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The Impact of Year-Round Schooling on Academic Achievement: Evidence from Mandatory School Calendar Conversions†

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In 2007, 22 Wake County, North Carolina traditional calendar schools were switched to year-round calendars, spreading the 180 instructional days evenly across the year. This paper presents a human capital model to illustrate the conditions under which these calendars might affect achievement. We then exploit the natural experiment to evaluate the impact of year-round schooling on student achievement using a multi-level fixed effects model. Results suggest that year-round schooling has essentially no impact on academic achievement of the average student. Moreover, when the data are broken out by race, we find no evidence that any racial subgroup benefits from year-round schooling. (*JEL* H75, I21, I28, J24)

Summer vacation, a much anticipated three-month break from school, has long been a staple of the US education system. Recent concern over tightening budgets and summer learning loss, however, has led to growing discussion over the merits of "modified" year-round school calendars. Such calendars spread the same number of school days over a longer period, effectively breaking up the long summer break into four or more smaller breaks throughout the year.[1] According to the National Association of Year-Round Education, over two million students attended a year-round school in 2007.[2] This number, about 4 percent of all US students, represents a marked increase from the 360,000 students (roughly 0.7 percent of all US students) who attended a year-round school in 1986.[3] While the number of year-round schools is on the rise, there is currently little consensus on the relative benefit (or cost) such a schedule affords. Rather, calendar conversions have

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[1] Thus, this type of year-round calendar is different from the "extended year" calendar, where the number of instructional days is increased.

[2] http://www.nayre.org (accessed February 3, 2011).

[3] "Year-Round Schooling," *Education Week*, September 10, 2004, accessed February 10, 2011, http://www.edweek.org/ew/issues/year-round-schooling/.

sparked heated education policy debates and have even led to the creation of groups whose sole purpose is to either support the growth of year-round education (The National Association of Year-Round Education) or to suppress its growing popularity (SummerMatters!!). This education policy issue has been especially divisive in Wake County, North Carolina where, in 2007 faced with unprecedented population growth, the Wake County Public School System (WCPSS) converted 22 elementary and middle schools to year-round calendars, and ordered all newly built schools to open on the year-round calendar. The move increased the number of year-round schools operating in the WCPSS to 46, more than doubling the number of schools operating on the year-round schedule. This policy initiative forced many students into mandatory year-round school (YRS) assignments and sparked widespread debates, including a legal challenge taken to the state Supreme Court.[4]

In this paper, we exploit the natural experiment created by the controversial WCPSS education policy initiative using a unique, restricted-use panel dataset from the North Carolina Education Research Data Center (NCERDC) to evaluate the impact of YRS on student achievement. In contrast to previous research, the panel design of our dataset, combined with both within-student and within-school variation, allows us to estimate a multi-level fixed effects model that includes student, school and grade-by-year fixed effects. With this model, we are able to separate the impact of YRS from the confounding impacts of other school, family, and individual characteristics. This contribution addresses the concerns in the literature about both student and school selection effects (McMillen 2001; Cooper et al. 2003; Graves 2010).

The YRS calendar studied in this paper is a version of the "multi-track" YRS model. Under this model, students attend school the same 180 days as a traditional-calendar student, but these days are spread across the full calendar year. Each child is placed into a particular track that comes with its own schedule, where at any point in time at least one track is on break.[5] This attribute allows the school to accommodate a larger number of students. For example, depending on enrollment, a multi-track school can hold 20 to 33 percent more students than a traditional calendar school.[6] As such, the multi-track system has been touted as a good solution to fast population growth, since this implies that for every three schools on the year-round calendar, one less school has to be built.[7] Research has found the multi-track system becomes the most cost effective solution once a school's population reaches 115 percent of its capacity (Cooper et al. 2003).

In addition to this cost savings attribute, proponents of YRS calendars argue that they are beneficial to students because, by redistributing vacation time into more

[4] Erin Coleman and Stacy Davis, "N.C. Supreme Court hears year-round school case," WRAL.com, December 16, 2008, accessed February 10, 2011, http://www.wral.com/news/local/story/4147682/.

[5] In addition to the most common multi-track year-round model, the county also has a handful of schools that operate on a modified version of the year-round calendar. The model is a single-track model in which all students in the school follow the same schedule. In the past, the schedule has fallen somewhere between the traditional and year-round calendar. Under the 2008–2009 calendar, for example, students in WCPSS's five modified instructional calendar schools had an eight week summer break and a two week fall, winter and spring break. Omitting these schools does not change the results (see online Appendix).

[6] http://www.wcpss.net/year-round/year-round-overview.html (accessed February 10, 2011).

[7] http://www.wcpss.net/year-round/capacity_gain.html (accessed February 10, 2011). Note that while the capacity of the school increases, that increase in capacity is not without other costs, especially in terms of administration and wear on the facilities (NCES 2000; Shields and Oberg 2000).

frequent shorter breaks, they help alleviate human capital loss during the long summer break ("summer learning loss"). Supporters further contend that the long break is particularly harmful for low-income, low-performing students who are less able to afford supplemental learning opportunities in the summer (Von Drehle 2010). These assertions are based on a wide literature on summer learning loss, which has found that student achievement stagnates over the summer, and that for low achieving and disadvantaged students especially, achievement can often decline while not in school (Jamar 1994; Cooper et al. 1996; Alexander, Entwisle, and Olson 2007).[8] Alexander, Entwisle, and Olson (2007) finds that by the end of ninth grade, almost two-thirds of the socioeconomic achievement gap can be explained by differential summer learning loss. Graves (2010, 2011) notes, however, that the ability of YRS to address this problem depends crucially upon the nature of the human capital accumulation process. In this paper, we formalize the logic of her argument, as well as that of other scholars. We present a simple model that illustrates YRS can only improve achievement if learning loss accelerates with the number of days out of school, or if there are diminishing returns to learning. Thus, even if disadvantaged students lose more human capital than their wealthier counterparts over summer, YRS cannot alleviate the problem unless there are specific nonlinearities in the human capital process (Graves 2010, 2011). Some critics also argue that more frequent breaks may actually disrupt the learning process (Rasberry 1992). Using our model, we show this could happen if learning was convex in the number of days of school.

Our study adds to a body of literature, primarily coming from outside of the field of economics, that is well-summarized by the meta-analysis performed by Cooper et al. (2003). The general consensus coming out of that review is that the impact of year-round education on student achievement is, on average, nearly negligible. On the other hand, the evidence suggests the modified calendar does benefit low performing and economically disadvantaged students. The primary drawback of these early studies, however, is their failure to account for non-random student and school selection. The studies included in Cooper et al. (2003) do not adequately control for student and school characteristics, and none attempt to control for both unobserved student and school heterogeneity. Cooper et al. (2003, 43) thus concludes that it "would be difficult to argue with policymakers who choose to ignore the existent database because they feel that the research designs have been simply too flawed to be trusted." Moreover, Cooper et al. (2003) report that those studies that do a better job controlling for student and school characteristics find smaller YRS effect sizes, indicating that the lack of proper controls may bias the results of previous studies upward. This result may be indicative of non-random selection of high-achieving students into YRS, or could also reflect the non-random implementation of year-round calendars in high-income, high-achieving areas.

