Re-examining the Impact of Education on Teenage Motherhood: Evidence from North Carolina

Poh Lin Tan[1]

Abstract

The literature on teenage motherhood suggests that women who have more years of education or better test scores are less likely to give birth during teenage years. However, quasi-experimental studies yield mixed evidence as to whether years of education have a negative causal impact on teenage childbearing, with studies using school entry laws showing no evidence of a causal relationship. This paper uses a similar empirical strategy with a highly detailed dataset which includes not only birth certificate data but also individually linked school administrative records. Consistent with previous research, the evidence suggests that individuals affected by school entry laws have fewer years of education but also better test scores. Using an IV regression strategy to distinguish the impacts of years of education and test scores, I show that both measures of educational success have negative causal impacts on teenage childbearing.

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[1] Sanford School of Public Policy, Duke University. I thank Philip J. Cook and Seth G. Sanders for their helpful comments.

Introduction

The literature on teenage motherhood suggests that women who do well in school or have more years of education are less likely to give birth during teenage years. Abrahamse, Morrison and Waite (1988), Manlove (1997) and Meade, Kershaw and Ickovics (2008), for example, show that teenage girls with better school performance are at lower risk of childbearing, even after controlling for a number of observed family characteristics. There are a number of theoretical causal pathways through which educational success could lower an individual's probability of teenage motherhood. First, educational success may increase the opportunity costs of early childbearing. Girls who do well in school have more options in higher education and in the labor market, and having a teenage birth would be more costly not only due to higher foregone earnings, but also due to non-pecuniary labor market returns in terms of job satisfaction and prestige (Cygan-Rehm and Maeder 2013).

Second, educational success may reduce tastes for teenage motherhood. Women who have more years of education or have higher test scores are likely to expect higher future household income due to higher own earnings as well as higher spousal earnings, since they are more likely to be married and to have a better educated partner. Economic theory suggests that the income elasticity of demand for child quality exceeds that for demand for child quantity, so that higher-income individuals choose to have fewer children and to invest more heavily in each child. Women with more education may thus prefer smaller family sizes and hence avoid early childbearing (McCrary and Royer 2011). In addition, girls with more years of education benefit from more exposure to positive influence from school-going peers (Cook and Kang 2013), which may lead to a more negative view of teenage motherhood. Third, educational success may increase contraceptive knowledge (McCrary and Royer 2011) or the ability to more accurately assess the risks of unprotected sexual activity.

Despite the plausible theoretical basis for a causal relationship between educational success and teenage motherhood, establishing this through data analysis can be difficult for several reasons. First, poor school performance and teenage childbearing may be driven by a common third factor such as poverty or negative peer environments, leading to omitted variable bias. Empirically, family and peer environments are difficult to fully control for – even within families, some women may receive more resources than their sisters. Second, fertility intentions and outcomes may affect school performance and probability of dropping out, leading to simultaneous causality bias.

To avoid these estimation issues, some researchers have turned to quasi-experimental approaches. One such approach uses changes in compulsory schooling laws, and studies based on data from Germany (Cygan-Rehm and Maeder 2013), Peru (Rivera 2013), the UK (Silles 2011), Norway and the US (Black, Devereux and Salvanes 2008) consistently find that increased years of schooling result in lower rates of teenage childbearing. Another quasi-experimental approach uses discontinuities in school entry age due to state cutoff dates for beginning kindergarten, where women born later than a certain date start kindergarten and each subsequent grade at an older age. Using a regression discontinuity model, McCrary and Royer (2011) find that women affected by state cutoff dates in Texas and California have significantly fewer years of education, but are no more likely to give birth at younger ages than women who were not affected. Hence, the evidence from existing quasi-experimental studies yield mixed evidence as to whether years of education have a negative causal impact on teenage childbearing.[2]

One potentially important drawback of using discontinuities in school entry age is that school entry age has a negative impact on years of education but also a positive impact on other measures of educational success such as test scores. In their paper, McCrary and Royer argue that the positive impact of school entry age on test scores diminishes with age and is unlikely to be large enough to fully balance the negative impact on years of schooling. The literature on school entry age and test scores, on the other hand, generally point to fairly strong effects that persist at least up to the eighth grade (Bedard and Dhuey 2006; Cook and Kang 2013; Elder and Lubotsky 2008). Using two nationally representative surveys, the Early Childhood Longitudinal Study and the National Educational Longitudinal Study, Bedard and Dhuey show that individuals affected by state school entry cutoff dates have math test scores that are higher by 0.36 of a standard deviation in fourth grade and 0.28 of a standard deviation in eighth grade; similarly, using North Carolina administrative public school records, Cook and Kang show that

[2] Black, Devereux and Salvanes (2011) also use discontinuities in school entry age in Norway and find that women affected by cutoff dates in Norway are less likely to experience teenage motherhood. However, since the authors do not find any impact of school entry age on completed schooling at ages 27 or older, their study does not provide evidence for whether education has a causal impact on teenage childbearing. Two possible explanations for why the authors, unlike McCrary and Royer (2011), find no impact on years of schooling are that: a) the effects of school entry age differ for Norway and the United States, and b) while individuals affected by cutoff dates are more likely to drop out of high school, they are also more likely to have more years of higher education (Bedard and Dhuey 2006), potentially due to better school performance.

affected students have higher reading test scores in sixth, seventh and eighth grade. Citing this literature, Dobkin and Ferreira (2010) argue that the opposite impacts of school entry age on years of schooling and test scores likely accounts for why school entry age has no impact on a wide range of future outcomes, including employment rates, wages and family income, home ownership and marital status.

This paper contributes to the literature in two ways. First, using discontinuities in school entry age and a highly detailed administrative dataset from North Carolina which includes not only information on years of schooling but also public school test scores, I find a negative causal relationship between years of schooling and teenage fertility once the impact of school entry age on test scores is accounted for, consistent with the literature on changes in compulsory schooling laws. Second, I show that education *attainment* (measured using years of schooling) is not the only form of educational success which matters for teenage fertility – instead, educational *achievement* (measured using test scores) also appears to have a negative causal impact. Consistent with Dobkin and Ferreira (2010)'s argument, the evidence in this paper suggests that school entry age has a negative impact on teenage fertility due to higher test scores, and that this negative impact is roughly equal in magnitude to its positive impact on teenage fertility due to fewer years of schooling.

To my knowledge, this is the first empirical study to examine the impact of school entry age on teenage childbearing (or any other outcomes) while taking into account performance on school test scores, as well as the first study which attempts to distinguish the impacts of years of education and test scores. In addition, the richness of the dataset used in this paper allows for a more detailed search for heterogeneous effects at not only the individual level, but also at the broader school and school district levels.

Methods

Let whether an individual becomes a teenage mother be a function of her educational success and other characteristics, including her birth outcomes and family, school and school district environments. Next, let her educational success be a function of a) her age in terms of number of days born after July 2 (negative for those born on July 1 or earlier), hereafter denoted as Age_i, b) a dichotomous indicator for whether she was born after the North Carolina cutoff date for beginning kindergarten, (one if she was born on October 17 or later, and zero otherwise), hereafter denoted as $Cutoff_i$, and c) her birth outcomes and family, school and school district characteristics X_i and cohort-specific effects θ_c.

Note that if date of birth is randomly assigned and hence uncorrelated with X_i and θ_c, the estimated coefficients for Age_i and $Cutoff_i$ are unbiased even if X_i and θ_c are not controlled for. Hence, one important advantage of this empirical approach is that it avoids the difficulty of fully controlling for environmental characteristics. Below, I discuss some robustness checks which test whether date of birth may indeed be treated as uncorrelated with X_i and θ_c. A second important advantage of using discontinuities in school entry age rather than other interventions such as changes in compulsory schooling laws is that "treatment" is targeted at specific individuals rather than at entire cohorts, so that there are no changes in labor market conditions which could potentially account for observed differences in outcomes (Black, Devereux and Salvanes 2011; Cook and Kang 2013).

However, as discussed above, one drawback of this approach is that school entry age has opposite impacts on two measures of educational success (years of education and test scores), both of which may have a negative impact on teenage childbearing. Hence, a regression discontinuity approach does not allow us to draw conclusions about the impact of either measure on teenage childbearing. To see this, note that if school entry age has no impact on teenage childbearing, then either of two explanations is possible: a) neither years of education nor test scores affect teenage fertility, or b) both measures affect teenage fertility, and the impacts offset each other to a large extent. Similarly, if school entry age has a positive or negative impact on teenage childbearing, then either of two explanations is possible: a) only years of education or test scores affect teenage motherhood, or b) both measures affect teenage motherhood, but one has a larger impact than the other.

