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What Happens to Summer Learning in Year-Round Schools?

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Abstract

Children’s learning slows dramatically during summer vacation, and the slowdown is especially severe for poor children with uneducated parents. In part to fight summer slowdown, more than three thousand schools have adopted *year-round* school calendars that redistribute the 180 days of the school year, replacing the traditional three-month summer vacation with a series of shorter breaks spaced more evenly throughout the year. In this paper, we compare learning rates in year-round and nine-month schools. During summer, children learn more quickly in year-round schools than in nine-month schools, but during the rest of the year, children learn more quickly in nine-month schools. On balance, over a twelve-month period, children learn about as much in year-round schools as in schools using a nine-month calendar. The results fit the view that summer setback is a symptom of disadvantages in children’s non-school environments—disadvantages that cannot be eliminated merely by rearranging the 180 days of the academic year.

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Summer Learning

Summer vacation is bad for children's academic achievement. In the early grades, children learn word and number skills much more slowly during summer vacation than they do during the school year (Cooper et al. 1996; Downey, von Hippel, and Broh 2004). In the later grades, when schoolwork grows more academic and less similar to out-of-school activities, most children return to school in the fall having forgotten over a month of the math, science, and social studies that they learned the previous spring (Cooper et al. 1996). This summer forgetting represents more than a lost opportunity; it also reduces the amount that can be taught during the school year. Instead of introducing new material, many teachers must begin the fall by reviewing a substantial fraction of what students learned the previous spring and then forgot over the summer.

Summer learning is particularly slow for poor children with less-educated parents (Heyns 1978; Entwisle and Alexander 1992; Downey, von Hippel, and Broh 2004). In fact, it is mainly during the summers between academic years that poor children lose ground to their middle-class peers (Alexander, Entwisle, and Olson 2001; Downey, von Hippel, and Broh 2004). Although poor children are already behind on the first day of kindergarten, during the school year they nearly hold their own by learning almost as fast, on average, as their more affluent peers (Downey, von Hippel, and Broh 2004). It is summer vacation that sets poor children further and further back (Alexander, Entwisle, and Olson 2001).

Compounded year after year, summer setback steadily erodes the competitive position of poor children, at first in academic achievement and later in the competition for jobs. By ninth grade, the achievement gap between poor and middle-class children is more than three times larger than it was at the start of first grade—and most of the expansion in the gap has occurred during summer vacations,

not during the school years (Alexander, Entwisle and Olson 2007). Poor children's low ninth-grade achievement, in turn, routes them away from college-preparatory courses and reduces their chances of graduating from high school, or of attending college if they do graduate (Alexander, Entwisle and Olson 2007). And if poor young adults lack a college degree, or at least a high-school diploma, it is much harder for them to escape poverty today than it was a generation ago (Barton 2005).

Sociologists attribute summer learning loss to deficiencies in children's home lives and neighborhood resources (Heyns 1978; Entwisle and Alexander 1992; Downey, von Hippel, and Broh 2004). Compared to schools, most children's home environments do relatively little to develop academic skills, and the level of stimulation is especially low in the homes of poor families (Lareau 2000; Linver, Brooks-Dunn, and Kohen 1999, 2002). In school, by contrast, children share a common environment that helps the poor learn almost as quickly as the middle class. Although some schools are better than others, the differences between rich and poor schools are much smaller than the differences between rich and poor homes (Downey, von Hippel, and Broh 2004).

Year-Round Schools

At least three educational interventions attempt to increase summer learning. The first and most obvious intervention is summer school. Most evaluations of summer school programs find that they have a positive effect on achievement (Cooper et al. 2000). The positive effect may be larger for middle-class children than for low-achieving poor children, but since poor children are more likely to attend (Cooper et al. 2000), on balance summer programs probably tend to reduce inequality across socioeconomic groups. Summer school programs are difficult to evaluate, because the children who enroll in them are typically much lower-achieving than the children who do not enroll. The best summer-school evaluations match participants to nonparticipants with similar achievement levels, or, even better, assign children at random to summer school or non-summer-school conditions. On

average, the best-designed studies find that summer school can boost achievement scores by 10% to 15% of a standard deviation (Cooper et al. 2000), which is a small effect but nevertheless impressive given that summer school programs last less than three months.

A second intervention is the *extended-year* school calendar, which lengthens the academic year from the usual 175-180 school days to more than 200 school days including three or more weeks in the summer. At least four east Asian countries—Singapore, Korea, Japan, and Hong Kong—have academic years with more than 200 school days, and these are also the highest scoring countries on international tests of mathematics skill (Trends in International Mathematics and Science Study 2003). Yet east Asian countries differ from the U.S. in other ways as well, and it is not clear to what extent their high test scores are due to their longer school years. In the U.S., less than one elementary school in a thousand uses an extended calendar¹, so that U.S. evaluations of the extended-year calendar are rare and can focus on as few as three schools (Gandara and Fish 1994). In addition, the effect of an extended-year calendar can be hard to evaluate because schools that extend the year often adopt other reforms at the same time. Charter schools in the Knowledge Is Power Program (KIPP), for example, have high scores and an extended-year calendar, but they also require extensive parental involvement, and they tend to attract students who are high-scoring (relative to their impoverished neighborhoods) before they even enroll (Carnoy et al. 2005).

A third attempt to increase summer learning is the *year-round* school calendar—which is the focus of this paper. Although still rare, the year-round calendar is much more popular than the extended-year calendar, and is currently in use at 3.5% of US public elementary schools.² Unlike an extended-year calendar (with which it is often confused), a year-round calendar does not actually

¹ Among the 935 schools surveyed by the Early Childhood Longitudinal Study, Kindergarten cohort, none scheduled more than 183 school days.

² In 2005, 2,353 elementary schools followed a year-round calendar (NAYRE 2005). This figure is about 3.5% of the 67,344 public schools with elementary grades in 1998-2000 (National Center for Education Statistics 2000a).

increase the days of instruction beyond the usual 180 or so. Instead, year-round schools take those 180 days and redistribute them, replacing the usual schedule—nine months on, three months off—with a more “balanced” schedule of shorter vacations alternating with shorter instruction periods across all four seasons of the year. There are several year-round calendars in use; the most popular alternate nine- or twelve-week instructional periods with three- or four-week vacations or “intersessions” (National Association for Year-Round Education [NAYRE] 2005).

Year-round calendars are not always meant to increase summer learning. In 40 percent of year-round schools, the year-round calendar is part of a *multi-tracking* plan whose purpose is to alleviate overcrowding (Cooper et al. 2003). In a multi-tracked school, the student body breaks into three or four groups who attend school on a staggered schedule. When one group is on vacation, the other groups are in session, so that only two-thirds to three-quarters of the students are in the building at a given time. Class sizes are reduced, and higher enrollments can be accommodated without building new schools. Some fast-growing school districts have become dependent on the year-round calendar as a way to handle increasing enrollments. In greater Las Vegas, for example, nearly half of all elementary schools use a year-round calendar, and it has been estimated that switching to a traditional calendar would require half a billion dollars in new school construction (Clark County School District Year-Round Calendar Study Group 2007).

In 60 percent of year-round schools, however, the calendar is not part of a multi-tracking plan and is evidently chosen for reasons other than crowding reduction. Both advocates and adopters of year-round calendars often claim explicitly that the calendar can reduce summer learning loss and therefore boost achievement (Stenvall 1999). The former president of the National Association for Year-Round Education, for example, has asked, “Why does it take so long for some secondary school communities to understand that one way to reduce summer learning loss is to reduce the summer

vacation?”(Ballinger 2000). The chief education officer of the Chicago Public Schools has said that most of Chicago’s 29 year-round schools adopted the year-round calendar in hopes of reducing summer learning loss (Sadovi and Little 2007); in fact, Chicago’s Mayor Daley believes that year-round calendars, as well as summer learning programs, are essential to international competitiveness—although his ultimate goal is extending the school year (Spielman and Grossman 2005).

Notice that the goals of crowd reduction and summer learning increases are not mutually exclusive. Even in multi-track schools, that is, there is often some expectation that year-round calendars will increase test scores.

But can year-round calendars really fix the problem of summer learning? Remember that year-round calendars do not eliminate or even reduce summer vacation; instead, they take the days of summer vacation and redistribute most of them to other times of the year. Unless there is something special about the months of June, July, and August, it seems likely that redistributing vacation time to other months will simply move the problem of summer learning around. Instead of a single season where academic learning slows down or reverses, on a year-round calendar we might expect a larger number of short slowdowns and reversals, distributed across all seasons of the year. These three- and four-week slippages will be less noticeable, and smaller in their individual impact—but over the course of a calendar year, the cumulative loss may be similar to the major setback usually observed during summer vacation.

Although sociologists occasionally voice support for year-round education (e.g., Alexander, Entwisle, and Olson 2007), there is little in the sociology of education to suggest that year-round calendars can remedy the disadvantages apparent during summer vacation. If summer setback is a symptom of deficiencies in children’s non-school environments (Downey, von Hippel, and Broh 2004), then year-round calendars do not really address the problem. Year-round calendars do not

increase the time that children spend in school, nor do they increase the academic stimulation of children's non-school environments.

While the sociological perspective is discouraging, cognitive psychology offers some hope that year-round calendars can increase achievement. Psychological experiments have repeatedly shown that “spaced practice” is better than “massed practice”. That is, people learn and retain more when they practice in a series of short sessions instead of a single long session—especially if the intervals between practice sessions are relatively short (for reviews see Dempster 1988 or Donovan and Radosevich 1999). This “spacing effect,” or “distribution-of-practice effect,” has been demonstrated in school-like activities such as memorizing vocabulary words and retaining ideas from readings and lectures (Dempster 1988). At first glance, then, we might hope that learning would increase if the three-month summer vacation were replaced with a series of three- or four-week breaks.

On the other hand, the kind of spacing effect observed in the laboratory may have little relevance to the school calendar. Most spacing research has focused on short time periods, with intervals of seconds, minutes, or hours between practice sessions. There has been less research that looked at intervals of days, and none that looked at intervals of weeks or months (Donovan and Radosevich 1999; J. Donovan, personal communication, Sept. 21, 2006). The research on longer time periods has been fairly discouraging; when the interval between practice sessions is longer than a day, spaced practice is only a little more effective than massed practice, and may be even *less* effective than massed practice if the mental tasks being learned are especially demanding³ (Donovan and Radosevich 1999, especially Table 3).

Although these findings suggest that the traditional three-month summer break is not optimal for learning, they also suggest that year-round breaks of three or four weeks may be no better.

³ This may be why computer programmers, for example, often prefer to work in unbroken marathon sessions.

Preview of Results

In this paper, we test the effect of year-round calendars using longitudinal data that lets us estimate learning rates both during the summer and during the nine months of the traditional academic year. This is the first piece of research to look directly at what happens to summer and non-summer learning under a year-round calendar.

The results suggest that year-round calendars do not, on average, increase total learning; instead, they merely redistribute periods of learning and forgetting across the calendar year. During the summer, children in year-round schools do learn more quickly than children who are on vacation, but during the other nine months, children in year-round schools learn more slowly than children in traditional schools. This pattern of results makes sense; during the summer, year-round schools are in session while nine-month schools are on vacation, but during the rest of the year, nine-month schools are in session almost continually, while year-round schools have regular breaks of three to four weeks. On balance, over a twelve-month period including both summer and the other three seasons, the amount learned in year-round schools is almost exactly the same as the amount learned in schools using a traditional nine-month calendar.

Although past research has suggested that year-round calendars may at least be helpful to disadvantaged children, our own results for such children were mixed. As it turns out, *most* children in year-round schools are at least moderately disadvantaged, so our finding that such children learned no more in year-round schools than did comparable children elsewhere may be viewed as evidence against the claim that year-round schools help the poor. If we look further down the socioeconomic ladder and focus on children who are disadvantaged even compared to the general year-round population, we find limited evidence that such children learn more on a year-round calendar than on a traditional calendar—but only in reading, not in mathematics, and only over one of the two twelve-

month periods that were covered in the study. Moreover, our confidence in the twelve-month reading gains was slightly undermined by the fact that the reading scores were rescaled partway through the survey, in a way that makes twelve-month reading gains harder to assess than similar gains for mathematics.

On balance, the results suggest that year-round calendars do little to fix the problem of summer learning. Instead, they mainly hide the problem by sweeping it under the rug of fall, winter, and spring.

Past Research on Year-Round Schools

In a meta-analysis, Cooper et al. (2003) reviewed 39 past studies of year-round school calendars. Depending on how the results are interpreted, the meta-analysis may be viewed as neutral, as mildly encouraging, or as uninformative.