Most recently, Graves (2010, 2011) uses detailed longitudinal school-level data from California to estimate the impact of the multi-track year-round calendar on academic achievement. By including school fixed effects and school-specific time trends, Graves (2010, 2011) is able to mitigate concerns over non-random,

[8] It is well documented that inequalities in student achievement are generally exacerbated over the summer months (Reardon 2003; Downey, von Hippel, and Broh 2004; Alexander, Entwisle, and Olson 2007).

year-round calendar implementation. In contrast to much of the prior research on YRS, Graves (2010, 2011) finds that achievement in multi-track, year-round schools is 1 to 2 percentile points *lower* than that in traditional calendar schools, and that low income and minority students may be especially harmed by YRS. However, without student-level data, she is not able to control for non-random student selection into YRS, which is a concern since the students in YRS differ in important ways from their peers in traditional schools. Thus, while the paper marks a significant improvement upon prior research, further research is necessary.

Our paper adds to this literature in the following ways. First, we formalize a simple human capital model to illustrate the conditions under which YRS may or may not affect achievement. Second, with student-level panel data, we are able to control for both observed and unobserved student and school heterogeneity, which is vital given the concerns in the literature about both student and school selection effects (McMillen 2001; Cooper et al. 2003). Finally, we use a large policy change as a natural experiment to aid in identification. Consistent with the existing literature, our results suggest YRS has essentially no impact on the academic achievement of the average student. Moreover, when the data are broken down by racial sub-group, the evidence indicates that, contrary to some previous studies (Graves 2011), disadvantaged racial groups neither benefit, nor are harmed, by YRS.[9] Taken as a whole, these results are consistent with the assertion that dividing a long summer break into several shorter breaks will neither improve or harm student achievement or address achievement gaps.

The remainder of the paper is organized as follows. Section I provides a brief description of YRS in Wake County, North Carolina. In Section II, we present a simple human capital model that is used to formally illustrate the assumptions under which YRS may or may not affect achievement and achievement gaps. Section III describes the data and descriptive statistics. Our empirical approach and results are presented in Section IV. Section V concludes.

I. Year-Round Schooling in Wake County, North Carolina

This study focuses on students in the public schools of Wake County, North Carolina, the largest school district in the state and the 18th largest in the nation.[10] In 1989, the school system opened the nation's first year-round magnet school. Since then, the number of Wake County students in YRS has steadily grown. The most significant policy change occurred in 2007 when the WCPSS converted 22 traditional calendar schools to the year-round calendar. This large conversion was largely initiated as a response to school crowding created by significant population growth.[11] During the 2003–2004 school year enrollment in the WCPSS was 108,970 and by 2008–2009 it had grown by more than 26 percent to 137,706 students.[12] The WCPSS responded to

[9] We find some limited evidence that Hispanic students are harmed by YRS, but the result is not consistent across specifications.

[10] http://www.wcpss.net/demographics/ (accessed February 7, 2011).

[11] http://www.wakegov.com/planning/demographic/education.htm (accessed June 9, 2011).

[12] http://www.wcpss.net/demographics/quickfacts/index_qf.html (accessed February 16, 2011). Despite considerable population growth, the demographics of the population have remained largely unchanged over the last decade (see http://www.census.gov/acs for details.)

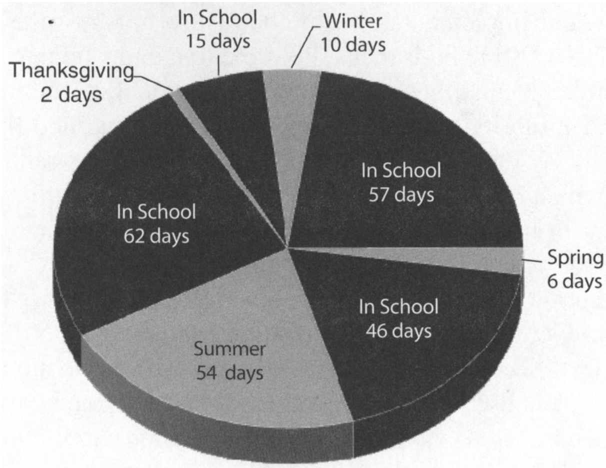

FIGURE 1A. TRADITIONAL CALENDAR

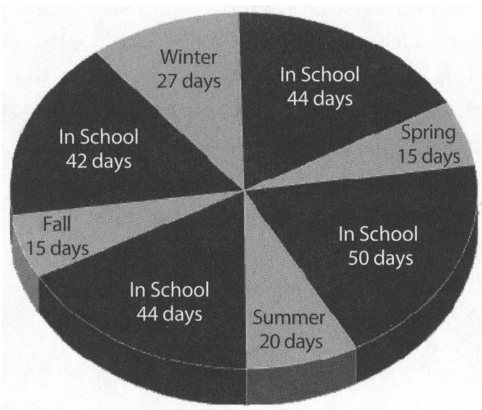

FIGURE 1B. YEAR-ROUND CALENDAR

growth by building new schools, however, growth often outpaced projections and led to situations where new schools were already too small when they opened, or were needed before they were completed. The YRS model was seen as a cost-effective solution. Currently, there are over 44,000 students attending YRSs in the WCPSS.[13]

As discussed above, the YRS calendar of interest in this paper is the multi-track year-round calendar, where all students attend school the same total number of days as the traditional calendar student (180), but these days are spread over an entire calendar year. Thus, compared to traditional calendars, under the year-round calendar the school days/breaks are more evenly distributed. Figures 1A and 1B illustrate the difference between these two calendars. Under the WCPSS YRS calendar, the

[13] Adam Owens, "Wake to end mandatory year-round schools," WRAL.com, January 6, 2010, accessed February 20, 2011, http://www.wral.com/news/education/story/674635/.

school year is separated into four quarters, with 45 days of instruction and 15-day breaks.[14] Students stay with the same teacher for the whole school year.

In addition to the natural experiment created by the 2007–2008 calendar conversions, the school system also has a policy of busing its students across the county to maintain within-school socio-economic diversity. The goal of the policy is to keep each school below 40 percent percent of its students receiving free or reduced-price lunch. This busing policy makes study of the YRS in this county desirable because students (and their parents) are given little choice over school assignment, particularly with respect to the academic calendar. In 2008–2009, for instance, just 4.8 percent of WCPSS selected into a year-round school and 2.3 percent chose a traditional calendar option.[15] Anecdotal evidence suggests many choice-based assignments into or out of YRS are primarily done for non-achievement related reasons. For example, many parents appeal their assignments in order to keep siblings together or to better fit family vacation plans or daycare schedules. Unfortunately, our data does not allow us to distinguish the "selectors" from the "non-selectors." However, to the extent this small group of selectors is problematic, the inclusion of school and student fixed effects should help alleviate these remaining selection effects.

Taken together, the mandatory school assignments and calendar conversions create more of a "natural lottery" allocation of students into YRSs, making problems of self-selection bias less of a concern. Moreover, in contrast to the previous literature, these attributes allow us to take advantage of the panel design of the dataset using a multi-level fixed effects approach to identify the impact of year-round schooling on achievement that is not confounded by time invariant school, family, or individual unobserved heterogeneity.

II. Human Capital Formation and YRS: A Basic Model

Students' academic skill, or their stock of human capital, tends to diminish when not in school and to grow when in school. This issue has been of particular concern for policymakers concerned with addressing summer learning loss (Jamar 1994; Cooper et al. 1996; Downey, von Hippel, and Broh 2004; Alexander, Entwisle, and Olson 2007). The ability of year-round education to address this problem relies upon the nature of the accumulative process of human capital skill during a school year. To illustrate the conditions under which YRS may or may not have the ability to affect academic achievement, consider the following general model of human capital accumulation drawn from Ben-Porath (1967):

$$h_{t+1} = g(I_t, h_t, \theta), \tag{1}$$

where human capital stock in period $t + 1$ depends on human capital stock in period t, innate ability, θ, and investment, I_t. In our model, investment in period t is considered to be a day spent learning in school. The current period human capital stock, h_t

[14] A copy of the 2008–2009 WCPSS multi-track, year-round calendar is available at http://www.wcpss.net/Calendars/2009-10/09-10-year-round.pdf.

