The Effects of Modified School Calendars on Student Achievement and on School and Community Attitudes

Harris Cooper and Jeffrey C. Valentine
University of Missouri, Columbia
Kelly Charlton
University of North Carolina, Pembroke
April Melson
University of Missouri, Columbia

This review synthesizes studies of the effects of modifying the academic calendar in Grades K–12 to do away with the long summer break while not increasing the length of the school year. The synthesis indicated that the quality of evidence on modified calendars is poor. Within this weak inferential framework, the average effect size for 39 school districts was quite small, d = .06, favoring modified calendars. Studies that used statistical or matching controls revealed an effect size of d = .11. Modified calendars were associated with higher achievement for economically disadvantaged students. Students, parents, and staffs who participated in modified calendar programs were positive about their experiences. Policymakers can improve acceptance of modified calendars by involving communities in the planning and by providing quality intersession activities.

KEYWORDS: alternative calendar, intersession, modified school calendar, year-round education.

In a 1994 report, the National Education Commission on Time and Learning urged school districts to develop school calendars that acknowledged (a) differences in student learning, and (b) the major changes taking place in American society. The report reflected a growing concern about school calendar issues on the part of local school boards, administrators, and teachers, especially as calendars related to students at risk for academic failure. For example, the long summer vacation that follows the typical school year has been associated with a decline in achievement test scores (Cooper, Nye, Charlton, Lindsay, & Greathouse, 1996) and has been implicated as a major source of the gap in learning between students from different economic backgrounds, due to students' differential access to learning opportunities in the summer (Alexander, Entwisle, & Olson, in press). Also, based primarily on the existence of learning decay over the summer, court decisions have required school districts to provide summer educational opportunities for students with learning disabilities (Public Law 89-10; see Katsiyannis, 1991). Finally, local school districts

often view modified school calendars as a means for responding to enrollment growth, inadequate capital improvement resources, and lifestyle changes of American families (Bradford, 1995).

We conducted a synthesis of the research on modified school calendars, that is, calendars that do away with the long summer break. We used statistical procedures to combine the results of studies that have examined the effects of calendar modifications on student achievement and on the attitudes and satisfaction of participants, including students, parents, teachers, and school administrators and staff. We not only sought to determine the overall effectiveness of modified calendars but also asked whether the effects of calendars differed for different types of communities, students, subject areas, and instructional configurations.

In the pages that follow, we will briefly describe (a) some background material on school calendar issues, (b) the arguments put forth by the proponents and opponents of modified school calendars, and (c) previous reviews of the literature on the effect of modified calendars on achievement test scores. Then we will present the methods and results of our research synthesis and discuss its implications for policy and practice.

Origins of, and Alternatives to, the Traditional School Calendar

In the early years of formal schooling in America, school calendars were designed to fit the needs of each particular community (Gold, 2002). In agricultural areas it was not unusual for children to attend school for only 5 or 6 months of the year, leaving them free to participate in the farming economy. Communities could be found with long summer breaks or with calendars that released children from school in spring, to help with planting, and in fall, to help with the harvest. During the same era, urban schools might be operating on 11- or 12-month schedules.

By the turn of the century, the great migration of families from the farm to the city and the general increase in family mobility created a need for curriculums standardized by grade level and a corresponding standardized amount of time that children were to spend in school. Parents and educators needed to know that children of roughly the same age would be expected to know roughly the same material no matter which area of the country they might be moving to. The present 9-month calendar, with schools closed during summer, emerged as the norm when 85% of Americans were involved in agriculture and when climate control in school buildings was limited. Today, about 3% of Americans' livelihoods are tied to the agricultural cycle, and air-conditioning makes it possible for schools to provide comfortable learning environments year-round (Association of California School Administrators, 1988).

Defining the Alternatives to the Traditional School Calendar

Suggestions for change in the normative school calendar can be grouped into two alternatives. Some proponents of calendar change call for an *extended school year* that increases the number of days that children spend in school. They point out that the United States ranks near the bottom among industrialized nations in the number of days that children attend school (National Center for Education Statistics, 1996). Proponents of greater availability of *summer school* opportunities would fall into this group as well.

The most prominent argument for increasing the number of school days is the potential to increase the amount learned by students. It is also argued that an extended

school year provides a closer fit with the lifestyles of today's American families, which often are headed by a single parent or by two parents with out-of-home employment (Farley, 1996). Those skeptical of extending the school year express concern about cost, teacher and student burnout, and whether increasing the quantity of schooling necessarily translates into increased achievement (Karweit, 1985; Mazzarella, 1984).