To address this drawback, the analysis in this paper makes use of both regression discontinuity as well as instrumental variable regression models. I begin by using regression discontinuity models to estimate the impacts of school entry age on years of education and tests scores. Following McCrary and Royer (2011) and Cook and Kang (2013), I use a local regression method which assigns more weight to data points closer to the cutoff date. Algebraically, the method chooses parameter values which minimize the following:

$$\sum_i(E_i - \beta_1 Cutoff_i - \beta_2 Age_i - \beta_3 Cutoff_i * Age_i - \beta_4 X_i - \theta_c)^2 K_h(Age_i),$$

where E_i is an educational outcome (years of education or test scores), and $K_h(Age_i)$ is a kernel function which assigns weights to individual observations where h is the bandwidth used. Cook and Kang (2013) note that the triangular kernel below has desirable statistical properties:

$$K_h(Age_i) = \left(1 - \frac{|Age_i|}{h}\right)1\left\{\frac{|Age_i|}{h} < 1\right\}.$$

I repeat the above analysis for teenage motherhood outcomes, where B_i is a dichotomous variable for whether the individual gave birth prior to age 20. I also examine whether school entry age has an impact on fertility at a) ages prior to 17, when most of the live births would have been conceived prior to the North Carolina minimum dropout age of 16, b) ages between 17 to 19, when the live births would have been conceived after the individual has been exposed to the possibility of dropping out. Cook and Kang (2013) show that the impact of school entry age on youth delinquency and criminality among males is negative at younger ages but positive at older ages, and argue that the effect of having higher test scores may dominate at younger ages while the effect of having fewer years of education may dominate at older ages when individuals have the option of dropping out of school.

The above analysis makes the important assumption that date of birth is randomly assigned and uncorrelated with individual-level characteristics. To test the validity of this assumption, I examine some graphical evidence of birth seasonality effects and test explicitly if these effects are discontinuous at the kindergarten entry cutoff date. Following previous papers (Black, Devereux and Salvanes 2011; Cook and Kang 2013), I restrict my sample to girls born a month or two before and after the cutoff date, which removes most of any birth seasonality effects, and compare the results from three different bandwidths: 45-day, 60-day and 75-day. Finally, I check if the regression discontinuity results are similar whether individual-level characteristics are controlled for or not.

Next, to distinguish the impacts of years of education and test scores on teenage motherhood, I turn to instrumental variable regression models. (Simply using regression discontinuity and controlling for one of the educational outcomes is not a suitable approach, since this method assumes, for instance, that girls born before and after the cutoff date with the same test scores are otherwise comparable. This assumption is problematic since girls born after the cutoff date have an age advantage, suggesting that the two groups likely differ on other unobserved attributes.) Since there are two endogenous variables, I use two instruments, $Cutoff_i$ and Age_i, as well as interaction terms between $Cutoff_i$ and a number of individual-level characteristics are included to allow for overidentification tests (see Table 10). Algebraically,

$$B_i = \beta_0 + \beta_1 TestScore_i + \beta_2 YearsEduc_i + \beta_3 X_i + \theta_c + \varepsilon_i,$$
$$TestScore_i = \alpha_0 + \alpha_1 Cutoff_i + \alpha_2 Age_i + \alpha_3 Cutoff_i{*}X_i + \eta_i,$$
$$YearsEduc_i = \gamma_0 + \gamma_1 Cutoff_i + \gamma_2 Age_i + \gamma_3 Cutoff_i{*}X_i + \upsilon_i,$$

where, unlike for the regression discontinuity models, the inclusion of the vector of individual-level characteristics X_i is not optional since their interaction terms with $Cutoff_i$ are used as instruments. Again, to remove most of any birth seasonality effects, I restrict my sample to girls born a month or two before and after the cutoff date, and compare the results from three different bandwidths: 45-day, 60-day and 75-day.

To check that this instrumental variable regression approach is valid, I test the relevance and exogeneity of my instruments using a) a test for weak instruments based on the Cragg-Donald statistic, which is a highly conservative test for regressions with more than one endogenous variable (Stock and Yogo 2002), and b) tests for endogenous instruments using the Anderson-Rubin and Basmann statistics for the limited information maximum likelihood (LIML) estimator. I also test if the results are robust to the exclusion of any single instrument.

To search for heterogeneous treatment effects, I repeat the above analysis for various subgroups at the individual, school and district levels (discussed in greater detail in the Data section).

Data

The dataset in this paper follows six North Carolina birth cohorts and consists of three components. The first component is each individual's birth certificate, which provides information about her birth outcomes and mother's race, age, educational level and marital status at the time of birth. The second component is each individual's public school administrative records, which provide information about whether she was registered in the public school system (excluding charter schools) and her age at registration for every grade between three and twelfth grade, as well as her end-of-grade test scores in the third and eighth grades. In addition, some information about the school and school district environments are available, including student poverty rates and the proportion of students who fail end-of-grade tests. The third component is any North Carolina birth certificate which lists an individual in the first and second component as the mother (in the rare case of multiple birth certificates, only data from the earlier certificate are used), and this component provides information about her age and educational attainment at birth. The dataset is de-identified and raw data can be obtained from the North Carolina Education Research Data Center, which performed all data linkages at the individual level.

The six birth cohorts are born between 1987 and 1992, where 1987 is the first year when linked birth certificates are available from the North Carolina Education Research Data Center. Since data are available up to 2012 for the second component, there is complete high school educational data for all cohorts up to age 20. However, since data are available only up to and including 2009 for the third component, there is complete information about the teenage childbearing histories only for the first three cohorts (1987-1989); for the last three cohorts (1990-1992), there is information only about their childbearing behavior up to and including age 16. Hence, while I analyze the impacts of school entry age on years of education and tests scores using data from all six birth cohorts, most of the analysis on teenage fertility uses data from the first three cohorts only.

Between 1987 and 1992, 276,614 female singleton live births were born in North Carolina to mothers residing in North Carolina. This sample excludes out-of-state births to North Carolina residents (4.1% of all births), which are slightly more likely to be born to younger, less-educated and unmarried white mothers (see Table A1). Of this original sample, I drop 10,918 observations (3.6%) which are neither non-Hispanic white nor non-Hispanic black (data on Hispanic ethnicity were not available in 1987 and all white and black individuals were included), and another 739 observations (0.3%) for which there were missing data for any of the following: birth parity, birth weight, mother's age, mother's education or mother's marital status. Most significantly, I drop 100,720 observations (36.4%) for which there are no public school records in third grade, eighth grade or at age 15, the maximum age before individuals are legally allowed to drop out of school in North Carolina. Hence, the final sample excludes individuals who did not survive to teenage years (including 2,419 observations with infant death certificates) as well as individuals who attended private or charter schools or moved out of state during these ages. These individuals are excluded not just to obtain a dataset with more complete educational data, but also because the remaining individuals are less likely to have moved out of state, so that any teenage births they might have are more likely to appear in the third component of the dataset. The excluded individuals tend to come from more advantaged socioeconomic backgrounds, with a lower proportion born to black or unmarried mothers (not shown here), and are hence generally at lower risk of teenage childbearing. Finally, I drop 10 observations for which there were missing data for school characteristics. Since most of the analysis is limited to observations born a month or two before and after the cutoff date, the final sample size for the largest bandwidth (75-day) is 68,771 observations, with the analysis on teenage fertility using 33,598 observations.

The dataset used in this paper has several important advantages for studying the impact of education on fertility. First and most importantly, unlike datasets used in previous studies, it includes information on individuals' performance in end-of-grade tests in the third and eighth grades, which allows me to distinguish the impacts of school entry age on years of education and test scores. All North Carolina public school students in the same grade in the same year take the same end-of-grade test, and each individual's test score is represented by her Z-score relative to the test results of all students (including male and other female students not included in the sample) who took the test. For greater efficiency, each individual's test score in the third or eighth grade is measured using the average of her verbal and math Z-scores. Around 1.6% and 1.3% of third and eighth grade test scores are missing; for these observations, test scores are imputed using the individual's test score in the other grade, her age at the time of tests, the number of grades she eventually completed (discussed in more detail below) and her age at school exit, as well as her birth outcomes, her family, school and school district characteristics and cohort effects. In the next section, the analysis shows that results are qualitatively similar whether or not the imputed test scores are included.

Second, this dataset follows the educational and teenage childbearing histories of six birth cohorts of women who attended public school in North Carolina between third grade and age 15, whereas previous studies such as McCrary and Royer (2011) use a more restricted sample which only include women who have had live births at younger ages. Hence, the analysis of the impact of school entry age on educational attainment in this paper applies to a wider socioeconomic range of women, i.e. women who are at lower as well as higher risks of teenage childbearing. This dataset is also considerably more suitable for estimating the impact of school entry age on early childbearing for a couple of reasons: a) data on teenage childbearing outcomes are available at the individual rather than the cohort level (McCrary and Royer calculate age-specific birth probabilities at the cohort level, computed as ratios of the number of live births to birth counts for each birth cohort), allowing for more precise estimation, and b) unlike in McCrary and Royer, this dataset is able to exclude women who did not attend local public school since they are more likely to have moved out of state and any births to them are more likely to be unobserved, hence reducing potential downward bias on birth probabilities.

Third, this dataset uses individual-linked administrative data rather than self-reported age and education data on birth certificates, which are not only higher quality but also provide information about each individual's educational attainment up to age 20 rather than at the time of birth. Since the majority of girls who give birth at school ages go on to graduate from high school without dropping out, and almost a third of those who drop out eventually graduate (Upchurch and MacCarthy 1990), the educational attainment of most teenage mothers is likely to have increased between the time of birth and age 20. Comparing data from administrative records and birth certificates for mothers aged 18 and below in this sample, I find that while self-reported age on birth certificates is highly similar to actual age, with 99.2% of observations being accurate, there is substantial disagreement between self-reported education and administrative school records, with 61.4% of observations having more years of education by age 20 than at the time of birth and only 28.3% remaining at the same level[3] (10.3% of observations have higher self-reported education than their public school records indicate, which may be due to private schooling or higher education). The disagreement is smaller if I compare self-reported age to number of grades attended rather than years of education, with 40.3% of observations having more years of education by age 20 and 40.0% remaining at the same level. Despite these large discrepancies which suggest that birth certificate data are a flawed proxy for completed educational attainment, estimation of the impact of school entry age on educational outcomes may not be biased if school entry age is uncorrelated with timing of fertility (Cook and

[3] These figures are calculated assuming that girls who were born after the cutoff date and started third grade one year later than their cohort peers did not instead start kindergarten at the same age as their peers and were retained in first or second grade (see discussion in the rest of this section). If we assume instead that these girls were retained in first or second grade, 57.1% of observations would have more years of education at age 20 than at the time of birth and 31.4% would have the same number of years of education in both periods.