[15] http://www.wcpss.net/demographics/reports/book09/VH-assignment.pdf (accessed June 17, 2011).

depreciates at rate δ_t, where, for generalization, the rate of depreciation is allowed to vary over time. For simplicity, we assume the only direct impact of human capital at time t on that at $t + 1$ comes through the carry over effect, or through $(1 - \delta_t)h_t$. Thus, equation (1) can be re-written as

$$h_{t+1} = f(I_t, \theta) + (1 - \delta_t)h_t. \tag{2}$$

In the remainder of this section, we use this simple framework to illustrate the assumptions under which YRS may or may not impact student achievement. Then, we use the model to highlight the importance of controlling for non-random selection of students into YRS. Finally, we address the implications of the model with respect to YRS and achievement gaps.

A. *Investment, Depreciation, and YRS*

We begin by illustrating a base case where both the return to investment and rate of depreciation are assumed to be constant. Then we illustrate the case where the rate of return on investment is constant, but depreciation rates increase the longer a student is out of school. Finally, we use the model to show how YRS might affect achievement if the rate of depreciation were constant, but the investment technology was either increasing or decreasing.[16]

Case 1: *Constant Rate of Return to Investment and Constant Rate of Depreciation.*— If $\partial h_{t+1}/\partial I_t = \partial f(I_t, \theta)/\partial I_t = c$ and $\delta_t = d$, where both c and d are constant, then both investment and depreciation of the human capital stock are assumed to be linearly related to growth in human capital stock. In this case, each day of school contributes equally to achievement growth regardless of the distribution of the school days. Likewise, each day away from school has the same negative impact on achievement, regardless of the length of the break. The best policies to address student learning loss in this case, therefore, are to: lengthen the school year, increase the marginal rate of return on investment (c), or decrease the rate of depreciation (d). In this situation, for the same given values of c and d, a YRS will produce the same achievement as a traditional calendar school, because each calendar includes the same number of days in school per year. Figure 2 provides a simple illustration of this case. While, at any point in time during the course of a year students may have different levels of human capital stock, by the end of the year, otherwise identical students in year-round schools will have the same level of achievement as students in traditional calendar schools. This is consistent with the arguments made by some learning-loss scholars (Downey et al. 2004; Von Hippel 2007).

Case 2: *Constant Rate of Return to Investment and Increasing Rate of Depreciation.*—A number of scholars have argued that year-round schooling should

[16] Of course, there are other possible combinations we could explore here. Since the model is used primarily for expositional purposes, we choose to illustrate those cases that are most consistent with the arguments for and against YRS.

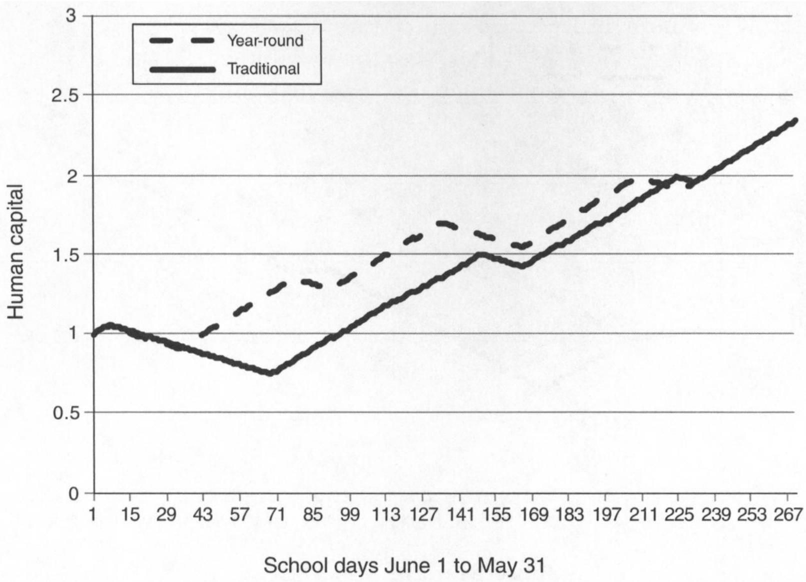

FIGURE 2. CONSTANT RATE OF INVESTMENT AND DEPRECIATION

counter the "summer learning loss" that is observed in the data (Cooper et al. 1996; Alexander, Entwisle, and Olson 2007). Downey et al. (2004) report, however that "learning rates are approximately constant for much of the school year." If this contention is true then any achievement gains arising from YRS must come from accelerating rates of depreciation over the summer. Figure 3 illustrates this hybrid case. Like the base scenario, the marginal rate of gross investment is assumed to be constant, $\partial h_{t+1}/\partial I_t = \partial f(I_t, \theta)/I_t = c$, but the rate of depreciation is assumed to increase with the number of days on a break away from school (i.e. increasing rate of depreciation). Formally, we assume that the rate of depreciation is a function of the number of consecutive days away from school, σ, so that $\delta_t = \phi(\sigma)$, where $d\delta_t/d\sigma > 0$. Under this scenario, the ideal school calendar includes many short breaks spread out over the year. Consequently, over the course of a school year, a YRS will produce higher achievement than a traditional calendar. This result is illustrated in Figure 3. In contrast to the situation depicted in Figure 2, after one school year, students in year-round schools now have higher levels of achievement than their traditional calendar counterparts.

Case 3: *Increasing or Decreasing Rate of Investment and Constant Rate of Depreciation.*—While the literature suggests learning rates are constant, if this assumption does not hold, it could be possible YRS would affect achievement even if depreciation rates were not increasing. First, consider the case where $\partial h_{t+1}/\partial I_t = \partial f(I_t, \theta)/\partial I_t > 0$ but $\partial^2 h_{t+1}/\partial I_t^2 = \partial^2 f(I_t, \theta)/\partial I_t^2 < 0$, so that the function f is concave in I_t. This assumption implies that the increase in achievement from day t to day $t + 1$ diminishes the longer students are in school without a break (i.e. diminishing returns to gross investment). Here, even if the rate of depreciation is assumed to be constant $(\delta_t = d)$, YRS would still lead to an increase in achievement (see Figure 4).