On one hand, most of the studies in the review found that children in year-round schools had higher average scores than students following a traditional school calendar. On the other hand, the percentage of studies showing an advantage for year-round schools was hardly overwhelming, at 62%, and the average effect size was trivial, at about 5% of a standard deviation. The effect did seem to be a bit larger, about 10% of a standard deviation, for students from disadvantaged communities.

The results of the meta-analysis should be viewed skeptically. The published literature is often biased toward showing that reforms have benefits, because research showing no benefits is harder to publish (Gerber, Green & Nickerson 2000; Gerber & Malhotra 2006).⁴ In addition, most research on

⁴ To their credit, Cooper et al. (2003) included unpublished studies in their meta-analysis, though of course they could not include studies that were never written up or released. Cooper et al.'s (2003) meta-analysis found two possible symptoms of publication bias: (1) they found larger effects in studies with smaller sample sizes; and (2) they found larger effects in dissertations than in other publications. However, these differences were observed only in a fixed-effects model; they disappeared when a random-effects framework was used.

year-round calendars has been poor in quality. As Cooper et al. (2003) pointed out, 59% of past studies did not control for confounding differences between students in year-round and traditional schools—which is troubling since, as we will show, year-round schools tend to have a variety of disadvantages. In addition, Cooper et al. (2003) were concerned that no prior study “explicitly controlled for the number of days that school had been in session before achievement outcomes were measured.” For example, if a spring test were given on May 30, students in a nine-month school would have almost a full year of instruction behind them, while students in a year-round school would still have a month of instruction to go. So the timing of a spring test can give nine-month students an unfair advantage. On a fall test, on the other hand, it is year-round students who have an advantage, since they start school more than a month before their nine-month counterparts.

McMillen (2001) pointed out other weaknesses of past studies, including “collapsing achievement outcomes into categories such as ‘at or below grade level’ ... failure to report any tests of statistical significance or measures of effect size..., failure to differentiate between year-round and extended-year schools,” avoidance of peer review, and failure to “account statistically for the nesting of students within schools.” McMillen’s (2001) own research—perhaps the best study of year-round schools to date—avoided most of these weaknesses. Focusing on public schools in North Carolina, McMillen (2001) compared 106 year-round schools to 1,364 nine-month schools in a design that accounted for student demographics, prior achievement, and the nesting of students within schools. The results showed no significant effects of school calendars on achievement, except for initially low-achieving students, whose final scores may have been boosted by a small amount, again less than 5% of a standard deviation.

In addition to the shortcomings pointed out earlier, no prior research has examined the effect of year-round calendars on summer learning. Although it is commonly assumed that the achievement

benefits of a year-round calendar would come mainly from an increase in summer learning⁵, no study has looked at this question directly. In saying this, we do not mean to criticize past researchers; it is impossible to look at summer learning unless students are given tests at the end of one academic year and the beginning of the next. In our study, we were fortunate to have data from such spring and fall tests—but such a testing schedule is very unusual.

Our Contribution

Our study, then, is the first to look explicitly at summer learning in year-round schools. Using longitudinal data, we track students' learning through the summer and through the nine months of the conventional academic year. Ours is also the first study to address Cooper et al's (2003) concern and account for the number of days that school has been in session at the time of each test. And we address McMillen's (2001) concern by accounting for the nesting of children in schools using a multilevel model.

We try to equalize year-round and nine-month schools on confounding variables, and a special feature of the data lets us show that our equalization is more or less successful. Specifically, the data let us estimate children's achievement levels in the summer before kindergarten—before school calendars have had a chance to affect learning. Before kindergarten there is a significant gap between the test scores of children bound for year-round and nine-month schools—but we can more or less eliminate this gap using a small set of control variables. Our ability to explain the pre-kindergarten gap provides some assurance that we have controlled adequately for preexisting differences between children in year-round and nine-month schools.

⁵ For example, Cooper et al. (2003) suggest that the average summer learning loss—about 10% of a standard deviation in achievement—represents an upper bound on the potential impact of year-round calendars. On the other hand, McMillen (2001) suggests that year-round calendars may boost achievement through “intersession” instruction—that is, extra instruction targeting at-risk students during the three- or four-week breaks between regular instruction periods. Of course, intersession instruction effectively increases the days of instruction well beyond 180, turning a year-round school into an extended-year school.

What kinds of schools use year-round calendars?

We begin by comparing the characteristics of nine-month and year-round calendars. This comparison sheds some light on what kinds of schools use year-round calendars, and why those calendars were adopted. The comparison also provides us with a list of differences to control for when we estimate the effect of year-round calendars on learning.

Data

To make this comparison, we use the Early Childhood Longitudinal Study, Kindergarten Class of 1998-99 (ECLS-K)—a national survey carried out under the direction of the National Center for Education Statistics, US Department of Education (<http://nces.ed.gov/ecls/Kindergarten.asp>). During kindergarten and first grade, the ECLS-K gave reading and math tests to 17,030 children enrolled in 992 public and private elementary schools⁶.

There are a couple of ways to check whether a school follows a year-round calendar. First, the ECLS-K includes a variable (called F4YrRnd) that explicitly indicates whether the school is year-round or not. In addition, the survey provides the first and last day of kindergarten, which can be used to calculate the length of the school's kindergarten year.⁷ The first variable is available for nine-tenths of schools; the second, for two-thirds.

For the most part, these two sources of information agree, but there are a few discrepancies that highlight coding errors and ambiguities. The length of the kindergarten school year has a cleanly

⁶ The number 992 refers to the schools where children began kindergarten. Some students later transferred out of their original schools, and the survey followed some of these students to their new schools. Our analyses exclude the schools that these children transferred to, and also exclude any tests that a child took after a transfer. In addition, since our focus is on the effect of summer vacation, we exclude any tests taken after a child attended summer school. After post-transfer and post-summer-school test scores were eliminated, there remained 17,020 children who contributed at least one test score—this is the number reported in the text above. Reassuringly, the results are not materially different if post-transfer and post-summer-school test scores are included and attributed to the school where the child began kindergarten.

⁷ Dates for the first and last day of first grade are available from the public release of the ECLS-K, but these dates have many missing values. Dates for kindergarten are more complete, but only available to researchers who hold a restricted-data license.

bimodal distribution, and we would expect that schools near the primary mode of 9.3 months⁸ would be classified by the survey as traditional, while schools near the secondary mode at 11.6 months would be classified as year-round. Only three schools violate this expectation⁹. In addition, there is a gray area between 10.2 and 10.6 months, where three schools are classified as year-round and three are not. We would like to eliminate this overlap, since there is no point in making distinctions between nominally “year-round” schools and nominally “traditional” schools whose school calendars are roughly the same.

To reconcile these discrepancies, we required that a year-round school have a kindergarten year of at least eleven months. Eleven months is an arbitrary cutoff, but changing it by a few weeks would not change the results, since the distribution of school-year length has a clean break with no schools at all between 10.65 and 11.05 months. Among schools that did not report the length of their school year, we called a school year-round only if it was classified as such by the survey.

Under this definition, 27 of the 748 public schools were classified as year-round, which is about the number we would expect since 3.5% of all US public elementary schools run year-round.¹⁰ None of the 244 private schools were classified as year-round, which again is expected since only 1 in 700 private schools uses a year-round calendar (Naylor 1995). Under a less conservative way of identifying year-round schools, up to 35 schools could be classified as year-round, but our results would not materially change.

⁸ Throughout this paper, we measure time using a “standard month” that is exactly $365/12=30.4$ days long (or $366/12$ days long in a leap year). This seems preferable to using a calendar month, which can range in length from 28 to 31 days.

⁹ Two schools with kindergarten years of 9.2 and 9.7 months are classified as year-round, while one school with an 11.1-month kindergarten year is classified as traditional.

¹⁰ In 2005, 2,353 elementary schools followed a year-round calendar (NAYRE 2005). This figure is about 3.5% of the 67,344 public schools with elementary grades in 1998-2000 (National Center for Education Statistics 2000a).

Comparison

Tables 1-3 provide a detailed comparison of year-round and nine-month schools. Missing values of the tabled variables were filled in using multiple imputation (Rubin 1987); for details of the imputation model, see the Appendix.

Year-round schools do not schedule any more school days than nine-month schools, but they spread those days out over a larger part of the year (Table 1). While nine-month schools typically begin in late August and end in early June, year-round schools typically start in mid-July and last until June is nearly over.

Among the 27 year-round schools in the survey, only one is in a rural area. This is not surprising, since the traditional academic calendar was developed in part to let rural children work on farms during the summer (Worsnop 1996). Although year-round schools are distinctly non-rural, they are not concentrated in cities; year-round calendars are about as popular in the suburbs as they are in urban areas.

Four-fifths of year-round schools are in the Western census region (Table 2), and half are in the state of California alone (NAYRE 2005). Year-round calendars may be popular in California because they got an early start there. California is an early adopter of school innovations. It was the second state to pass a charter-school law; it has the country's most ambitious class-size reduction program (Stecher and Bohrnstedt 2000); and it was a pioneer in year-round education as well. In 1971 two districts in San Diego County were among the first districts to adopt a year-round calendar; by 1974 thirteen other California districts had followed suit, and today there are 164 California districts with year-round schools (NAYRE 2005). It may be that, like the charter-school movement, the calendar-reform movement has spread by geographic contagion (Renzulli and Roscigno 2005). That is, once one district or state tries a year-round calendar, the reform starts to look more promising to its

neighbors, even if there is no clear evidence that the practice is effective. (The idea that a neighbor's adoption of a practice can make it look more appealing is known as "social proof" [Cialdini 2000]). Indirect evidence for geographic diffusion can be seen in the states around California: not only does California have far more year-round schools than any other state, but its southeastern neighbor Arizona has the second-most year-round schools, and its eastern neighbor Nevada is number five (NAYRE 2005).¹¹ On the other hand, there are only three year-round schools in California's northern neighbor, Oregon (NAYRE 2005).

Outside the West, resistance to year-round schools is institutionalized. In seven Southern states, groups have sprung up with names such as Save Alabama Summers and Texans for a Traditional School Year (<http://schoolyear.info/stcoalition.html>). Some of these groups get funding and support from the American Camping Association and the International Association of Amusement Parks and Attractions (Cumming 1993; MacFarquhar 1995; Chaker 2005), which are understandably reluctant to lose summer customers. In 2006, the Texas group cheered the passage of a state law forbidding schools from starting before the fourth Monday in August (79th Texas Legislature, 3rd called session, House Bill 1). With this law Texas joined six other Southern and Midwestern states with laws requiring that all public schools start on the same day (Janofsky 2005). Four of these states—North Carolina and Texas in the South, Wisconsin and Minnesota in the Midwest—require schools to start no earlier than the last week of August (Janofsky 2005).

Children attending year-round schools tend to be poor, but their poverty is moderate rather than profound. Compared to nine-month schools, year-round schools have a 15% higher rate of poor students who eat reduced-price lunches—but the percentage of *very* poor students who eat *free* lunches is no higher at year-round schools than it is elsewhere (Table 2). Similarly, on a measure of

¹¹ This pattern is not an artifact of differences in state populations. Although California is the most populous state, its share of the U.S. population (12%) is much smaller than its share of year-round schools (50%). Similarly, although Arizona and Nevada rank second and fifth in their share of year-round schools, they rank only 19th and 35th in population.

socioeconomic status (SES) constructed by the ECLS-K—a standardized composite of household income, parents’ education, and parents’ occupational status¹²—children at year-round schools typically have lower-than-average SES, but the SES gap between children at year-round schools and nine-month schools is only about half of a standard deviation (Table 3).

There are other signs of disadvantage at year-round schools. Not only are nearly all year-round schools public rather than private, but year-round schools are disproportionately likely to suffer from crowding (Table 2). On a 1-to-5 measure of crowding, year-round principals give their schools an average score of 3.4, compared to an average score of 2.5 in nine-month schools. This difference is not at all surprising, since one purpose of the year-round calendar is to alleviate overcrowding.

Crowding may also be the reason why year-round schools are twice as likely as other schools to offer half-day rather than full-day kindergarten. Half-day kindergarten can be an answer to overcrowding if half the children come to school in the morning and half come in the afternoon.¹³ Crowding has also been suggested as the reason why year-round schools are popular in California (Cooper 2005), but the evidence on this point is mixed. It is not clear that schools are more crowded in California than elsewhere¹⁴, and if it were there would still be plenty of crowded schools in states where year-round calendars are rare.

It is not surprising that year-round calendars are more prevalent in disadvantaged communities. The problems that the year-round calendar is supposed to address—both overcrowding and summer

¹² The ECLS-K measure of SES is a sum of standardized variables, but the sum is not itself standardized. To make interpretation easier, we have standardized the sum.