Kang 2013; McCrary and Royer 2011; this paper). In the data analysis below, I compare results produced using administrative and birth certificate data.

Fourth, the dataset includes detailed information about individual characteristics at the individual, school and district levels (see Table 1), allowing a more effective search for heterogeneous treatment effects. (While some information about school lunch eligibility is available from school administrative records, these data are not used due to missing data for some grades in some years as well as missing data for students in schools where school lunch programs are not operational.)

Table 1: Sample Background Characteristics

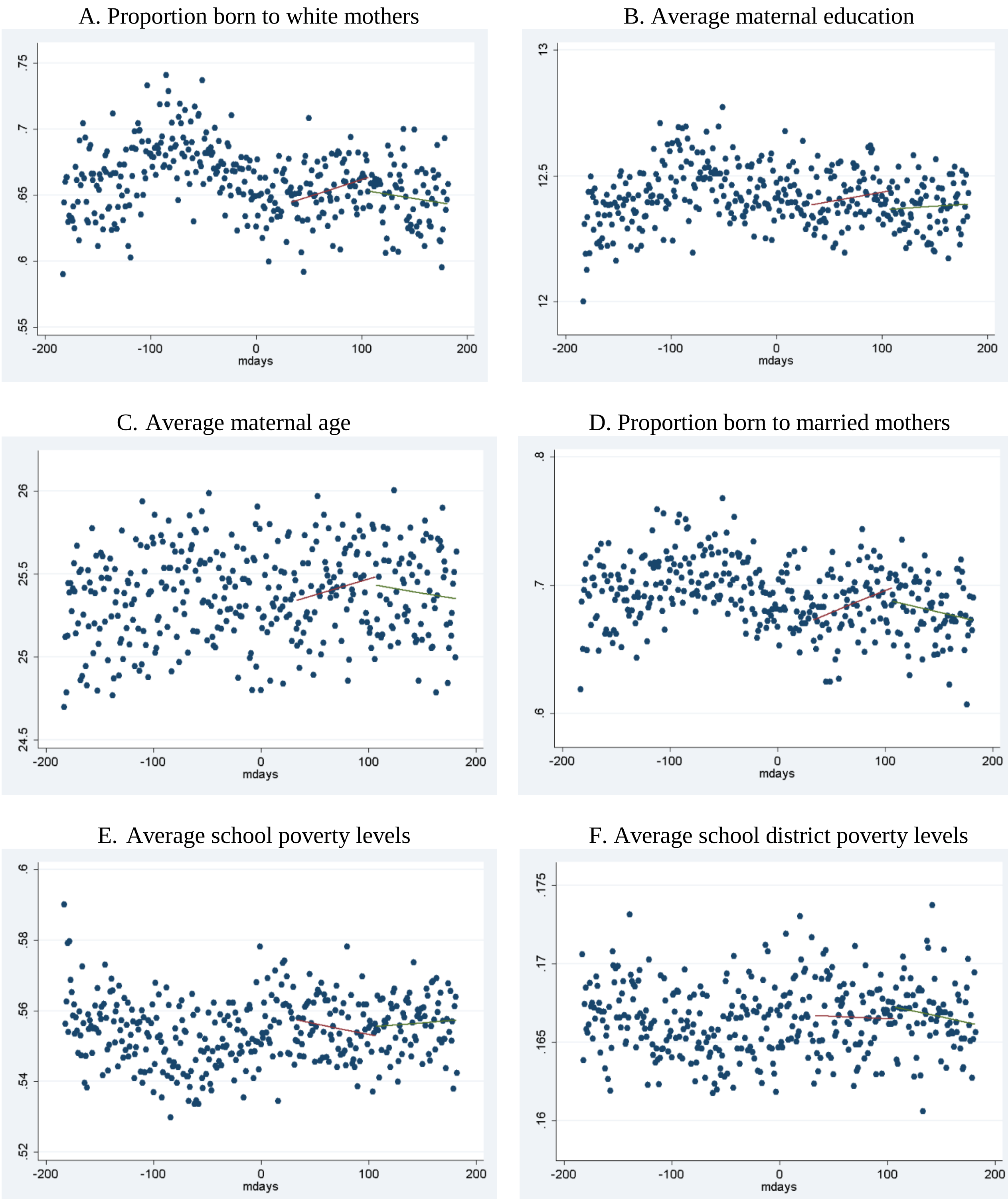
	Total sample	Individuals born up to 60 days before the cutoff date	Individuals born up to 60 days after the cutoff date
Individual-level characteristics			
First-born child to mother (%)	44.45	44.46	44.45
Birth weight (g)	3273	3265	3270
Mother is white (%)	65.94	65.68	64.79
Maternal age at time of birth	25.38	25.45	25.40
Maternal education at time of birth	12.41	12.42	12.37
Mother was married at time of birth (%)	69.16	68.83	67.95
School-level characteristics			
% of students who are poor	55.44	55.48	55.67
No. of crimes per 100 students	0.583	0.583	0.584
% of students passed their end-of-grade tests	77.26	77.23	77.15
School district-level characteristics			
% of population who are poor	16.65	16.65	16.69
% of students with one parent	22.74	22.72	22.78
% of students passed their end-of-grade tests	77.61	77.63	77.57
Number of observations	164,237	27,798	27,195

I conduct a couple of validity checks to ensure that the above dataset is suitable for the purposes of this paper. First, I test if "treatment" is randomly assigned, i.e. whether girls born before and after the kindergarten entry cutoff date are otherwise highly similar in a number of observable characteristics. Figure 1 shows graphical evidence of non-trivial birth seasonality effects, with girls born in the second quarter having the highest proportion of white and married mothers as well as mothers with the highest average level of education. Birth seasonality effects in the third and fourth quarters appear to be substantially weaker, suggesting that restricting the sample to one or two months surrounding the cutoff date (October 16) would remove much of the potential bias. Focusing on seasonality effects in these two birth quarters, Table 1 shows that individuals born up to 60 days before the cutoff date are 0.9% more likely to have a white or married mother than those born up to 60 days after the cutoff date, and the former are also born to mothers who have on average 0.05 more years of age as well as education. Perhaps surprisingly, evidence of seasonality effects is weaker for the larger 75-day window (with a 0.6% difference in maternal race and marital status and a 0.04 difference in years of education) and comparable between the 60-day and 45-day windows. Using local regression methods, I find some evidence of discontinuities in maternal race and marital status (individuals born after the cutoff date are 3.4% and 4.4% more likely to have a white and married mother, significant at the 10% level for the 75-day window), but not for maternal age or education. In the data analysis, I attempt to control for any seasonality effects by controlling for a host of individual, school and school district characteristics (see Table 1). The results are very similar whether or not the controls are used, similar to previous papers using discontinuities in school entry age (Black, Devereux and Salvanes 2011; Dobkin and Ferreira 2010).

Second, I examine whether "treatment" compliance is high, i.e. whether girls born before the cutoff date almost always begin kindergarten entry one year earlier than those born after the cutoff date. Non-compliance is possible since parents have the option of sending their children to private or out-of-state kindergartens which do not observe the same cutoff dates, and can even choose not to send their children to kindergarten at all (Cook and Kang 2013). While perfect compliance is not necessary for unbiased estimation of "intention-to-treat" effects as long as treatment itself is randomly assigned, compliance rates have implications for how an important variable, years of education, is calculated. As discussed below, examining compliance rates can also yield insight into whether the results are being driven by particular socioeconomic groups.

Previous studies suggest that compliance with school entry laws are generally high (Elder and Lubotsky 2008), especially for women (Cook and Kang 2013). In this dataset, while most girls born after the cutoff date generally start school one year later than others in their birth cohort (so that the treatment group generally complies with treatment), a substantial proportion of girls born just before the cutoff date also starts school later (so that part of the control group also receives treatment). To infer compliance rates in this paper, I use entry age at third grade rather than at kindergarten since data on kindergarten, first or grade attendance are not available.

Figure 1: Background Characteristics by Date of Birth



Notes: The horizontal axis refers to date of birth, where zero refers to July 2. Since there are two leap years (1988 and 1992), values to the far left may reflect values for different dates. The red and green lines are separately computed using OLS for the 60-day window around the cutoff date (October 16).

Among girls born up to 60 days before the cutoff date, 77.5% start third grade at age 8 together with the majority of their birth cohort, with 21.7% and 0.8% starting at ages 9 and 10. On the other hand, among girls born up to 60 days after the cutoff date, only 2.3% start at age 8, while 92.0% and 5.7% start at ages 9 and 10. If the comparison uses a narrower 45-day window or broader 75-day window, the differences in entry age at third grade weakens and grows respectively[4], so that the "intention-to-treat" estimates based on the 75-day window may be closest to the "treatment" effects of school entry age.