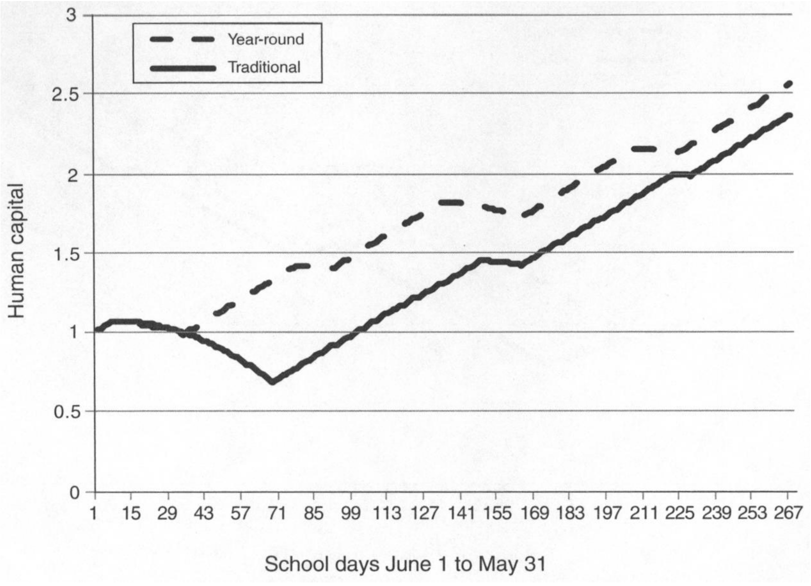

Figure 3. Constant Rate of Investment and Increasing Rate of Depreciation

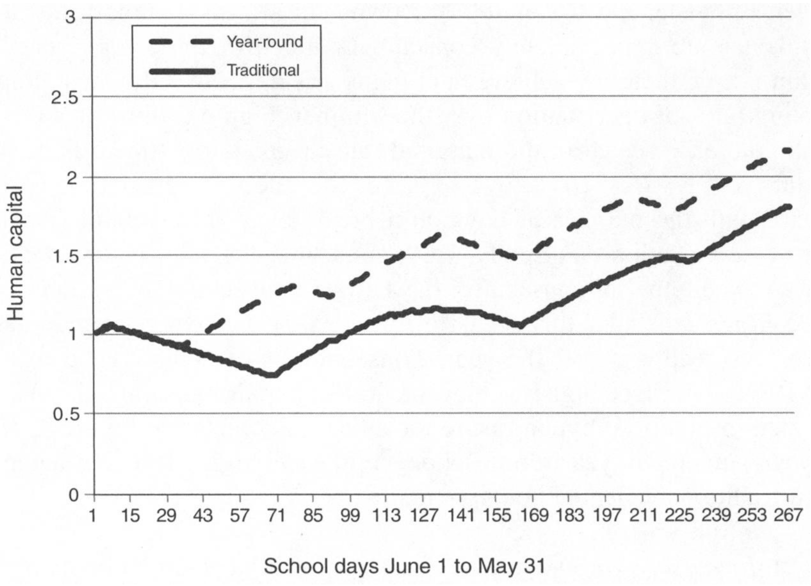

Figure 4. Diminishing Return to Investment and Constant Rate of Depreciation

Alternatively, if we assume instead that $\partial h_{t+1}/\partial I_t = \partial f(I_t, \theta)/\partial I_t > 0$ but $\partial^2 h_{t+1}/\partial I_t^2 = \partial^2 f(I_t, \theta)/\partial I_t^2 > 0$ such that the function f is convex in I_t, this would imply the increase in achievement from day t to day $t + 1$ increases the longer students are in school without a break (i.e. increasing returns to gross investment).

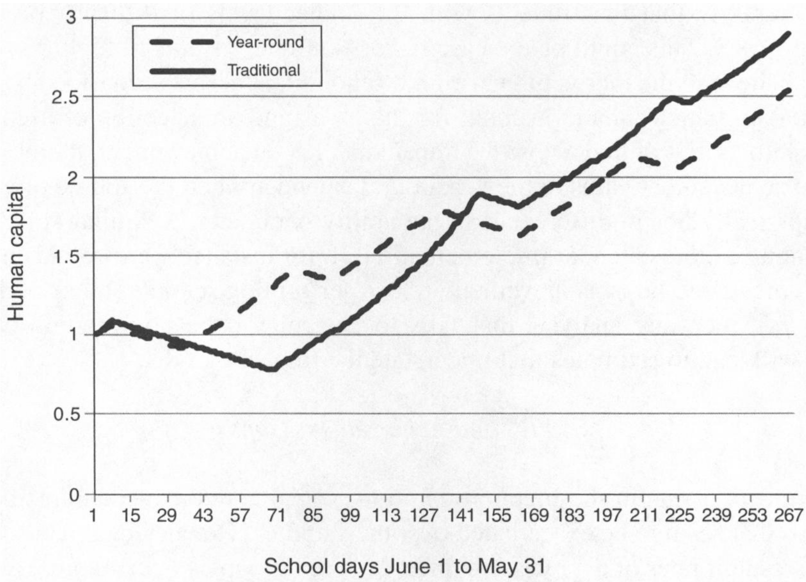

Figure 5. Increasing Return to Investment and Constant Rate of Depreciation

Under this scenario, if depreciation rates are constant, YRS would actually be less beneficial than a traditional calendar (see Figure 5).[17]

This implication is consistent with critics such as Rasberry (1992) and Graves (2010) who argue YRS may be detrimental due to the shorter, more disrupted blocks of learning time.[18]

B. *The Role of Ability*

Up to this point, the discussion has largely ignored the role of innate ability, θ. We now expand upon our analysis, using this framework to highlight the importance of controlling for non-random selection of students into YRS. First, consider the simplest case where both the marginal return to gross investment and the rate of depreciation are constant. Recall, under this scenario, at the end of a school year, YRS should have no discernable impact on student achievement.

Assume the marginal rate of gross investment is higher for higher-ability students, $\partial f(I_t, \theta)/\partial\theta > 0$. Under this assumption, for a given level of I_t, h_t and δ_t, higher ability students will have higher achievement on day $t + 1$, so that $h_{t+1}(\theta^H) > h_{t+1}(\theta^L)$, where $\theta^H > \theta^L$. Likewise, even if gross investment does not depend on ability, $\partial f(I_t, \theta)/\partial\theta = 0$, if the rate of depreciation is allowed to depend on ability, $\delta_t(\theta)$ and $d\delta_t/d\theta < 0$, then for a given level of I_t and h_t, higher ability students will carry over more of their human capital stock to period $t + 1$ and, once again, $h_{t+1}(\theta^H) > h_{t+1}(\theta^L)$.

[17] Of course, if depreciation were instead increasing, the net impact of YRS would depend upon the relative magnitudes of these effects.

[18] These scholars also argue that the disruption problem could be due to decreasing rates of depreciation the longer a student is on break. This case would also result in lower achievement in YRS than under traditional calendars.

Now, suppose that the students with the higher levels of θ disproportionately select into YRS. This could be the case if, for example, parents of higher achieving students believe in the merits of year-round school. Under this scenario, any empirical evaluation which fails to account for this non-random selection of high ability students into YRS will find a positive impact of YRS on achievement, thereby attributing the achievement gains to the year-round calendar when the source of achievement gaps really lies in differences in the ability parameter. A similar story can be told for non-random calendar implementation. If, for instance, year-round calendars are implemented to boost achievement in low-performing schools (those with lower average θs), then any analysis that fails to control for the non-random calendar reforms will lead to estimates that understate the impact of YRS.

C. *YRS and Achievement Gaps*

Much of the focus in the literature is on the racial and socio-economic achievement gaps that seem to be exacerbated over the summer (Downey et al. 2004). These gaps can result if rates of depreciation differ between groups. For example, low socio-economic-status students are often thought to spend the summer in lower-quality learning environments (Jamar 1994; Cooper et al. 1996; Downey et al. 2004; Von Drehle 2010), where, for example, there may be fewer accessible materials for independent reading. Given our discussion above, it is clear that these gaps will not necessarily be remedied by a year-round calendar. In particular, if the rate of return on gross investment and the rate of depreciation are both constant across time (as depicted in Figure 2), even if they vary between students, YRS will not be able to narrow these gaps. Only if the rate of depreciation increases with the number of consecutive days away from school, $d\delta_t/d\sigma > 0$, and differs between groups, will the group that experiences more depreciation benefit disproportionately from year-round education.