¹³ Alternatively, we might suspect that half-day kindergarten is a sign of disadvantage; that is, we might think that schools compensate for low resource levels by staying open only half the day. But this explanation contradicts the evidence; in fact, schools with half-day kindergarten are more likely to be affluent than disadvantaged (Wallston and West 2004).

¹⁴ We could not find school crowding measures at the state level, but we could find such measures at the level of census regions. In the ECLS-K, public schools were not notably more crowded in the western census region than elsewhere; in the west, the average crowding score was 2.72, while elsewhere the average crowding score was 2.69. A different survey using a different measure of crowding did find that western schools were more crowded than schools elsewhere (NCES 2000b). Although more than half of the western census region lives in California, the point remains, however, that crowding alone cannot explain why California has so many year-round schools.

setback—are more acute in poor schools than elsewhere. Beyond that, though, disadvantaged schools are more likely to try nearly *any* educational reform. Schools serving poor children are widely, though often unfairly, viewed as failing (Downey, von Hippel, and Hughes 2005), and this label makes them targets for educational reform. Schools that are perceived as failing may be more receptive to change, and if they are not receptive, they have few resources and little credibility to resist reforms imposed by outsiders.

When year-round calendars are proposed in middle-class school districts, they run into organized resistance. Newspaper reports suggest that the parents leading the opposition to year-round schools are from the upper middle class. A leader of the Florida resistance, for example, is described as “a non-practicing lawyer and mother of two,” while a co-founder of Save Georgia Summers evidently has an affluent parent’s flexible schedule and access to sympathetic doctors, since she reports spending a weekday “in the allergist’s office to get a note” excusing her child from hot August bus rides (Janofsky 2005).

Poor parents, by contrast, tend to be less politically mobilized (Jenkins and Wallace 1996; Oberschall 1973; Verba, Brady, and Schlozman 1995). And given their low purchasing power, poor families may be less likely to attract support from business interests such as the theme-park and summer-camp industries. So when a year-round calendar is proposed in a poor community, it has a better chance of taking root.

With year-round schools concentrated in poor Western cities and suburbs, it is not surprising that about half the students in year-round schools are Hispanic. In fact, Hispanic children are three times as common in year-round schools as elsewhere (Table 3). We might also expect year-round schools to have a large number of African-American students, but actually black children are three times *less* common in year-round schools than they are in other schools. This probably has something to do with

geography. Although Hispanic and African-American children both tend to be poor, nearly half of Hispanics live in the Western states, where year-round schools are popular, while most African-Americans live in the South (U.S. Census Bureau 2002), where the resistance to year-round schools is strongest.

A final sign of disadvantage is that the children who attend year-round schools are younger than the children who attend nine-month schools. Disadvantaged parents are less likely to hold their children back from kindergarten, perhaps because poor parents have fewer resources to care for children outside of school (Downey and Hickman 2003). As a result, on July 18, 1998, when the average year-round kindergarten opened its doors, most of the children there were less than 64 months old—six weeks younger than children in traditional schools, who still had over a month to go before the start of kindergarten (Table 3). In our models of achievement and learning, it will be important to account for differences in age as well as test dates and school calendars—since, all else being equal, a younger child will tend to have lower test scores than an older one.

Learning Rates in Nine-Month and Year-Round Schools

To sum up, then, year-round calendars are most common in the crowded public schools of Western cities and suburbs, and are mainly attended by moderately poor Hispanic children. We will now account for these characteristics of year-round schools as we evaluate the impact of year-round calendars on children's learning rates.

Data: Test Scores and Test Dates

A major asset of the ECLS-K is that its testing schedule allows us to estimate how quickly children learn during the summer as well as during the academic year. The ECLS-K provides this information by measuring reading and math skills four times in the first two years: in the fall and

spring of kindergarten 1998-99 and in the fall and spring of first grade 1999-2000. By comparing the spring and fall test scores, we can estimate the amount learned during kindergarten, summer, and first grade. The ECLS-K continued to follow children through fifth grade, but later test occasions were spaced two years apart and so do not permit separate estimates of summer and school-year learning.

Excellent tests of reading and mathematics skill were designed especially for the ECLS-K (Rock and Pollack 2002). The tests were over 90% reliable and tried to measure the full range of young children's abilities—from rudimentary skills, such as recognizing isolated letters and numbers, to advanced skills, such as reading words in context and solving multiplication and division problems. Tests were administered in two stages: first students took a routing test to determine their general skill level (low, medium or high); then they took a more specialized test to determine more precisely where they fell among students of generally comparable skill. After testing, item response theory (IRT) was used to estimate how many questions each child would have answered correctly had they been given a full list of 92 questions in reading and 64 questions in math. The result was an IRT math score with a maximum of 64, and an IRT reading score with a maximum of 92. We can think of each point on the scale as representing the amount learned in two to four weeks of school, since our later estimates suggest that most children gained between 1 and 2.5 points per month when school was in session (Table 5).

The kindergarten reading tests originally contained just 72 questions, and were scored on a 72-point scale. In first grade, though, 20 extra questions were added to the reading test, and the kindergarten scores were rescaled to match the 92-point scale used for the first grade scores. This rescaling involved using IRT to estimate how many of the first grade questions *would* have been answered correctly had they been asked in kindergarten. Such estimates depend on the association

between the first-grade questions and the questions that were asked in both first grade and kindergarten.

Although the approach to rescaling the kindergarten reading tests was sophisticated, we are concerned by the implicit assumption that some component of kindergarten reading skill can be inferred from questions that were only asked in first grade. This assumption appears to run the risk of overestimating kindergarten skill by equating it to first grade reading skill—or, equivalently, of underestimating the amount learned in the summer between kindergarten and first grade.

Consistent with this theoretical concern, we did find some empirical evidence that summer reading gains may have been underestimated between kindergarten and first grade.

1. First, the average summer reading gain was not significantly different from zero (cf. Downey, von Hippel, and Broh 2004). This is just what we would expect if the estimates inappropriately equated kindergarten and first-grade reading skill.
2. Second, the estimates of summer reading gains were lower than the estimates for summer math gains. This runs counter to the usual finding that summer vacation is more detrimental to math skills than to reading (Cooper et al. 1996).
3. Third, even in year-round schools, estimated reading gains were slower during the summer than they were during fall, winter, and spring (Table 5a). This does not make sense, since year-round schools offer instruction at a more or less even rate across all four seasons. By contrast, on the mathematics test, which was not rescaled, summer and non-summer learning rates were very similar in year-round schools (Table 5b).

In short, we are concerned that rescaling may have hurt the estimates of summer reading gains, and therefore hurt the estimates of reading gains over periods that include the summer—such as the twelve-month period between the beginning of kindergarten and the beginning of first grade.

Another reason to be concerned about the reading scores is that reading tests were not given to students who lacked proficiency in English. This is especially concerning because English proficiency was not equally distributed across year-round and nine-month schools. Hispanic students were more likely to lack English proficiency, especially at the beginning of kindergarten, and Hispanic students were also more likely to enroll in year-round schools. More specifically, in the fall of kindergarten 37% of Hispanic students did not take the reading test, vs. 10% of the black and white students.¹⁵ Meanwhile, 53% of the students in year-round schools were Hispanic, vs. just 16% of the students in nine-month schools.

Because of our concerns about the reading tests, we de-emphasize our reading results in favor of the results for mathematics. The mathematics tests are less problematic because (1) the mathematics tests were not rescaled, and (2) Hispanic students were no more likely than other ethnic groups to miss the mathematics tests. Note that our decision to de-emphasize the reading tests does not affect the conclusion of the paper. Although some of the patterns were different for reading than for mathematics, the conclusion that year-round schools do not increase average learning was consistent.

Not every child was tested on every occasion, and the number of test scores was especially low in the fall of first grade, when only a random 30% subsample of schools was visited for testing (Table 4). The undersampling of schools in the fall of first grade is unfortunate, since that test is crucial for estimating the summer learning rate. But even though our power to estimate summer learning is limited, our summer-learning estimates are still valuable since no prior study of year-round schools has been able to estimate summer learning at all.

It would be nice if we could compensate for the missing test scores using multiple imputation. Unfortunately, though, test scores are our dependent variable (Y), and although imputations of Y can

¹⁵ 33% of the Asian students missed the first kindergarten reading test as well. Unlike the Hispanic students, though, the Asian students were just as likely to miss the first kindergarten math test.

be used in analysis, they do not add any information to the regression estimates (Little 1992); under most circumstances, in fact, imputed *Ys* make the estimates worse (von Hippel 2007). In our analyses, therefore, we used observed *Ys* to impute missing independent variables (*Xs*), but we deleted imputed *Ys* before fitting our models. This approach to missing data is known as *multiple imputation, then deletion*, or MID (von Hippel 2007). Further details on the imputation model are given in the Appendix.

Average test scores and test dates

Average scores for each of the four test occasions are given in Table 4. On every test occasion, the average score in year-round schools was one to four points lower in year-round schools than in nine-month schools. In mathematics, the gap was about the same on the last test as on the first, but in reading the gap seems to have grown over time.

Comparing scores between year-round and nine-month schools, though, requires more than a comparison of means. Not only do nine-month and year-round schools differ in their institutional and demographic characteristics, but there are also some tricky issues related to the timing of school years and tests.

First, each school took tests on a different date.¹⁶ In the fall of kindergarten, for example, 90% of children took the tests in October or November, but 10% took them as early as September or as late as December. The difference in school calendars was confounded with the difference in test dates; in the fall of kindergarten, children in year-round schools took their tests an average of three weeks before children in nine-month schools (Table 4).

Even if all schools had been tested on the same dates, their scores would not be comparable because year-round schools start earlier and end later than nine month schools. On average, year-round

¹⁶ Within schools, different children took the tests on different dates, but the within-school differences were trivial. Typically, children from the same school differed by no more than a day or two in their test dates.

schools start about five weeks before nine-month schools (Table 1), which gives year-round schools a head start in learning material for their fall tests. Nine-month schools, by contrast, have an advantage on the spring tests, since nine-month students finish their 180 or so days of instruction two or three weeks before year-round students do (Table 1).

Furthermore, even if each school was tested on the first and last day of its own school year, year-round and nine-month scores would *still* not be comparable, because their students wouldn't be the same age. As we remarked earlier, on the first day of year-round kindergarten, children in year-round schools are eleven weeks younger, on average, than children in nine-month schools are on the first day of nine-month kindergarten (Table 3). As we will see later, a one-month difference in initial age is associated with about a one-third point difference in initial test scores (Table 6), so an eleven-week difference in age corresponds to about a point's difference in achievement.

To adjust for these differences, our models of achievement and learning will include controls for students' age and for the days elapsed between each test date and the first and last day of each school year.

Basic model

To compare learning rates across nine-month and year-round schools, we fit a multilevel growth model (Raudenbush and Bryk 2002, chapter 6) to the test dates and test scores in Table 4. Our model answered Cooper et al.'s (2003) concern and "explicitly controlled for the number of days that school had been in session before achievement outcomes were measured." That is, the model made adjustments for the difference between the test dates in Table 4 and the first and last dates of the school years in Table 1; in a sense, the model extrapolated back from the test dates to estimate the scores that *would* have been received had the tests been given on the first and last day of the school year. (For a detailed specification of our multilevel growth model, see the Appendix.)

The model estimated monthly learning rates over three successive periods. These periods are almost the same in year-round schools as in nine-month schools. In nine-month schools, the periods are (1) kindergarten, (2) summer vacation, and (3) first grade—which have slightly different start and end dates in each school. In year-round schools, the three periods are (1) August 26, 1998 to June 6, 1999, (2) June 7 to August 24, 1999, and (3) August 25, 1999 to June 5, 2000—which are the average dates for kindergarten, summer vacation, and first grade in nine-month schools (Table 1). In other words, in year-round schools, we measure how quickly children learn while *other* children—children in nine-month schools—are in kindergarten, on summer vacation, and in first grade.

Table 5 gives estimates from a basic model that simply compares the average learning rates in nine-month and year-round schools. As predicted, during summer the year-round students learned more quickly than the nine-month students, but during the other parts of the year—when nine-month students were in kindergarten and first grade—the year-round students learned more slowly than children in nine-month schools. These differences fit the idea that children learn faster when they spend more time in school. Year-round children spend more time in school during summer, but nine-month students have more school days during fall, winter, and spring.

When nine-month schools are in session, their students don't just learn faster than year-round students—they learn roughly 10-20% faster, which is about the difference we would expect if learning increases with exposure to school. During the nine-month school year, nine-month schools have about 20% more school days per month than do year-round schools: nine-month schools have about 19 school days per month (178 days over 9.3 months), while year-round schools have about 15½ (176 days over 11.3 months). So it is not surprising that, when nine-month schools are in first grade, their students learn reading at a rate of 2.56 points per month, which is .53 points per month, or 21%, faster than students in year-round schools (Table 5b). In mathematics, the difference between year-round and

nine-month schools is smaller, but in the same ballpark: when nine-month schools are in first grade, their students learn mathematics at a rate of 1.55 points per month, which is .14 points per month, or 9%, faster than students in year-round schools (Table 5a).

