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

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October 27, 2020

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Abstract

The U.S. tax code offers sizable tax credits to families with children, and eligibility for those credits depends on the calendar year in which a child is born. Thus, tax benefits begin a year earlier for a family if a child is born in December rather than a few days later in January, creating 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 regression discontinuity techniques with an omitted region, with the omitted region used to account for endogenous birth timing around the New Year, to calculate the effect of the change in after-tax income on outcomes for children and young adults observed in restricted access Census 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 likely 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 attenuate with age.

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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 (Michelmore and Dynarski, 2017; Reardon, 2011), less likely to graduate high school (Stark, Noel 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 (?Currie, 2009), more likely to have experiences in the criminal justice system, including incarceration (Chetty et al., 2019), more likely to earn less as adults (Chetty et al., 2014) and more likely to have lower life longevity (Ferrie and Rolf, 2011).

However, the causal mechanisms underlying these relationships remain an open question. Outcomes for children reflect the resources available to their families, but they also reflect the preferences of their families over how to raise children and the real and perceived costs of choices 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 outcomes of poorer children and richer children simply reflect economic resources at home, then more robust transfer programs to poorer families could have sizable impacts on later life outcomes (Aizer et al., 2016; Dahl and Lochner, 2012; Chetty, Friedman and Rockoff, 2011; Akee et al., 2010). However, if the difference reflects decisions families make in raising children regardless of resources, then interventions aimed at better preparing adults for their responsibilities as parents may more effectively address the disparity (Michalopoulos et al., 2019; Lavy, Lotti and Yan, 2020; Gubbels, van der Put and Assink, 2019; Mayer et al., 2019). 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).

The existing literature on the causal relationship between family income and later life outcomes will be reviewed more in depth in the next section, and this paper adds to that literature by offering evidence from a source of variation that fills gaps in the existing research. The source of income variation in this paper is a comparatively small change in income that happens specifically in the first year of a child's life, is especially large for low-income families and can be tracked for years after a child is born. Specifically, this paper exploits the discontinuity in after-tax income for families that occurs in infancy 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 for that child one year earlier than if a child is born after New Year's. This discontinuity in tax policy means that the parents of children born one day earlier have larger after-tax income, specifically $2,000 more or a 5% increase in after-tax income for families with children born in tax year 2016. Notably, this increase in

income is large for families who are more likely disadvantaged, including Black families and families with low education attainment.

This paper calculates the effect of this discontinuous change in after-tax income using a regression discontinuity design with an omitted region. As will be discussed, endogenous birth timing around the New Year is a major concern in this setting, and this paper accounts for this issue by omitting from the estimation process a region of observations around the New Year, where this omitted region is estimated and identified using bunching estimation techniques (Chetty et al., 2011; Kleven and Waseem, 2013; Saez, 2010). Under the assumption that no other treatments coincide with the passing of the New Year, and the assumption that the evolution of an outcome can be approximated using linear 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 in school (Xia and Kirby, 2009), so improvements in the share of students grade-for-age indicate multi-dimensional improvements in student development. Consistent with validity of the research design, there is no discontinuity in pre-school attendance and Kindergarten entrance around the New Year, as children born before and after the New Year enter pre-school and Kindergarten on-time at similar rates.[1] 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. This finding is robust to a variety of checks, including restrictions to students who live in their birth state. 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 0.96 percentage points.

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 are currently in poverty. Since the effects are largely driven by changes

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

in outcomes for these likely disadvantaged populations, these results are consistent with a strongly non-linear relationship between income and child outcomes.

The results also show that the effects of more income in early childhood persist after high school. In the spirit of Kling, Liebman and Katz (2007), this paper combines income, participation in the labor force, high school degree attainment and usage of SNAP into a single measure of economic self-sufficiency. In the years after young adults turn 19, there are suggestive but not statistically significant discontinuities in this measure that exist in the population at large, but there are small discontinuities for young Black adults and adults born in counties with comparatively lower education attainment. These discontinuities last until young adults reach their mid-20s, with the discontinuities largely driven by differences in high school education attainment and earned income, but they slowly fade thereafter,. Specifically, these estimates imply that Black adults and adults born in counties with comparatively lower education attainment see slightly over a 0.5 percent increase in total earnings from ages 20 to 30. 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.

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 family 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.

Preexisting Literature and Research Contribution

This paper draws together two distinct strands of the literature: research into relationships between family economic resources and outcomes for children, and research that examines this particular discontinuity in after-tax income.

This paper's primary contribution lies in the research on the relationship between family economic resources and child outcomes, as the source of variation here has unique strengths that add to the existing literature. This research literature, given the endogeneity issues with observed family income and unobserved variables, tends to rely on instrumenting for family income. Some of these papers use instruments based around occupation characteristics. For example, Shea (2000) and Chevalier et al. (2013) use industry,

job and policy-based instruments and data on characteristics of fathers' jobs to instrument father's income. Both of these papers find a stronger relationship between permanent income and outcomes for children than contemporaneous father's 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 Michelmore (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. The variation these papers rely on is clearer than the previous job instrument papers, but the reliance on the comparatively small samples in the PSID or NLSY means that these papers are likely underpowered for analyzing impacts.

Other papers use large administrative datasets to look at the link between variation in after-tax income and child outcomes. 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 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, Friedman and Rockoff (2011) use tax records linked to New York school performance data and use the non-linear structure of the EITC to estimate that a $1,000 increase in contemporaneous EITC income increases performance on test scores by 0.06 to 0.09 standard deviations. Chetty and coauthors extrapolate that the benefits from this improved performance in school translate a $1,000 increase in contemporaneous EITC income into a 0.54 percentage point increase in lifetime earnings. Black et al. (2014) use variation in after-tax income coming from a child care price subsidy in Norway resulting in an average $1,700 increase in income for children at age 5. Black et al. (2014) find large effects on achievement, including a 0.2 to 0.6 standard deviation increase in test scores a decade later from a $1,000 increase in income, although they note that these shifts likely reflects a permanent change in family income occurring in the years after the treatment. The focus on the effects of contemporaneous income makes it difficult to investigate long-term impacts in Manoli and Turner (2018) and Chetty, Friedman and Rockoff (2011)). Black et al. (2014) is probably the closest to this paper in that the authors look at the long-term impacts a relatively small change in income, but the change in income is indirect and comes for a child at

age 5, and the paper does not look at outcomes in adulthood.

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 source of variation in income for families eligible for the profits of the casino, and Loken (2010) and Loken, Mogstad and Wiswall (2012) use regional shocks to income from oil development. These papers find substantial marginal effects of additional income on outcomes for children in the lower part of the income distribution, but their variation reflects changes in permanent income. Bulman et al. (2017) and Cesarini et al. (2016) use variation in lottery earnings, and find relatively small to non-existent effects on outcomes for children.

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 et al. (2013) argues that the relationship is small, but Dahl and Lochner (2012) and Chetty, Friedman and Rockoff (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 and outcomes can be traced for years thereafter. 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, Ludwig and Magnuson, 2011; Currie and Almond, 2011), so focusing on changes in family income that happen early in life may reveal more pronounced long-term impacts, especially since the source of variation looked at here is pronounced for lower income families.

Second, a handful of papers that have looked specifically at parents' responses to this discontinuity, and this paper offers more evidence on the nature of this response. 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, Sallee and Turner (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, Sallee and Turner (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. Only one other paper has used this specific discontinuity to look at its effects on parents and children. Wingender and LaLumia (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 fits into this literature looking at this specific benefit by extending the analysis of effects of this discontinuity to children, and analyzing how the characteristics of families evolve over the discontinuity. Given that some impacts on parent behavior have already been established, looking at impacts on children is a natural extension. Additionally, this paper provides demographic evidence complementary to the tax record evidence in LaLumia, Sallee and Turner (2015) that the characteristics of parents evolve smoothly across the New Year outside of a window around the specific holiday.

Data

The data in this paper come from three sources - the Current Population Survey (CPS), the long form sample of the 2000 Census (otherwise known as the 1-in-6 or 17 percent sample), and the 2001-2016 American Community Survey (ACS).

The CPS is a monthly sample of the non-institutional civilian adult population of the U.S. The detailed information on income in the March CPS provides data for estimating tax obligations and estimating the size in the discontinuity of after-tax income 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 general patterns of grade repetition by grade.

The long form of the 2000 Census was a survey mailed to one-sixth of all U.S. households, covering 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 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

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2011 the Census Bureau has targeted approximated 3.5 million households (U.S. Census Bureau, 2014). The ACS covers many questions similar to those in the 2000 Census long form, but some question definitions are 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. The data in the 2000 Census and 2001-2016 ACS is the foundation of the causal regression discontinuity analyses later.

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 the 2000 Census and 2001-2016 ACS surveys ask about education attainment and how grade-for-age status is assigned. The sample restrictions necessary for the grade-for-age calculations determine what sample the paper uses for all causal analyses.

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 be a specific age by a certain date before being eligible to enter either Kindergarten or 1st grade in that state. Comprehensive data on these state policies for Kindergarten entrance were collected by (Bedard and Dhuey, 2012), and they generously provided their most recent data covering 1955 to 2015. This data was compiled directly from state statutes and legislative history on school entry policies, and cross-checked against a variety of other data sources. 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.

Three 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. Second, some states make the eligibility cutoff January 1st or December 31st. In the years that such cutoffs are present, children born before and after the New Year would, in addition to the treatment described, also experience the treatment of different grade eligibility rules. This paper also drops these individuals from any further calculation. 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. The nature of these questions is described more in Appendix A, but the consequence of this limitation is that grade-for-age status can only be consistently calculated in pre-school, Kindergarten, 1st grade, 5th grade, 7th grade, and 9th through 11th grade.

Thus, the sample of data varies by year and age of individuals, but these restrictions in aggregate mean that the sample for analysis in this paper is adults and children born 1980 and later who were born in states that had statewide Kindergarten entrance cutoffs away from the New Year at the date that the student would have entered Kindergarten in that state.

Overview of Tax Policy Relating to Children

The variation that drives this paper is the discontinuity in after-tax income income for families in the first year of an infant's life depending on the birth timing of the child. As discussed earlier, parents are eligible for tax benefits relating to a child starting in the tax year that a child is born. Specifically, there are four main child-related tax benefits that depend on timing of birth: personal exemptions for a dependent, the EITC, the Child Tax Credit (CTC) and the Child and Dependent Care Credit. This section describes features of these benefits relevant to this analysis, but the four tax benefits are described in further detail in Appendix B.

Figure 1 estimates the discontinuity in after-tax income produced by these four benefits for having a child born before the New Year. Without access to administrative data on tax records, it is difficult to precisely calculate the value of this discontinuity, but Figure 1 offers the best approximation to this calculation possible with March Current Population Survey (CPS) data.[2] These estimates are in line with calculations from administrative data; LaLumia, Sallee and Turner (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, rising from about $800 in 1980 to a little over $2,000 in 2016. A more thorough discussion of the history of changes to the four tax benefits mentioned above is in Appendix B, but in general, the rise in the discontinuity reflects increased generosity of the EITC and CTC over time through raised benefit levels and expanded eligibility for additional children in a household. Furthermore, the discontinuity is non-zero and positive for the vast majority of families. The share of parents with either no change in their tax liabilities or an increase in their tax liabilities is around 10 percent prior to 1994 and falls to about 6 percent thereafter. These parents have zero change in tax liabilities for three reasons: either they have very low income, they

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[2]Specifically, 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. More details are in Appendix A.

have received the maximum of relevant tax credits, or they have very high incomes and high deductions.[3] Thus, the vast majority of families experience a modest increase in after-tax income.

Figure 1 also shows the same average changes in after-tax income for families where a child's mother has a high school degree of education 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, but they start to diverge as time goes on. The relatively large change in after-tax income for these groups reflects the fact that these tax benefits can be especially large for lower income families, as the EITC in particular is targeted at lower income families. Critical to the size of these tax benefits for these families is the fact that the EITC is a refundable tax credit and the CTC is partially refundable,[4] meaning that individuals who have low tax obligations can actually see a positive tax return from the government.

Figure 2 presents these changes in after-tax income as being percentage increases in after-tax income, 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 larger for families where the mother has a high school degree or less and for Black families than it is for all families on average.[5] In particular, the lines rapidly diverge as the generosity of the CTC and EITC ramp up in the 1990s, demonstrating how these programs create especially large percentage jumps in income for disadvantaged households.

