950)
Mother's Weeks of Work Last Year	29.36 (0.031)	2.157** (1.039)	0.664 (0.5903)	0.456 (0.5056)
Mother Worked Last Year	0.711 (0.001)	0.0454* (0.0239)	0.0179 (0.0132)	0.0137 (0.0110)

Note: Table records discontinuity in covariates listed on left. Results estimated using children in the 2000 Census born between 1999 and 2000. 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	Regression Discontinuity Treatment Effect Estimates by Bandwidth		
		Omit December 11th - January 9th		
		1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth
K	0.970 (0.001)	0.0061 (0.0055)	-0.0023 (0.0025)	-0.0022 (0.0020)
1st	0.931 (0.001)	0.00280 (0.0121)	0.00520 (0.0059)	0.00610 (0.0045)
5th	0.915 (0.001)	-0.00520 (0.0083)	-0.00180 (0.0048)	0.00200 (0.0041)
7th	0.903 (0.001)	0.0158 (0.0102)	0.0105* (0.0057)	0.0102** (0.0044)
9th	0.878 (0.001)	0.0139** (0.0059)	0.0084** (0.0042)	0.0088*** (0.0032)
10th	0.864 (0.001)	0.00200 (0.0120)	0.00560 (0.0066)	0.00500 (0.0052)
11th	0.855 (0.001)	0.0245*** (0.0076)	0.0205*** (0.0043)	0.0211*** (0.0033)
9th - 11th	0.877 (0.001)	0.0123** (0.0059)	0.0113*** (0.0032)	0.0114*** (0.0024)

Note: Table records discontinuity in grade-for-age status. Results estimated using children in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.

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

Grade	Race	Control Mean	Regression Discontinuity Treatment Effect Estimates by Bandwidth		
			Omit December 11th - January 9th		
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth
5th	White	0.922 (0.001)	0.000600 (0.0080)	-0.00130 (0.0049)	0.00170 (0.0041)
	Black	0.871 (0.001)	-0.0194 (0.0188)	-0.0127 (0.0110)	-0.00550 (0.0093)
	Difference		-0.0200	-0.0114	-0.00720
7th	White	0.912 (0.001)	0.00990 (0.0105)	0.00680 (0.0059)	0.00670 (0.0045)
	Black	0.845 (0.001)	0.0218 (0.0223)	0.0311** (0.0118)	0.0315*** (0.0097)
	Difference		0.0119	0.0244*	0.0248**
9th-11th	White	0.879 (0.001)	0.00720 (0.0065)	0.0102*** (0.0036)	0.0102*** (0.0028)
	Black	0.793 (0.001)	0.0207 (0.0207)	0.0132 (0.0111)	0.0170** (0.0088)
	Difference		0.0135	0.00310	0.00690

Note: Table records discontinuity in grade-for-age status. Results estimated using children in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.

Table 4: Regression Discontinuity Estimate of Treatment Effect on Grade-For-Age Status in School by Family Income

Grade	Poverty Status	Control Mean	Regression Discontinuity Treatment Effect Estimates by Bandwidth		
			Omit December 16th - January 6th		
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth
5th	Above Poverty Threshold	0.933 (0.001)	-0.00380 (0.0079)	-0.00290 (0.0056)	-0.00110 (0.0046)
	Below Poverty Threshold	0.863 (0.001)	0.00220 (0.0306)	-0.000800 (0.0135)	0.00530 (0.0109)
	Difference		0.00590	0.00200	0.00640
7th	Above Poverty Threshold	0.923 (0.001)	0.0172** (0.0085)	0.0111** (0.0053)	0.0095** (0.0041)
	Below Poverty Threshold	0.846 (0.001)	0.0176 (0.0230)	0.0117 (0.0144)	0.0132 (0.0115)
	Difference		0.000400	0.000600	0.00380
9th-11th	Above Poverty Threshold	0.893 (0.001)	0.00520 (0.0043)	0.0072*** (0.0027)	0.0072*** (0.0022)
	Below Poverty Threshold	0.786 (0.001)	0.0196 (0.0201)	0.0162 (0.0109)	0.0198** (0.0087)
	Difference		0.0143	0.00900	0.0126

Note: Table records discontinuity in grade-for-age status. Results estimated using children in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.