There are two plausible reasons for why a substantial proportion of girls born just before the cutoff date start third grade at age 9 rather than at age 8. First, parents may be reluctant to let their children be the youngest in the class, so that these children may begin kindergarten at age 6 rather than age 5. Second, girls born just before the cutoff date tend to have much weaker school performance (discussed in greater detail in the next section) and are hence more likely to be held back a year in kindergarten, first or second grade. Distinguishing between these two reasons is important to the analysis in this paper, since girls who start third grade late for the second reason have one more year of education than girls who start late for the first reason. Using nationally representative data, Bedard and Dhuey (2006) find that the first reason accounts for 57.4% of students born up to 30 days before state cutoff dates who enter third grade at ages 9 or older, while the second reason accounts for 42.6%. (In their sample, 41.4% of students born before the cutoff date enter third grade at higher-than-expected ages, which is substantially higher than in this paper, possibly due to their narrower window and the inclusion of boys, who are more likely to be held back.) On the other hand, statistics on retention rates between kindergarten and third grade in North Carolina public schools suggest that the second reason is likely to be more important. According to the Kindergarten Readiness Issue Group (2003), the probability of being retained between kindergarten and third grade rose from around 9% in 1991-1992 to around 17% in 2001-2002, possibly reflecting the large increase in the proportion of students from Hispanic immigrant families during this period. Since only 11.6% of girls in this dataset start third grade at higher-than-expected ages, a majority of them are likely to have been retained at an early age. In the data analysis in the next section, I show that the results are qualitatively similar for both methods of calculating years of education.

[4] For the 45-day window, 75.9% and 91.9% of girls born before and after the cutoff date start third grade at the expected ages; for the 75-day window, 78.8% and 92.0% of girls born before and after the cutoff date do so.

Comparing compliance rates among various socioeconomic groups, the evidence suggests that girls born just before the cutoff date who start school later (or the part of the control group who also receives treatment) are disproportionately likely to be from disadvantaged families. Among all girls born up to 60 days before the cutoff date, those who started third grade at age 9 are 1.6% and 8.3% less likely to be born to white and married mothers, and their mothers have on average 0.69 and 0.61 fewer years of age and education. Consistent with these lower compliance rates, the data analysis in the next section shows that the "intention-to-treat" effects of school entry age are substantially smaller for girls from disadvantaged backgrounds. My results differ from those in Dobkin and Ferreira (2010), who find instead lower compliance rates and smaller effects for individuals from white and more educated families for California and Texas.

Third and finally, I test for selective attrition among the "treatment group", i.e. girls born after the cutoff date. Selective attrition may occur if school entry age has a positive impact on test scores and if girls with higher test scores are more likely to attend private or charter schools, which may lead to biased "intention-to-treat" effects. Using local regression methods, I find no evidence of a discontinuity at the cutoff date in the probability of being included in the sample, consistent with Cook and Kang (2013) who use a similar dataset.

Results

On average, individuals in this dataset completed 11.7-11.9 years of education (not including kindergarten) by age 20, depending on whether those born before the cutoff date who started school later are assumed to have been retained in first or second grade or to have started first grade at a later age. While 87.8-93.5% have at least 12 years of education, only 76.4% attended the 12th grade, reflecting relatively high retention rates.

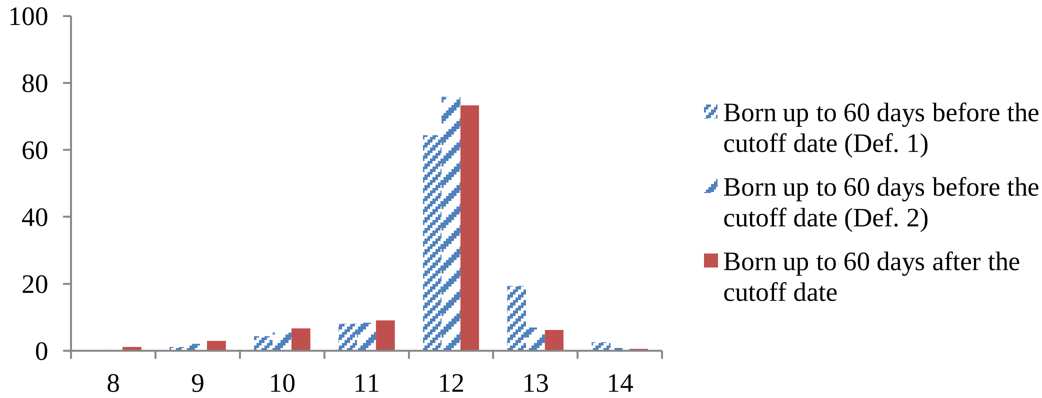
Women born up to 60 days before the cutoff date have more years of education on average (11.8-12.0, compared to 11.7) and are more likely to attend the 12th grade (77.7%, compared to 74.1%) than those born up to 60 days after the cutoff date (see Figure 2A-B). McCrary and Royer (2011) attribute these differences to the fact that girls born after the cutoff date reach the minimum dropout age (16 in North Carolina) after receiving fewer years of education – however, as Figure 2C shows, very few students actually choose to exit the school system at age 15, i.e. before taking an end-of-grade test at age 16, even though a substantial proportion would have been legally allowed to drop out. To explain why girls born after the cutoff date continue to drop out at higher rates in the 10th and 11th grades (see Figure 2B), Cook and Kang (2013) suggest that exposure to the possibility of dropping out may have a cumulative effect, while Dobkin and Ferreira (2010) theorize that the differences are due to biological age effects. The evidence in this paper finds support for the biological age argument: Figure 2C shows that the probability of dropping out increases with age regardless of whether an individual is born before or after the cutoff date, and that once biological age is taken into account, girls born after the cutoff date actually have lower or equal dropout rates, consistent with their higher test scores (discussed in detail below).

Consistent with the above arguments, Figure 3 shows that girls who are born earlier in the year are at higher risk of dropping out, so that there is an upward trend in average number of years of education and grades attended up to the cutoff date. The upward trend for years of education is considerably steeper if we assume that girls born before the cutoff date who start third grade late were retained and hence received an additional year of education, resulting in a larger discontinuity at the cutoff date. Table 2 provides estimates of the discontinuities at the cutoff date computed using the 45-day, 60-day and 75-day windows, where each figure refers to the coefficient for being born after the cutoff date ($Cutoff_i$) from an individual regression. The results suggest that girls born immediately after the cutoff date have around 0.2 fewer years of education and attend around 0.13 fewer grades than girls who were born a few days before them. These estimates are comparable to those in McCrary and Royer (2011), who find discontinuities in years of education of 0.14 and 0.24 for California and Texas respectively. Table 2 also shows that the estimates are fairly robust across windows and almost identical whether or not controls for individual, school and school district characteristics or missing values for grades attended are included. Previous studies using discontinuities in school entry age also find that regression results are very similar whether or not controls are introduced, suggesting that seasonality effects are orthogonal to the impact of school entry age on educational success.

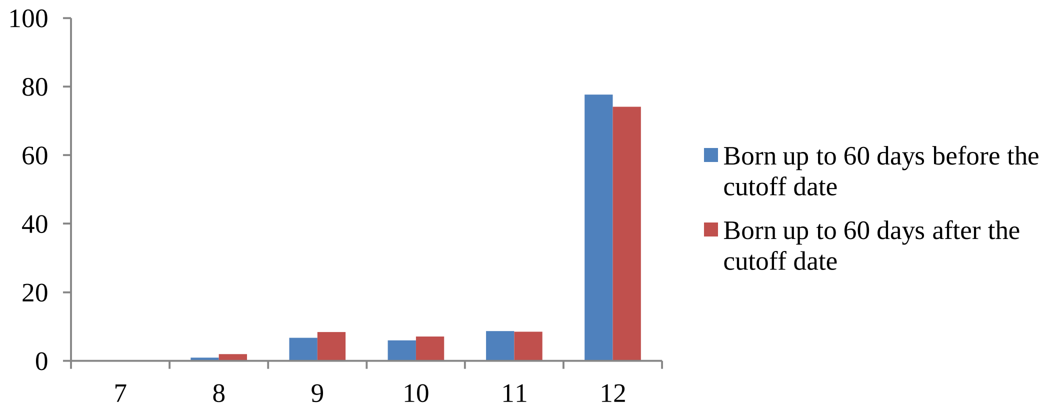
Out of curiosity, I repeat the analysis using only observations for which there are self-reported educational data on birth certificates, similar to the approach in McCrary and Royer (2011). (Unlike the authors, I further restrict my sample to women who gave birth prior to age 19 for increased comparability, since the administrative educational data are only for up to the 12th grade.) Table 3 shows that the estimated discontinuities are generally larger for this negatively