However, 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 a year earlier 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.[6] So, the effect of having

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[3]Inconsistent take-up of benefits in the first year of life is a concern for estimating the size of the discontinuity, but is difficult to accurate adjust for it in the specific calculation used here. As documented in LaLumia, Sallee and Turner (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. To offer a bound on this potential source of bias, a separate analysis described in Appendix A replaces the 5 percent of newborns in the simulated data each year whose families have the largest change in tax refunds with 0s. As the 5 percent of newborns who are not claimed but could be are 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 10 percent for all tax years years up to 2010. Therefore, this particular source of bias in the estimated discontinuity in after-tax income is likely modest.

[4]The CTC 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).

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

[6]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).

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.[7]

Birth Timing Patterns

Causal analysis of the effect of this change in after-tax income needs to account for the fact that parents and doctors have some degree of control over birth timing. Doctors may deliver children using Cesarian section (C-section) surgery (32 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), delivery methods that can be used to alter timing of birth (Martin et al., 2018). There is clear evidence of this control over birth timing in the well-known "weekend effect" where fewer births happen on weekends. As is clear in Figure 4, there are large dips in counts of births on the weekends. This fall on the weekends reflects a substantial decrease in C-section surgeries, but there is a smaller but still noticeable fall in vaginal births as well (Martin et al., 2010). Figure 4 also shows that mothers 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 and have specific preferences over birth timing while others parents do not to the same degree.

After regression adjusting for day of week in Figure 6, the distributions of births and the characteristics of births are much smoother.[8] However, there are clear disruptions in the distribution of births, 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. Similar to mothers who give birth on weekends, mothers with births that occur on holidays have slightly

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

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

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

where the first set of indicator variables $\mathbb{1}[d=i]$ are a set of six dummy variables (excluding Monday), and the second set of indicator variables $\mathbb{1}[d_H = i]$ are 11 dummy variables for each day within 5 days of each major holiday (indexed by H). The second set of dummy variables exclude from the estimation process all days around holidays, and the first set of dummy variables indicate the average births that are observed on a given day that differ from the births observed on Monday (the omitted category variable). Then, the regression adjusted counts of births would be:

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

lower average years of education than mothers with births that do not occur on holidays, but the average years of education return to previous levels quickly in the days around a holiday. Focusing in particular around New Year's, there is a drop in births on New Year's Day, and a slightly larger drop on Christmas Day, with larger counts of births occurring before and after these holidays. Interestingly, there are relatively few births after New Year's Day compared to before, suggesting that parents and their doctors with some level of control over birth timing are more likely to move births 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, Sallee and Turner (2015) find limited evidence of shifting in birth-timing around the New Year that seems specifically tax-related, with most tax-correlated shifting concentrated in a narrow window around the New Year.[9]

Methods

Evidence in the previous section suggests that the treatment of being born before New Year's Day is not random for some children, at least within a window of New Year's Day. However, the distribution of births outside of days around New Year's Day appears relatively smooth, save for other holidays. Intuitively, while parents can shift births in a specific region, 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 C develops microeconomic theory foundations to justify such a way of thinking, but this general intuition inspires a regression discontinuity strategy with an omitted region (sometimes referred to as a "doughnut regression discontinuity").

Specifically, this paper estimates the following model:

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

Where Y is some outcome, d is the distance in days to the New Year's, c is the scale of polynomial in d, $\boldsymbol{X}$ is a list of additional covariates (specifically, state fixed effects and day of week fixed effects), and the estimation process includes days in some range $[D_1, D_2]$ 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

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[9]Furthermore, LaLumia, Sallee and Turner (2015) 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.

process:

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

Following the recommendations in the theoretical literature regarding regression discontinuity estimation, this paper restricts attention to local linear regressions where $c = 1$ (Hahn, Todd and der Klaauw, 2001) and 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 et al., 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 Bureau.[10]

Before discussing the sufficient conditions this paper builds up and the validation strategies suggested by such conditions, it is useful to review the typical assumptions for regression discontinuity analyses without omitted regions. As described by Lee and Lemieux (2010) 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:

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

$$\tag{5}$$

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

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

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

2. Test the null hypothesis that $f(d|X)$ is smooth at the threshold, with a rejection of smoothness at the

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

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

In this setting, without an omitted region, both these traditional assumptions are clearly violated, as there is clear strategic timing of births with more births occurring around New Year's than on New Year's, 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 observations evolve smoothly and the conditional means functions can be estimated consistently, then estimating the conditional means functions in equation 9 with the remaining data and extrapolating to 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.

To validate this set-up, note that, assuming the manipulated region is identified correctly and the conditional means consistently estimated, the first test should still be applicable in a valid regression discontinuity design with an omitted region. However, the second test is no longer applicable as a substantial share of the data is omitted, and extrapolating an estimated density into an omitted region rapidly reduces power.

Using this estimation strategy depends on properly identifying the region of manipulated birth timing around the New Year. Currently, there is no standardized way researchers use to identify an omitted region for this form of regression discontinuity estimation. Many papers use ad hoc visual analyses of the size of the manipulated region (Barreca et al., 2011; Gauriot and Page, 2019; Almond and Doyle, 2011), but some papers suggest more regularized methods that are not applicable in this setting.[11] This paper proposes a data-driven method widespread in the public economics bunching estimation literature (Chetty et al., 2011; Saez, 2010; Kleven and Waseem, 2013) to impose some level of empirical structure on the choice. Specifically, this method sets an upper bound on the region of manipulation (after January) and then uses density estimation techniques 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 graphed in Figure 6.[12]

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[11]Dahl, Loken and Mogstad (2014) are able to use other years where a treatment does not exist as a counterfactual to estimate the extent of the regions that are not manipulated. Hoxby and Bulman (2016) suggest a method of estimating the region 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 (2016) for estimating bias.

[12]The process described here could be run for birth counts separately by year of birth, creating different omitted regions for different years of birth. This strategy would likely make the most sense with full count natality data, but given the need to

Next, this paper follows a three 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:

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

Where the first term is a flexible polynomial of order c. Similar to Kleven and Waseem (2013), this paper uses $c = 5$, although the results are 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:

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

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

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

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

Note that $Gap_{\underline{d},\bar{d}}$ shows the gap between the counterfactual births and the observed births. Kleven and Waseem (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 on and after New Year's Day.[13]

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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.

[13]In some respects, this estimation process is akin to ensuring that the remaining data meet a condition similar to the second validity test described above that was no longer applicable in this setting. Omitting dates that demonstrate shifted births 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. Of course, the density estimation process here ensures that, by design, the estimated density is smooth, but the estimation process drops observations from the analysis process that effectively do not meet a smoothness condition. Thus, the estimation process of the omitted region ensures that the remaining data left after this process is complete follows the logic of the second condition described above.

Note 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 bootstraps the estimation procedure in 2000 replications, 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 accomodate clustering on running variable, as is common in the applied regression discontinuity literature to account for potential model misspecification (Lee and Card, 2008).

Results

First, this paper estimates the size of the omitted region using the techniques just described. Figure 5 shows results from the density estimation procedures described in equations 5, 6 and 7. Following Kleven and Waseem (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 size of 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 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, Loken and Mogstad, 2014). As is clear visually, the density of births appears to return to a smooth distribution outside of these dates.[14]

Validating Omitted Regions

As discussed before, one test for the validity of this design with this omitted region is to test for discontinuous differences in pre-treatment and untreated covariates using the research design. If the research design is valid, there should be no detectable differences except those observed at random. Table 1 shows the results from regression discontinuity estimates testing whether the characteristics of children's parents and their households vary discontinuously using the omitted region and three separate bandwidths.[15] These

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[14]A 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 in Figure 7, as the timing of Thanksgiving (falling on the fourth Thursday in November) varies from year to year. The results estimating the estimated region available on request.

[15]Although the results regarding outcomes for children below use pooled data from the 2001-2016 ACS and the 2000 Census, this section uses only the data from the 2000 Census and looks at the characteristics of infants and their families for children

tests look at household and parent income, intensive and extensive parent labor force participation in the previous year, education attainment of parents, race of child, marital status of parents and household size.

11 out of 114 tests show significant discontinuities at the 5 percent level. This rejection rate is within the levels that would be expected with random sampling variation and independent tests if the null hypothesis of no discontinuous change in characteristics were true. Additionally, as these tests are likely positively correlated, rates of rejection expected under this null hypothesis would be even higher than they are here. 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 non-treated covariate smoothness condition implied by equation 5, and the estimation procedure seems valid.

Effect of Family Income in Infancy on Grade-for-Age Status 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. The primary school outcome observable in the Census and ACS data is grade-for-age status. 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.

Table 2 reports all basic results testing whether children are grade-for-age by grade, with Figures 7A - 7D and Figures 8A - 8D showing graphical depiction of these regression discontinuities. In the year that students are eligible for Kindergarten, Table 2 and Figure 7A show that enrollment in Kindergarten or a higher grade in the year of Kindergarten eligibility shows no discontinuity across the threshold. This result suggests that there is no detectable difference in parents delaying their child's entrance into Kindergarten, a practice often referred to as "red-shirting."

This lack of a discontinuity in Kindergarten attendance is important for contextualizing later results, as this result suggests that any subsequent detected discontinuities in grade-for-age status reflect students being retained in a grade and not Kindergarten red-shirting. It is difficult to interpret the meaningfulness of changes in grade-for-age status from red-shirting, as the population of students who are red-shirted do not on average have lower cognitive skills and social maturity before they enter school than children who

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 2000 Census asks 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, (Wingender and LaLumia, 2017) find evidence of a labor supply response from a change in after-tax income. 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 in the previous year, but will be incomplete for all months thereafter, and thus the population of children born after New Year's. 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.

are not red-shirted (Bassok and Reardon, 2013).[16] In contrast, repeating a grade after entering school is usually interpreted as a negative signal about a student's social, emotional or academic readiness for the next grade. Students who are retained in a grade are more likely to have poorer academic performance prior to retention, lower social skills and poorer emotional adjustment, and more problem behaviors in class, including inattention and absenteeism (Xia and Kirby, 2009).[17] Thus, any subsequent detected changes in grade-for-age status in this setting are an indication of changes in these conditions that make students more likely to be retained within a grade.[18]

Figure 7A also shows an important pattern in the omitted region that is worth noting for all subsequent graphs in Figures 7 and 8. The students born right after the New Year appear to be slightly less likely to have entered Kindergarten on time than the students born right before. These data were excluded from the estimation process for the reasons discussed earlier regarding strategic birth timing, and this particular drop likely reflects both the fact that students born after the New Year did not get the income boost and the fact that these children are likely negatively selected compared to the children born before the New Year. As was discussed previously regarding Figure 5, these children born right after the New Year come from households where mothers have, on average, slightly lower educational attainment.

Table 2 and Figure 7B shows that a small 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 6 shows, Kindergarten is one of the grades students are most likely to repeat, so 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 (results available on request). These results offer suggestive evidence that a discontinuity has opened up in the share of students grade-for-age, but that discontinuity is relatively modest.[19] These results are confirmed when

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

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

[18]Retention 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 2018, 16 states have 3rd grade retention policies that require students to repeat a grade if those students have not reached some minimum threshold of achievement (Education Commission of the States, June 2018b). 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). Thus, the meaningfulness of this outcome may differ from location to location, with some teachers in some states much more willing to use it as a tool than others.

[19]While repetition of Kindergarten may represent a type of red-shirting (Deming and Dynarski, 2008), it is worth noting that the characteristics of children who repeat Kindergarten are on average 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 later grades, have below-average school work, and be described by their teachers as having behavioral issues (National Center for Education Statistics, 2000). Thus, an increase in retention rates in

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looking at the share of students grade-for-age in 5th grade in Table 2 and Figure 7C. 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. Thus, by 5th grade, the cumulative effects of retention 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.

Moving forward to 7th grade in Table 2 and Figure 8A, a larger detectable discontinuity has opened up in the share of students grade-for-age. The regression discontinuity estimate shows that students born before the New Year have a 1.02 percentage point increase in the probability of being grade-for-age. Again, similar to the transition from Kindergarten into first grade, the increase in the discontinuity here makes sense, given that Figure 6 again shows that there is a gradual increase in retention rates by grade from 5th grade to 7th grade. As is clear from visual inspection of Figure 8A, this result appears somewhat sensitive to the upper bound of dates excluded, but this result is suggestive evidence of an eventual shift in grade-for-age status taking place.