Two other conclusions about achievement gaps arise from this model and should be noted. First, the timing of the testing will be important for the measurement of the achievement gap: there should be less of a gap at the end of the year than at the beginning if time in school narrows the gap while time out of school widens the gap (Downey et al. 2004). Second, a policy that lengthens the school year, instead of simply re-ordering the time in school, could increase achievement (Pischke 2007) and decrease achievement inequality.

III. Data

This paper uses a restricted-use dataset from the NCERDC (1999–2009), housed in the Center for Child and Family Policy at Duke University. The NCERDC, created in 2000 through a partnership with the North Carolina Department of Public Instruction, holds and manages data on North Carolina's public schools. The large dataset has information at the student, school, teacher, and district level for all public school students in the state of North Carolina from 1995 to 2009.[19] In addition, we

[19] This study uses student data coming primarily from the individual-level EOG (end-of-grade) test and school level information from the School Report Card files.

TABLE 1—SAMPLE CONSTRUCTION: GRADE BY COHORT AND YEAR

Cohort	2005–2006	2006–2007	2007–2008	2008–2009
1	Grade 6	Grade 7	Grade 8	
2	Grade 5	Grade 6	Grade 7	Grade 8
3	Grade 4	Grade 5	Grade 6	Grade 7
4	Grade 3	Grade 4	Grade 5	Grade 6
5		Grade 3	Grade 4	Grade 5

merge these data with WCPSS school capacity data that is publicly available on the WCPSS website. Our analysis sample includes WCPSS students in grades three through eight in years 2006 (two years before the large policy change) through 2009 (the year after the policy change). This gives us two "treatment" years of data for the 22 schools affected by the policy change. The primary identification strategy (discussed in the next section) relies on repeated observations of students over time. The sample is therefore restricted to those cohorts with at least three consecutive years of data available, as illustrated in Table 1.

To determine whether a student attended a year-round school, we use a test date indicator taken from the end-of-grade files.[20] Table 2 shows the breakdown of year-round and traditional calendar schools and students across the time period of interest. As shown in the table, the number of YRSs operating in the county more than doubled in 2007–2008 due to the major calendar conversion policy. The number of students in YRSs has also increased across time. However, because the majority of YRSs are at the elementary school level and our 2008–2009 sample is limited to students in the fifth grade and above, the number of students observed in a YRS in our analysis sample drops in 2008–2009.

The primary outcomes of interest are a student's end-of-grade math and reading test scores. These tests, administered over a week in the spring, are given to all NC students in the third through the eighth grade. The WCPSS testing schedules are adjusted by calendar and track to ensure that the tests are administered at approximately the same point in the school year in terms of learning days. For instance, in 2010–2011, the first date of testing for traditional calendar students occurred on May 11, 156 days into the school year, while YRS students on track 3 started testing on June 1, or day 154.[21] Since these tests schedules are adjusted in this way, the amount of schooling is roughly the same for all students, making the tests comparable across calendar type. A comparison of test scores closer to the beginning of the year would risk biasing the results in favor of year-round schools. To make test scores comparable across grade and year, we follow Bifulco and Ladd (2006) and normalize scores such that grade-by-year test score means are equal to zero with a standard deviation of one.

[20]For students who attended a year-round school, this variable is coded as "YROxx", where xx indicates the year of the test. In cases where this indicator variable is not constant across all students within the same school in a particular year, we refer to publically available information on the WCPSS website regarding school type and manually change the indicator, if necessary.

[21]Track 1 began testing on May 11 (day 153), Track 2 began testing on May 6 (day 159), and track 4 began on June 1 (day 158). Test schedules are available at http://www.wcpss.net/evaluation-research/reports/calendars/t_calendars.html (accessed June 9, 2011).

TABLE 2—BREAKDOWN OF STUDENTS AND SCHOOLS IN ANALYSIS SAMPLE BY ACADEMIC CALENDAR

	2005–2006	2006–2007	2007–2008	2008–2009
Panel A. Students by type				
Traditional	28,199	37,153	30,071	23,349
Year-round	6,278	7,603	15,431	11,871
Total	34,477	44,756	45,502	35,220
Panel B. Schools by type				
Traditional	97	101	79	80
Year-round	14	14	42[a]	46
Total	111	115	79	126

Notes: The large difference in sample size in 2005–2006 and 2008–2009 is a function of the sample restriction illustrated in Table 1. In these years, there are four (versus five) grades included. Moreover, since the majority of year-round schools are elementary schools and the sample in 2008–2009 only includes grade 5–8, there are fewer students observed in year-round schools. The number of students attending YRS in the WCPSS did, in fact, increase during this school year.

[a] In addition to the 22 schools that were converted in 2007–2008, six new YRS opened. Thus, the total number of YRS operating in WCPSS increased by 28 in this year.

Since the year-round effect is estimated using several model specifications, the sample is limited so that the estimates are comparable.[22] The final analysis sample includes 50,657 unique students across a total of 126 schools. Table 3 shows summary statistics of these students by the type of school the student is observed attending during the time period of study. Means for achievement variables, demographic characteristics and parents' education are reported for two groups: (i) those who are observed in only a traditional school, (ii) those who are observed at least once in a YRS. The table also shows the difference in means across the two groups. Of the 50,657 students included in the sample, 32,103 attended a traditional school throughout the entire period of observation, while 18,554 students attended a YRS at least once.

Compared with students who are observed in only a traditional calendar school, students observed at least once in a YRS have both statistically significant higher average math and reading test scores. This difference in means may indicate a positive impact of YRS on achievement or, as outlined in Section II, may simply reflect differential selection into YRS by higher achieving students. Additionally, students observed in a YRS are more likely to be white and less likely to be African-American. The table also shows statistically significant differences by parents' education level. However, a higher percentage of students in the year-round group are from later cohorts and thus do not have information on parental education available. It is, therefore, difficult to draw conclusions from the parental education variables. Nevertheless, the racial differences between these students do indicate that it is possible that the sample of students in year-round schools is different than that of traditional schools, suggesting simple empirical models that fail to control for the non-random selection of students into YRS will result in biased estimates of YRS. No previous study of YRS has been able to control for this type of selection.

[22] Growth models with fixed effects require at least three observations per student. We want to compare models for the same students. This requirement, which largely eliminates new entrants or leavers from the WCPSS, along with non-missing achievement and control variables, drops the number of students in the sample from 60,657 to 50,657.

TABLE 3—SUMMARY STATISTICS OF STUDENTS IN ANALYSIS SAMPLE

	Traditional only	At least once in year-round	Difference
Panel A. Achievement			
Math score	−0.027	0.088	−0.115**
Reading score	−0.033	0.038	−0.071**
Panel B. Demographics			
Male	50.58%	51.43%	−0.008*
White	51.13%	58.04%	−0.069 **
African-American	28.46%	21.91%	0.066**
Hispanic	10.24%	10.33%	−0.001
Indian	0.27%	0.32%	0.000
Asian	5.67%	5.09%	0.006**
Mixed	4.22%	4.31%	−0.001
Panel C. Parents' education			
Less than high school	3.65%	2.91%	0.007**
High school	16.00%	12.53%	0.035**
Some college	3.22%	2.39%	0.008**
2-year degree	6.21%	5.07%	0.011**
4-year degree	29.24%	26.76%	0.025**
Graduate school	6.41%	5.46%	0.010**
Missing	35.27%	44.89%	−0.096**
Observations	32,103	18,554	

** Significant at the 5 percent level.
* Significant at the 10 percent level.

To control for school-level characteristics other than YRS, we merge our student-level data taken from the EOG files with school-level characteristics taken from the School Report Card files. School-level information taken from these files includes the student-to-teacher ratio, type of school (elementary or middle), average class size, number of students, teacher licensure, teacher experience, and teacher turnover. Importantly, since year-round calendars were implemented in WCPSS primarily to ease over-crowding, we control for school crowding in order to identify the impact of the year-round calendar. Crowding information is not available in the NCERDC data; however, the WCPSS makes annual school data reports available on their website. These files have detailed capacity and crowding information at the school level.