Table 5: Regression Discontinuity Estimate of Treatment Effect on Grade-For-Age Status in School by Mother's Education Level

Grade	Mother's Education Level	Control Mean	Regression Discontinuity Treatment Effect Estimates by Bandwidth		
			Omit December 11th - January 9th		
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth
5th	Mother's Education above HS	0.941 (0.001)	-0.00650 (0.0078)	-0.00320 (0.0052)	-0.000600 (0.0043)
	Mother's Education not above HS	0.887 (0.001)	-0.00540 (0.0153)	-0.00200 (0.0072)	0.00440 (0.0061)
	Difference		0.00110	0.00130	0.00500
7th	Mother's Education above HS	0.932 (0.001)	-0.00120 (0.0081)	-0.000700 (0.0047)	0.00110 (0.0038)
	Mother's Education not above HS	0.874 (0.001)	0.0207 (0.0180)	0.0168 (0.0107)	0.0159* (0.0085)
	Difference		0.0219	0.0175	0.0148
9th-11th	Mother's Education above HS	0.916 (0.001)	0.00350 (0.0058)	0.00190 (0.0031)	0.00340 (0.0025)
	Mother's Education below HS	0.825 (0.001)	0.0105 (0.0117)	0.0173** (0.0067)	0.0173*** (0.0053)
	Difference		0.00700	0.0155**	0.0139**

Note: Table records discontinuity in grade-for-age status. Results estimated using children in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.

Table 6: Regression Discontinuity Estimate of Treatment Effect on Grade-For-Age Status in School - Children Living in Same State as Birth

Grade	Control Mean	Regression Discontinuity Treatment Effect Estimates by Bandwidth		
		Omit December 11th - January 9th		
		1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth
5th	0.915 (0.001)	0.00150 (0.0093)	0.00190 (0.0056)	0.00420 (0.0047)
7th	0.904 (0.001)	0.0177 (0.0114)	0.0110* (0.0063)	0.0100** (0.0050)
9th-11th	0.867 (0.001)	0.0172*** (0.0055)	0.0129*** (0.0032)	0.0125*** (0.0025)

Note: Table records discontinuity in grade-for-age status. Results estimated using children in the 2000 Census and 2001-2016 ACS with children living in the same state as birth. Estimation strategy described in text.

Table 7: Baseline Results for Regression Discontinuity Estimate of Treatment Effect on Grade-For-Age Status in School - Restricting to Subsample of Birthstates

Grade	Control Mean	Regression Discontinuity Estimates by Omitted Region	Treatment Effect	
Omit December 11th - January 9th				
		1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth
5th	-0.0126 (0.001)	-0.00680 (0.0080)	-0.00180 (0.0056)	(0.0048)
7th	0.905 (0.001)	0.0315*** (0.0081)	0.0211*** (0.0050)	0.0195*** (0.0041)
9th-11th	0.868 (0.001)	0.00960 (0.0087)	0.0099** (0.0046)	0.0096*** (0.0034)

Note: Table records discontinuity in grade-for-age status. Results estimated using children in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.

Table 8: Baseline Results for Regression Discontinuity Estimate of Treatment Effect on Grade-For-Age Status in School - Restricting to Subsample of Birthstates

Grade	Control Mean	Regression Discontinuity Estimates by Omitted Region	Treatment Effect	
Omit December 11th - January 9th				
		1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth
5th	-0.0126 (0.001)	-0.00680 (0.0080)	-0.00180 (0.0056)	(0.0048)
7th	0.905 (0.001)	0.0315*** (0.0081)	0.0211*** (0.0050)	0.0195*** (0.0041)
9th-11th	0.868 (0.001)	0.00960 (0.0087)	0.0099** (0.0046)	0.0096*** (0.0034)

Note: Table records discontinuity in grade-for-age status. Results estimated using children in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.

Table 9: Baseline Results for Regression Discontinuity Estimates of Treatment Effects for Young Adults

Outcome	Age Range	Control Mean	Regression Discontinuity Treatment Effect Estimates by Bandwidth		
			Omit December 11th - January 9th		
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth
Composite Measure	19-27	0.0000 (1)	-0.0473 (0.0484)	0.0028 (0.0261)	0.0101 (0.0216)
Composite Measure	19-22	0.0000 (1)	0.0481 (0.0597)	0.0249 (0.0409)	0.0197 (0.0336)
Composite Measure	23-27	0.0000 (1)	-0.1204* (0.0643)	-0.0152 (0.0301)	0.0016 (0.0253)
Composite Measure	28-32	0.0000 (1)	-0.0819 (0.0748)	-0.0175 (0.0447)	-0.0236 (0.0374)
Graduated High School	19-27	0.9161 (0.0006)		0.0006 (0.0029)	0.0012 (0.0023)
Graduated High School	19-22	0.9092 (0.0009)		0.0008 (0.0044)	0.0011 (0.0038)
Graduated High School	23-27	0.9210 (0.0007)		0.0002 (0.0034)	0.0011 (0.0028)
Graduated High School	28-32	0.9321 (0.0009)		-0.0047 (0.0034)	-0.0044* (0.0026)
Earned Income	19-27	16780 (42.6)		-143 (182)	-111 (155)
Earned Income	19-22	9582 (43)		7.5 (169)	14 (133)
Earned Income	23-27	21920 (62.7)		-280 (292)	-217 (244)
Earned Income	28-32	33100 (129)		-1.76 (675)	-376 (569)
In Labor Force	19-27	0.7763 (0.0009)		0.0048 (0.0048)	0.0048 (0.0037)
In Labor Force	19-22	0.7238 (0.0014)		0.0070 (0.0086)	0.0041 (0.0064)
In Labor Force	23-27	0.8138 (0.0010)		0.0028 (0.0051)	0.0051 (0.0039)
In Labor Force	28-32	0.8234 (0.0014)		-0.0032 (0.0081)	-0.0010 (0.0068)
SNAP	19-27	0.1528 (0.0007)		0.0015 (0.0050)	0.0004 (0.0040)
SNAP	19-22	0.1480 (0.0011)		-0.0021 (0.0091)	-0.0020 (0.0072)
SNAP	23-27	0.1561 (0.0010)		0.0041 (0.0043)	0.0022 (0.0035)
SNAP	28-32	0.1566 (0.0013)		-0.0036 (0.0069)	-0.0026 (0.0055)