Figure 2: Years of Education, Highest Grade Attended and Age at School Leaving

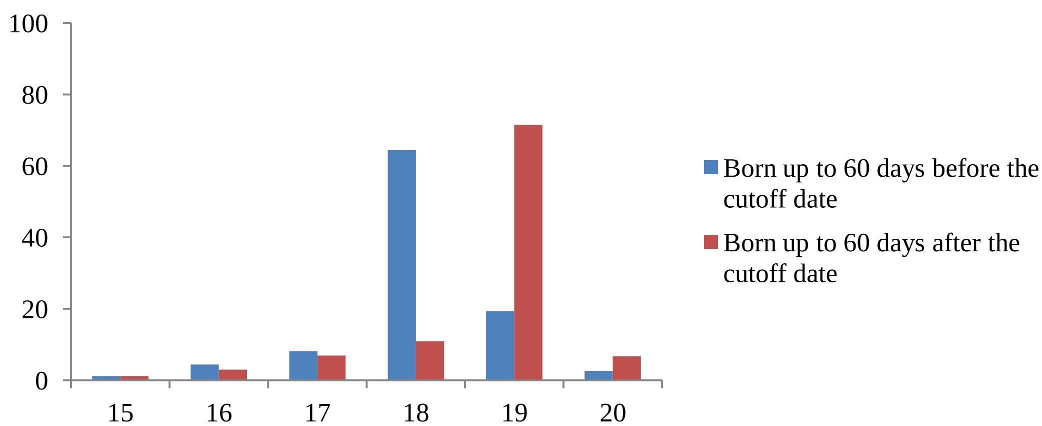
A. Years of Education



B. Highest Grade Attended



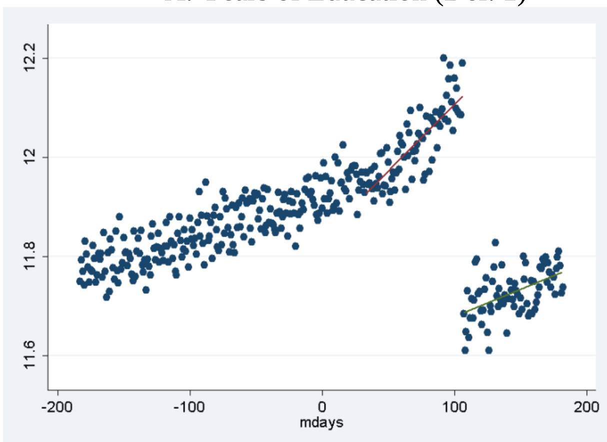
C. Age at School Leaving



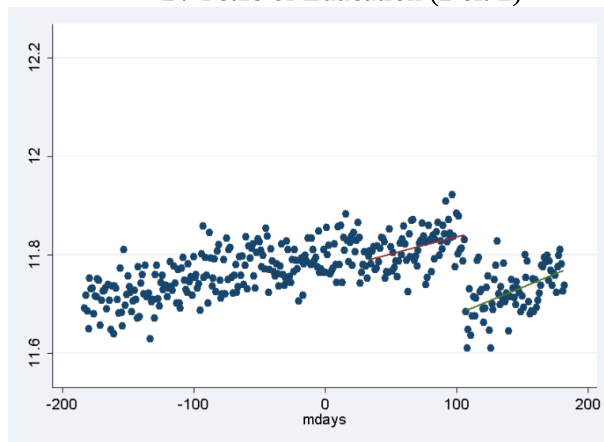
Notes: For Figure 2A, years of education for those “born up to 60 before the cutoff date (Def. 1)” are calculated assuming that those who started third grade late were retained, while years of education for those “born up to 60 before the cutoff date (Def. 2)” are calculated assuming that they began first grade at a later age.

Figure 3: Years of Education and Highest Grade Attended by Date of Birth

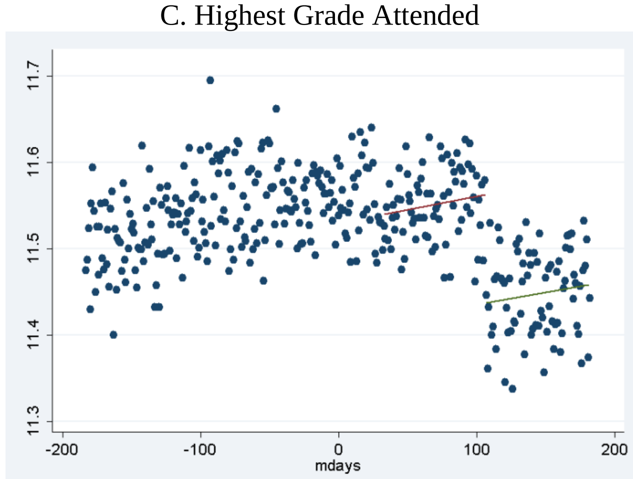
A. Years of Education (Def. 1)



B. Years of Education (Def. 2)



C. Highest Grade Attended



Notes: The horizontal axis refers to date of birth, where zero refers to July 2. Since there are two leap years (1988 and 1992), values to the far left may reflect values for different dates. Years of education (Def. 1) are calculated assuming that those who started third grade late were retained, while Years of education (Def. 2) are calculated assuming that they began first grade at a later age. The red and green lines are separately computed using OLS for the 60-day window around the cutoff date (October 16).

Table 2: Impact of Being Born After the Cutoff Date on Educational Attainment (dependent variables in bold)

	45-day	60-day	75-day
Years of education (Def. 1)			
No controls	-0.240**	-0.215***	-0.235***
Controls included	-0.233**	-0.214***	-0.234***
Years of education (Def. 2)			
No controls	-0.257**	-0.186***	-0.186***
Controls included	-0.240**	-0.182***	-0.183***
Years of education (Def. 3)			
No controls	-0.248**	-0.201***	-0.210***
Controls included	-0.236**	-0.198***	-0.209***
Highest grade attended			
No controls	-0.195	-0.132*	-0.130**
Controls included	-0.161	-0.131*	-0.137***
Controls included and missing data excluded	-0.163	-0.133*	-0.138***
Number of observations	41,139	54,993	68,771

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: Years of education (Def. 1) are calculated assuming that those who started third grade late were retained, Years of education (Def. 2) are calculated assuming that they began first grade at a later age and Years of education (Def. 3) is equal to the mean of the two. Each value refers to the coefficient on being born after the school entry cutoff date from an individual regression, with the dependent variable given in bold. Coefficients are estimated using regression discontinuity models. Controls refer to individual, school and school district characteristics (see Table 1). All regressions control for cohort fixed effects.

Table 3: Impact of Being Born After the Cutoff Date on Educational Attainment (dependent variables in the first column)

	Entire sample	Sample restricted to teenagers who gave birth before age 19
Self-reported years of education	-	-0.309
Years of education (Def. 1)	-0.240**	-0.477***
Years of education (Def. 2)	-0.257**	-0.382**
Grades attended	-0.195	-0.362*
Number of observations	68,771	6,749

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: Years of education (Def. 1) are calculated assuming that those who started third grade late were retained, while Years of education (Def. 2) are calculated assuming that they began first grade at a later age. Each value refers to the coefficient on being born after the school entry cutoff date from an individual regression, with the dependent variable given in the first column. Coefficients are estimated using regression discontinuity models and the 75-day window, controlling for individual, school and school district characteristics (see Table 1) and cohort fixed effects.

selected group of women, consistent with the heterogeneity analysis below in this section. Hence, the discontinuities in years of education at the cutoff date in California and Texas may be somewhat smaller than suggested in McCrary and Royer (2011), and may be smaller than in North Carolina due to the higher minimum dropout age in these states (18, rather than 16).

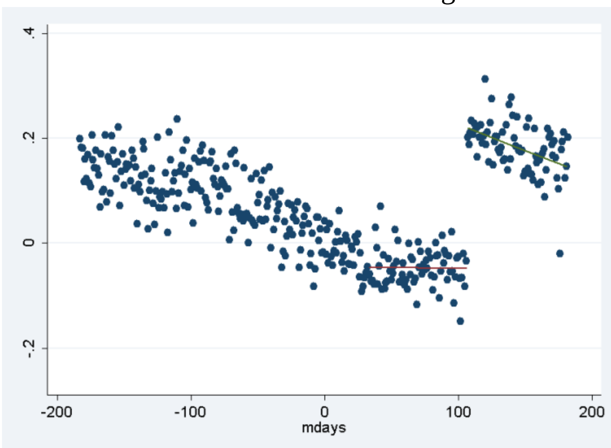
Thus far, the analysis shows that school entry age has a negative impact on education *attainment*, measured using years of schooling and grades attended. I now show that school entry age also has a positive impact on another form of educational success: educational *achievement*, measured using test scores. As previously mentioned in the Data section, individual test scores are represented by their Z-scores relative to the test results of all students who took the test, so that the average test score should be around zero if the sample is representative of North Carolina public school students. For this dataset, the average test scores in this dataset are 0.07 and 0.00 for reading and math in the third grade, and 0.06 and 0.01 for reading and math in the eighth grade, where the reading scores are slightly higher since only non-Hispanic white and black girls born in North Carolina, who are unlikely to be taking English as a second language, are included.

While women born up to 60 days before the cutoff date have more years of education on average than those born up to 60 days after the cutoff date, they have lower average test scores not only in third grade (-0.09, compared to 0.15) but also in eighth grade (-0.04, compared to 0.09). The test score gaps are comparable for reading and math (0.23 and 0.26 of a standard deviation in third grade, and 0.14 and 0.13 in eighth grade). As these figures suggest (also see Figure 4), the "intention-to-treat" effects of school entry age on test scores are substantially weaker in eighth grade than in third grade, which is due in large part to the fact that individuals born right before the cutoff date are more likely to be retained between ages 11 and 15 than those born after the cutoff date (Cook and Kang 2013), reducing compliance rates (since a larger part of the control group also receives treatment). Table 4 provides estimates of the discontinuities at the cutoff date, which are on the order of 0.35 of a standard deviation in third grade and 0.2 in eighth grade and are, once again, fairly robust across windows and the inclusion of controls and missing test scores. The evidence is consistent with previous studies which find that the impact of school entry age persists at least up to the eighth grade (Bedard and Dhuey 2006; Cook and Kang 2013; Elder and Lubotsky 2008), suggesting that the overall impact on educational success is substantially more mixed than suggested in McCrary and Royer (2011).