Lastly, looking at 9th, 10th and 11th grades in Table 2 and Figures 8B through 8D, the discontinuity in the share grade-for-age here appears to eventually grow in magnitude and is slightly larger in magnitude than the discontinuities reported for earlier grades 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 at the 5 percent level. Furthermore, the results depicted in Figures 8B through 8D appear to become less and less sensitive to the upper bound on dates omitted, unlike Figure 8A. As a final measure, Table 2 and Figure 8D show the average discontinuity in grade-for-age status 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.

While estimates of specific discontinuities are often noisy, the pattern of the evolution of the discontinuity across grades is worth noting. By 1st grade, a slight discontinuity that is statistically insignificant opens up, and by 5th grade the discontinuity is still small. While it is difficult to read much into this early pattern, it may be weak evidence of a small if undetectable gap beginning. The estimated discontinuity in grade-for-age status in 7th and 9th grade is larger, and in high school, it continues to grow. While these estimates are imprecise, they suggest a gradual increase over time in the size of the discontinuity, with perhaps the largest

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increases happening in grades where students are most likely to be retained as depicted in Figure 5.[20]

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

Tables 3 through 5 and Figure 9 break these results down further by showing how these results vary among subgroups. Here, for concision, the only grades analyzed are grades 5, 7 and then 9, 10 and 11 conjointly. 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 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. These are groups that have lower income at time of a child's birth.

When comparing Black children with White children in Table 3 and Figure 9B, 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. 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 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

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

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 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, 2019). 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, Kim and Sakamoto, 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.

In general, these results suggest stronger effects observed for groups that are more likely to be disadvantaged at a child's birth. This result suggest that the impacts of this additional income are strongly nonlinear.

Robustness Checks on Grade-for-Age Status Results

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 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 for 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, Smith and Wozniak, 2011). Thus, the findings discussed before are robust to whatever error is added from the misassignment of state of residence at age 5.

Effect of Income in Infancy on Outcomes in Early Adulthood

Having investigated the consequences of this discontinuity in family income for outcomes in school, the next step is to examine what long-term consequences this discontinuity has 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 a 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 being retained 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, 2006). 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 on later achievement and labor force outcomes 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 eventual high school graduation (Schwerdt, West and Winters, 2017). Thus while the initial treatment in infancy is clear, other compensating treatments happen subsequently that may complicate interpretation of effects in adulthood.

As the discontinuity in grade-for-age status was concentrated in less educated and likely disadvantaged households, changes in outcomes in early adulthood are likely 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 likely disadvantaged households as they get older. This paper uses two strategies to identify these groups. First, this paper looks at outcomes among Black children. While Black children did not display consistently statistically different results in grade-for-age discontinuities than White children, Black children had larger point estimates of estimated changes in grade-for-age status. Second, this paper looks at outcomes for children born in counties that have lower 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 were observable for young adults no longer living at home, conditioning on features of counties of birth is the best available proxy for this group of individuals.

For relevant later life outcomes, this paper looks at high school completion rates, earned income, labor

force participation, and SNAP receipt for children born in 1980 forward. Additionally, as these outcomes have more variation than the previous analysis of grade-for-age status, this paper follows Kling, Liebman and Katz (2007) in combining these four measures of outcomes into a single unitary measure of economic sufficiency. This single measure allows more power in measuring effects that move in the same 'positive' direction. To compute this measure, this paper normalizes each outcome 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 10A through 10C 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 demonstrate the underlying variation in outcomes and are not meant to be interpreted as causal impacts. As is clear, there is little detectable difference in high school graduation rates, nor in labor force attachment in the population as a whole. However, 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 11A combines all four measures into a unitary measure of economic self-sufficiency for all adults. Note that, by construction, this measure has average value 0 for people born in January, but there is still a standard error on the estimate as it is an average and has sampling variation. Figure 11A 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 11B and 11C show similar graphs 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 average value 0 for people born in January within this subgroup. Here, the patterns are much noisier given the smaller sample sizes, but similarly the gap varies from .09 to .01 standard deviations, and attenuates over time to low numbers by the time adults reach their late 20s and early 30s. Among Back 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 depending on age.

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 and Figure 12A shows

reuslts for the self-sufficiency measure. Given the small nature of the effects observed in Figures 10 and 11A, 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.[21] As is clear in Figure 11A, however, there are individual outliers within these age groups that can be important for driving measured effects. Table 9 and Figure 12A 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 -0.02, 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 suggest a weak treatment effect in early adulthood that falls over time as young adults age into their mid to late 20s, although strictly speaking no effects are distinguishable from 0.

Heterogeneous Effects by Subgroups on Outcomes in Early Adulthood

Table 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 larger treatment effect for Black young adults is driven by a treatment effect on earned income and labor force participation, both positively signed but again not distinguishable from 0 at the 10 percent level. Note

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[21]Note that age 18 is excluded here. Given the way the sample is constructed, young adults aged 18 are expected to have completed high school if they graduated on time. By definition, the previous differences in grade-for-age status ensure that high school graduation rates at age 18 would be different. Young adults aged 19, on the other hand would be expected to have completed high school if they graduated either on time or one year later. Including age 18 would increase measured effects, but the difference expressed would reflect the differences in education attainment created by graduation timing, hence it was excluded from consideration here.

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, and the gap between the two again is statistically distinguishable at the 5 percent level.

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 in the sample as a whole, where estimated treatment effects are largest in earlier years and appear to attenuate with time.

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 0.05 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.

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 in the widest bandwidth. 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.

Finally, looking at adults aged 28-32, the 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 given the lack of power in picking up what will likely be minor effects, but they appear to tell a consistent story. While effects of the income discontinuity seem to persist in terms of impacts on education attainment and earnings after turning 19, these impacts apparently attenuate with time as students age into their late 20s and early 30s. Again,

as before, estimated effects are largest for comparably disadvantaged groups.

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 for at least some subgroups. Few other papers have used such a specific, sharply defined, and relatively modest treatment that affects income in the first year of a child's life, so it is difficult to directly compare these findings with similar research on the effect of family income, but some comparisons are possible.

First, the results here suggest a strongly non-linear relationship between family income and student achievement that has been found in other settings. Loken, Mogstad and Wiswall (2012) and Akee et al. (2010) find that changes in family income for lower-income groups in particular have large impacts on outcomes for children in school and in early adulthood, whereas effects of changes in family income at other income levels are much lower.

Second, this paper suggests that a $1,000 change in family income in infancy results in a non-trivial change in the probability of retention in a grade, and other papers find that temporary income shocks of $1,000 through the same tax benefit mechanism also show strong relationships between family income and school achievement. 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, Friedman and Rockoff (2011) and Dahl and Lochner (2012) find that $1,000 of contemporaneous income results in a 0.06 to 0.09 standard deviation rise in contemporaneous test scores (depending on the specification) and Black et al. (2014) find even larger effects of 0.1 to 0.6 standard deviations in test scores at age 15 from a $1,000 shock at age 5. Such changes in tests scores, especially if they happen in the lower part of the test score distribution, may have non-trivial impacts on retention. While there is no known causal work other than this paper linking changes in family income and changes in student retention, data from Florida on test scores and retention patterns suggest that a 0.06 to 0.09 standard deviation change in test scores correlates to a reduction in the probability of students being retained in grade 4 by 0.6 to 0.8 percentage points.[22] While this relationship from the Florida data is not causal, it is suggestive that changes in test scores from a $1,000 change in after-tax income may result in similar effects on retention as those measured in this paper.

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[22]This estimate comes from the evidence reported in Schwerdt, West and Winters (2017). In Figure 2A, the authors offer average retention rates by test scores in the years prior to a test score-based retention policy existing, and in Appendix Figure A-2 the authors show the distribution of test scores. Shifting the distribution of test scores in the lower regions of the distribution up by 0.06 to 0.09 standard deviations produces this estimated result. Baseline retention rate in this data among all students is 1.87 percent.

Third, this paper finds that a $1,000 change in income in family infancy results in modest long-term changes in outcomes in adulthood, and other papers show a similar relationship. Chetty, Friedman and Rockoff (2011) provide a method of linking changes in test scores to changes in future earnings, and then use these estimates along with the previously described causal estimates of impacts of income to convert the impact of $1,000 in after tax income into a change in 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, Friedman and Rockoff (2011) conclude that a $1,000 increase in after-tax income in later primary and high school dates 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 a $1,000 increase in after-tax income in infancy 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 the fact that these estimated impacts on earnings are within similar ranges is suggestive evidence that the effects measured here are within the bounds that might be expected.

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

A wide array of research in social science suggests that family conditions in infancy and early childhood are particularly consequential for patterns of long-term development for children. First, gaps in measured cognitive and non-cognitive abilities between children open up at early ages and are observable clearly before students enter school (Loeb and Bassok, 2007; Cunha and Heckman, 2007), as are observable gaps in health (Figlio et al., 2014; Case, Lubotsky and Paxson, 2002; Currie and Almond, 2011), and these gaps are highly correlated with family economic resources. Second, a literature in biology suggests the existence of 'critical periods' for development where inputs are especially important for later life outcomes

(Reviewed in Cunha et al. (2006)). Lastly, research shows that some policy interventions that affect the resources available to low-income families can have both short-term consequences (Hoynes, Miller and Simon, 2015; Almond, Hoynes and Schanzenbach, 2011; Rossin-Slater, 2013) and long-term consequences for outcomes for children (Black et al., 2014; Hoynes, Schanzenbach and Almond, 2016; Aizer et al., 2016; Milligan and Stabile, 2009). Those papers find effects observed across multiple dimensions in health, cognitive

skills, non-cognitive skills, and other metrics of child development. Thus, it would not be surprising that an income shock in early childhood, like the one analyzed here, would relate to multi-faceted improvements in outcomes for children that may have larger long-term effects than income shocks later in life.

Finally, note that this literature on the effects of family conditions in infancy and early childhood on later life outcomes offers a few clues as to potential mechanisms. First, disadvantaged families who see the largest jump in after-tax income are highly likely to be income constrained with infants in early childhood. Over the sample period included here, around 50% of black newborns and 35% of newborns in families where the mother has a high school degree or less are in poverty, but by the time those children turn 15 that share drops to 40% and 23% respectively. Hence, changes in the income of these families in early childhood might have significant impacts on consumption patterns, as differences in income between families correlate to differences in spending patterns on children (Caucutt, Lochner and Park, 2017). Research shows that changes in income from tax credits result in changes in spending on resources that might affect child development (McGranahan and Schanzenbach, 2013), although it should be noted that much of the research on spending patterns of EITC recipients suggests that recipients use it to pay down debt and spend on transportation (Goodman-Bacon and McGranahan, 2008; Mendenhall et al., 2012).[23] To the degree that these spending patterns might enable slightly higher labor force attachment in subsequent years, such patterns may result in improvements in permanent income that may further improve the economic standing of families over time (Ramnath and Tong, 2017; Black et al., 2014). However, even if consumption patterns on children and permanent income are unaffected, the simple act of loosening the family's budget constraint may have impacts on how parents interact with their children. Research has found that parental stress, parental depression, martial conflict, are all highly correlated with low income in families, and in turn correlated with adverse outcomes for children (Wadsworth et al., 2005; Conger et al., 1994; Gershoff et al., 2007). Thus, even small changes in the economic resources of families can have consequences for important early life experiences of children, either through changes in consumption patterns, changes in permanent income, or changes in the family environment.

Conclusion

This paper demonstrates compelling effects of family income in early childhood on outcomes in childhood and early adulthood. Specifically, this paper shows that a $1,000 change in family income in infancy results in a 0.96 percentage point increase in the probability of being grade-for-age in high school. These effects are largely driven by treatment effects in likely disadvantaged children, specifically Black children, children in

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

families currently in poverty, and children from families with low education attainment. Small but suggestive effects on adult outcomes in earnings, labor force attachment, high school graduation status and SNAP usage persist into early adulthood, in particular among Black young adults, and adults from counties with low education attainment. As these effects are largest among disadvantaged groups, they suggest a non-linear relationship between changes in income and changes in outcomes.