School-level summary statistics are reported in Table 4. The table reports means of the school-level variables for three sets of schools: (i) schools that are always on a traditional calendar, (ii) schools that are always on a year-round calendar, and (iii) schools that switch academic calendars due to the 2007 policy change. Compared with the traditional calendar schools, YRSs are less crowded, have a slightly higher average student-to-teacher ratio, are more likely to be elementary schools, have a slightly higher average class size, and have a larger student enrollment. On average, YRSs also have a higher percentage of fully licensed teachers, more experienced teachers, and a lower teacher turnover rate.

It is also instructive to examine summary statistics for the 22 schools that changed calendar type during the 2007–2008 school year. Table 5 reports summary

TABLE 4—SUMMARY STATISTICS OF SCHOOLS IN ANALYSIS SAMPLE

	Traditional only	Year-round only	Both
Percent crowding	104.48	93.21	98.06
Student-to-teacher ratio	13.51	14.66	14.49
Elementary	0.73	0.79	0.86
Middle	0.28	0.21	0.14
Class size	22.08	23.25	22.19
Number of students	720.25	846.43	836.33
Percent teachers fully licensed	96.36	97.48	95.78
Percent teachers with 0–3 yrs experience	24.07	21.78	25.87
Percent teachers with 4–10 yrs experience	31.72	36.42	32.56
Percent teacher turnover	17.27	14.78	15.87
Number of schools	80	24	22

TABLE 5—SUMMARY STATISTICS FOR TRADITIONAL ONLY AND CONVERTING SCHOOLS

		Converters		
	2006–2007 Traditional	2006–2007 (Traditional)	2007–2008 (Year-round)	Difference
Percent crowding	103.482	104.423	84.177	20.245**
Student-to-teacher ratio	13.604	14.558	14.175	0.383
Class size	21.863	21.621	21.689	−0.068
Number of students	711.215	832.318	827.364	4.955
Percent teachers fully licensed	97.646	97.900	98.073	−0.173
Percent teachers with 0–3 yrs experience	23.738	26.168	25.591	0.577
Percent teachers with 4–10 yrs experience	32.023	31.705	33.100	−1.395
Percent teacher turnover	22.341	20.191	9.577	10.614**
Number of schools	79	22	22	

** Significant at the 5 percent level.
* Significant at the 10 percent level.

statistics for these schools the year before the switch (under a traditional calendar) and the year of the switch (under a year-round calendar). The table also includes summary statistics for the traditional schools that were not switched to a year-round calendar.

Not surprisingly, these 22 schools are significantly less crowded after the mandatory year-round calendar conversion. On average, these schools drop from 104.4 percent over-crowded to 84.2 percent over-crowded.[23] There are no statistically significant changes with respect to class size, enrollment, and teacher licensure or teacher experience. This result suggests that YRS has little impact on these characteristics. Interestingly, average teacher turnover is significantly lower under the year-round calendar. The comparatively large turnover rate in the year preceding the policy change may suggest that teachers anticipated the policy change and left

[23] We use the crowding measure provided by the district, which assumes that the school capacity increases when the year-round calendar is adopted. This allows us to measure an impact of year-round schooling holding the level of school crowding constant. Since this crowding level is related to the policy itself, we also estimate specifications with no crowding adjustment and with a crowding variable that assumes the pre-policy school capacity. In each case our results remain very similar (see online Appendix).

these 22 schools before the calendar was converted. However, the turnover rate in 2005–2006 (19.7 percent) is comparable to the 2006–2007 (20.2 percent) turnover rate, suggesting YRS may instead be the cause. In either case, turnover rates appear to be an important control variable. Finally, compared to the nonswitching schools there are very few differences in aggregate school statistics, though the schools that stay on the traditional calendar are slightly less crowded, have somewhat more experienced teachers, and higher teacher turnover. The similarity between the two sets of traditional calendar schools helps ease concerns over non-random selection of the converting schools.

IV. Empirical Approach and Results

Many prior studies that have estimated the impact of YRS have relied on simple cross-sectional analysis to estimate the impact of YRS on student achievement (Cooper et al. 2003).[24] For instance, the impact of YRS might be estimated using the following equation:

$$(3) \qquad Y_{is} = \alpha YRS_s + \mathbf{X}_i\beta + \mathbf{S}_s\delta + \varepsilon_{is},$$

where, Y_{is} is the test score of individual i in school s, YRS_s is an indicator variable that set equal to one if school s operates on a year-round schedule, $\mathbf{X}_i$ is a vector of individual and family background characteristics of individual i, $\mathbf{S}_s$ is a vector of school level characteristics of school s, and ε_{is} is an error term. The effect of YRS is then recovered by α. This parameter reflects the mean difference in test scores between students who attend YRSs and those who attend traditional calendar schools, controlling for the included covariates. The advantage of this estimation strategy is that identification of the YRS effect in these models requires only between-school variation in the year-round variable. Consequently, the data requirements are minimal. The implicit assumption underlying such models, however, is that students who attend YRSs are comparable to students who attend traditional calendar schools, or that the control variables included in the model sufficiently capture all of these differences. As highlighted in Section II, this assumption is problematic for two reasons. First, students who enroll in YRSs might be systematically different from their peers in ways that are not observed in the data (i.e. groups may have different values of ability, θ). Second, the implementation of a year-round calendar is likely non-random, and thus YRSs might differ from traditional schools in unobserved ways. Estimates that fail to account for these differences will likely be biased.

These limitations, which are inherent, to varying degrees, in previous studies on YRS, can be addressed with a dataset that has repeated observations on students over time, provided there is both within-student and within-school variation in calendar type. If these conditions are met, school and student fixed effects can be used to

[24] In fact, in several of the early studies examined by Cooper et al. 2003, the empirical analysis consists of a simple difference in means of achievement in traditional calendar and year-round schools. Graves (2010, 2011), McMillen (2001), and Von Hippel (2007), however, each use more sophisticated specifications.

capture permanent differences in students and schools, of which many are likely correlated with both achievement and year-round schooling.