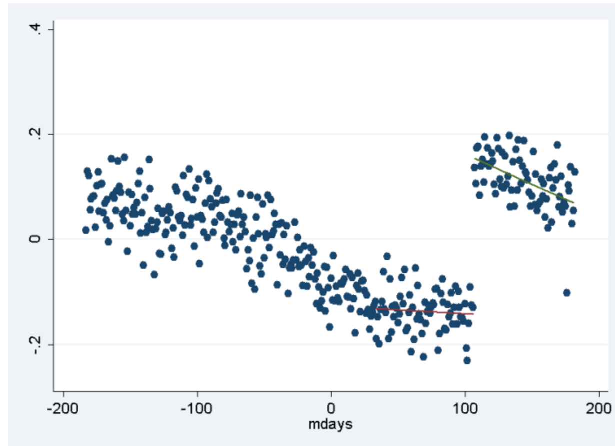
Table 5 shows the heterogeneous "intention-to-treat" impacts of school entry age on educational attainment and achievement, with the top rows showing estimates of the discontinuities at the cutoff date for socioeconomically advantaged groups and the bottom rows showing estimates for their disadvantaged counterparts. If we assume that girls born before the cutoff date who start third grade late were retained and hence received an additional year of

Figure 4: Third and Eighth Grade Test Scores by Date of Birth

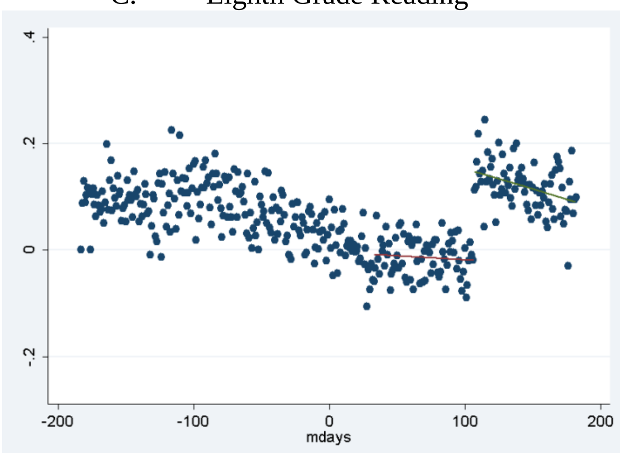
A. Third Grade Reading



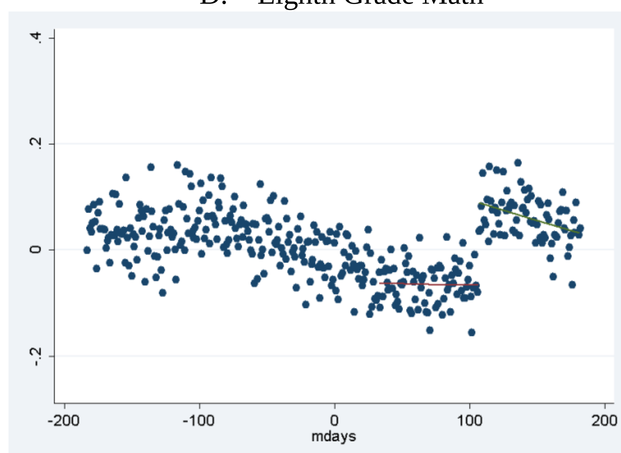
B. Third Grade Math



C. Eighth Grade Reading



D. Eighth Grade Math



Notes: The horizontal axis refers to date of birth, where zero refers to July 2. Since there are two leap years (1988 and 1992), values to the far left may reflect values for different dates. The red and green lines are separately computed using OLS for the 60-day window around the cutoff date (October 16).

Table 4: Impact of Being Born After the Cutoff Date on Test Scores (dependent variables in bold)

	45-day	60-day	75-day
Third grade test scores			
No controls	0.279***	0.353***	0.368***
Controls included	0.310***	0.338***	0.345***
Controls included and missing data excluded	0.310***	0.343***	0.349***
Eighth grade test scores			
No controls	0.141	0.208***	0.224***
Controls included	0.173**	0.192***	0.200***
Controls included and missing data excluded	0.169**	0.186***	0.195***
Number of observations	41,139	54,993	68,771

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: Each value refers to the coefficient on late entry from an individual regression, with the dependent variable given in bold. Coefficients are estimated using regression discontinuity models. Controls refer to individual, school and school district characteristics (see Table 1). All regressions control for cohort fixed effects.

education, the discontinuities in years of education are generally larger for disadvantaged groups since they are more likely to be retained by third grade; if we assume that girls born before the cutoff date who start third grade late also started kindergarten at a later age, the discontinuities are generally smaller, again due to the lower compliance rates (among the control group). One complication to the above story is that while compliance rates are lower for disadvantaged groups, the actual treatment or biological age effects on dropout rates appear to be larger except for blacks, and are exceptionally small for girls born to more educated mothers (not shown here), so that there is only weak evidence of a discontinuity in educational attainment for this subgroup. The results are consistent with McCrary and Royer (2011), who also find smaller "intention-to-treat" effects for blacks.

As expected given their higher compliance rates, girls from more advantaged backgrounds also experience larger discontinuities in test scores and are more likely to continue to see these differences persist to the eighth grade. Elder and Lubotsky (2008), who find similar heterogeneous effects using nationally representative survey data, offer another explanation for these findings: unlike for years of education, the discontinuities in test scores are primarily due to differences in skill accumulation prior to kindergarten rather than biological age effects, which are substantially larger for higher SES families. The evidence in this paper suggests that broader environmental factors may also matter – while test score discontinuities in third grade are very similar in low-poverty and high-poverty school and school districts, they appear to decline less rapidly in low-poverty settings, potentially due to increased ability tracking.

Table 5: Impact of Being Born After the Cutoff Date on Years of Education, Grades Attended and Test Scores
(dependent variables in the first column)

	Mother is white	Mother is older	Mother is more educated	Mother is married	Low-poverty school	Low-poverty school district
Years of education (Def. 1)	-0.264***	-0.213***	-0.057	-0.201***	-0.170***	-0.266***
Years of education (Def. 2)	-0.262***	-0.213***	-0.128*	-0.196***	-0.164***	-0.237***
Grades attended	-0.189***	-0.177***	-0.104	-0.144**	-0.113*	-0.138**
Third grade test scores	0.383***	0.440***	0.428***	0.387***	0.344***	0.356***
Eighth grade test scores	0.241***	0.298***	0.278***	0.266***	0.247***	0.229***
Number of observations	44,778	36,510	22,356	46,963	33,973	36,899
	Mother is black	Mother is younger	Mother is less educated	Mother is unmarried	High-poverty school	High-poverty school district
Years of education (Def. 1)	-0.194***	-0.262***	-0.327***	-0.323***	-0.302***	-0.192**
Years of education (Def. 2)	-0.047	-0.153**	-0.216***	-0.169*	-0.204***	-0.116
Grades attended	-0.043	-0.098	-0.158**	-0.141	-0.160**	-0.133*
Third grade test scores	0.268***	0.235***	0.305***	0.257***	0.342***	0.333***
Eighth grade test scores	0.113**	0.086	0.163***	0.058	0.149**	0.165**
Number of observations	23,993	32,261	46,415	21,808	34,798	31,872

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.
Notes: Years of education (Def. 1) are calculated assuming that those who started third grade late were retained, while Years of education (Def. 2) are calculated assuming that they began first grade at a later age. Each value refers to the coefficient on late entry from an individual regression, with the dependent variable given in the first column. Coefficients are estimated using regression discontinuity models and the 75-day window, controlling for cohort fixed effects.

Compared to the regression discontinuity results for educational outcomes, those for teenage fertility are much weaker. As mentioned in the Data section, the analysis is largely based only on data for the first three birth cohorts for whom there are complete teenage fertility histories. For this sample, 18.7% had a teenage birth, with only 3.3% giving birth prior to age 17. The proportions of teenage mothers among girls born up to 60 days before and after the cutoff date are almost identical (19.1% and 19.0%), and are very similar for the 45-day (18.8%, compared to 19.3%) or 75-day window (19.4%, compared to 18.8%). Figures 5 and 6 show that there are virtually no differences between the fertility schedules of the two groups, and Tables 6 and 7 show that the lack of results extends to all windows and subgroups. The evidence is hence once again generally consistent with McCrary and Royer (2011), although the discontinuity point estimates of around -2.5% are closer to the statistically significant results of -1.8% in Black, Devereux and Salvanes (2011) than the precisely estimated zeros in the former. Unlike Cook and

Kang (2013), I find no positive "intention-to-treat" effects on behavioral outcomes below age 17, possibly due to the low incidence of childbearing at these ages. An important corollary of this result is that being born after the school entry cutoff date ($Cutoff_i$) is likely to be an exogenous instrument for educational outcomes, since it is not directly associated with fertility change.