These results are on the upper end of estimated relationships between family income and outcomes for children, but they fit in line with a broad literature suggesting that changes in family economic resources in early childhood may have substantial long-term impacts on outcomes for children through changing family spending patterns and improving future family earnings, but also changing the home life circumstances that young children have early in life.

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. Directions for future research in this project include examining effects on siblings, and investigation into mechanisms of effects in consumption data.

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Figures and Tables

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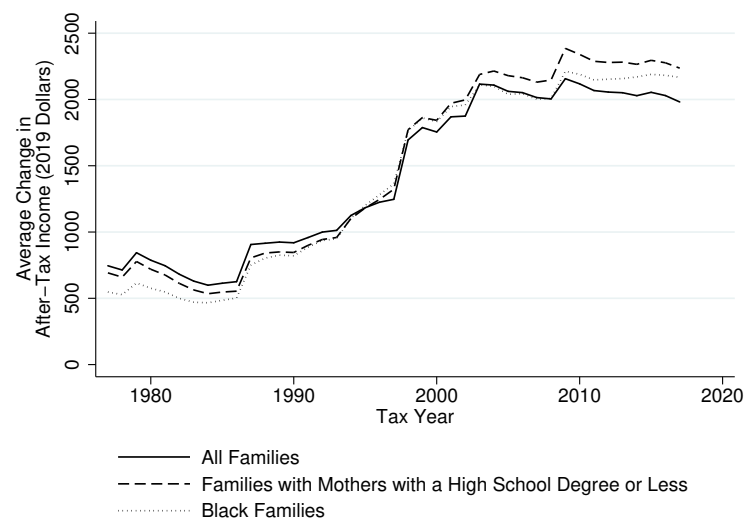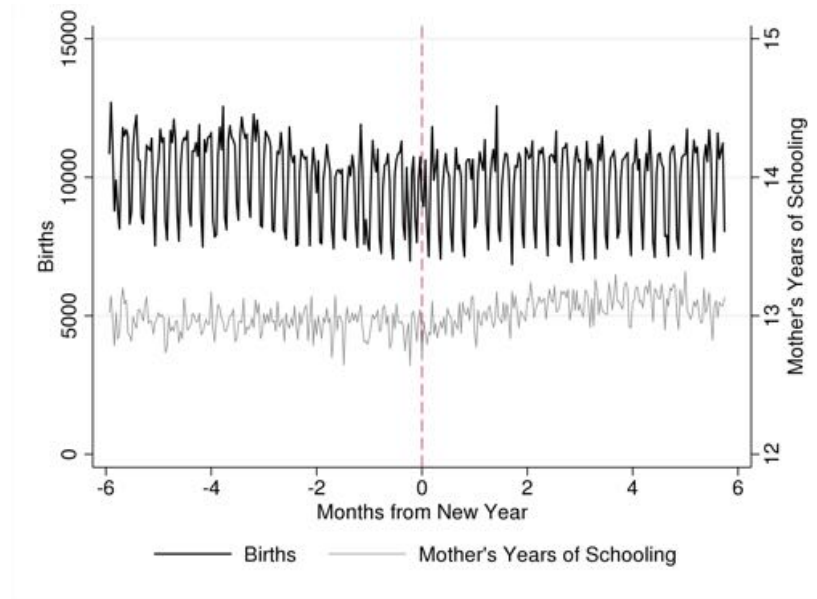

Note: Figure depicts average estimated discontinuity in after-tax income for families for having a child born in December compared to January by tax year of birth in 2019 dollars. Estimation process draws inspiration from Hoynes et al. (2015) - additional details on estimation in Appendix B. Standard error bars here omitted for clarity.

Figure 2: Average Percent Increase in After-Tax Income from Having Newborn in December Compared to January (2019 Dollars)
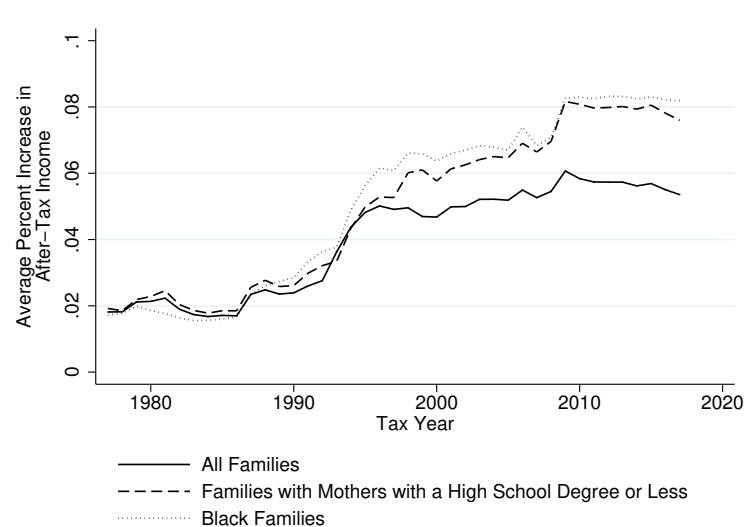
Note: Figure depicts percent increase in after-tax income for all families, for families where the mother has a high school degree or less, and Black families, for having a child born in December compared to January by tax year of birth in 2019 dollars. Same estimation process as described in Figure 1 and Appendix B. Standard error bars omitted for clarity.

Figure 3: 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 4: Births by Day of Year Adjusted by Day of Week
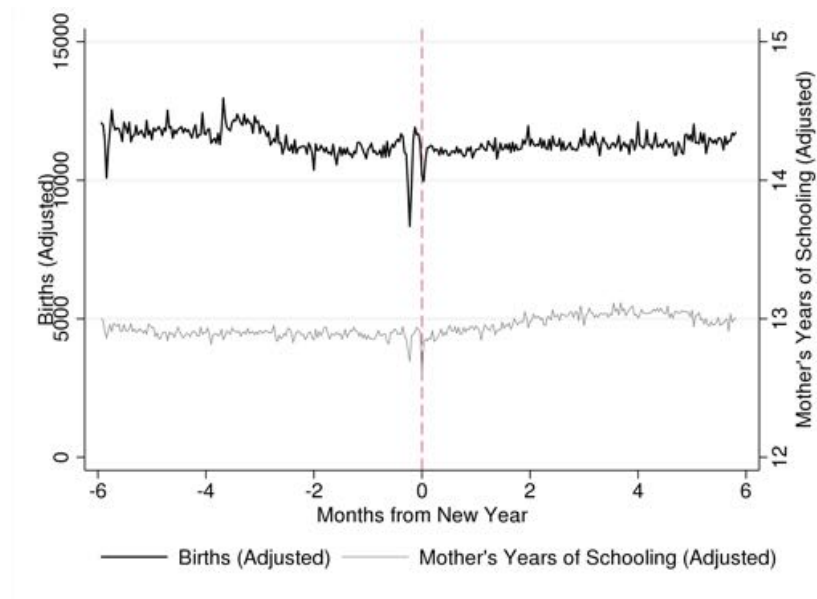
Note: Figure depicts average births by day of year estimated in the 2000 Census from July 1st 1996 to June 30th 1997, centered on the New Year in 1997, and regression-adjusted for day of birth following equations (1) and (2) in the text.

Figure 5: Estimated Birth Timing Manipulation
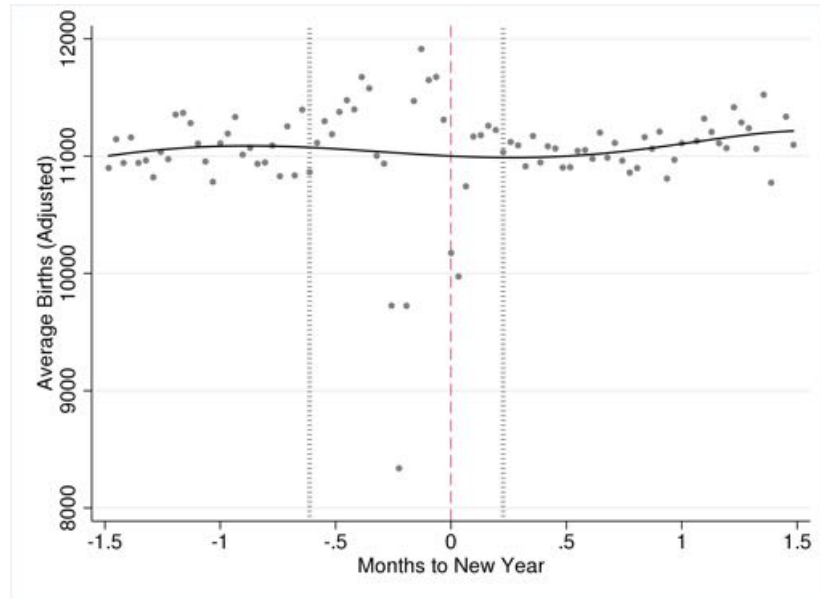
Note: Figure depicts average births by day of year from 1989-1994 regression-adjusted for day of birth following equations (1) and (2). Vertical bars indicate manipulation region omitted from calculation. Upper bound selected visually at 9 days after the New Year. Lower bound selected through estimation process described in the text.

Figure 6: Average Share of Students Retained in Grade - 1990-2005
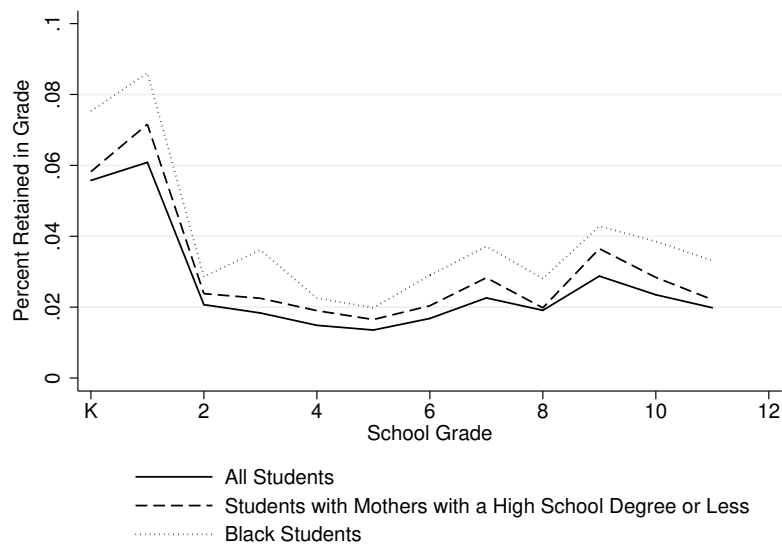
Note: Figure depicts average share of students retained in each grade over the years 1990 to 2005 estimated in the October CPS. Standard error bars omitted for clarity.