Consider, for instance, the following model:

$$(4) \qquad Y_{igst} = \alpha YRS_{st} + \mathbf{X}_{it}\beta + \mathbf{S}_{st}\delta + \varphi_i + \phi_s + \gamma_{gt} + \varepsilon_{igst},$$

where Y_{igst} is the outcome of interest (i.e. test scores) for student i in grade g at school s at time t, YRS_{st} is an indicator variable that set equal to one if school s operates on a year-round schedule at time t, $\mathbf{X}_{it}$ is a vector of individual and family background characteristics of individual i at time t, $\mathbf{S}_{st}$ is a vector of school level characteristics of school s at time t, φ_i is a student specific fixed effect, ϕ_s is a school fixed effect, γ_{gt}, is a set of grade-by-year fixed effects, and ε_{igst} is an error term. Models of this type are typically specified in one of two ways. First, if the dependent variable is a test score as written in (4), then the parameter α captures the impact of YRS on the level of achievement, the student and school fixed effects capture an average level of achievement within a student or school, and the grade-by-year effects capture any unobserved effects that may differ within a grade in a particular year (e.g., grade specific yearly changes in tests). We refer to this specification as the "levels" specification. The second approach is to estimate a similar specification with the dependent variable defined as the change in test scores between periods, as shown in specification (5):

$$(5) \quad Y_{igst} - Y_{ig-1st-1} = \alpha YRS_{st} + \mathbf{X}_{it}\beta + \mathbf{S}_{st}\delta + \varphi_i + \phi_s + \gamma_{gt} + \xi_{igst},$$

$$\text{where } \xi_{igst} = \rho_{igst} - \rho_{igst-1}$$

and ξ_{igst}, and ρ_{igst} are error terms. Using this approach, the parameter α captures the impact of YRS on the change in achievement over the course of the year, and the student and school fixed effects capture average rates of change in achievement within student and school. As before, the grade-by-year effects capture any unobserved effects that may differ within a grade in a particular year. We refer to this as the "growth" specification.

These panel data techniques identify the impact of YRS using the exogenous variation in YRS that remains after controlling for the multi-level fixed effects. Empirical specifications of the education production function similar to (4) and (5) are common in the economics of education literature and are widely preferred over models that only capture contemporaneous inputs such as that specified by equation (3). Rivkin, Hanushek, and Kain (2005), for instance, use similar models to evaluate the impact of teacher effectiveness on achievement. Using the same NCERDC data, Bifulco and Ladd (2006) also use a similar formulation to test the impact of charter school attendance on achievement in North Carolina.[25] More

[25] For a detailed discussion of the conceptual and empirical concerns regarding estimation of the education production function, see Hanushek (1979) and Todd and Wolpin (2003). One issue that is relevant to our study is the

recently, Hanushek and Rivkin (2009) use multi-level fixed effects models to estimate the impact of teacher experience and racial concentration of schools on the racial academic achievement gap.

In our study sample, we observe more than 10,000 students in both a traditional and a year-round calendar. This variation in year-round calendar attendance arises in our dataset for two reasons. First, we observe students switching schools over the time period of interest (within-student variation). Most obviously, as students progress through their schooling, many will advance from elementary to middle school or middle school to junior high, etc. Since very often only a subset of the schools a student attends is year-round, this allows us to observe the same student under two calendars. Additionally, in this school district, students are assigned to schools in such a way that the socio-economic backgrounds of students are balanced across schools. This requires switching students from one school to another to maintain the balance. Importantly, these types of switches give us variation that is independent from the normal switching due to grade advancement and not due to self-selection.

The second source of variation in YRS arises because we observe 22 schools before and after they are converted to a year-round schedule for the 2007–2008 school year (within-student-and-school variation). Because this variation comes from a school-wide policy change, this second source of variation in school calendars not only provides variation that is not due to self-selection, but also provides a source of within-student variation that does not rely upon school switches. This attribute allows us to address concerns expressed in the literature about identifying school effects from students who switch schools (Bifulco and Ladd 2006).

We estimate variations of equations (4) and (5) with three primary sets of model specifications. First, a baseline model (similar to most past studies) is estimated. This model includes neither student nor school fixed effects. Then, student fixed effects are added in the second specification. Finally, the last set of models includes student and school fixed effects. Since all of our demographic characteristics are constant across time, the vector $\mathbf{X}_{it}$ is dropped from models with student fixed effects. However, in models without student fixed effects, standard demographic controls are included (gender, race, and parents' education). Since YRS is a school-level variable, all standard errors are clustered at the school-level.

A. *YRS and Student Achievement: Main Results*

Table 6 reports results from levels and growth models for the three different model specifications. Math score results are presented in panel A, while reading score results are given in panel B. The first specification includes neither student nor school fixed effects, and thus provides a baseline estimate of YRS. These models

assumptions that these specifications make about the technology of cognitive achievement. If we adopt the standard framework that Todd and Wolpin (2003) and Hanushek, Kain, and Rivkin (2006) use, the key question is about the depreciation of past achievement. Our "levels" specification assumes a 100 percent depreciation of past achievement, while the "growth" specification assumes no depreciation. The biases associated with misspecification in either case are different, and thus we include both. Other specifications, including those that include lagged test scores, yield similar results (see online Appendix).

TABLE 6—ESTIMATES OF YEAR-ROUND SCHOOLING ON MATH AND READING TEST SCORES

	Test score levels			Test score growth		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Math scores						
Year-round	0.067*	0.046**	−0.002	0.027*	0.026	−0.003
	(0.035)	(0.016)	(0.028)	(0.016)	(0.024)	(0.051)
Panel B. Reading scores						
Year-round	0.040	0.036**	0.016	0.014	0.017	−0.012
	(0.027)	(0.012)	(0.023)	(0.009)	(0.015)	(0.032)
Student fixed effects	No	Yes	Yes	No	Yes	Yes
School fixed effects	No	No	Yes	No	No	Yes
Observations		159,955			133,372	
Students		50,657			50,657	

Notes: All models include grade-by-year fixed effects and time-varying school controls (crowding, student-to-teacher ratio, elementary, middle, class size, number of students, percent teachers fully licensed, percent teachers with 0–3 years experience, percent teachers with 4–10 years experience, and percent teacher turnover). Models without individual fixed effects include controls for gender, race and parents' education. Robust standard errors, clustered at the school level, are in parentheses.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

essentially are multi-cohort cross-sectional models. The estimated effect in these models reflects the average difference in test scores (or growth) between traditional and year-round students, controlling for observed demographic and school characteristics and grade-by-year effects. The year-round effect in these base models is identified through the between-school, within-student, and within-student-and-school variation.

The findings from the baseline model are suggestive of a positive impact of YRS on achievement. However, as discussed earlier, these results may be misleading due to non-random student selection into year-round schools. To alleviate student self-selection concerns, student fixed effects are added to the models. Results from these model specifications (columns 2 and 5 of Table 6) are similar to the baseline results and imply YRS has a positive impact on both math and reading score levels and on their growth. Math score estimates are precisely estimated and indicate students who attend a YRS score 0.046 standard deviations higher than their traditional calendar counterparts and have about a 0.026 standard deviation advantage in terms of growth in test scores. Effects of this size are plausible given that the magnitude of summer learning loss is often estimated to be about 0.1 standard deviations per year. Nevertheless, effects of this size are small: they amount to an achievement boost of one to two percentile points. Estimates on reading score levels are only slightly lower, suggesting students in YRS score 0.036 and 0.017 standard deviations higher in terms of levels and growth, respectively.