Figure 5: Teenage Childbearing Survival Curves by Late Entry

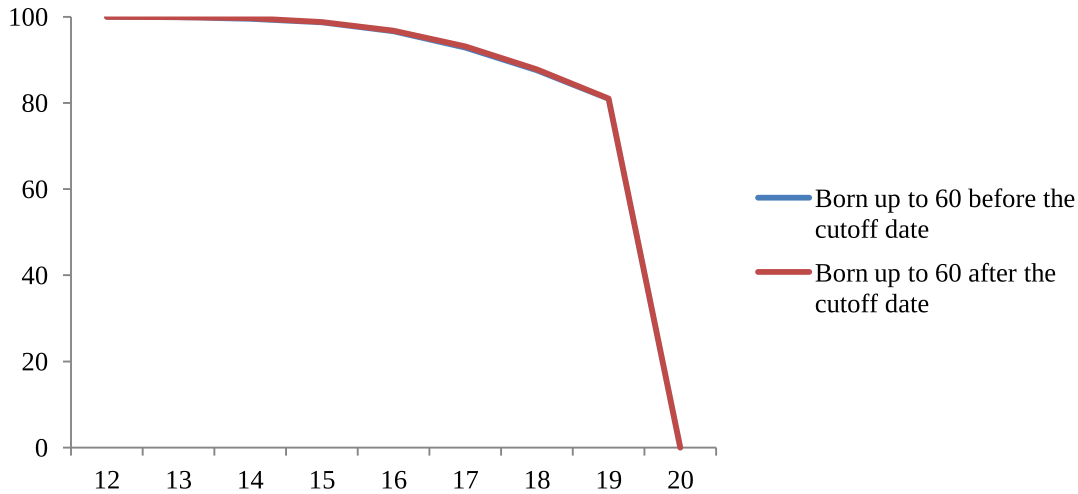
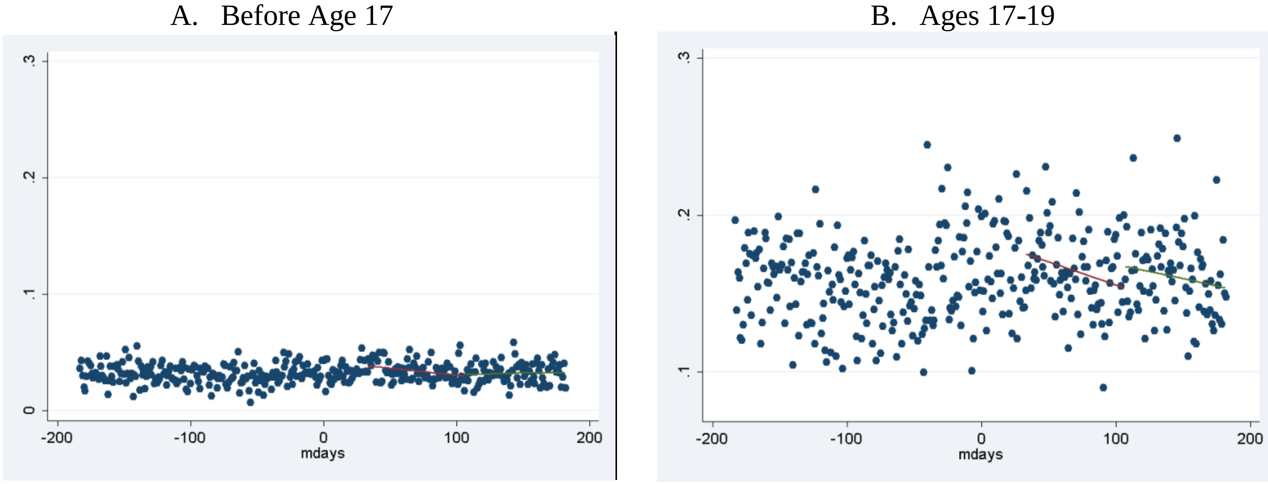


Figure 6: Proportions of Teenage Mothers by Date of Birth

A. Before Age 17

B. Ages 17-19



Notes: The horizontal axis refers to date of birth, where zero refers to July 2. Since there are two leap years (1988 and 1992), values to the far left may reflect values for different dates. The red and green lines are separately computed using OLS for the 60-day window around the cutoff date (October 16).

Table 6: Impact of Being Born After the Cutoff Date on Teenage Childbearing (dependent variables in bold)

	45-day	60-day	75-day
Motherhood by age 20			
No controls	-0.023	-0.024	-0.021
Controls included	-0.048	-0.035	-0.026
Number of observations	20,022	26,844	33,598
Motherhood by age 17			
No controls	-0.018	-0.015	-0.013
Controls included	-0.019	-0.014	-0.011
Number of observations	41,139	54,993	68,771
Motherhood at ages 17-19			
No controls	0.012	-0.003	-0.003
Controls included	-0.015	-0.016	-0.009
Number of observations	19,351	25,961	32,446

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.
Notes: Each value refers to the coefficient on late entry from an individual regression, with the dependent variable given in bold. Coefficients are estimated using regression discontinuity models. Controls refer to individual, school and school district characteristics (see Table 1). All regressions control for cohort fixed effects.

Table 7: Impact of Being Born After the Cutoff Date on Teenage Childbearing

	Mother is white	Mother is older	Mother is more educated	Mother is married	Low-poverty school	Low-poverty school district
	-0.040	-0.026	-0.020	-0.020	-0.046	-0.034
Number of observations	21,969	17,754	10,532	23,803	16,306	17,885
	Mother is black	Mother is younger	Mother is less educated	Mother is unmarried	High-poverty school	High-poverty school district
	0.005	-0.023	-0.028	-0.036	-0.006	-0.016
Number of observations	11,629	15,844	23,066	9,795	17,292	15,713

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.
Notes: Each value refers to the coefficient on late entry from an individual regression, with the dependent variable given in the first column. Coefficients are estimated using regression discontinuity models and the 75-day window, controlling for cohort fixed effects.

I now turn to the results for the instrumental variable regression model, which distinguishes the simultaneous impacts of years of education and test scores on teenage motherhood. As discussed in the Model section, I use multiple instruments for the two endogenous variables: being born after the cutoff date ($Cutoff_i$), age in terms of number of days born after July 2 (Age_i), as well as interaction terms between $Cutoff_i$ and a number of individual-level characteristics. Each pair of figures in Table 8 refers to the coefficients on years of education and test scores from an individual regression, with teenage childbearing outcomes as the dependent variable. While there is little evidence of instrument endogeneity for any of the model specifications according to the Anderson-Rubin and Basmann statistics, the instruments appear to be strong only for model specifications which use the 75-day window and third grade test scores, consistent with previous literature and the above analysis which show declining "intention-to-treat" effects with age. The test for weak instruments is based on the Cragg-Donald statistic as recommended by Stock and Yogo (2002), who note that the test is overly conservative (and hence less likely to reject the null hypothesis of weak instruments) when there are multiple endogenous variables. All IV regression results are produced using the Limited Information Maximum Likelihood (LIML) estimator, which are more robust to instrument weakness (Staiger and Stock 1997; Stock and Yogo 2002) at the cost of larger standard errors.

The IV regression results based on the 75-day window and third grade test scores suggest that years of education and test scores both have very large negative impacts on teenage childbearing. In particular, under the second definition of years of education, an additional year of schooling is associated with a decrease in the probability of teenage childbearing by 23.2 percentage points, which exceeds the base probability of teenage birth in this sample (18.7%). If the first definition of years of education is used instead, the impact of an additional year of schooling is only half as large at 12.5 percentage points, but the evidence continues to imply that improved educational attainment can drastically reduce teenage childbearing, which is consistent with the fact that the majority of teenage mothers are high school dropouts who are not pregnant at the time of school leaving (Upchurch and McCarthy 1990).

Combining these IV regression results together with the regression discontinuity estimates from Tables 2 and 4, I investigate whether the overall "intention-to-treat" effect of being born after the cutoff date on teenage childbearing can be approximated as the sum of a) the product of the "intention-to-treat" effects of being born after the cutoff date on years of education, and the impact of years of education on teenage childbearing, and b) the product of the "intention-to-treat" effects of being born after the cutoff date on test scores, and the impact of test scores on teenage childbearing. Under the first definition of years of education, the predicted "intention-to-treat" effect on teenage childbearing is 0.029 + (-0.061) = -0.032, where the first component is positive since both sub-components are negative and the second component is negative since one sub-component is positive and the other is negative. Under the second definition, the predicted effect is 0.043 + (-0.042) = 0.001, and finally, if I take the average of these two measures of years of education (henceforth the third definition of years of education), the predicted effect is 0.037 + (-0.059) = -0.022, which comes closest to the actual (non-significant) regression discontinuity point estimate of -.026 from Table 6.

Table 9 compares the IV regression results when teenage childbearing outcomes at ages below 17 and at ages 17-19 are used as the dependent variable, using estimates based on the 75-day window, the third definition of years of education and third grade test scores. Consistent with the literature on the effects of increasing compulsory education on fertility (Rivera 2013), I find stronger evidence that educational outcomes matter for childbearing at later teenage years, possibly due to the low incidence rates at younger ages. Hence, the joint evidence from quasi-experimental approaches points strongly towards the view that policies which increase educational attainment, and promote high school completion are likely to reduce teenage childbearing, particularly at older ages when the risks of childbearing are especially high.

To check that the results in Table 8 are not driven by any single instrument, I repeat the IV regression analysis using the 75-day window, the third definition of years of education and third grade test scores, but this time excluding each of the seven instruments in turn. Table 10 shows that the point estimates from the seven regressions vary in magnitude but are consistently large and negative. For three of the regressions, there is evidence of instrument weakness when an instrument is excluded, so that the more rigorous estimates are based on model specifications where these instruments are used and the interacted variable (race or maternal marital status) is included in the list of covariates (see discussion in the Model section). Given this constraint in the covariates that can be excluded and the imprecision of the regression analysis when the sample is restricted to individual subgroups, an analysis of heterogeneous effects for this part of the paper is not provided.