Other proponents of calendar change call for arrangements in which children might or might not attend school for more days but the long summer vacation disappears. Children might go to school for 9 weeks and then have 3 weeks off, or go for 12 weeks and have 4 weeks off. These are possible forms of a *modified school calendar*. In this article we focus exclusively on studies of schools that modified their calendars but did not increase the length of their school year.

Modified calendars have been especially popular in school districts where there is a great need for additional schools and classrooms (Shields & Oberg, 2000). For example, under a modified calendar children can be placed in alternating vacation sequences. Thus some students can be on vacation at any given time while the building is in use year-round. This arrangement increases the number of students that a particular school facility can accommodate. The strategy is called *multi-tracking*. When all children in a school are on the same modified calendar, it is called *single-tracking*. Single-tracking typically is adopted because the implementers believe that it has educational benefits or is good for families. In many instances, schools that have adopted multi-tracked modified calendars return to single-tracked modified calendars when the space problem subsides.

Current Use of Modified Calendars in the United States

In school year 2000–2001, more than 2.16 million students in 45 states attended more than three thousand schools that operated without the long summer break (National Association for Year-Round Education [NAYRE], 2000). About three-quarters of those students attended elementary schools, with the remainder equally divided between middle and high schools. The 2000–2001 totals represent an increase of about 3.8% in students over the previous school year. The state with the largest number of students in modified calendar schools was California, followed by Hawaii, Arizona, Nevada, and Texas. It is not irrelevant that these states were among those experiencing the largest population growth rates in the country. Approximately 40% of schools using modified calendars were on a multi-track system. Approximately 30% of schools on modified calendars used a "9 weeks on, 3 weeks off" configuration, and another 30% used a "12 weeks on, 4 weeks off" configuration. The shortest cycle in use was "5 weeks on, 1 week off"; the longest cycle was "18 weeks on, 6 weeks off." The U.S. elementary school with the longest continuously running modified calendar is located outside St. Louis, Missouri, in the Frances Howell School District. It has been in operation for nearly 30 years.

Arguments For and Against Modified Calendars

When the possibility of instituting a modified school calendar is introduced to a community, the debate over its benefits and drawbacks typically involves both academic and nonacademic concerns (Shields & Oberg, 2000). The debates are informative in that they reveal points of interest that should be used by researchers in formulating a broad array of questions for their studies. Regrettably, empirical

studies on nonacademic issues are few. Therefore, we will cite relatively little research on nonacademic issues and rely instead on arguments to which we were introduced through communication with advocacy groups that either favor (e.g., NAYRE, 2002) or oppose (e.g., SummerMatters!!, 2001) modified calendars. Our quantitative synthesis will focus exclusively on achievement and attitudes.

Nonacademic Arguments

Citizens concerned about the efficient use of public money often weigh in on modified calendar debates. They press the advocates of modified calendars for evidence that money actually is saved when schools use multi-tracking. They note that if schools are open 12 months of the year, then maintenance workers, office workers, administrators, and some teaching specialists have to be paid 12-month rather than 9-month salaries. Modified calendar proponents acknowledge these costs but counter that if new schools were built or modular classrooms added to existing buildings, additional staff would have to be hired. They cite cost analyses that indicate that when a school's population grows past 115% of its building capacity, multi-tracking becomes more cost effective than other alternatives (Coleman & Freehorn, 1993).

Teachers also often voice initial skepticism about the impact of modified calendars. They fear that without the long summer break, they will be more susceptible to burnout. Multi-track employees who have 12-month contracts may be most susceptible to burnout, especially building principals who find it difficult to take vacations knowing that their school is occupied but they are not there. In addition, in multi-tracked schools teachers often have to keep their personal teaching materials in movable storage closets and must change rooms when they return from vacations. To counter these arguments, advocates of calendar change point to teachers in schools with alternative calendars who report that the more frequent but shorter breaks prevent burnout rather than cause it. In addition, proponents argue, teachers in multi-tracked schools often use their breaks to substitute-teach in classes that are in session. In this way, regular teachers make extra money, and students benefit from improved instruction because their substitute teachers have more experience and knowledge of the curriculum.