By controlling for all time invariant student differences, the student fixed effects specifications alleviate many student self-selection biases. If, however, year-round *schools* systematically differ from traditional calendar schools in unobserved ways, the estimates reported in columns 2 and 5 may still be biased because of these omitted school level inputs. Moreover, some of the students used to identify the impact of

YRS in these models are those students who are observed switching schools (either from traditional to YRS or vice versa). While many are forced to switch due to the county's diversity busing policy, if this set of school switchers differs from the larger group of students enrolled in a YRS, or if the school switch itself has an independent impact on achievement, these results could be misleading.[26]

Because the school calendar is a school-wide characteristic and 22 schools have changed their academic calendars over the study period, we are able to address this limitation by exploiting the within-student-and-school variation in our data to directly control for time invariant student and school characteristics using student and school fixed effects. This model essentially exploits the natural experiment created by the WCPSS policy change using a general differences-in-differences type framework. Results from these models are presented in columns 3 and 6 of Table 6. The results from these model specifications tell a very different story. In contrast to the results reported in columns 1, 2, 4, and 5, which indicate a positive, statistically significant impact of YRS on both math and reading test score levels and growth, estimates from columns 3 and 6 imply that YRS has essentially no impact on either math or reading achievement. Nearly every estimate is close to zero in magnitude and in all growth models is slightly negative. The primary conclusion coming out of these models is that failure to control for *school-level* unobserved heterogeneity leads to estimates that largely over-state the impact of YRS on student achievement. This is consistent with the hypothesis that year-round schools are placed in high-growth and possibly high achieving areas.

B. *YRS and Student Achievement: Results by Race*

As noted earlier, year-round calendars have often been promoted as a fix for "summer learning loss" which tends to exacerbate racial and economic academic inequalities. Unfortunately, we do not have reliable student level income or socioeconomic status variables, but we do observe students' race.[27] Table 7 shows the results of eight specifications similar to those shown in previous tables, but with the year-round indicator variable interacted with a set of race categories.[28] Math scores results are reported in panel A, while results for reading scores are reported in panel B. All models include student fixed effects. Columns 1 and 3 report results from models that do not include school fixed effects, while results from models that do include school fixed effects are reported in columns 2 and 4.

The results reported in Table 7 mirror the main results of the paper. In models that do not control for unobserved school characteristics, the estimates imply YRS has a small positive impact on both math and reading test score levels and growth.

[26] Although theoretically problematic, empirically these switchers don't appear to bias the results. Estimates from student fixed-effects models without the 22 converted schools, which identify the impact of YRS using only the switchers, are nearly identical to the main results reported in Table 6 (see online Appendix).

[27] Unfortunately, though parents' education and free lunch status would be good proxies and are available in the NC data, they are missing for a substantial portion of our sample. Parents' education is missing for nearly half of the YR students (see summary statistics in Table 3). The free lunch status variable is available in 2006, but is not included in the 2007–2009 data that we also use in this study, making it unusable.

[28] We do not omit any racial category; instead we omit the YRS indicator, so the coefficients can be interpreted as the advantage that YRS give to each racial group relative to their counterparts in schools with traditional calendars.

TABLE 7—ESTIMATES OF YEAR-ROUND SCHOOLING ON MATH AND READING TEST SCORES BY RACE

	Test score levels		Test score growth	
	(1)	(2)	(3)	(4)
Panel A. Math scores				
Year-round × White	0.048**	−0.012	0.032	−0.002
	(0.019)	(0.029)	(0.033)	(0.058)
Year-round × African-American	0.053***	0.025	0.052**	0.026
	(0.016)	(0.027)	(0.022)	(0.044)
Year-round × Hispanic	0.023	0.004	−0.034	−0.050
	(0.021)	(0.030)	(0.029)	(0.048)
Year-round × Asian	0.081**	0.022	−0.017	−0.037
	(0.029)	(0.035)	(0.030)	(0.060)
Year-round × Indian	−0.056	−0.093	−0.156	−0.153
	(0.084)	(0.087)	(0.132)	(0.133)
Year-round × Mixed	0.015	−0.035	0.027	0.000
	(0.019)	(0.029)	(0.033)	(0.000)
Panel B. Reading scores				
Year-round × White	0.039**	0.016	0.018	−0.008
	(0.012)	(0.023)	(0.017)	(0.033)
Year-round × African-American	0.034**	0.016	0.038**	0.001
	(0.016)	(0.025)	(0.018)	(0.034)
Year-round × Hispanic	0.007	−0.005	−0.043*	−0.073**
	(0.016)	(0.027)	(0.023)	(0.036)
Year-round × Asian	0.060**	0.039	0.038	0.026
	(0.024)	(0.032)	(0.037)	(0.050)
Year-round × Indian	0.035	0.024	0.245	0.245
	(0.089)	(0.095)	(0.199)	(0.205)
Year-round × Mixed	0.053**	0.029	−0.006	−0.036
	(0.025)	(0.034)	(0.005)	(0.041)
School fixed effects	No	Yes	No	Yes
Observations	159,955	159,955	133,372	133,372
Students	50,657	50,657	50,657	50,657

Notes: All models include grade-by-year fixed effects and time-varying school characteristics (crowding, student-to-teacher ratio, elementary, middle, class size, number of students, percent teachers fully licensed, percent teachers with 0–3 years experience, percent teachers with 4–10 years experience, and percent teacher turnover). Robust standard errors, clustered at the school level, are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

However, when school fixed effects are included in the models, the estimates are no longer statistically significant and some are of opposite sign, indicating YRS has little to no impact on achievement.

Estimates on reading test scores for African-American students are consistently positive and have a higher magnitude than those of white students, but even with this relatively large sample of students, the effects are not statistically significant. This indicates that if there is a positive impact of YRS on any racial group, the effect is likely quite small. The impact on Hispanic students in reading is estimated to be negative, but the effect is only significant in one of the two specifications with school fixed effects. Moreover, the Hispanic population in our sample is not large enough to draw firm conclusions, but this may be a population that is impacted differently, since a larger proportion of the population consists of English language learners for whom the technology of achievement might be different.[29]

V. Conclusion

Despite heated debates over year-round schooling and its rapid adoption across the country, we find little evidence that a year-round calendar will benefit the average student. Once we account for permanent unobserved student and school characteristics in our fixed effects models, we find that the achievement of students in YRS is very similar to those using traditional calendars. Our results imply that dividing a long summer break into more frequent shorter breaks does not have a positive impact on achievement as measured through standardized test scores. In our model of learning loss, these results are consistent with a constant rate of return to investment—similar to what was found by Downey et al. (2004)—and a constant rate of depreciation, which together indicate that the timing of learning is not important, only the amount of learning. Though this simple model can provide a framework for our work and future research on the topic, without more detailed data on achievement throughout the year, we are not able to directly estimate the parameters of this model.

While our data are valuable in many respects, there are some limitations to using this sort of focused natural experiment approach. First, we only observe outcomes for two years after the policy change. As a result, we can only estimate short-run effects. If there is an adjustment period, after which teachers and schools can better take advantage of the new calendar type, the long run results might show a larger positive impact of year-round schooling. There is some limited evidence for this in Graves' (2010) study which finds smaller negative impacts in later years. A second limitation is that, given that the year-round calendar was placed in schools with a particular population—or because particular students moved to the year-round schools—the same policy might have a different impact if applied to a very different population.

While these results are similar to others in the literature the argument for year-round calendars does not depend solely on the estimated impact on academic performance. For example, the transition in Wake County was made in order to take advantage of cost savings, and many year-round schooling opponents cite negative impacts on the community and family life (Shields and Oberg 2000). Nevertheless, the quasi-experimental nature of our research design should help push this literature in a direction that will allow schools to make calendar decisions based on more accurate information.

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[29] While our sample of Hispanic students is not large enough to draw firm conclusions regarding Hispanic students, the negative result is consistent with results reported by Graves (2010, 2011) who finds California schools on a year-round calendar rank one to two percentile points lower nationally than California schools on a traditional calendar, and that some minority groups are disproportionately harmed by YRS. Since Graves relies on data from California, which has a much larger sample of Hispanic/Latino students than our sample, this may suggest the findings reported in that paper are driven by the negative impact of YRS for Hispanic students.

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