Note: Table records discontinuity in adult outcomes by age group. Results estimated using in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.

Table 10: Regression Discontinuity Estimate of Treatment Effects on Composite Outcomes for Young Adults by Race and Age

Outcome	Age Range	Group	Regression Discontinuity Treatment Effect Estimates by Bandwidth		
			Omit December 11th - January 9th		
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth
Composite Measure	19-27	White	-0.0658 (0.0582)	-0.0121 (0.0334)	-0.0059 (0.0269)
		Black	0.0775 (0.1208)	0.1240** (0.0744)	0.0990* (0.0550)
		Difference	0.1433	0.1361**	0.1049**
Composite Measure	19-22	White	0.0382 (0.0742)	0.0206 (0.0440)	0.0090 (0.0375)
		Black	0.1101 (0.1489)	0.1340 (0.1086)	0.1100** (0.0660)
		Difference	0.0719	0.1134*	0.1010**
Composite Measure	23-27	White	-0.1434* (0.0788)	-0.0364 (0.0432)	-0.0175 (0.0349)
		Black	-0.0148 (0.2210)	0.1124 (0.1162)	0.0971 (0.0949)
		Difference	0.1286	0.1488	0.1146
Composite Measure	28-32	White	-0.0257 (0.1019)	-0.0142 (0.0567)	-0.0351 (0.0458)
		Black	-0.1497 (0.2525)	0.0333 (0.1361)	0.0333 (0.1095)
		Difference	-0.1240	0.0475	0.0684

Note: Table records discontinuity in adult outcomes by age group and race. Results estimated using in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.

Table 11: Regression Discontinuity Estimate of Treatment Effects on Composite Outcomes for Young Adults by Mothers' Education Attainment and Age

Outcome	Age Range	Group	Regression Discontinuity Treatment Effect Estimates by Bandwidth		
			Omit December 11th - January 9th		
			1.5 month bandwidth	2 month bandwidth	2.5 month bandwidth
Composite Measure	19-27	Avg. Mothers' Ed Above Lowest Quartile	-0.0772 (0.0589)	-0.0150 (0.0287)	-0.0029 (0.0241)
		Avg. Mothers' Ed Below Lowest Quartile	0.0496 (0.0888)	0.0692 (0.0518)	0.0555 (0.0375)
		Difference	0.1269	0.0842	0.0584
Composite Measure	19-22	Avg. Mothers' Ed Above Lowest Quartile	0.0146 (0.0773)	0.0190 (0.0485)	0.0243 (0.0401)
		Avg. Mothers' Ed Below Lowest Quartile	0.1527 (0.1206)	0.0497 (0.0667)	0.0049 (0.0541)
		Difference	0.1381	0.0308	-0.0194
Composite Measure	23-27	Avg. Mothers' Ed Above Lowest Quartile	-0.1490** (0.0732)	-0.0415 (0.0321)	-0.0237 (0.0277)
		Avg. Mothers' Ed Below Lowest Quartile	-0.0173 (0.1144)	0.0838 (0.0746)	0.0912 (0.0590)
		Difference	0.1317	0.1253	0.1149*
Composite Measure	28-32	Avg. Mothers' Ed Above Lowest Quartile	-0.0303 (0.0860)	0.0102 (0.0558)	-0.0080 (0.0467)
		Avg. Mothers' Ed Below Lowest Quartile	-0.2692* (0.1490)	-0.1171 (0.0873)	-0.0754 (0.0730)
		Difference	-0.2389	-0.1273	-0.0674

Note: Table records discontinuity in adult outcomes by age group and average mothers' education attainment in county of birth. Results estimated using in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.