Finally, I examine the impact of being born after the cutoff date on childbearing at ages 20-21, where the analysis is based only on data for women born in the first two cohorts (born in 1987-1988) since fertility histories for these ages are not complete for the third cohort (born in 1989). For this sample, 14.3% of women first gave birth at ages 20-21, with similar proportions among girls born up to 60 days before and after the cutoff date (11.9% and 11.6%). Table 11 shows that women born on either side of the cutoff date continue to exhibit very similar fertility behavior at these ages, and that there is no statistically significant evidence that years of

Table 8: Impact of Years of Education and Test scores on Teenage Childbearing

	45-day	60-day	75-day
Results based on third grade test scores			
Years of education (Def. 1)	0.086	-0.040	-0.125*
Third grade test scores	0.134	-0.059	-0.177*
Evidence of weak instruments	Yes	Yes	No
Evidence of endogenous instruments	No	No	No
Years of education (Def. 2)	-0.058	-0.164	-0.232**
Third grade test scores	-0.013	-0.084	-0.120***
Evidence of weak instruments	Yes	Yes	No
Evidence of endogenous instruments	No	No	No
Results based on eighth grade test scores			
Years of education (Def. 1)	0.029	-0.076	-0.127*
Eighth grade test scores	0.091	-0.191	-0.320**
Evidence of weak instruments	Yes	Yes	Yes
Evidence of endogenous instruments	No	No	No
Years of education (Def. 2)	-0.141	-0.182*	-0.230***
Eighth grade test scores	-0.100	-0.167*	-0.213***
Evidence of weak instruments	Yes	Yes	Yes
Evidence of endogenous instruments	No	No	No
Number of observations	20,022	26,844	33,598

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: Years of education (Def. 1) are calculated assuming that those who started third grade late were retained, Years of education (Def. 2) are calculated assuming that they began first grade at a later age, while Years of education (Def. 3) are equal to the average of the two. Each pair of values in the table refers to the coefficients on years of education and test scores from an individual regression, with the probability of motherhood by age 20 as the dependent variable. Coefficients are estimated using IV regression models, controlling for individual, school and school district characteristics (see Table 1) and cohort fixed effects.

Table 9: Impact of Years of Education and Test Scores on Teenage Childbearing (dependent variables in bold)

	By age 20	**By age 17**	**At ages 17-19**
Years of education (Def. 3)	-0.177**	-0.037	-0.192**
Third grade test scores	-0.170**	-0.039**	-0.172**
Evidence of weak instruments	No	No	No
Evidence of endogenous instruments	No	No	No
Number of observations	33,598	68,771	32,446

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: Each pair of values in the table refers to the coefficients on years of education and test scores from an individual regression, with the dependent variable given in bold in the first row. Coefficients are estimated using IV regression models and the 75-day window, controlling for covariates and cohort fixed effects.

Table 10: Impact of Years of Education and Test Scores on Teenage Childbearing

	Excluded instrument						
	Cutoff	Relative age in days	Cutoff* cohort	Cutoff* race	Cutoff* mother's marital status	Cutoff* school poverty	Cutoff* school district poverty
Years of education (Def. 3)	-0.177**	-0.156	-0.185**	-0.121	-0.221**	-0.190**	-0.169*
Third grade test scores	-0.170**	-0.153	-0.178**	-0.122*	-0.208**	-0.182**	-0.163*
Evidence of weak instruments	No	Yes	No	Yes	Yes	No	No
Evidence of endogenous instruments	No	No	No	No	No	No	No
Number of observations	33,598						

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.
Notes: Each pair of values in the table refers to the coefficients on years of education and test scores from an individual regression, with the probability of motherhood by age 20 as the dependent variable. Coefficients are estimated using IV regression models and the 75-day window, controlling for individual, school and school district characteristics (see Table 1) and cohort fixed effects.

Table 11: Impact of Being Born after the Cutoff Date, Years of Education and Test Scores on Childbearing at Ages 20-21

	45-day	60-day	75-day
Regression discontinuity			
No controls	-0.079	-0.031	-0.001
Controls included	-0.088	-0.038	-0.004
IV regression			
Years of education (Def. 3)	0.109	-0.317	-0.158
Third grade test scores	0.098	-0.276	-0.141
Evidence of weak instruments	Yes	Yes	Yes
Evidence of endogenous instruments	No	Yes	No
Number of observations	10,434	14,062	17,582

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.
Notes: For regression discontinuity results, each value refers to the coefficient on being born after the cutoff date from an individual regression, with childbearing at ages 20-21 as the dependent variable. Controls refer to individual, school and school district characteristics (see Table 1). All regressions control for cohort fixed effects. For IV regression results, each pair of values in the table refers to the coefficients on years of education and test scores from an individual regression, with the probability of teenage motherhood by age 20 as the dependent variable. All results are estimated controlling for cohort fixed effects.

education and test scores affect childbearing outcomes at these ages, possibly due to the smaller sample size. Hence, unlike the literature on changes in compulsory schooling which finds that increased high school educational attainment reduces childbearing at teenage as well as slightly older ages (Black, Devereux and Salvanes 2008), the evidence from state cutoff dates for kindergarten admission, and specifically that from North Carolina, does not indicate whether the effects of increased educational attainment extend into early adulthood.

Conclusion

This paper contributes to the quasi-experimental literature on the impact of education on teenage fertility in two ways. First, using a highly detailed administrative dataset from North Carolina which includes not only information on years of schooling but also public school test scores, I show that the evidence from discontinuities in school entry age suggests a negative causal relationship between years of schooling and teenage fertility, consistent with the literature on changes in compulsory schooling laws. Second, I show that education *attainment* (measured using years of schooling) is not the only form of educational success which matters for teenage fertility – instead, educational *achievement* (measured using test scores) also appears to have a negative causal impact.

The empirical results in this paper are generally strongly consistent with those in previous research using discontinuities in school entry age. The most closely related work is by McCrary and Royer (2011), who use data for the Texas and California birth cohorts of 1969-1987, while the analysis in the paper is based on data for the North Carolina birth cohorts of 1987-1992. Despite the cohort and demographic differences between these two populations (including a much higher proportion of Hispanics in the former population), as well as potential differences in the "intention-to-treat" effects given the higher minimum dropout age in Texas and California (18, rather than 16), the estimated discontinuities in years of education and fertility in both papers are highly comparable. The regression discontinuity results for test scores in this paper are also in line with the work by Bedard and Dhuey (2006), Cook and Kang (2013) and Elder and Lubotsky (2008), who similarly find that discontinuities in test scores persist up to the eighth grade and tend to be larger for women from more advantaged family backgrounds. Finally, the IV regression results support the arguments of Cook and Kang (2013) and Dobkin and Ferreira (2010), who suggest that the mixed impact of school entry age on educational success accounts for its ambiguous impacts on multiple life outcomes, including youth criminality, employment rates, wages and marital status.

There are several potentially important limitations in this paper. First, following previous related research, I assume that the impact of school entry age on teenage childbearing is primarily through its impact on educational success, rather than through any effects on social popularity and peer characteristics. The impacts of school entry age on teenage fertility through peer effects are likely to be ambiguous, since having older peers is associated with higher test scores, while having younger peers is associated with a lower probability of diagnosed with a learning disability (Elder and Lubotsky 2008), higher self-confidence and lower exposure to risky peer behaviors (Black, Devereux and Salvanes 2011). While the dataset used in this paper has little information on immediate peer characteristics, the similarity of the results for childbearing at ages below 20 (when school peer effects may be stronger) and at ages 20-21 (when school peer effects may be weaker) suggests that any bias from omitting the impacts on peer effects is likely to be small. Second, I assume that the marginal impact of an additional year of education on teenage fertility is similar across all grades, which may not be true since the risks of childbearing are especially high at older teenage ages. Finally, the analysis in this paper does not indicate whether the negative impacts of education on teenage childbearing are due to increased pregnancy prevention or higher abortion rates, which is likely to be important from a policy perspective.

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Appendix

Table A1: Maternal Characteristics in North Carolina, 1987-1992

	Total	% white	% below 20 years old	% with less than high school degree	% unmarried
	Vital Statistics				
1987	93,501	69.3	15.7	23.2	24.9
1988	97,579	67.6	16.0	23.2	26.3
1989	102,105	66.8	16.4	22.9	27.7
1990	104,525	66.5	16.2	22.7	29.4
1991	102,362	66.0	16.2	23.2	31.6
1992	103,967	65.9	15.4	22.1	31.3
	Birth certificates				
1987	89,704	69.0	15.2	22.7	24.1
1988	93,507	67.2	15.4	22.7	25.5
1989	97,996	66.5	15.9	22.3	27.0
1990	100,357	66.2	15.7	22.1	28.7
1991	98,123	65.7	15.7	22.6	30.8
1992	99,811	65.6	15.0	21.5	30.5

Notes: Vital Statistics data include out-of-state births to mothers residing in North Carolina, while birth certificate data include only in-state births to mothers residing in North Carolina. Vital Statistics data were obtained online from the Centers for Disease Control and Prevention at http://www.cdc.gov/nchs/data_access/vitalstats/VitalStats_Births.htm. Birth certificate data were obtained from the North Carolina Education Research Data Center.