In mathematics, the summer effects also square with expectations. During summer, the year-round children learned mathematics at an estimated rate of 1.11 points per month, which is about the same as their learning rate during nine-month kindergarten (1.16 points per month) and not significantly slower than their learning rate during nine-month first grade (1.41). It is not surprising that children in year-round schools learn mathematics at such a steady rate; after all, year-round schools are in session all year round. By contrast, nine-month schools are out of session during summer, and it shows; students from nine-month schools learn two or three times slower during summer than during the kindergarten and first grade school years.

In reading, the results also suggest that year-round children learn faster than nine-month children—but none of the children seems to learn as fast as we would expect. According to the estimates, year-round children learned two or three times slower during the summer than during the nine-month kindergarten school year (Table 5b)—which is hard to believe since year-round students were in school all year-round, and didn't exhibit much summer slowdown in mathematics. In nine-month schools, the estimates suggest that children gained no skills at all over the summer (Table 5b)—which is also hard to believe since we would expect to see basic reading skills reinforced at home. In fact, children *must* learn some reading skills at home, or their average reading score on the first day of kindergarten would be zero, instead of 19 or so (Table 5b). It is hard to imagine why nine-month children would display summer gains in mathematics but not in reading; prior research, in fact, has found just the opposite, suggesting that “summer break [is typically] more detrimental for math than for reading” (Cooper et al. 1996, p. 227).

In short, then, summer reading gains seem disconcertingly small compared to summer math gains. We have not found a convincing substantive reason, and it may be that the true reason comes from inadequacies in the data. As we mentioned earlier, ECLS-K statisticians added points to the kindergarten reading scores, and it may be that they added too many. If they did add too many points to the kindergarten reading scores, that could explain why summer reading gains, between kindergarten and first grade, look too small.

Although many of the reading patterns make sense—for example, it makes sense that summer reading gains are larger in year-round schools than in nine-month schools—we have some reservations about results that involve comparing reading scores between kindergarten and first grade. On the whole, the mathematics scores, which were not rescaled between kindergarten and first grade, may provide a more trustworthy estimate of summer learning than do the reading scores.

Figure 1a plots average mathematics gains in nine-month and year-round schools. The learning trajectory for nine-month children flattens considerably during the summer, while the trajectory for year-round children climbs steadily all year round. During the summer, year-round children gain much more than nine-month children, while during the school year, year-round children gain slightly less.

The lower frame of Figure 1a shows how the achievement gap between year-round and nine-month children waxes and wanes over the twenty-odd months following the beginning of kindergarten. The year-round children close part of the gap during summer, but the nine-month children pull away again when their schools are in session. At the start of kindergarten, the nine-month children are already a point-and-a-half ahead of the year-round children, and they double that lead during kindergarten. During the summer after kindergarten, year-round children gain back the point-and-a-half that they lost during kindergarten, but during first grade they lose it again. Although we do

not have estimates for the summer after first grade, it would not be surprising if year-round children again gained back the ground that they lost during the first-grade school year.

Figure 1b plots the results for reading. The reading patterns are generally similar to the results for mathematics, but there are two departures. First, in both year-round and nine-month schools, it appears that learning slows dramatically during the summer. As we mentioned earlier, though, this could be an artifact since the reading scores were rescaled in a way that could lead to underestimating summer gains. The second difference between the reading and math results is that the lead of nine-month schools grows more quickly during first grade than it did during kindergarten. We don't have an explanation for this pattern—but it certainly does not suggest an advantage for year-round schooling.

Because year-round and nine-month schools distribute learning differently across the calendar year, the only fair way to compare them is to look at *total gains over a twelve-month period*. Over any twelve-month period, year-round and nine-month schools have about the same number of school days (Table 1) and about the same number of days off.

Table 5 estimates twelve-month gains for two overlapping periods. The first period runs from the beginning of nine-month kindergarten to the beginning of nine-month first grade (August 26, 1998 to August 25, 1999), and the second period runs from the end of nine-month kindergarten to the end of nine-month first grade (June 6, 1999 to June 5, 2000). These twelve-month gains are estimated as a weighted sum of the monthly learning rates. For example, for the twelve months starting on the first day of nine-month kindergarten (August 26, 1998), the twelve-month gain is estimated as 9.34 months times the kindergarten learning rate plus 2.66 months times the summer learning rate.

In mathematics, the twelve-month gains are very similar in nine-month and year-round schools. Over the twelve months starting August 26, 1998, both year-round and nine-month children gained a

little less than 14 points, and over the twelve months starting June 6, 1999, both year-round and nine-month children gained about 16 points. The estimated differences between nine-month and year-round gains were less than 2%, and nowhere close to statistical significance.

In reading, the results for the twelve months starting August 26, 1998 are similar to the results for mathematics; that is, the year-round and nine-month reading gains differ by an insignificant amount estimated at less than 2%. But for the twelve months starting June 7, 1999, children in nine-month schools gained significantly more—12% more—than children in year-round schools. On the face of it, this finding suggests that year-round schools may reduce annual reading gains, but, again, we place limited trust in this result since we have doubts about the summer estimates for reading.

In sum, it appears that year-round schools do little or nothing to increase annual achievement gains. Among the four twelve-month differences that we examined, only one difference was substantial and significant—and that difference favored the traditional nine-month calendar.

Fuller model

Although the results of our basic analyses are suggestive, they are not convincing because they do not control for confounding differences. To make a fairer comparison, we now fit a fuller model that includes controls for the demographic and institutional differences between nine-month and year-round schools.

At the school level, this fuller model controls for crowding, for school sector (public vs. private), and for geography (Western vs. non-Western; rural vs. suburban vs. urban). At the child level, the model controls for ethnicity (white, black, Hispanic, Asian, other) and for SES, which the ECLS-K defines as a composite of parental education, parents' occupational status, and household income.

In addition, we included two variables that affect exposure to school and non-school environments. At the school level we controlled for the difference between half- and full-day

kindergarten, while at the child level, we controlled for age at the start of nine-month kindergarten (August 26, 1998).¹⁷ In an alternate version of the model, we also controlled for summer school attendance, but in the version displayed in Table 6 we simply omitted any tests that were taken after a child attended summer school. The results of these two approaches were about the same.

Variables were coded so that the reference group contained the types of children and schools that are most likely to follow a year-round calendar—that is, the reference group young Hispanics of lower-than-average SES, who attended crowded public schools with half-day kindergarten programs in Western cities. We highlighted this reference group by centering continuous variables (crowding, SES, age) around the mean values for year-round schools, and by coding dummy variables so that the reference categories were Hispanic children, public schools, urban schools, Western schools, and schools offering half-day kindergarten.

The effects of the control variables are about what we would expect from past research (Table 6). Older children, children of high SES, and children in private or suburban schools all had high initial test scores, while children in rural schools scored relatively low. Every non-Hispanic ethnic group—including blacks, whites, and Asian-Americans—had higher initial test scores than Hispanic children. In reading, the gap between high- and low-SES children opened faster during the summer than during the kindergarten or first grade school years, but in mathematics this seasonal pattern was less pronounced (cf. Entwisle and Alexander 1992; Downey, von Hippel, and Broh 2004). Net of SES, the gap between black and non-black children grew faster during the school year than during summer vacation (cf. Entwisle and Alexander 1994; Downey, von Hippel, and Broh 2004). Net of demographic differences, kindergarten learning was somewhat faster in schools offering full-day rather than half-day kindergarten programs (cf. Lee et al. 2006).

¹⁷ As we remark a little later, age was mean-centered, so it does not actually matter on which day we calculate age—provided we use the same day for every child.

In this fuller model with its controls for confounding variables (Table 6), the estimated effects of year-round school were very similar to what they were in the basic model (Table 5). Again, the results suggested that, compared to nine-month schools, year-round schools had faster average learning rates during summer, but slower average learning rates during the rest of the year. As in the basic model, not all the summer and school-year differences were significant, but the direction of the differences consistently favored year-round schools in the summer, and favored nine-month schools the rest of the year. In addition, the size of the year-round differences seemed plausible, and very similar to the effect sizes in the basic model (Table 1).

Again, the crucial question is how much children learned over a twelve-month period. And again, in mathematics, the twelve-month gains were very similar in nine-month and year-round schools. Over the twelve months starting August 26, 1998, both year-round and nine-month children gained, net of the control variables, almost 13 points, and over the twelve months starting June 6, 1999, both year-round and nine-month children gained almost 17 points. Again, the estimated differences between nine-month and year-round gains were less than 2%, and nowhere close to statistical significance.

In reading, the results for the twelve months starting August 26, 1998 were again similar to the results for mathematics; that is, the year-round and nine-month reading gains differ by an insignificant amount estimated at less than 2%. But again, for the twelve months starting June 7, 1999, children in nine-month schools gained significantly more—11% more—than children in year-round schools. Notwithstanding our reservations about estimates of summer reading gains, none of these results suggests that year-round calendars have any benefit.

We could fit this model with a different set of control variables—and we have—but the key question is whether the variables in Table 6 are sufficient to compensate for important differences

between nine-month and year-round schools. In most data sets, we never know whether we have enough control variables, but the ECLS-K data have a feature that gives us some idea when a model is adequately controlled. Specifically, the ECLS-K tells us children's achievement levels at the beginning of kindergarten, before any school variable has had a chance to make its mark. If we have enough control variables to explain away the start-of-kindergarten gap between year-round and nine-month schools, then we may well have enough control variables to accurately estimate the later effect of year-round schooling.

If we want to estimate the start-of-kindergarten achievement gap, one possibility is to look at scores from the very first test. But the first test is already too late for the comparison; in most schools, it was given in October or November 1998, when students had already been attending kindergarten for a couple of months.

Instead of actual scores for the first test, we could use the model's estimates of the scores that *would* have been earned had tests been given on the average starting date for nine-month kindergarten—that is, on August 26, 1998. But this isn't good enough, either: although nine-month children were untouched by school on this date, children in year-round schools had already been in school for over a month (Table 1).

To compare children's before-school achievement levels, then, we have to go back even further—to the start of *year-round* kindergarten, which on average began July 18, 1998. On that date, neither year-round nor nine-month children had any exposure to school.

In year-round schools, estimating achievement scores as of July 18, 1998, is straightforward. Year-round students are in kindergarten between July 18 and August 26, so all we have to do is take their kindergarten learning rate (e.g., 1.09 points per month in mathematics), multiply it by the months elapsed between July 18 and August 26 (1.28 months), and subtract the result from year-round

children's average August 26 math score (14.26). This calculation yields an estimated July 18 score of 12.86 in mathematics, and 14.84 in reading.

In nine-month schools, estimating July 18, 1998 achievement scores is a little harder, since it requires information on how quickly children learn between July 18 and August 26—that is, during the month and a half before the start of kindergarten. Fortunately, we can estimate this pre-kindergarten learning rate using the effect of age on initial test scores (Table 6). On the first day of nine-month kindergarten (August 26, 1998), an extra month in age predicts an extra 0.38 points in mathematics and an extra 0.32 points in reading.¹⁸ This suggests that, in the months before kindergarten, children are learning at an adjusted mean rate of about 0.38 points per month in mathematics, and 0.32 points per month in reading.¹⁹

Now, to estimate July 18, 1998, achievement scores for children about to enter nine-month schools, all we have to do is take the average learning rate for the months before kindergarten (e.g., 0.38 points per month in mathematics), multiply this rate by the months elapsed between July 18 and the start of nine-month kindergarten on August 26 (1.28 months), then subtract the product from the adjusted start-of-kindergarten achievement level for children in nine-month schools (13.62). In mathematics, this calculation yields a July 18 score of 13.13; in reading, it yields an average July 18 score of 15.15.

In sum, on July 18, 1998—before kindergarten had started in either year-round or nine-month schools—we estimate that children who were starting year-round schools would have scored, on

¹⁸ This is the effect for children in nine-month schools. In year-round schools, the effect of age is about the same, since the interaction between age and year-round schooling is small and insignificant (Table 6).