Opposition to doing away with the summer break is also voiced by some parents. They say that summer vacation provides an opportunity to spend a long period of time with their children without the influence of schools. Proponents of change counter that the modified calendars do not shift the balance of influence between schools and families. The actual time spent in school doesn't necessarily increase. Some parents also fear that multi-tracking can disrupt family life if children from the same family are not placed in the same vacation sequence. Proponents point out that most multi-tracked schools will put siblings on the same track if parents so desire. However, proponents say, some parents want their children on separate vacation schedules. That way, parents who work in the home can provide individual and undivided attention to each child when that child is on break. Concern is also expressed by parents of children who are active in extracurricular activities. They fear that sports teams, bands, and other clubs that compete with or travel to schools on the traditional calendar will be adversely affected because some team members could be on vacation when the big game or competition occurs. Proponents counter that experience suggests that students participate even when they are on vacation. They use as

an example Tempe High School, an alternative calendar school in Tempe, Arizona, whose football team won the state championship in 1996.

Some high school students express the concern that they will lose the opportunity to be employed over the long summer break. Advocates of modified calendars say that employers often adopt job-sharing arrangements. For example, several high school students can move in and out of the same grocery store job as they go from school to break and back. Indeed, advocates say this strategy can reduce dropout rates because adolescents do not settle into a job routine. And students know the job will be there for them the next time they have a break. However, advocates admit that the majority of modified calendar schools are at the elementary level, partly because of concerns about the impact of a modified calendar on adolescents' afterschool activities.

The nationally organized opposition to calendar change comes not from teachers or parents but from economic interests that are threatened by the loss of the long summer break. An organization that lobbied against school calendar reform, called Time To Learn, was partially funded by the International Association of Amusement Parks and Attractions (Worsnop, 1996). The American Camping Association and the daycare industry also keep a close eye on calendar reform. The organization promoting modified calendars, NAYRE, obtains funding from its annual national conference, membership dues, and publications (Charles Ballinger, personal communication, April 21, 1999).

Academic Arguments

Proponents of modified school calendars raise concerns about the possible negative impact of summer vacations on learning (Stenvall, 1999). They suggest that children learn best when instruction is continuous. The long vacation breaks the rhythm of instruction, leads to forgetting, and requires that significant time be spent on review of material when students return to school in the fall. In addition, proponents say, the long summer break can have a greater negative effect on the learning of children with special educational needs. For example, students who speak a language other than English at home may have their acquisition of English language skills set back by an extended period without using them. Finally, there is growing concern that whatever negative impact summer vacations have on learning might be uneven across children from different economic groups. Tying summer vacations to equity issues, Jamar (1994) wrote, "Higher SES students may return to school in the fall with a considerable educational advantage over their less advantaged peers as a result of either additional school-related learning, or lower levels of forgetting, over the summer months" (p. 1).

Opponents of modified calendars provide several counterarguments (c.f., Time To Learn, 1996). Few argue that modified calendars have a negative effect on student learning, but many are skeptical about the alleged benefits. First, they state that changing the calendar does not address the real issues in education, such as effective teaching methods, parent involvement, and curriculum restructuring. Second, they suggest that shorter, more frequent school breaks actually would give students more opportunities to forget things and, therefore, increase the need for time spent on review. Finally, they point to a study by Mitchell and Mitchell (1999) that examined a large California school district using multi-tracking. The researchers found that different tracks corresponded to differences in student achievement levels and

5

student economic and ethnic backgrounds and that poor-performing tracks were associated with fewer teacher resources (e.g., teachers with less experience).

Advocates of change point not only to the prevention of summer learning loss but also to the importance of multiple, shorter breaks for children who are struggling in school. They claim that teachers who see a student struggling on the traditional calendar are often frustrated by their limited ability to intervene until summer. Breaks that occur after 9 or 12 weeks provide an opportunity for more timely remedial activities. Advocates hold up as positive examples schools that make remedial, enrichment, and acceleration classes available to students during the multiple breaks. Opponents counter that remedial intersessions will be of limited effectiveness because they simply force students who may already have negative attitudes toward school to endure more of the same failing instructional techniques.

Summer Vacation and Achievement Test Scores

Research on Summer Learning Loss

A recent research synthesis examined the effects of summer vacation on student achievement (Cooper et al., 1996). Thirty-nine studies were found that examined the effects of summer vacation, 13 of which provided enough information for use in a meta-analysis. The meta-analysis indicated that summer learning loss equaled at least 1 month of instruction. On average, children's achievement test scores were at least 1 month lower when they returned to school in fall than they had been when the children left school in spring.