Figure 7: Estimated Discontinuities in Grade-for-Age Status - Primary and Pre-Primary School

(a) Kindergarten
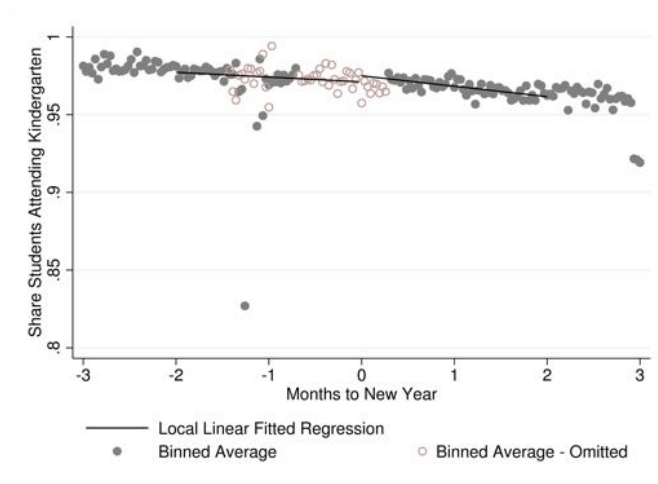
(b) 1st Grade
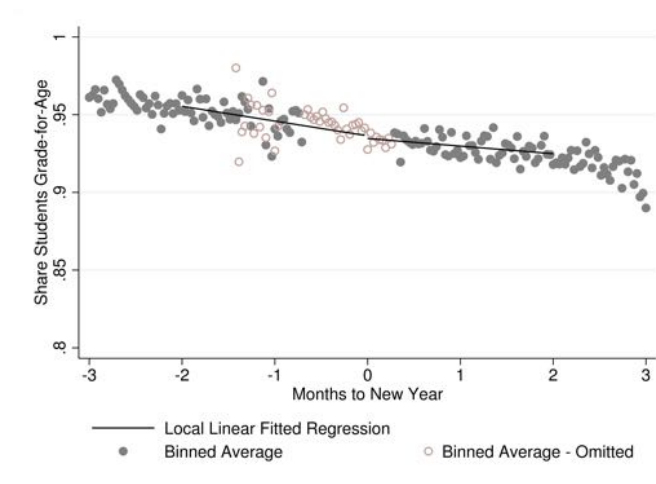
(c) 5th Grade
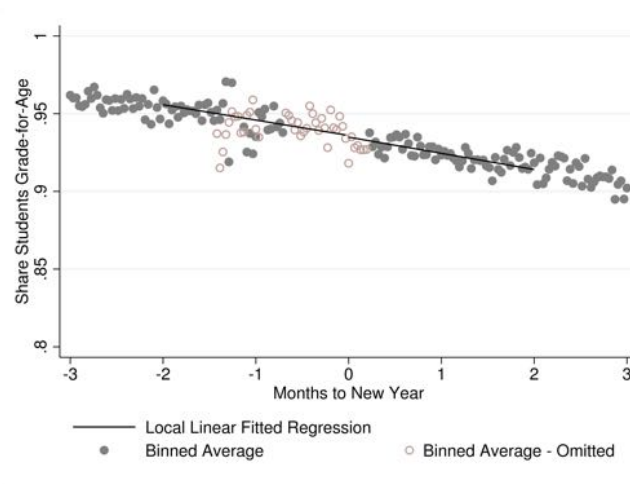
Note: Figures depicts discontinuity in share of students attending Kindergarten, and share of students grade-for-age in 1st grade and 5th grade around the New Year. Red empty circles are data omitted from estimation process, and grey solid circles are data that could be included. The estimated line uses a bandwidth of two months around the New Year, and the solid grey circles covered by the estimated line represent data included in the estimation process. See Table 2 for point estimates. Estimation process detailed in text.

Figure 8: Estimated Discontinuities in Grade-for-Age Status - Middle and Secondary School

(a) 7th Grade
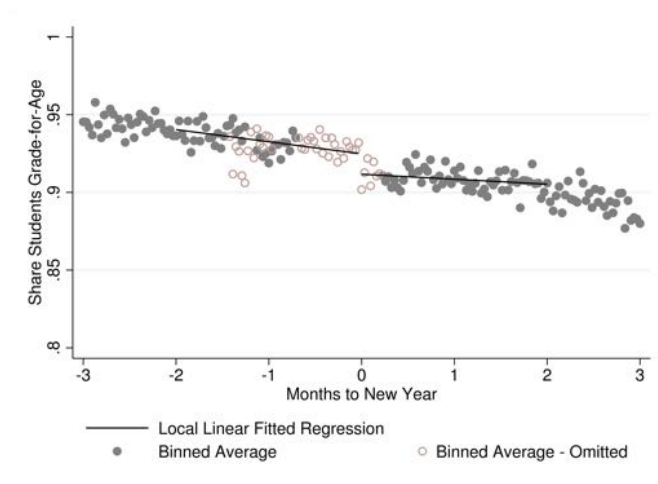
(b) 9th Grade
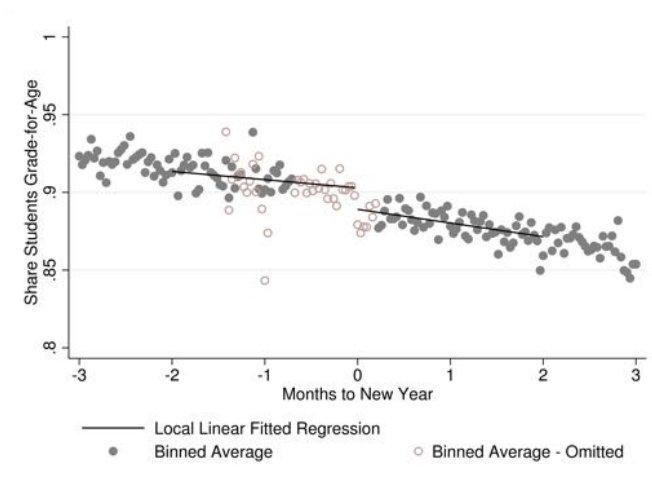
(c) 10th Grade
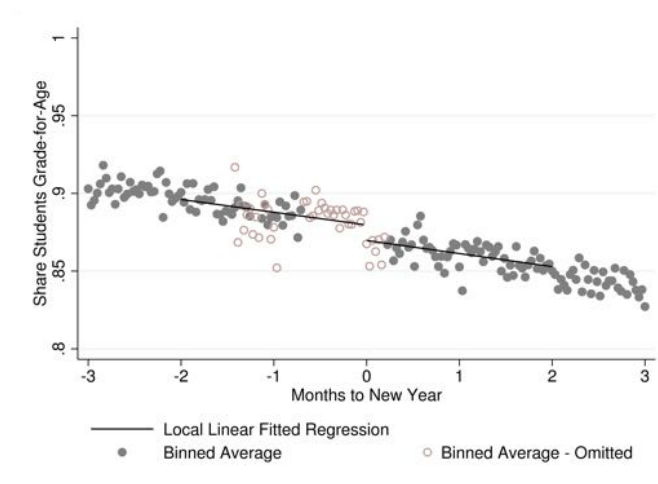
(d) 9th-11th Grade
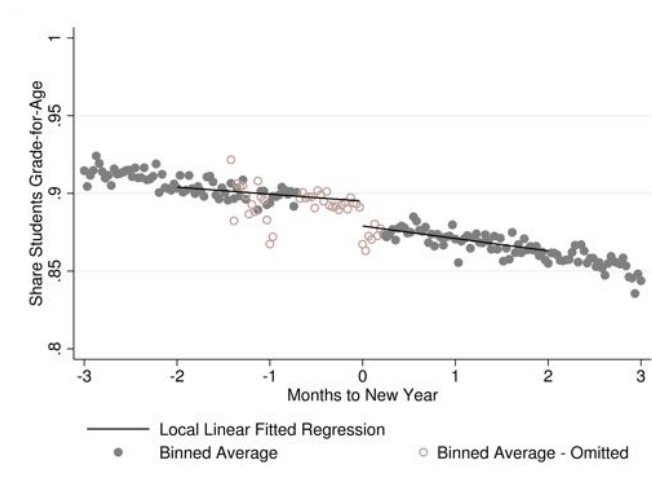
Note: Figures depicts discontinuity in share of students grade-for-age in 7th grade, 9th grade, 10th grade, and 9th through 11th grade around the New Year. Red empty circles are data omitted from estimation process, and grey solid circles are data that could be included. The estimated line uses a bandwidth of two months around the New Year, and the solid grey circles covered by the estimated line represent data included in the estimation process. See Table 2 for point estimates. Estimation process detailed in text.

Figure 9: Estimated Discontinuities in Grade-for-Age Status by Grade and Subgroup

(a) Full Sample

(b) Separated by Race

(c) Separated by Mother's Education Attainment
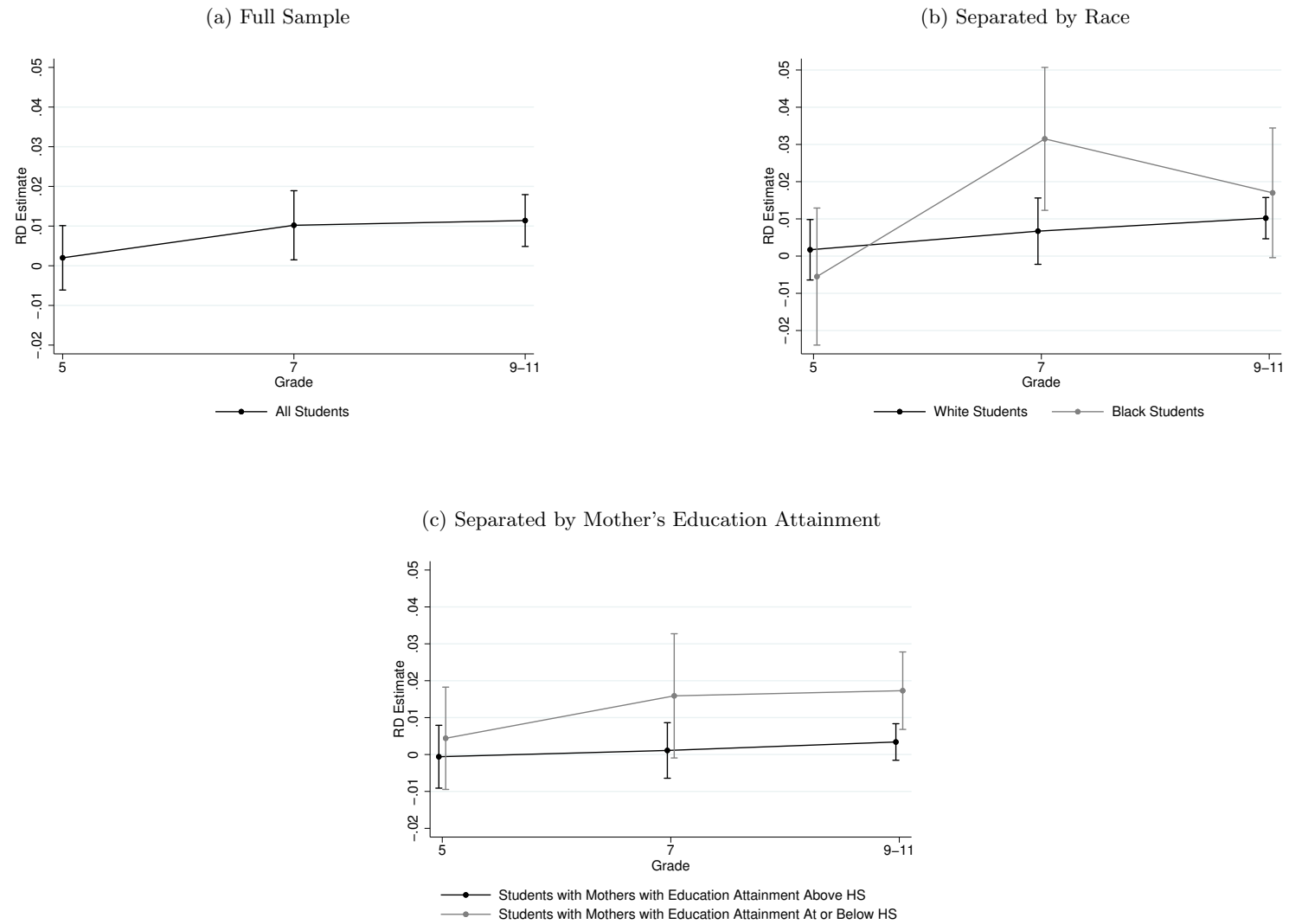
Note: Figures depicts estimated regression discontinuities in grade-for-age status for being born before the New Year in grades 5, 6 and 9-11 recorded in Tables 2, 3, and 5 with a bandwidth of two months. Estimation process detailed in text.