¹⁹ We would expect the before-kindergarten learning rate to be comparable to the summer-vacation learning rate, since children are out of school during both periods. In mathematics, these rates are roughly comparable; the before-kindergarten rate is 0.38 points per month, while the summer vacation rate is 0.67 points per month. But in reading, the before-kindergarten learning rate of 0.32 points per month is much faster than the summer vacation rate; in fact, the summer vacation rate of -0.03 points per month suggests that children do not learn reading at all during summer vacation. As we have already suggested, though, summer reading gains may be underestimated due to measurement artifacts in the ECLS-K.

average, just 0.27 points lower in mathematics and 0.31 points lower in reading than would comparable children who were waiting to enter nine-month schools (Table 6). These differences are nonsignificant and quite small²⁰, which suggests that our full model does a reasonably thorough job of controlling preexisting differences between the children who attend nine-month schools and the children who attend year-round schools. It may be that the differences could be controlled more completely, but any improvement would be small and hard to demonstrate given the limited precision of the statistical estimates. In addition, with only 27 year-round schools in the sample, we would be uncomfortable including more than the half-dozen school-level control variables that are already present in the model.

Figure 2 plots average learning rates in year-round and nine-month schools. Again the patterns suggest that during the summer, year-round schools pull ahead, but during the rest of the year, nine-month schools make up the lost ground. Aside from having a different start date—July 18 instead of August 26, 1998—Figure 2 looks very similar to Figure 1. The main difference is that Figure 2’s use of control variables has pulled the year-round and nine-month trajectories much closer together than they were in Figure 1.

Our ability to estimate scores and learning rates starting on July 18, 1998 raises the possibility of estimating gains over an additional twelve-month period. In addition to estimating twelve-month gains starting on August 26, 1998, or June 7, 1999, we can estimate gains for the twelve-month period starting on July 18, 1998. A nice property of this twelve-month period is that, since it ends in mid-summer, it makes relatively little use of summer learning estimates. This is a useful property since, as we have said, we don’t quite trust our estimates of summer reading gains. Following our strategy for

²⁰ A 0.3 point gap is equivalent to missing about a week of school, or to being less than a month younger than average when school begins.

previous twelve-month estimates, we estimate the gains for July 18, 1998-July 18, 1999 by adding up the gains for shorter periods. Details of the calculation are provided in the footnote to Table 6.

Over the twelve months starting on July 18, 1998, we do not find significant differences between the gains in year-round schools and nine-month schools. Specifically, we estimate that, compared to similar children from nine-month schools, children from year-round schools had expected twelve-month gains that were just 0.50 points higher in mathematics and 0.96 points higher in reading. Neither of these differences is close to significant.

In sum, we have now estimated gains in two subjects over three overlapping twelve-month periods. Among these six comparisons, half favored year-round schools, and half favored nine-month schools. Five of the differences were nonsignificant, and the sixth—the largest and only significant difference—favored nine-month schools. Averaging across the six comparisons, we estimate that annual gains were less than 1% larger in year-round than in nine-month schools—an absolutely trivial difference.

Do Year-Round Schools At Least Help the Disadvantaged?

Past research has suggested that, even if year-round schools don't help most students, they can help students from especially poor families (McMillen 2001, Cooper et al. 2003). This suggestion seems plausible at first, because year-round calendars increase summer learning, and summer is when poor children do most of their falling behind (Alexander, Entwisle, and Olson 2007). We can see poor children's summer setback in Table 6a, where the reading gap between high- and low-SES children widens faster during the summer than during the kindergarten or first grade school years. On the other hand, in mathematics, Table 6b's evidence for summer setback is less pronounced.

Since year-round schools replace most of summer vacation with regular school instruction, it makes sense that year-round schools would reduce summer learning gaps between children of high

and low socioeconomic status (SES). During summer, year-round schools reduce children's exposure to their home environments—and it is at home that poor children experience their greatest disadvantages (Lareau 2000; Linver, Brooks-Dunn, and Kohen 1999, 2002).

But again, the summer benefits of year-round schools may come with a cost. As we pointed out earlier, year-round schools do not just add days of instruction to the summer; they also *subtract* days of instruction from the rest of the year. So although year-round schools reduce poor children's time at home during summer, they increase home exposure during fall, winter, and spring.

On balance we might predict that, though year-round schools reduce the growth of achievement gaps during the summer, they accelerate the growth of achievement gaps during the school year. To test this prediction, we follow past research by including in our model an interaction between year-round schooling and child-level SES (cf. McMillen 2001, Cooper et al. 2003). If year-round schools reduce the summer growth in SES achievement gaps, we would expect the summer to reveal a *negative* interaction between SES and year-round schools—indicating that a *decrease* in SES is associated with an *increase* in the benefits of year-round schools. Conversely, during the regular kindergarten and first grade school years, we would expect a *positive* interaction between year-round schooling and SES. We might also predict that these interactions will be stronger in reading than in mathematics, since it is in reading that the effect of summer vacation on SES achievement gaps is most pronounced (Table 6a vs Table 6b).

The results, in the bottom lines of Table 6a vs Table 6b, are mostly consistent with our predictions. In reading, there is negative and substantial year-round SES X year-round interaction during summer, counteracted by a positive and substantial interaction during nine-month first grade. During nine-month kindergarten, though, the interaction is not positive as we predicted, but slightly negative, though only marginally significant. In mathematics, the seasonal interactions follow the

same negative-negative-positive pattern as in reading, but the effect sizes are much smaller and, for the most part, not significantly different from zero.

When an effect is positive in one season and negative in another, the only fair way to evaluate its net impact is over a full twelve months. Over the twelve months starting August 26, 1998, then, the total effect of the SES X year-round interaction is negative and significant for reading, and negative though nonsignificant for mathematics. And over the twelve-month period starting June 9, 1999, the total effect of the interaction is positive though nonsignificant for reading, and negative and nonsignificant for math.²¹

So do year-round schools have special benefits for low-SES children? The results are equivocal. We have tested the SES X year-round interaction in two subjects over two twelve-month periods. Among those four tests, just one was statistically significant, and that test relied on an estimate of summer reading gains in which we are not entirely confident. Nevertheless, it is fair to say that the results lean just slightly toward the view that year-round schools constrain the growth of the reading achievement gap between high- and low-SES children.

Discussion

Our results fit the view that year-round schools do little to increase or decrease average achievement gains—instead, they just distribute gains more evenly across the year. The results suggest that neither year-round nor nine-month calendars have a clear advantage over the other. Any advantage of year-round schools, if it exists, is elusive, probably small, and may be confined to children whose socioeconomic status is low even for the year-round population.

²¹ We did not estimate the effect of the interaction over the twelve months starting on July 18, 1998. To do so, we would have had to use students' age, and this implies a three-way interaction between age, SES, and year-round schools—actually a four-way interaction when you consider that multilevel growth models work by letting student and school characteristics interact with time (see the Appendix). When estimating a three- or four-way interaction involving mostly continuous variables, it is hard to have much confidence in the results since the risk of misspecification is so great.

The data we used to address this question are not perfect, though they are the best data we have. Just 27 year-round schools participated in the ECLS-K survey, and just 7 took the first grade autumn test needed to estimate summer learning rates. Because the sample of year-round schools was small, our estimates of summer learning in year-round schools are imprecise, with wide confidence intervals, and some of this imprecision seeps into our estimates of twelve-month gains. In addition, reading tests were rescaled between the kindergarten and first grade school year, and that rescaling raises doubts about comparisons that bridge these two years.

A further concern is that the ECLS-K did not give tests at the very beginning and end of the school year. Fall tests were typically given in mid-October, and we have to extrapolate backward to estimate the scores that would have been obtained at the start of the school year in late August or, for year-round schools, in mid-July. Likewise, spring tests were typically given in early May, and we have to extrapolate forward to estimate the scores that would have been obtained at the end of the school year June. These extrapolations rely on the assumption that learning is approximately linear during the school year—that is, that students learn about as fast in September and June as they do between October and May. Linearity may be a serviceable approximation,²² but it is only an approximation, and the true first- and last-day scores probably differ a bit from our estimates.

The findings are limited to elementary school, and more specifically to the kindergarten and first grade school years. This limitation is less severe than it may seem, however, since about three quarters of year-round students are in elementary rather than secondary schools (Cooper et al. 2004). For example, all of the year-round schools in Las Vegas, and all but one of the year-round schools in Chicago, are at the elementary level (Clark County School District Year Round Calendar Study Group 2007; Ihejirika 2007). Middle schools and high schools find it difficult to adopt the year-round calendar, at least in its multitrack form, because it is hard to ensure that all required courses and a

²² The justification for differential calculus is that linear approximations work well over short intervals.

reasonable sampling of electives are available to students on three or four different tracks (Dale Erquiaga²³, personal communication, October 2007). An elementary school, by contrast, where children spend most of the day in a single classroom, is relatively easy to put on a year-round calendar.

It remains the case that the patterns in kindergarten and first grade could be different from those later on. However, if our explanation for our results is correct—if the amount learned depends on the amount of time spent in school, regardless of how that time is arranged—then we can hypothesize that similar results would be obtained for older students. Further research is needed to test this hypothesis.

Against the faults of the ECLS-K, we have to weigh the fact that no other data set can speak to the question of summer learning in year-round schools. It would be nice if the ECLS-K data were better, but until someone collects data that sheds a brighter light on summer learning in year-round schools, the evidence from this study is invaluable because it is unique.

Conclusion

Do year-round schools accelerate summer learning? Yes, they do—but they also *reduce* learning during the rest of the year. Year-round calendars don't schedule any more school days than nine-month calendars, so they can only add school days to summer by subtracting school days from fall, winter, and spring. Because learning increases and decreases with the number of scheduled school days, year-round schools do not really solve the problem of summer setback—they simply spread it out across the year.

Just as year-round schools take the long summer vacation and break it up into several shorter breaks, it may be that year-round schools replace the usual large summer setback with three or four smaller setbacks that happen during the year-round calendar's three- and four-week vacations. Although we cannot observe these micro-setbacks directly, they would explain why the catching up

²³ Dale Erquiaga was the facilitator for the Clark County Public Schools Year Round Calendar Study Group.

that year-round children do during the summer is almost exactly canceled by their falling behind during the rest of the year.

These results are consistent with the sociological view that summer setback comes from cognitive disadvantages in students' homes and neighborhoods (Heyns 1978; Entwisle and Alexander 1992; Downey, von Hippel, and Broh 2004). Non-school disadvantages cannot be erased by a year-round calendar, because a year-round calendar does not reduce the time that children spend outside school, nor does it increase the academic stimulation of children's out-of-school activities.

Although our findings contradict a major argument for year-round calendars, the results need not be taken as a repudiation of year-round schools. Instead, the results are more or less neutral with regard to the academic effects of the year-round calendar: over twelve-month periods, year-round schooling appears neither to increase learning nor to decrease it. On purely academic grounds, we cannot advocate a year-round calendar, but we cannot recommend against it, either. If a school has some non-academic reason to favor a year-round calendar—for example, to cope with overcrowding—it appears that the year-round calendar can be adopted at little or no academic cost. And if a school has already switched to a year-round calendar, we see no reason—at least, no *academic* reason—for the school to switch back. On the other hand, if a school is considering a year-round calendar in hope of boosting academic achievement, it seems unlikely that those hopes will be realized; changing to a year-round calendar may not be worth the disruption that comes with the change.

Finally, the results provide further evidence that academic learning can be increased simply by increasing the number of days that children spend in school. During fall, winter, and spring, nine-month schools have more days of instruction than year-round schools, and children in nine-month schools learn proportionately more. During the summer, by contrast, year-round schools offer dramatically more instruction than nine-month schools, and children in year-round schools learn

dramatically more. It is no great stretch to imagine that a school which increased total days of instruction—instead of merely redistributing days from one part of the year to another—would also increase total learning. This is the premise of extended-year schools, and may be part of the advantage of east Asian schools that offer 200 or more days of instruction per year. Perhaps future attempts to solve the problem of summer learning should focus on extending or supplementing the school year rather than just rearranging it.

Appendix

Analysis Model

We estimated achievement and seasonal learning rates by fitting a multilevel growth model (Raudenbush and Bryk 2002). Specifically, we fit a 3-level model in which test scores (level 1) were nested within children, and children (level 2) were nested within schools (level 3).

At level 1, the dependent variable is the score Y_{ics} on the test given to child c in school s at time t . This test score is modeled as a linear function of the months that have elapsed in the three periods of interest: (1) fall 1998-spring 1999, (2) summer 1999, and (3) fall 1999-spring 2000:

$$Y_{ics} = \alpha_{0cs} + \alpha_{1cs} \text{FALL98SPRING99}_{ics} + \alpha_{2cs} \text{SUMMER99}_{ics} + \alpha_{3cs} \text{FALL99SPRING00}_{ics} + e_{ics} \quad (1)$$

To make comparisons easier, the summer period is defined to be about the same year-round schools as in nine-month schools. In a nine-month school, the summer period begins and ends with the first and last day of summer vacation. In a year-round school, summer vacation is minimal, so the summer period is defined to begin on June 5 and end on August 25—which are the average dates for summer vacation in a nine-month school (Table 1).²⁴ In effect, then, we define year-round children's summer learning as the amount that they learn while *other* schools are on summer vacation.