The meta-analysis also found differences in the effects of summer vacation on various skill areas. Summer loss was more pronounced for math facts and spelling than for other tested skill areas. The explanation rested on the observation that both math computation and spelling skills involve the acquisition of factual and procedural knowledge, whereas other skill areas, especially math concepts, problem solving, and reading comprehension, are more conceptually based. The findings of cognitive psychology suggest that, without practice, facts and procedural skills are most susceptible to forgetting (e.g., Cooper & Sweller, 1987). The meta-analysis also suggested that summer loss was more pronounced for math overall than for reading overall. The authors speculated that children's home environments might provide more opportunities to practice reading skills than to practice mathematics.

In addition to the influence of subject area, numerous differences among students were examined as possible moderators of the effect of summer vacation. Overall, neither the students' gender nor their ethnicity appeared to have a consistent influence on summer learning loss. Likewise, the meta-analysis revealed little evidence to suggest that a student's I.Q. had an impact on the effect of summer break. However, a study by Sargent and Fidler (1987) provided some evidence suggesting that children with learning disabilities have a special need for extended-year schooling. Finally, educators have expressed special concern about the impact of summer vacation on the language skills of students who do not speak English at home, but the meta-analysis produced little evidence bearing on this issue.

Family economics was also examined as an influence on what happens to children over the summer. The meta-analysis revealed that all students, regardless of

the resources in their homes, experienced roughly equal losses in math skills over the summer. However, loss of reading skills varied, and the variations were linked to economic differences among students. On some measures, middle-class children showed gains in reading achievement over the summer and disadvantaged children showed losses. Reading comprehension scores of both income groups declined, but more so for disadvantaged students. Again, the authors speculated that income differences could be related to differences in opportunities to practice and learn reading skills over the summer, with more books and reading opportunities available for middle-class children (see also Alexander et al., in press).

Implications of Summer Learning Loss

The existence of summer learning loss can be used to argue for increasing students' access to summer learning opportunities. A research synthesis reported by Cooper, Charlton, Valentine, and Muhlenbruck (2000) used both meta-analytic and narrative procedures to integrate the results of 93 studies of summer school. The results revealed that summer programs focusing on remedial, accelerated, or enriched learning had a positive impact on the knowledge and skills of participants. Although all students benefited from summer school, students from middle-class homes showed larger positive effects than students from disadvantaged homes. Remedial programs had larger effects when a program was relatively small and when instruction was individualized. Remedial programs may have more positive effects on math than on reading. Mandatory parental involvement also appeared to be related to more effective programs. Students at all grade levels benefited from remedial summer school, but students in the earliest grades and in secondary school may have benefited most.

The existence of summer learning loss also could be used to argue for adopting changes in the school calendar, perhaps by eliminating the long summer break. However, the authors of the meta-analysis pointed out that the existence of summer learning loss could not ipso facto be taken to mean that modified calendars that redistribute vacations throughout the year produce higher achievement among students. Rather, the review of summer vacation effects on achievement underscores the need for systematic review of other calendar-related literatures. Like extended school years and summer school, modified calendars need to be evaluated on their own merits. As noted by calendar change opponents, modified calendars may simply substitute several periods of loss for one.

Previous Reviews of Research on Modified School Calendars

We could locate only one research synthesis on modified school calendars that had appeared in a peer-reviewed journal during the past two decades. Kneese (1996a) performed a quantitative integration of 15 studies of modified school calendars conducted between 1984 and 1994. Studies were located through searches of the ERIC and Dissertation Abstracts International databases and obtained from specialists in the field of year-round education, including several studies from NAYRE. Kneese used the standardized mean difference in achievement test scores (see description below) as an effect size. Comparisons were made either (a) between scores of students who did and did not attend modified calendar schools, with varying degrees of student matching; or (b) between student scores from before and after a year (or longer) spent attending a modified calendar school. The effect of the modified calendar on

achievement was positive regardless of whether it compared two separate groups of students using normal curve equivalent scores ($d = +.12$) or other achievement scores ($d = +.15$), or one group's pre- and post- gain scores ($d = +.11$ for normal curve equivalent scores and $d = +.20$ for other scores). Kneese labeled these effects "positive, but very small" (p. 67). She went on to suggest that evaluations of single-track modified calendar schools produced larger positive effects than evaluations of multi-track modified calendar schools and that male students appeared to perform better than female students in modified calendar schools. Kneese also suggested that larger effects favoring modified calendars appeared in more recent studies. No clear pattern emerged from gain scores regarding the difference between the two calendar types over successive years of implementation. Kneese pointed out that many of the conclusions about variables that might moderate the effects of calendars were based on small numbers of studies. Also, the conclusions about moderators were not based on formal statistical tests.