Figure 10: Average Adult Outcomes by Age Group

(a) Share Graduated High School

(b) Average Earned Income

(c) Share in Labor Force
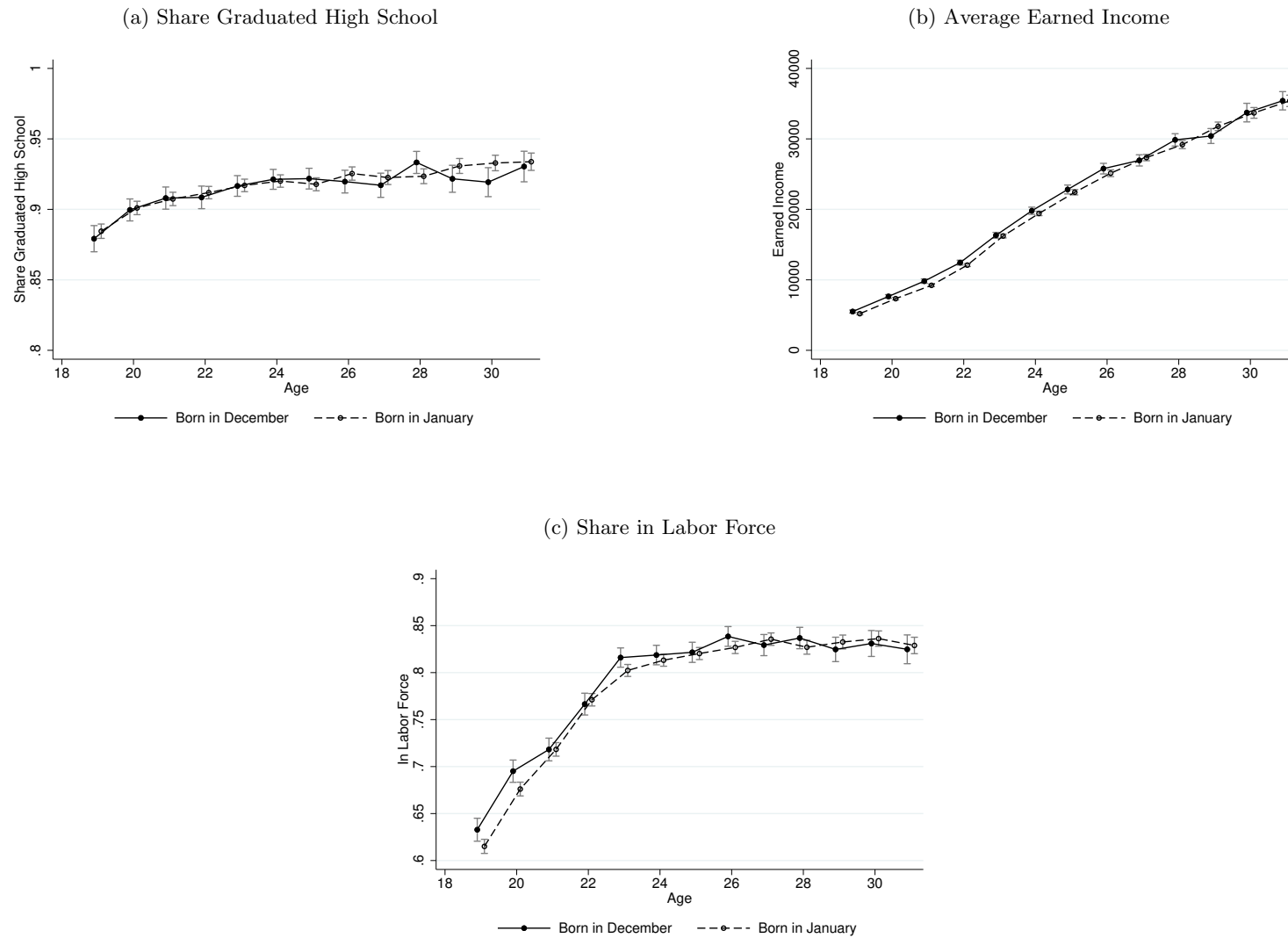
Note: Figure depicts average trends in the whole population of the variables described omitting adults born December 11th through January 9th. "December" births are children born from November 15th to December 10th, and "January" births are children born from January 10th to February 15th.

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Figure 11: Average Composite Measure of Outcomes by Age Group and Subgroup

(a) Full Sample
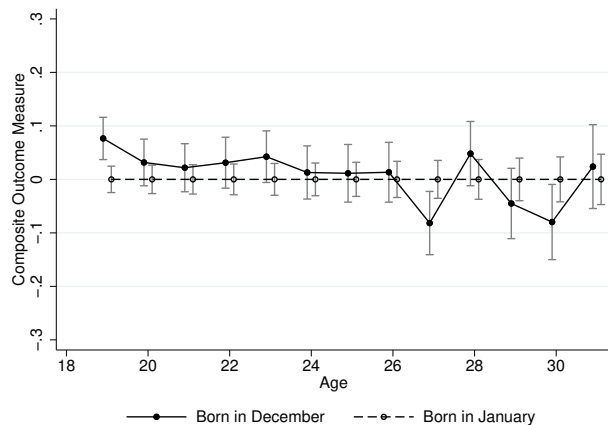
(b) Black Adults
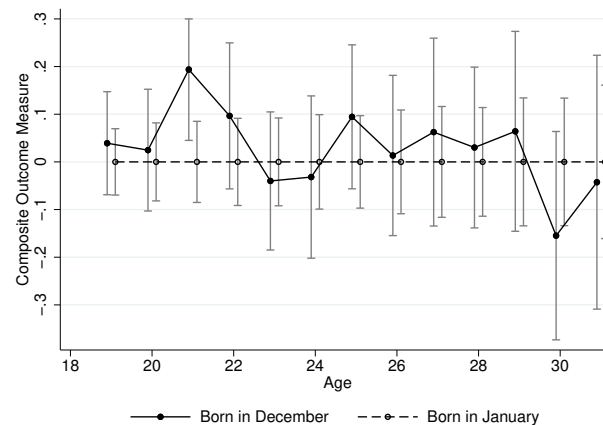
(c) Adults Born in Counties with Average Mothers' Education Attainment in Lowest Quartile
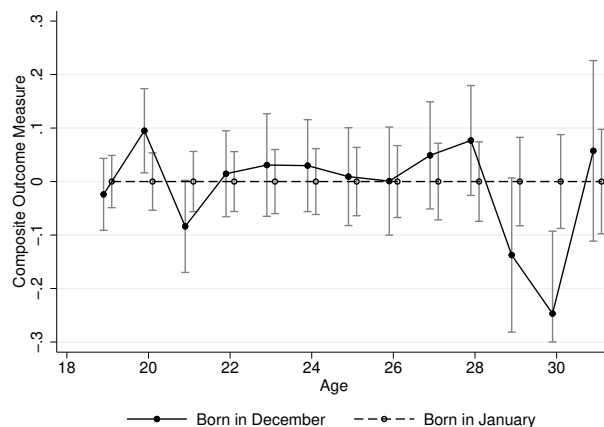
Note: Figure depicts average trends in a composite measure of adults' outcomes by age, omitting adults born December 11th through January 9th. The composite measure reflects labor force participation, earned income, SNAP receipt and high school graduation status. Process that creates this measure described in text. Note that the measure takes on average value 0 for individuals born after the New Year by construction, but there is a standard error present due to sampling variation.

Figure 12: Estimated Discontinuities in Composite Measure of Outcomes by Age Group and Subgroup

(a) Full Sample

(b) By Race

(c) By County Education Level
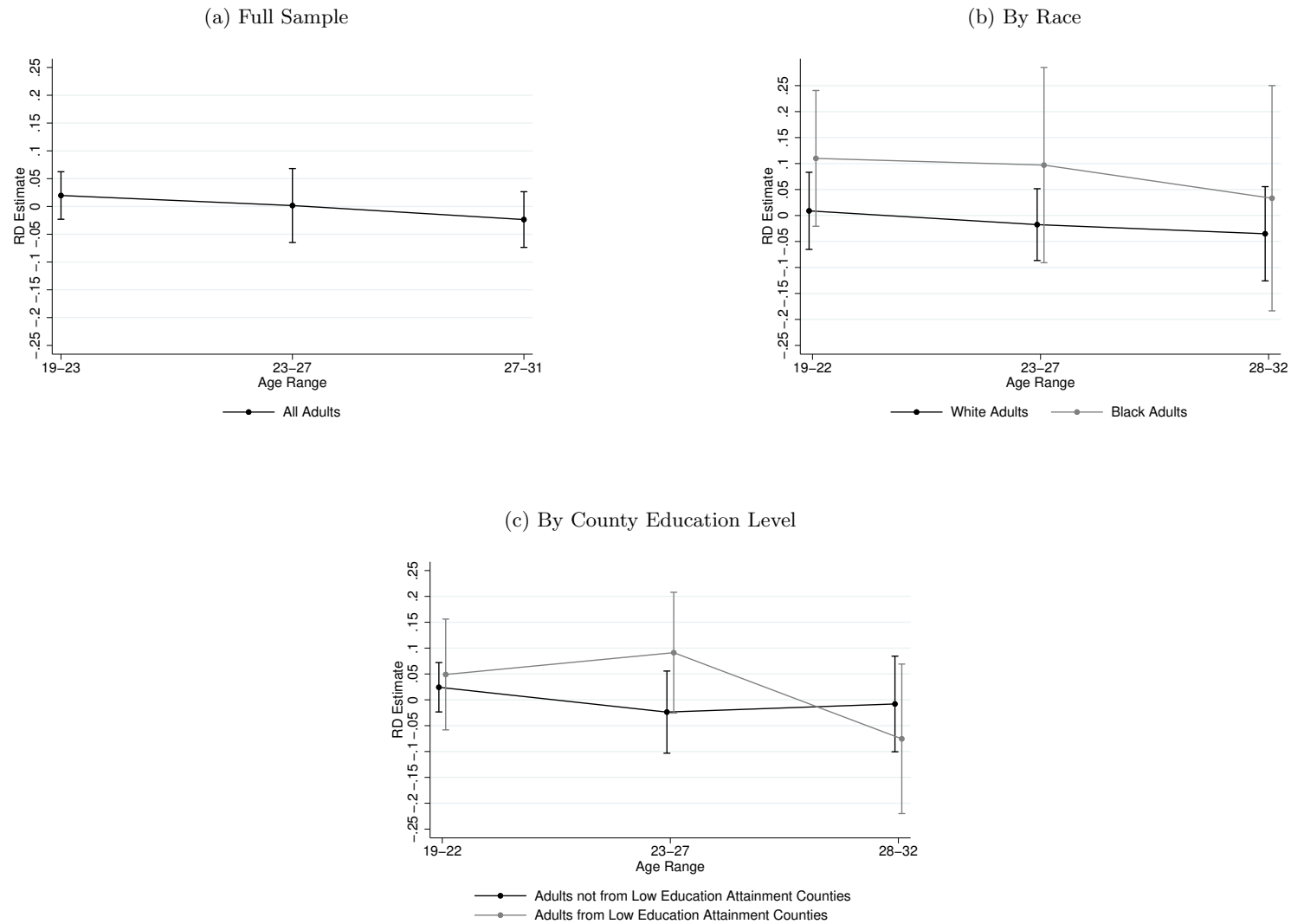
Note: Figures depicts estimated regression discontinuities for an adult being born before the New Year in composite measure of outcomes for adults aged 19-22, 23-27 and 28-32. Results recorded in Tables 7, 8, and 9 with a bandwidth of two months. Estimation process detailed in text.

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	-0.0410	-0.0227*	-0.0172*
	(0.001)	(0.0258)	(0.0119)	(0.0097)
Child is Black	0.117	0.00140	0.00400	0.00240
	(0.001)	(0.0129)	(0.0066)	(0.0055)
Child is non-White, non-Black	0.159	0.0396**	0.0187**	0.0147*
	(0.001)	(0.0193)	(0.0092)	(0.0077)
Child State of Residence Same as Birth	0.955	-0.00430	-0.00240	-0.00480
	(0.001)	(0.0101)	(0.0053)	(0.0042)
Total Children in Household	1.937	-0.0480	-0.0295	-0.0299
	(0.001)	(0.0466)	(0.0218)	(0.0197)
Child Lives with Both Parents	0.706	0.00980	-0.00900	-0.00560
	(0.001)	(0.0235)	(0.0122)	(0.0093)
Child's Household Has Any Earned Income	0.807	0.0457**	0.0144	0.00600
	(0.001)	(0.0178)	(0.0092)	(0.0077)
Child's Household Has Any Other Income	0.112	-0.00510	0.000500	0.000600
	(0.001)	(0.0130)	(0.0078)	(0.0061)
Child's Household Has Any Retirement Income	0.0300	-0.00390	0.00290	0.00440
	(0.001)	(0.0066)	(0.0039)	(0.0031)
Child's Household Has Any Supplemental Income	0.0150	0.00300	0.00330	0.00310
	(0.001)	(0.0058)	(0.0035)	(0.0030)
Child's Household Has Any Welfare Income	0.0600	-0.0138	-0.00200	-0.00340
	(0.001)	(0.0215)	(0.0105)	(0.0081)
Child's Household's Earned Income	41500	2300	474.8	79.18
	(71600)	(1700)	(700)	(800)
Child's Household's Other Income	469.8	-8.781	4.689	20.45
	(182.3)	(85.16)	(53.63)	(42.86)
Child's Household's Supplemental Income	84.83	11.92	13.89	14.39
	(23.32)	(30.15)	(19.57)	(16.46)
Child's Household's Total Income	42000	1600	1300	814.7
	(84000)	(1600)	(843.7)	(712.7)
Child's Household's Wage Income	39500	1300	70.91	-137.6
	(68500)	(1800)	(950.9)	(790.2)
Child's Household's Welfare Income	119.3	-72.69**	-27.29	-18.18
	(19.20)	(35.38)	(19.45)	(15.35)
Maximum Age of Parents	30.72	0.142	0.103	0.0206
	(0.002)	(0.3087)	(0.1557)	(0.1246)
Maximum Wage Income of Parents	0.880	0.0174	0.00170	-0.00160
	(0.001)	(0.0113)	(0.0069)	(0.0055)
Maximum Welfare Income of Parents	0.0480	-0.00960	-0.00240	-0.00540
	(0.001)	(0.0132)	(0.0080)	(0.0066)
Maximum Wage Income of Parents	33000	999	1000	806
	(54500)	(1600)	(848.2)	(670.9)
Maximum Education Attainment of Parents	13.68	0.136	-0.00610	-0.0222
	(0.001)	(0.1117)	(0.0629)	(0.0489)
Either Parent is in Labor Force	0.897	0.00260	-0.00300	-0.00250
	(0.001)	(0.0104)	(0.0068)	(0.0049)
Maximum Usual Hours of Work of Parents	41.24	0.248	0.0283	-0.0144
	(0.013)	(0.9542)	(0.5250)	(0.4227)

Note: Table records estimated discontinuities in child and family covariates for a child being born before the New Year . Results estimated using children in the 2000 Census born between 1999 and 2000. Estimation strategy described in text.