In equation (1), the slopes α_{1cs} , α_{2cs} , and α_{3cs} represent monthly rates of learning during the three periods of interest. The intercept α_{0cs} represents the child's achievement level on the first day of kindergarten (i.e., the first day of FALL98SPRING99). This initial achievement level is not the same as the first test score, because the first test was not given until two months after the start of kindergarten; instead, the intercept α_{0cs} is an extrapolation to the score that the child *would* have received had the

²⁴ The results do not change much if we move these dates by a week or two.

test been given on the first day of kindergarten. The residual term e_{ics} is measurement error—the difference between the test score Y_{ics} and the child’s true achievement level at the time of the test. The variance of this measurement error can be calculated from test-reliability estimates in Rock and Pollack (2002). For example, the first math test was 92% reliable and the total variance of the test scores was about 50; this implies that the variance of the measurement error was 8% of 50, or 4. Likewise, the error variances on the second, third, and fourth math test were 4.6, 5.5, and 5.4, and the error variances on the four reading tests were 5.2, 5.9, 6.4, and 6.0. It is helpful to know these error variances, since by incorporating them into our modeling process, we can separate random variation in measurement error from true variation in achievement and learning.

At levels 2 and 3 of the model, we model variation in achievement and learning rates by including child- and school-level random effects. The level 2 and 3 models can be written in one step, as follows:

$$\alpha_{0cs} = \gamma_{00} + b_{0s} + a_{0c} \tag{2}$$

$$\alpha_{1cs} = \gamma_{10} + b_{1s} + a_{1c}$$

$$\alpha_{2cs} = \gamma_{20} + b_{2s} + a_{2c}$$

$$\alpha_{3cs} = \gamma_{30} + b_{3s} + a_{3c}$$

Here the dependent variables α_{0cs} , α_{1cs} , α_{2cs} , and α_{3cs} are the child-level parameters from equation (1), which represent the initial achievement level and subsequent learning rates for child c in school s . The fixed effects γ_{00} , γ_{10} , γ_{20} , and γ_{30} represent the grand average levels of achievement and learning; the school-level random effects b_{0s} , b_{1s} , b_{2s} , and b_{3s} represent the departure of school s from the grand

average; and the child-level random effects a_{0s} , a_{1s} , a_{2s} , and a_{3s} represent the departure of child c from the average for school s .²⁵

The model may be expanded to include covariates X_1, X_2, \dots :

$$\alpha_{0cs} = \gamma_{00} + \gamma_{01} X_1 + \gamma_{02} X_2 + \dots + b_{0s} + a_{0c} \quad (3),$$

$$\alpha_{1cs} = \gamma_{10} + \gamma_{11} X_1 + \gamma_{12} X_2 + \dots + b_{1s} + a_{1c}$$

$$\alpha_{2cs} = \gamma_{20} + \gamma_{21} X_1 + \gamma_{22} X_2 + \dots + b_{2s} + a_{2c}$$

$$\alpha_{3cs} = \gamma_{30} + \gamma_{31} X_1 + \gamma_{32} X_2 + \dots + b_{3s} + a_{3c}$$

For example, X_1 may indicate whether the school follows a year-round calendar ($X_1=1$ if it does, $X_1=0$ if it doesn't), and X_2 may be a control variable representing some other quality of school s or child c .

In this expanded form of the model, the fixed-effect intercepts γ_{00} , γ_{10} , γ_{20} , and γ_{30} represent the average achievement level and learning rate for a child with covariate values of zero ($X_1=X_2=\dots=0$).

The fixed-effect slopes γ_{01} , γ_{12} , ... represent the change in average achievement level or learning rates associated with a one unit change in each covariate.

The different levels of the model (equations (1) and (3)) may be combined to represent the model in a single “mixed” equation:

$$\begin{aligned} Y_{ics} = & (\gamma_{00} + \gamma_{01} X_1 + \gamma_{02} X_2 + \dots + b_{0s} + a_{0c}) \\ & + \text{FALL98SPRING99}_{ics} (\gamma_{10} + \gamma_{11} X_1 + \gamma_{12} X_2 + \dots + b_{1s} + a_{1c}) \\ & + \text{SUMMER99}_{ics} (\gamma_{20} + \gamma_{21} X_1 + \gamma_{22} X_2 + \dots + b_{2s} + a_{2c}) \\ & + \text{FALL99SPRING00}_{ics} (\gamma_{30} + \gamma_{31} X_1 + \gamma_{32} X_2 + \dots + b_{3s} + a_{3c}) \end{aligned} \quad (4)$$

²⁵ The school-level random effects are assumed to be correlated among themselves, and so are the child-level random effects, but random effects at the school level are assumed to be independent of those at the child level. Both the school- and child-level random effects are assumed to have a normal distribution.

which shows how differences in learning rates are modeled by letting the covariates (X_1, X_2, \dots) interact with the time variables (FALL98SPRING99_{ics}, SUMMER99_{ics}, FALL99SPRING00_{ics})

Imputation Model

We compensated for missing values using a multiple-imputation strategy (Rubin 1987) that filled in each missing value with ten plausible imputations. The imputation model assumed that the missing and observed values followed a multivariate normal distribution. Although some of the variables were dummy variables, dummy variables can be imputed under a normal model without introducing bias (Allison 2005; Horton et al. 2003). To account for correlations among tests on the same child, the data were formatted so that each child's test scores appeared on a single line alongside the other variables (Allison 2002). To account for the interactions in equation (4), we multiplied the component variables before imputation and imputed the resulting products like any other variable (Allison 2002).²⁶ To account for the difference between child- and school-level variables, we first created a school-level file that included school averages of the child and test variables. We imputed this school file ten times; merged the imputed school files with the child and test file; then imputed the child and test variables conditionally on the school variables.

Although our imputation model included test scores, none of the imputed test scores was used in the analysis. Excluding imputations of the dependent variable is a strategy known as multiple imputation, then deletion (MID), which can substantially increase efficiency when there are many missing values on the dependent variable, as there are here (von Hippel 2007). MID can also reduce the effects of shortcoming in the imputation model (von Hippel 2007). Here, for example, the

²⁶ As is often the case, there was substantial collinearity between the interactions and the component variables. The imputation model compensated for this collinearity by using a ridge prior, as suggested by Schafer (1997).

imputation model could not account for the clustering of children from the same school.²⁷ Since many of the imputed values were deleted, however, this shortcoming mattered less than it otherwise might. Perhaps for this reason, our results changed very little when we tried imputing values in different ways.

²⁷ Schafer and Yucel (2002) describe a method that accounts for clustering in a random-effects framework. Their software for doing this has not been maintained, however, and it assumed that only the dependent variable Y (not the covariates X) had missing values.

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Tables

Table 1. School calendars. Means.

	Nine-month (N=965)	Year-round (N=27)	Difference
Kindergarten school days	178	176	-2.5* days (-4.4,-0.6)
First grade school days	179	177	-1.6* days (-3.0,-0.2)
Kindergarten begins	August 26, 1998	July 18, 1998	-1.30*** months (-1.49,-1.10)
Kindergarten ends	June 6, 1999	June 30, 1999	0.81*** months (0.64,0.98)
First grade begins	August 25, 1999	July 13, 1999	-1.43*** months (-1.64,-1.22)
First grade ends	June 5, 2000	June 23, 2000	0.59*** months (0.40,0.79)

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Parentheses enclose 95% confidence intervals. Means and mean differences are weighted.

Table 2. School characteristics. Means and percentages.

	Nine-month (N=965)	Year-round (N=27)	Difference
Rural	25%	3%	-23%*** (-29%,-17%)
Urban	39%	50%	11% (-10%,32%)
Suburban	36%	48%	12% (-09%,33%)
Western census region	23%	84%	62%*** (46%,77%)
Public	69%	100%	31%*** (27%,34%)
Percent free lunch	7%	7%	0% (-3%,3%)
Percent reduced-price lunch	28%	43%	15%* (-2%,27%)
Percent minority	36%	67%	31%*** (19%,44%)
Half-day kindergarten	44%	86%	42% (29%,55%)
Crowded (1-5)	2.5	3.4	0.9** (0.4,1.5)

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Parentheses enclose 95% confidence intervals. All statistics are weighted.

Table 3. Child characteristics. Means and percentages.

	Nine-month	Year-round	Difference
Hispanic	16%	53%	36%*** (24%,49%)
Asian	6%	11%	5% (-1%,12%)
Black	15%	5%	-9%*** (-13%,-5%)
White	58%	27%	-31%*** (-43%,-2%)
Mixed race	2%	3%	0% (-2%,2%)
Native American/Pacific Islander	3%	2%	-1% (-3%,0%)
SES (standardized)	0.01	-0.44	-0.44** (-0.72,-0.16)
Age on July 18, 1998 (average start of year-round kindergarten)	65.29 months	63.81 months	-1.47*** months (-2.09,-0.86)
Age on August 26, 1998 (average start of nine-month kindergarten)	66.57 months	65.09 months	-1.47*** months (-2.09,-0.86)

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Parentheses enclose 95% confidence intervals. Comparisons account for the clustering of children within schools.

Table 4. Test dates and test scores. Means.

			Kindergarten		First grade	
			Fall	Spring	Fall	Spring
Date		nine-month	October 11, 1998	May 6, 1999	October 13, 1999	May 6, 2000
		Year-round	October 31, 1998	May 3, 1999	October 8, 1999	May 1, 2000
		Difference	-0.66*** months (-0.91,-0.42)	0.11 months (-0.16,0.38)	0.17 months (-0.49,0.82)	0.18† months (-0.03,0.39)
Schools visited		nine-month	897	758	303	789
		Year-round	27	17	7	22
Math	Scores	nine-month	19.91	27.96	32.96	43.55
		Year-round	17.22	25.20	28.83	41.13
		Difference	-2.69*** (-4.09,-1.28)	-2.76** (-4.55,-0.96)	-4.13* (-7.49,-0.78)	-2.41* (-4.41,-0.42)
	Children tested	nine-month	14,852	16,081	4,171	13,859
		Year-round	421	433	96	422
Reading	Scores	nine-month	23.28	33.38	38.87	56.07
		Year-round	21.96	31.79	35.34	51.64
		Difference	-1.32 (-3.14,0.51)	-1.59 (-3.52,0.34)	-3.53† (-7.57,0.51)	-4.44** (-7.14,-1.73)
	Children tested	nine-month	14,136	15,568	4,069	13,652
		Year-round	305	346	70	390

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Parentheses enclose 95% confidence intervals. Comparisons account for the clustering of children within schools.

Table 5. Seasonal learning rates in traditional and year-round schools. Multilevel growth models.

Table 5a. Mathematics

	<i>Achievement,</i>	<i>Monthly learning rates</i>			<i>Twelve-month gains^a</i>	
	start of nine-month kindergarten (8/26/98)	Nine-month kindergarten (8/26/98-6/6/99)	Summer (6/7/99-8/24/99)	Nine-month first grade (8/25/99-6/5/00)	Kindergarten + summer (8/26/98-8/25/99)	Summer + first grade (6/6/99-6/5/00)
Nine-month	16.95*** (16.71,17.20)	1.32*** (1.30,1.34)	0.50*** (0.41,0.58)	1.55*** (1.52,1.58)	13.68*** (13.42,13.95)	15.86*** (15.65,16.08)
Year-round	15.27*** (13.83,16.71)	1.16*** (1.04,1.28)	1.11† (-0.20,2.42)	1.41*** (1.03,1.80)	13.77*** (10.45,17.10)	16.18*** (15.08,17.28)
Difference	-1.68* (-3.15,-0.22)	-0.16** (-0.28,-0.04)	0.61 (-0.72,1.94)	-0.14 (-0.50,0.23)	0.09 (-3.41,3.59)	0.31 (-0.79,1.42)

Table 5b. Reading

	<i>Achievement,</i>	<i>Monthly learning rates</i>			<i>Twelve-month gains^a</i>	
	start of nine-month kindergarten	Nine-month kindergarten	Summer	Nine-month first grade	Kindergarten + summer	Summer + first grade
Nine-month	19.42*** (19.14,19.71)	1.66*** (1.63,1.69)	-0.05 (-0.15,0.06)	2.56*** (2.52,2.61)	15.48*** (15.09,15.87)	23.96*** (23.48,24.44)
Year-round	18.37*** (16.64,20.10)	1.47*** (1.29,1.64)	0.76† (-0.06,1.57)	2.03*** (1.77,2.30)	15.75*** (13.16,18.34)	21.09*** (18.77,23.42)
Difference	-1.05 (-2.81,0.71)	-0.19* (-0.37,-0.01)	0.80† (-0.06,1.66)	-0.53*** (-0.80,-0.26)	0.27 (-2.46,2.99)	-2.87* (-5.52,-0.22)

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Parentheses enclose 95% confidence intervals.