Unpublished syntheses of varying breadth and technique can be found. For example, Palmer and Bemis (1999) presented a vote count of the significance of 78 findings of studies conducted between 1980 and 1997. The authors did not say how many studies the findings came from or how the studies were located. Overall, they reported 27 findings indicating better achievement in modified calendar schools, 6 findings favoring traditional calendar schools, and 42 findings revealing no difference. Zykowski, Mitchell, Hough, and Gavin (1991) presented a "comprehensive review of research and professional literature" on modified school calendars (p. 1). They included narrative descriptions of six studies and a summary of an earlier review that included nine studies. The authors concluded that the results were "mixed" and "fail to show significant differences in student achievement between year-round and traditional programs" (p. 1), but there did "not appear to be harmful achievement effects when students attend YRE [year-round schools]" (p. 31).

In 2000, NAYRE published an update of Kneese's review (Kneese, 2000a). In it, 30 studies were used to generate 90 comparisons between modified and traditional calendar schools. No effect sizes were presented, but Kneese did report that, in a directional vote count, 67.8% of the comparisons showed higher achievement among modified calendar schools. We could find no similar synthesis of research offered by opponents of modified school calendars, but opponents do offer several annotated bibliographies that critique studies showing a positive effect of modified calendars (SummerMatters!!, 2001).

Our research synthesis improves on past efforts in several ways. First, we attempted to retrieve the largest possible body of empirical evidence on modified school calendars, including sources available from both proponents and opponents of calendar change. Second, we used multiple coders to extract reliable information from each study. Third, we retrieved effect size estimates from studies whenever the reported data permitted, and we combined the effect sizes using conservative assumptions regarding the independence of effect estimates and estimate errors. Fourth, we used formal statistical procedures to test for the influence of moderating variables on the impact of modified calendars. Finally, we conducted a statistical synthesis of data relating to the attitudes of students, parents, teachers, administrators, staff, and the general public who had experienced modified calendars. This analysis examined not only attitude valence but also influences on attitude valence and strength.

8

Methods for Achievement-Related Data Synthesis

Literature Search Procedures

To locate all studies of modified calendars would be virtually impossible no matter how thorough the search. Therefore, bias against the null hypothesis is a concern. That is, the possibility exists that studies with statistically significant results are more easily retrievable than studies with null findings. To mitigate this possibility, we collected studies from a wide variety of sources (including school districts that chose to revert to the traditional school calendar after trying a modified calendar) and included search strategies meant to uncover both published and unpublished studies.

First, we searched three electronic databases: ERIC (January 1965 to March 2002), PsycINFO (January 1967 to March 2002), and Dissertation Abstracts International (1861 to March 2002). For each of these databases we searched using the terms "alternative calendar," "modified school calendar," "year-round school," and "year-round education." The search identified approximately 500 potentially relevant studies. All of the abstracts obtained from the electronic databases were examined, and relevant documents were retrieved.

Next, we used two strategies to ensure that we tapped sources that might have differential access to studies favoring traditional and modified calendars. We contacted the executive directors of NAYRE and Time To Learn and requested that they send us any studies of modified school calendars that they were aware of. NAYRE sent us 77 documents from its library. Time To Learn sent us no documents but did send an annotated bibliography, which we checked for documents previously unknown to us. We also sent 99 letters requesting studies to school districts where one or more schools were known to have used a modified calendar at one time but apparently had discontinued the practice. Thirty-five schools sent us information on their programs. Finally, we hand-searched the reference sections of retrieved documents to identify any additional reports that might address the effects of using a modified calendar. These search processes led us to review more than 400 complete documents.

Criteria for Including Studies

To be included in our research synthesis, each study had to meet several criteria. First and most obviously, each study had to include one group of students who had attended a school in session for approximately 180 days but with multiple vacations distributed throughout the calendar year, beyond the typical distribution of 1 or 2 weeks in winter, 1 week in spring, and at least 2 months during summer. We excluded any study that could be considered an evaluation of an extended school calendar, that is, a calendar in which the school year exceeded approximately 180 days of instruction.

Second, each study had to include some data collection on students. Studies were not included if they merely described a modified calendar program, presented a cost analysis, or detailed a feasibility study.

Third, each study had to assess K–12 students. We excluded studies conducted solely on preschool children or postsecondary students.

Finally, each study had to include some program comparison. Specifically, it had to compare students attending a modified calendar school with students attending a

9

traditional calendar school. We excluded studies in which students' own pre-treatment scores served as controls for their post-treatment scores.