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 Weeks of Work Last Year of Parents	43.04	0.842	-0.0117	-0.0482
	(0.013)	(0.9000)	(0.4911)	(0.4152)
Either Parent Worked Last Year	0.936	0.00840	0.00260	0.00340
	(0.001)	(0.0081)	(0.0052)	(0.0040)
Age of Mother	28.44	0.428	0.0868	0.0229
	(0.002)	(0.3302)	(0.1772)	(0.1443)
Mother Has Any Wage Income	0.681	0.0548**	0.0192	0.0111
	(0.001)	(0.0236)	(0.0133)	(0.0109)
Mother Has Any Welfare Income	0.0480	-0.00810	-0.00660	-0.00860
	(0.001)	(0.0122)	(0.0072)	(0.0060)
Mother's Wage Income	15000	2900***	1300**	850.0*
	(26500)	(1000)	(567.4)	(466.8)
Mother's Education Attainment	13.27	0.3927***	0.0721	0.0196
	(0.001)	(0.1312)	(0.0841)	(0.0676)
Mother is in Labor Force	0.554	0.0302	0.00210	0.00100
	(0.001)	(0.0295)	(0.0156)	(0.0123)
Mother is Married	0.836	0.00420	0.00560	0.00840
	(0.001)	(0.0175)	(0.0107)	(0.0079)
Mother is Single Household Head	0.0770	0.00570	0.0127**	0.00730
	(0.001)	(0.0106)	(0.0061)	(0.0052)
Mother's Usual Hours of Work	25.86	1.949**	0.8732*	0.653
	(0.022)	(0.8465)	(0.4650)	(0.3950)
Mother's Weeks of Work Last Year	29.36	2.157**	0.664	0.456
	(0.031)	(1.039)	(0.5903)	(0.5056)
Mother Worked Last Year	0.711	0.0454*	0.0179	0.0137
	(0.001)	(0.0239)	(0.0132)	(0.0110)

Note: Table records estimated discontinuities in child and family covariates for a child being born before the New Year . 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	0.0061	-0.0023	-0.0022
	(0.001)	(0.0055)	(0.0025)	(0.0020)
1st	0.931	0.00280	0.00520	0.00610
	(0.001)	(0.0121)	(0.0059)	(0.0045)
5th	0.915	-0.00520	-0.00180	0.00200
	(0.001)	(0.0083)	(0.0048)	(0.0041)
7th	0.903	0.0158	0.0105*	0.0102**
	(0.001)	(0.0102)	(0.0057)	(0.0044)
9th	0.878	0.0139**	0.0084**	0.0088***
	(0.001)	(0.0059)	(0.0042)	(0.0032)
10th	0.864	0.00200	0.00560	0.00500
	(0.001)	(0.0120)	(0.0066)	(0.0052)
11th	0.855	0.0245***	0.0205***	0.0211***
	(0.001)	(0.0076)	(0.0043)	(0.0033)
9th - 11th	0.877	0.0123**	0.0113***	0.0114***
	(0.001)	(0.0059)	(0.0032)	(0.0024)

Note: Table records estimated discontinuity in grade-for-age status for a child being born before the New Year by grade of student for full population. 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	0.000600	-0.00130	0.00170
		(0.001)	(0.0080)	(0.0049)	(0.0041)
	Black	0.871	-0.0194	-0.0127	-0.00550
		(0.001)	(0.0188)	(0.0110)	(0.0093)
	Difference		-0.0200	-0.0114	-0.00720
7th	White	0.912	0.00990	0.00680	0.00670
		(0.001)	(0.0105)	(0.0059)	(0.0045)
	Black	0.845	0.0218	0.0311**	0.0315***
		(0.001)	(0.0223)	(0.0118)	(0.0097)
	Difference		0.0119	0.0244*	0.0248**
9th-11th	White	0.879	0.00720	0.0102***	0.0102***
		(0.001)	(0.0065)	(0.0036)	(0.0028)
	Black	0.793	0.0207	0.0132	0.0170**
		(0.001)	(0.0207)	(0.0111)	(0.0088)
	Difference		0.0135	0.00310	0.00690

Note: Table records estimated discontinuity in grade-for-age status for a child being born before the New Year by grade of student among White and Black children. 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	-0.00380	-0.00290	-0.00110
		(0.001)	(0.0079)	(0.0056)	(0.0046)
	Below Poverty Threshold	0.863	0.00220	-0.000800	0.00530
		(0.001)	(0.0306)	(0.0135)	(0.0109)
	Difference		0.00590	0.00200	0.00640
7th	Above Poverty Threshold	0.923	0.0172**	0.0111**	0.0095**
		(0.001)	(0.0085)	(0.0053)	(0.0041)
	Below Poverty Threshold	0.846	0.0176	0.0117	0.0132
		(0.001)	(0.0230)	(0.0144)	(0.0115)
	Difference		0.000400	0.000600	0.00380
9th-11th	Above Poverty Threshold	0.893	0.00520	0.0072***	0.0072***
		(0.001)	(0.0043)	(0.0027)	(0.0022)
	Below Poverty Threshold	0.786	0.0196	0.0162	0.0198**
		(0.001)	(0.0201)	(0.0109)	(0.0087)
	Difference		0.0143	0.00900	0.0126

Note: Table records estimated discontinuity in grade-for-age status by grade of student for a child being born before the New Year among children with different levels of contemporaneous family income. 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	-0.00650	-0.00320	-0.000600
		(0.001)	(0.0078)	(0.0052)	(0.0043)
	Mother's Education not above HS	0.887	-0.00540	-0.00200	0.00440
		(0.001)	(0.0153)	(0.0072)	(0.0061)
	Difference		0.00110	0.00130	0.00500
7th	Mother's Education above HS	0.932	-0.00120	-0.000700	0.00110
		(0.001)	(0.0081)	(0.0047)	(0.0038)
	Mother's Education not above HS	0.874	0.0207	0.0168	0.0159*
		(0.001)	(0.0180)	(0.0107)	(0.0085)
	Difference		0.0219	0.0175	0.0148
9th-11th	Mother's Education above HS	0.916	0.00350	0.00190	0.00340
		(0.001)	(0.0058)	(0.0031)	(0.0025)
	Mother's Education below HS	0.825	0.0105	0.0173**	0.0173***
		(0.001)	(0.0117)	(0.0067)	(0.0053)
	Difference		0.00700	0.0155**	0.0139**

Note: Table records estimated discontinuity in grade-for-age status for a child being born before the New Year by grade of student among children with different levels of mother education attainment. 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	0.00150	0.00190	0.00420
	(0.001)	(0.0093)	(0.0056)	(0.0047)
7th	0.904	0.0177	0.0110*	0.0100**
	(0.001)	(0.0114)	(0.0063)	(0.0050)
9th-11th	0.867	0.0172***	0.0129***	0.0125***
	(0.001)	(0.0055)	(0.0032)	(0.0025)

Note: Table records estimated discontinuity in grade-for-age status by grade of student for a child being born before the New Year among children living in the same state as birth. Results estimated using children in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.

Table 7: 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	-0.0473	0.0028	0.0101
		(1)	(0.0484)	(0.0261)	(0.0216)
Composite Measure	19-22	0.0000	0.0481	0.0249	0.0197
		(1)	(0.0597)	(0.0409)	(0.0336)
Composite Measure	23-27	0.0000	-0.1204*	-0.0152	0.0016
		(1)	(0.0643)	(0.0301)	(0.0253)
Composite Measure	28-32	0.0000	-0.0819	-0.0175	-0.0236
		(1)	(0.0748)	(0.0447)	(0.0374)
Graduated High School	19-27	0.9161		0.0006	0.0012
		(0.0006)		(0.0029)	(0.0023)
Graduated High School	19-22	0.9092		0.0008	0.0011
		(0.0009)		(0.0044)	(0.0038)
Graduated High School	23-27	0.9210		0.0002	0.0011
		(0.0007)		(0.0034)	(0.0028)
Graduated High School	28-32	0.9321		-0.0047	-0.0044*
		(0.0009)		(0.0034)	(0.0026)
Earned Income	19-27	16780		-143	-111
		(42.6)		(182)	(155)
Earned Income	19-22	9582		7.5	14
		(43)		(169)	(133)
Earned Income	23-27	21920		-280	-217
		(62.7)		(292)	(244)
Earned Income	28-32	33100		-1.76	-376
		(129)		(675)	(569)
In Labor Force	19-27	0.7763		0.0048	0.0048
		(0.0009)		(0.0048)	(0.0037)
In Labor Force	19-22	0.7238		0.0070	0.0041
		(0.0014)		(0.0086)	(0.0064)
In Labor Force	23-27	0.8138		0.0028	0.0051
		(0.0010)		(0.0051)	(0.0039)
In Labor Force	28-32	0.8234		-0.0032	-0.0010
		(0.0014)		(0.0081)	(0.0068)
SNAP	19-27	0.1528		0.0015	0.0004
		(0.0007)		(0.0050)	(0.0040)
SNAP	19-22	0.1480		-0.0021	-0.0020
		(0.0011)		(0.0091)	(0.0072)
SNAP	23-27	0.1561		0.0041	0.0022
		(0.0010)		(0.0043)	(0.0035)
SNAP	28-32	0.1566		-0.0036	-0.0026
		(0.0013)		(0.0069)	(0.0055)

Note: Table records estimated discontinuity in adult outcomes for an adult being born before the New Year by age group for the full sample. Results estimated using adults in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.

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

Outcome	Age Range	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	-0.0121	-0.0059
			(0.0582)	(0.0334)	(0.0269)
		Black	0.0775	0.1240**	0.0990*
			(0.1208)	(0.0744)	(0.0550)
		Difference	0.1433	0.1361**	0.1049**
Composite Measure	19-22	White	0.0382	0.0206	0.0090
			(0.0742)	(0.0440)	(0.0375)
		Black	0.1101	0.1340	0.1100**
			(0.1489)	(0.1086)	(0.0660)
		Difference	0.0719	0.1134*	0.1010**
Composite Measure	23-27	White	-0.1434*	-0.0364	-0.0175
			(0.0788)	(0.0432)	(0.0349)
		Black	-0.0148	0.1124	0.0971
			(0.2210)	(0.1162)	(0.0949)
		Difference	0.1286	0.1488	0.1146
Composite Measure	28-32	White	-0.0257	-0.0142	-0.0351
			(0.1019)	(0.0567)	(0.0458)
		Black	-0.1497	0.0333	0.0333
			(0.2525)	(0.1361)	(0.1095)
		Difference	-0.1240	0.0475	0.0684

Note: Table records estimated discontinuity in adult outcomes for an adult being born before the New Year by age group among White and Black adults. Results estimated using adults in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.