^aEach twelve-month gain is a linear combination of monthly learning rates. The gain from 8/26/98-8/26/99 is 9.34 months times the kindergarten learning rate plus 2.66 months times the summer learning rate. Likewise, the gain from 6/7/99-6/7/2000 is 2.66 months times the summer learning rate plus 9.34 months times the first-grade learning rate.

Table 6. Seasonal learning rates in traditional and year-round schools, with controls for confounding differences

Table 6a. Math

	Achievement		Monthly learning rates			Twelve-month gains		
	Start of year-round kindergarten (7/18/98)	Start of nine-month kindergarten (8/26/98)	Nine-month kindergarten (8/26/98-6/6/99)	Summer (6/7/99-8/24/99)	Nine-month first grade (8/25/99-6/5/00)	8/26/98-8/25/99	6/6/99-6/5/00	7/18/98-7/17/99
Nine-month	13.13*** (12.71,13.55)	13.62*** (13.20,14.04)	1.19*** (1.13,1.25)	0.67*** (0.30,1.03)	1.62*** (1.51,1.73)	12.88*** (11.95,13.81)	16.88*** (16.28,17.47)	12.51*** (11.91,13.11)
Year-round	12.86*** (11.96,13.76)	14.26*** (13.41,15.11)	1.09*** (0.97,1.21)	1.02** (0.27,1.77)	1.49*** (1.23,1.75)	12.92*** (10.87,14.97)	16.66*** (15.47,17.86)	13.01*** (11.58,14.44)
Difference	-0.27 (-1.19,0.65)	0.64 (-0.23,1.52)	-0.10 (-0.22,0.03)	0.35 (-0.42,1.12)	-0.12 (-0.38,0.13)	0.04 (-2.05,2.12)	-0.21 (-1.44,1.01)	0.50 (-0.95,1.95)
<i>Control variables</i>								
<u>School level</u>								
Crowded (1-5)		-0.02 (-0.16,0.11)	-0.01 (-0.02,0.01)	0.04 (-0.05,0.13)	-0.02 (-0.06,0.01)			
Full-day kindergarten		-0.08 (-0.39,0.24)	0.14*** (0.10,0.18)	-0.11 (-0.36,0.14)	-0.09* (-0.17,0.00)			
Private		1.68*** (1.30,2.06)	-0.02 (-0.08,0.03)	0.00 (-0.29,0.29)	-0.12* (-0.23,0.00)			
Non-western		-0.18 (-0.55,0.19)	-0.02 (-0.07,0.03)	-0.19 (-0.43,0.06)	0.11* (0.02,0.21)			
Rural		-0.67*** (-1.07,-0.28)	0.06* (0.00,0.11)	0.11 (-0.17,0.39)	-0.08† (-0.17,0.01)			
Suburban		0.41* (0.08,0.75)	0.02 (-0.03,0.06)	0.12 (-0.17,0.41)	-0.03 (-0.13,0.07)			
<u>Child level</u>								
<i>Ethnicity (Hispanic omitted)</i>								
Asian		2.65*** (2.11,3.18)	0.02 (-0.05,0.08)	0.36* (0.03,0.70)	-0.19*** (-0.30,-0.08)			
Black		0.52* (0.11,0.94)	-0.13*** (-0.19,-0.08)	-0.05 (-0.38,0.29)	-0.09 (-0.20,0.02)			
White		2.22*** (1.89,2.56)	0.06** (0.02,0.10)	0.00 (-0.27,0.28)	-0.02 (-0.11,0.07)			
Other non-Hispanic		0.74** (0.20,1.28)	0.01 (-0.06,0.08)	-0.22 (-0.68,0.24)	-0.02 (-0.15,0.12)			
SES (standardized)		2.28*** (2.15,2.40)	0.06*** (0.05,0.08)	0.06 (-0.03,0.15)	-0.02 (-0.06,0.01)			
Age (in months) on August 26, 1998		0.38*** (0.35,0.40)	0.00 (0.00,0.01)	0.00 (-0.01,0.02)	-0.02*** (-0.02,-0.01)			

Interactions

SES X year-round	0.11 (-0.61,0.82)	-0.08† (-0.17,0.01)	-0.08 (-0.72,0.56)	0.03 (-0.18,0.24)	-0.95 (-2.72,0.81)	-0.21 (-1.44,1.01)
Age X year-round	0.01 (-0.14,0.17)	0.02† (0.00,0.04)	-0.05 (-0.15,0.05)	0.02 (-0.02,0.05)		

Table 6b. Reading

	Achievement		Monthly learning rates			Twelve-month gains		
	Start of year-round kindergarten (7/18/98)	Start of nine-month kindergarten (8/26/98)	Nine-month kindergarten (8/26/98-6/6/99)	Summer (6/7/99-8/24/99)	Nine-month first grade (8/25/99-6/5/00)	8/26/98-8/26/99	6/7/99-6/7/00	7/18/98-7/18/99
Nine-month	15.15*** (14.57,15.72)	15.56*** (14.99,16.14)	1.60*** (1.52,1.68)	-0.03 (-0.35,0.28)	2.51*** (2.37,2.65)	14.85*** (13.80,15.91)	23.32*** (22.18,24.46)	15.31*** (14.47,16.14)
Year-round	14.84*** (13.55,16.13)	16.71*** (15.49,17.93)	1.46*** (1.28,1.65)	0.54 (-0.29,1.37)	2.10*** (1.77,2.43)	15.09*** (12.49,17.68)	21.06*** (18.99,23.12)	16.27*** (14.16,18.38)
Difference	-0.31 (-1.62,1.00)	1.15† (-0.10,2.39)	-0.14 (-0.33,0.05)	0.57 (-0.27,1.41)	-0.40* (-0.75,-0.06)	0.23 (-2.33,2.80)	-2.26* (-4.48,-0.04)	0.96 (-1.14,3.06)
<i>Control variables</i>								
<i>School level</i>								
Crowded (1-5)		-0.04 (-0.21,0.13)	0.00 (-0.03,0.02)	0.04 (-0.06,0.14)	-0.04* (-0.09,0.00)			
Full-day kindergarten		0.23 (-0.19,0.65)	0.25*** (0.18,0.31)	-0.17 (-0.41,0.07)	-0.21** (-0.34,-0.09)			
Private		1.96*** (1.46,2.45)	-0.03 (-0.10,0.05)	0.13 (-0.13,0.39)	-0.02 (-0.14,0.11)			
Non-western		-0.19 (-0.67,0.30)	-0.10** (-0.18,-0.03)	-0.14 (-0.40,0.13)	0.15* (0.02,0.27)			
Rural		-0.99*** (-1.51,-0.48)	0.03 (-0.05,0.11)	0.20 (-0.13,0.54)	-0.07 (-0.22,0.09)			
Suburban		0.61** (0.18,1.04)	0.00 (-0.07,0.07)	0.05 (-0.22,0.32)	0.04 (-0.09,0.16)			
<i>Child level</i>								
<i>Ethnicity (Hispanic omitted)</i>								
Asian		3.01*** (2.33,3.70)	0.10* (0.01,0.18)	0.39† (-0.05,0.83)	-0.05 (-0.22,0.12)			
Black		1.30*** (0.76,1.85)	-0.19*** (-0.26,-0.12)	0.17 (-0.16,0.50)	-0.09 (-0.21,0.04)			
White		1.99*** (1.54,2.44)	-0.02 (-0.08,0.04)	0.01 (-0.26,0.28)	0.10† (-0.02,0.21)			
Other non-Hispanic		0.94** (0.24,1.64)	-0.03 (-0.12,0.06)	-0.10 (-0.50,0.30)	0.07 (-0.09,0.23)			
SES (standardized)		2.77*** (2.61,2.93)	0.08*** (0.06,0.10)	0.18*** (0.09,0.27)	0.04* (0.01,0.08)			
Age (in months) on August 26, 1998		0.32*** (0.29,0.36)	0.00 (0.00,0.00)	0.02* (0.00,0.04)	-0.01*** (-0.02,-0.01)			

Interactions						
SES X year-round	0.20 (-0.83,1.23)	-0.11† (-0.24,0.02)	-0.89* (-1.76,-0.03)	0.37* (0.07,0.66)	-3.41** (-5.92,-0.90)	1.04 (-0.33,2.42)
Age X year-round	0.02 (-0.21,0.26)	0.01 (-0.02,0.04)	0.03 (-0.11,0.18)	-0.01 (-0.06,0.05)		

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Parentheses enclose 95% confidence intervals. † $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$.

Parentheses enclose 95% confidence intervals. All quantitative variables (crowding, SES, age) were centered around the mean values for year-round schools—so that the reference child was an Hispanic with an SES score of -0.44 who was 63.79 months on July 18, 1998, and the reference school is a public school in a Western city with a school-crowding score of 3.4.

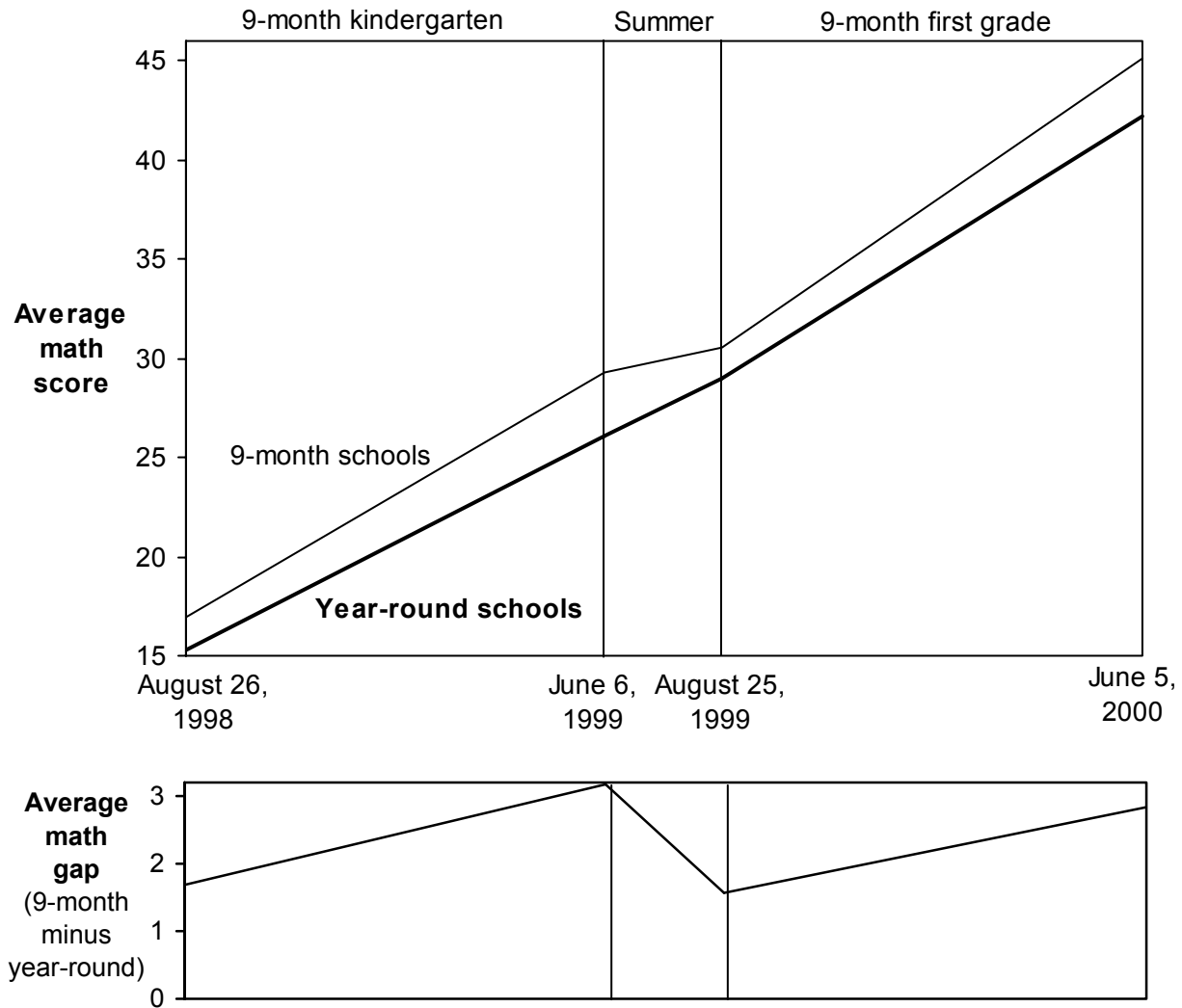
^aThe effect of age on initial achievement may be interpreted as the learning rate before the first day of kindergarten.

^bIn nine-month schools, therefore, the achievement gains from 7/18/98 to 7/18/99 is estimated as 1.28 months times the pre-kindergarten learning rate, plus 9.34 months times the kindergarten learning rate, plus 1.38 months times the summer learning rate. In year-round schools, achievement gains over the same period are estimated as 10.62 months times the learning rate for 8/26/98-6/6/99 plus 1.38 months times the learning rate for 6/6/99-7/18/99.

Figures

Figure 1. Average test scores in year-round and nine-month schools.

1a. Mathematics



1b. Reading

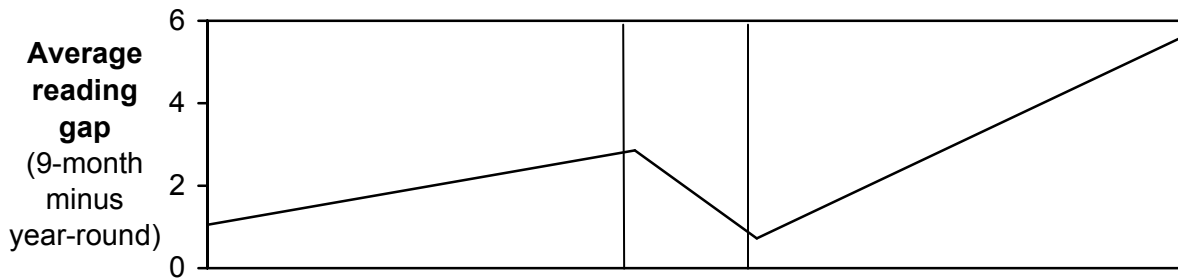
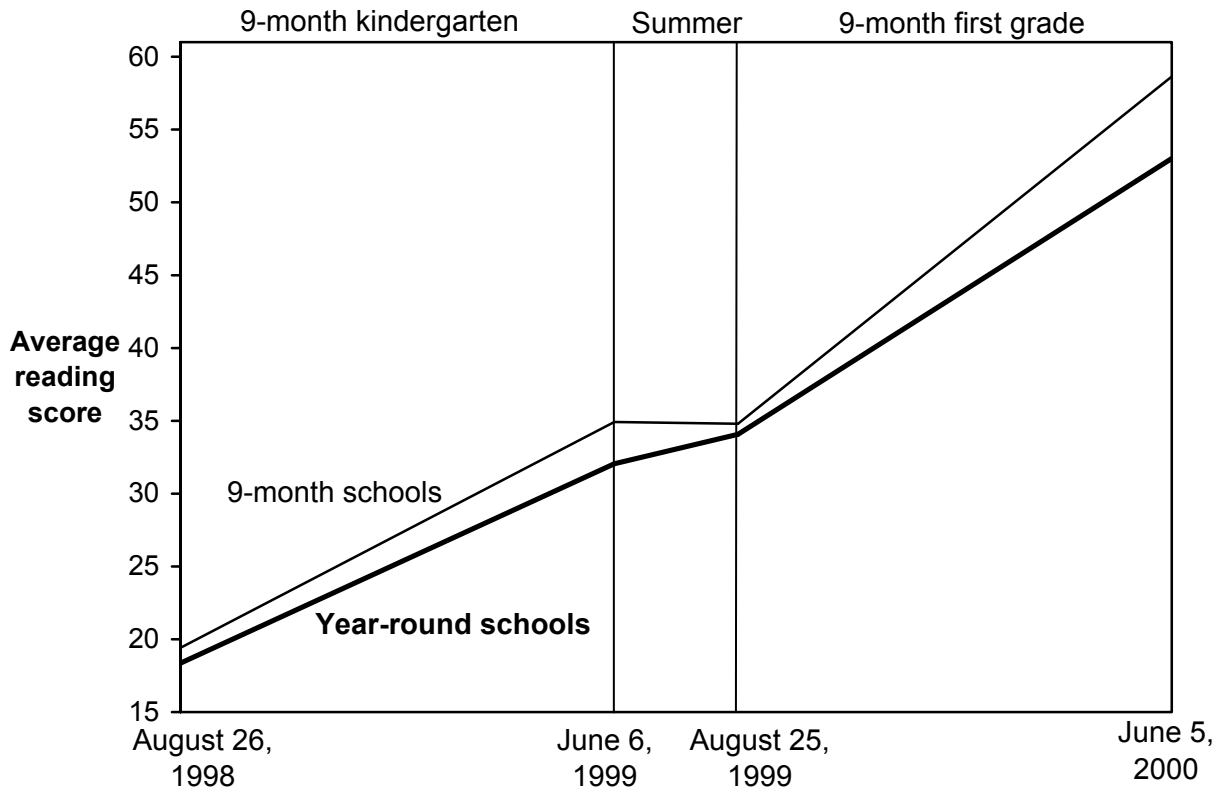
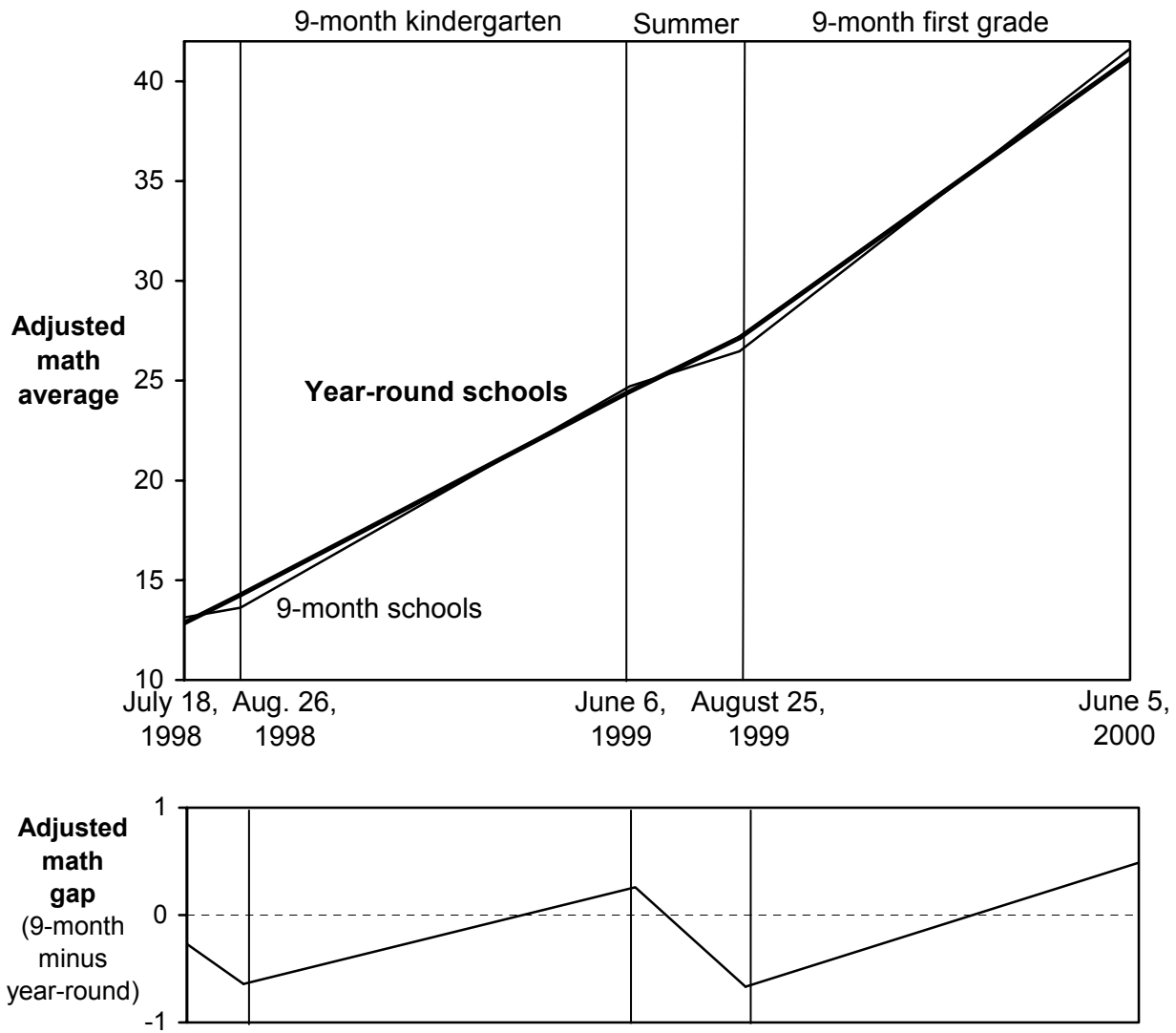
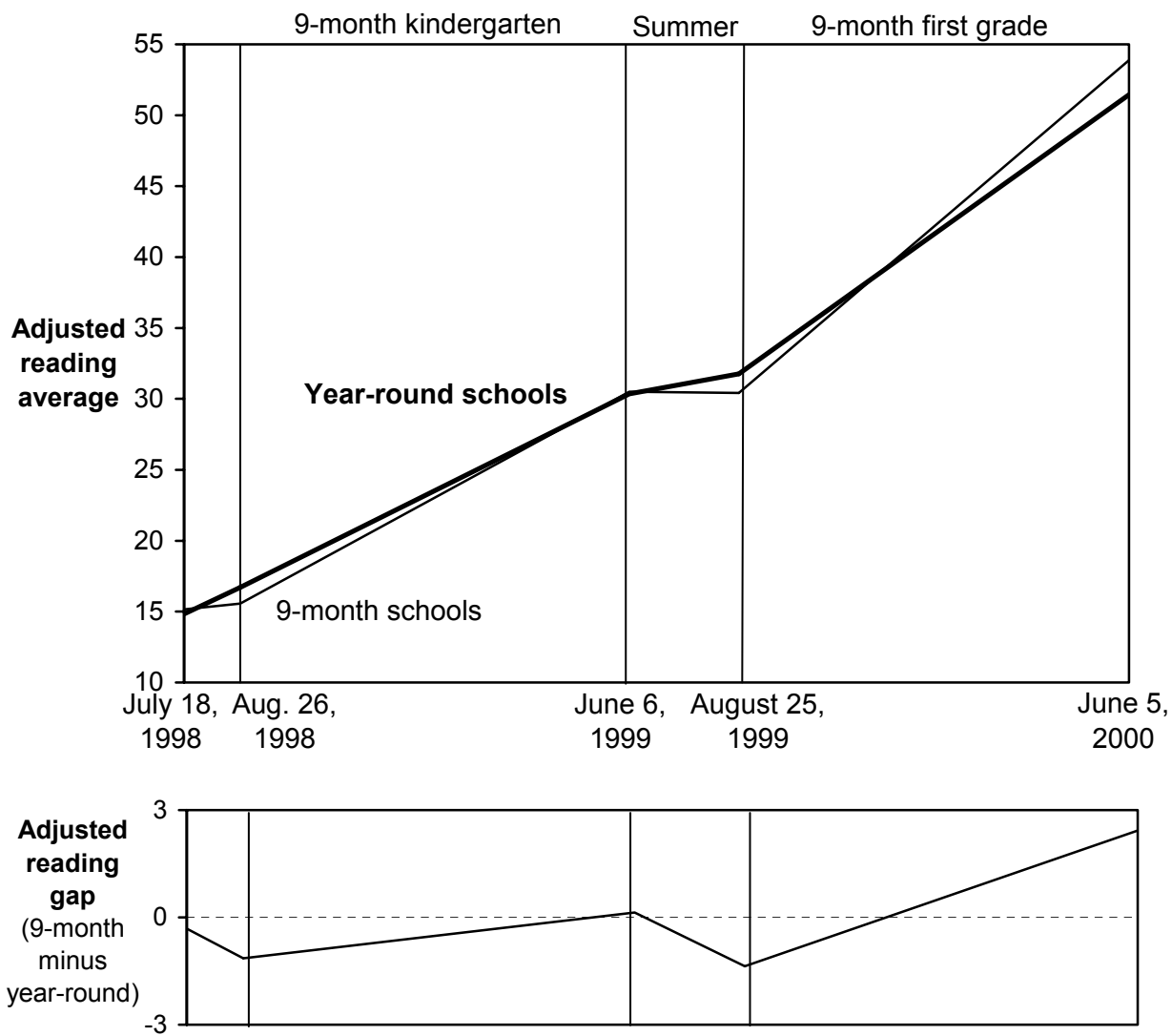


Figure 2. Test scores in year-round and nine-month schools, adjusted for the differences in Table 6.
 2a. Mathematics



2b. Reading



Note. These averages are adjusted to represent schools and children who would typically follow a year-round calendar. The typical year-round student is an Hispanic with an SES score of -0.44 who was 63.79 months old on July 18, 1998. The typical year-round school is a public school in a Western city or suburb with a school-crowding score of 3.4.