Table 9: 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	-0.0150	-0.0029
			(0.0589)	(0.0287)	(0.0241)
		Avg. Mothers' Ed Below Lowest Quartile	0.0496	0.0692	0.0555
			(0.0888)	(0.0518)	(0.0375)
		Difference	0.1269	0.0842	0.0584
Composite Measure	19-22	Avg. Mothers' Ed Above Lowest Quartile	0.0146	0.0190	0.0243
			(0.0773)	(0.0485)	(0.0401)
		Avg. Mothers' Ed Below Lowest Quartile	0.1527	0.0497	0.0049
			(0.1206)	(0.0667)	(0.0541)
		Difference	0.1381	0.0308	-0.0194
Composite Measure	23-27	Avg. Mothers' Ed Above Lowest Quartile	-0.1490**	-0.0415	-0.0237
			(0.0732)	(0.0321)	(0.0277)
		Avg. Mothers' Ed Below Lowest Quartile	-0.0173	0.0838	0.0912
			(0.1144)	(0.0746)	(0.0590)
		Difference	0.1317	0.1253	0.1149*
Composite Measure	28-32	Avg. Mothers' Ed Above Lowest Quartile	-0.0303	0.0102	-0.0080
			(0.0860)	(0.0558)	(0.0467)
		Avg. Mothers' Ed Below Lowest Quartile	-0.2692*	-0.1171	-0.0754
			(0.1490)	(0.0873)	(0.0730)
		Difference	-0.2389	-0.1273	-0.0674

Note: Table records estimated discontinuity in adult outcomes for an adult being born before the New Year by age group among adults born in counties where average mothers' education is below the lowest quartile and above the lowest quartile. Results estimated using adults in the 2000 Census and 2001-2016 ACS. Estimation strategy described in text.

Appendices

Appendix A Additional Detail on Variables and Data

As mentioned in the text, this paper uses the 2000 long-form Census and the 2001-2016 ACS to estimate causal impacts, and uses the CPS to estimate the size of the discontinuity in after-tax income. This appendix discusses data quality issues associated with these sources sequentially.

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

As described in the text, this paper assigns grade-for-age status to students based on 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 be a specific age by a certain date before being eligible to enter either Kindergarten or 1st grade in that state. Comprehensive data on these state policies for Kindergarten entrance were collected by Bedard and Dhuey (2012) and they generously provided their most recent data covering 1955 to 2015. Using this data, this paper assigns expected completed grades to students assuming that they entered Kindergarten or first grade in the first year that they were eligible for those grades and then progressed through all other grades sequentially without repeating a grade. A student is grade-for-age 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.

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

Third, there are only a handful of grades where grade-for-age status can be reliably assigned due to the nature of the grade attainment and enrollment questions in the 2000 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/Preschool through 4th grade, 5th grade through 6th grade, 7th grade through 8th grade, and 9th, 10th, 11th and 12th grades. These same surveys only allow respondents to list whether

they have recently attended Nursery School/Preschool, Kindergarten, 1st through 4th grade, 5th grade through 8th grade, and 9th grade through 12th grade. Therefore, the best grades to measure grade-for-age status would be grades where students would be expected to have completed a grade where the student's family could have listed completion or attendance of a prior grade. These grades would be pre-Kindergarten, Kindergarten, 1st, 5th, 7th, 9th, 10th and 11th grades. To see why, for example, 6th grade cannot be included, note that whether or not a student has completed 5th or 6th cannot be distinguished from that student's information in the 2000 Census and the 2001-2007 ACS.[24]

Fourth, the response day of a household will affect the grade a student may have completed or attended. In both the Census and the ACS, the education attainment question asks for the highest grade completed by a respondent. Thus, the date of response to an individual survey matters for determining the grade a student has completed by date of response. 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 likely list that student as having completed the fifth grade. To account for this issue, this paper assumes that households responding to surveys between January 1st and April 10th will still have their children enrolled in the grade that they would have enrolled in at the beginning of the school year, and the children will be recorded as having finished the previous grade they finished before enrolling in this grade. Households that respond to surveys between July 1st and December 31st will either have completed the previous grade (if the student passed and is grade-for-age) or will only have completed the grade before that (if the student was retained and is not grade-for age). As grade-for-age status cannot be ascertained reliably for the intervening months, this paper drops those months from consideration for all calculations.[25]

One complication worth noting about timing of response to questions is that the vast majority of responses to the 2000 Census happen in March through the end of April, while the sampling structure of the ACS ensures there are responses throughout the year (Stackhouse and Brady, 2003). For school grades that are nearly always organized by regular school calendars, the previously mentioned adjustment regarding date of response offers an accurate method of calculating average likelihood of a child being grade-for-age within that grade. However, when looking at pre-Kindergarten enrollment, the lack of standard enrollment policies

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[24]Grade-for-age status in the text is calculated with completion data for 5th, 7th, 9th, 10th and 11th grades, and with current enrollment data for pre-Kindergarten, Kindergarten and 1st grade.

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

across states and disctricts ensures that more children tend to be enrolled in pre-Kindergarten programs for months closer to the beginning of the next school year. As the Census responses happen disproportionately in the later months before the lead-up to the next school year, data from the 2000 Census would increase the estimated average level of enrollment in pre-Kindergarten for the year prior to Kindergarten enrollment. While including the 2000 Census data does not impact the significance of discontinuities reported in the paper, this paper restricts attention to individuals in the ACS 2001-2016, as the average in this data offers a more accurate estimate of average likelihood of being enrolled in nursery school or pre-school in the year prior to school enrollment.

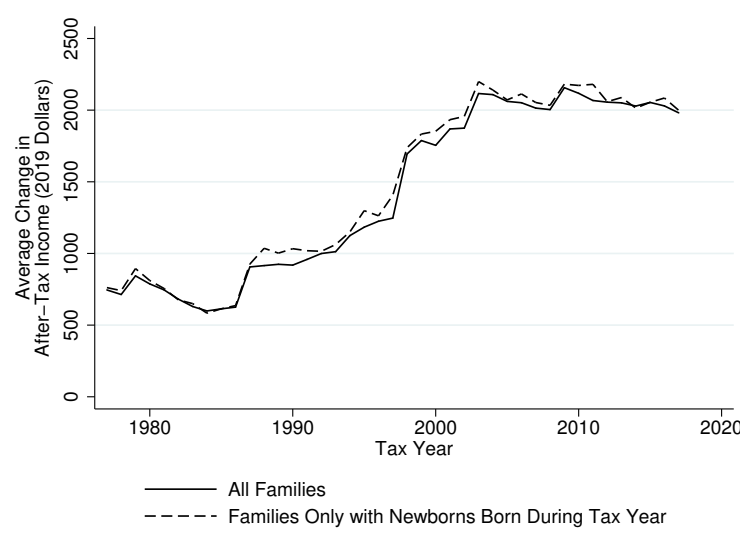## Estimating the Discontinuity in After-Tax Income using CPS Dta

As described in the text, this paper uses the March CPS files to estimate the size of the discontinuity in after-tax income for a family for having a child in December rather than January. The estimation process draws inspiration from Hoynes, Miller and Simon (2015). The sample for the estimation process are parents with an infant under three who are in the March Current Population Survey (CPS) in a four year radius for the year after the tax year. So for example, when calculating the discontinuity for the 1986 tax year, this paper uses all parents with an infant under three in a four year radius of the 1987 March CPS (1983 to 1991). All parents with an infant under 3 are then treated as having an infant who is under 1 who could have been born in January or December. Note that the central year in the data included is the year after the relevant tax year, as the CPS income data reflect income from the previous calendar year, which is the relevant year for computing taxes for the tax year. The inclusion of other years and other ages is only to increase power when calculating effects for smaller likely disadvantaged groups. A later part of this section investigates potential bias introduced by this choice.

Using this sample, this paper calculates tax obligations for having a child born in December by summing income measures at the family level and calculating the total state and federal tax burden using TAXSIM assuming that the family with the infant under 3 is the relevant tax filing unit. This paper calculates tax obligations for having a child born in January using the same data with the same income measures with reducing the number of dependents under the age of 13 by one (as if the infant is born after December). The tax discontinuity is then the difference between the two calculated tax obligations, and the percent change in after-tax income is this change divided by after-tax income for having a child born in January. Individuals with no reported income are included in all calculations, but they comprise a small share of households over all years, and are included as a 0 percent change in income.

As a check on the potential for bias created by including parents with slightly older children and other

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

Figure A.1: 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.

Appendix B   Tax Policies Related to Children

As discussed in the paper, the discontinuity depicted in Figures 1 and 2 reflects three separate features of how the tax system treats infants: personal exemptions for dependents, the Earned Income Tax Credit (EITC) and the Child Tax Credit (CTC). These four tax benefits have changed substantially over time.

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 a benefit that gradually increases in income until it reaches a maximum level and eventually phases out to 0. Importantly, this benefit is refundable, meaning that it can both reduce tax obligations and result in a tax refund where a parent receives a refund for the difference between tax obligations and the size of the EITC credit. 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, 2018b), 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. [26]

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, 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 of information on child care expenses in the CPS, it is omitted from consideration here, although it would

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[26]One notable change from 1986 complicating analysis of take-up in this data is the fact that, beginning in tax year 1987, tax filers were required to list the Social Security Number for exemptions for dependents that they claimed. It is well-known that this requirement resulted in a drop of the number of dependents claimed from 77 million in tax year 1986 to 70 million in tax year 1987. Thus, it is possible that there is not as sharp a discontinuity in claiming of dependents around the New Year in years prior to 1987 as parents with children born after the New Year may be claiming them inappropriately. There is no way to accomodate this fact in this data.

on average increase the size of the discontinuity.[27]

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[27]The 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, 2018a)

Appendix C Theoretical Foundations of Birth Shifting

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

Consider the following one period family utility optimization problem:

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

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

In the first equation, C is spending on a new born, δ is a multiplier on C drawn from a distribution (where higher levels of δ indicate high marginal utility of investments in C), F is spending on the rest of the family, L is a unitary measure of labor for the household, d is the realized date of birth (centered such that $d = 0$ is New Year's day) and d' is the date of birth that would happen without a parent altering the timing of birth, and $f(d - d')$ is a cost function that reaches a minimum when $d = d'$. This term reflects the fact that altering the exact date of birth of a child away from the expected due date, either by Cesearian section or induced labor is costly to a family in terms of consequences to an infant and a mother's health. Given the relatively smooth distribution of births outside of holidays depicted in Figure 4, assume that d' is randomly assigned. The final term, η is a utility cost to being born on the New Year independent of tax benefits.

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

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

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

2. d' is realized;

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

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

To gain an understanding of how this model works, note that as long as w, p_C, p_F and $g(d')$ are the same, then L should be the same for everyone.

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

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

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

Where C_{-1}, F_{-1}, $C_{d'}$, $F_{d'}$ represent consumption choices such that budget sets balance at either $d = -1$ or $d = d'$. Note that, for any given level of δ and η, the convex cost in d' means that there is some maximum date past which individuals will not move the timing of their birth. Furthermore, note that for each level of d', the individuals who move the timing of their birth will have larger values of δ, indicating a larger marginal utility of spending on children. Finally, note that the utility cost of having a birth at $d' = 0$ ensures that individuals will also move away from having a birth on New Year's Day in particular.

This model has important implications for what happens near the discontinuity. First, unlike the infinite cost setting before, actual observed birthdays d will not be randomly distributed, and a larger mass of individuals will move from the days after New Year's Day to the day right before New Year's Day. Second, comparing spending patterns of individuals right before the New Year to spending patterns of individuals born on New Year's day is no longer indicative of the pure income effect of increasing a family's economic

resources. The individuals born after the New Year will include people with comparatively low values of δ, indicating that their spending on their infants will be comparatively lower, and the individuals born before the New Year will include people with comparatively higher values of δ, indicating that their spending on their infants will be comparatively higher. Thus, a comparison of their spending will both indicate the pure effect of the increase in after-tax income, but also the difference in the distribution of δ that comes from the people selecting to have births before the New Year having higher marginal utility of spending on children. These differences would mean that a naive comparison of spending on children at the New Year would offer a biased upwards treatment effect.

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

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

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