We identified 66 studies that met the above criteria. Of those, 47 contained enough information to calculate an effect size. The 47 studies originated from 40 different school districts. Two presented a comparison of achievement under modified and traditional school calendars in two districts in a manner that did not allow the effect to be calculated for each district separately. Thus, in our usable data, we had 39 school districts in our effect size analysis. An additional 19 reports included only enough information to determine the direction of the findings but not enough to calculate an effect size; these studies originated from 19 school districts. Finally, we found an additional 3 studies that examined data at a statewide level.

Information Retrieved from Studies

Coding Scheme

Our database included 50 characteristics for each study, grouped in six broad categories: (a) demographics of the research report, (b) characteristics of the research design, (c) characteristics of the school district and modified calendar program, (d) characteristics of the sample of students, (e) indicators of the outcomes of the effectiveness of the different calendars, and (f) estimates of the effect of calendar variation.

Effect Size Estimation

We used the standardized mean difference to estimate the effect of calendar variation on measures of student achievement. The *d* index (Cohen, 1988) is a scale-free measure of the separation between two group means. Calculating the *d* index for any comparison involves dividing the difference between the two group means either by their average standard deviation or by the standard deviation of the control group. These calculations result in a measure of the difference between the two group means expressed in terms of either their common standard deviation or the standard deviation of the untreated population. Thus a *d* index of .25 indicates that one-quarter standard deviation separates the two means. In the synthesis, we subtracted the traditional calendar mean from the modified calendar mean and divided the difference by their average standard deviation. Thus positive scores indicate that the modified calendar students had better achievement outcomes.

We calculated effect sizes on the basis of the means and standard deviations of students' achievement indicators, if available. If means and standard deviations were not available, we retrieved the information needed from inferential statistics to calculate *d* indexes (see Rosenthal, 1994). We also used codes to indicate whether the effect sizes that were derived from test statistics came from (a) an unadjusted comparison of two means (e.g., based on two-group *t* test, dependent *t* test, one-way ANOVA); (b) pre-post change scores (e.g., comparing growth for modified calendar students with growth for traditional calendar students); or (c) an ANCOVA.

Coder Reliability

Information from complete reports selected for inclusion was extracted independently by two of the authors. First, they noted and discussed discrepancies; if they did not reach agreement, a third author was called upon to decide the issue.

Most of the information could be gathered with no inferences necessary on the part of the coders: sample size, grade level of participating students, and type of year-round schedule usually fell into this category. However, in some instances the coders needed to make inferences about a sample or program included in a study. For example, if a study reported that a school had more than 50% of students on a free lunch program, the study was coded as "low socioeconomic status" (low SES). In addition, if a study had been conducted after 1990 and demographic information (such as the ethnic makeup of the school) could not be obtained from the report itself, we retrieved relevant data from the Quality Education Data (QED, 2000) survey. QED is an online national education database that includes information on enrollments, relative wealth, spending levels, and the ethnic makeup of public and private schools and school districts.

Methods of Data Integration

Before conducting any statistical integration of the effect sizes, we first counted the number of positive and negative effects. For studies offering effect size information, we calculated the median and range of effects. Also, we examined the distribution of sample sizes and effect sizes to determine if any studies were associated with statistical outliers. Applying Tukey's (1977) definition, we identified sample sizes and d indexes that were more than 3 interquartile ranges beyond the 75th percentile. These values were set at the value of their next nearest neighbor.

Calculating Average Effect Sizes

We used both weighted and unweighted procedures to calculate average effect sizes across all comparisons. In the unweighted procedure, we gave each effect size equal weight in calculating the average d index. In the weighted procedure, we first multiplied each independent effect size by the inverse of its variance. We then divided the sum of these products by the sum of the inverses.[1] We also calculated 95% confidence intervals for the weighted average d indexes. If the confidence interval did not contain $d = 0$, then the null hypothesis of no calendar effect could be rejected.

Identifying Independent Hypothesis Tests

One problem that arises in calculating effect sizes involves deciding what constitutes an independent estimate of effect (Cooper, 1998). We used the school district as the unit of analysis. Our reason for using the school district was to ensure that each estimate of the modified calendar's effect was truly independent of other estimates in the data set. Clearly, multiple estimates of effect within the same school would not be independent because (a) they could be based on achievement measures given to the same sample of students, or (b) circumstances surrounding the innovation (e.g., rationale, calendar type, implementation) would affect all students in the school. Estimates based on schools within districts would not be subject to the former problem but could be subject to the latter. Operationally, using the district as the unit of analysis required averaging across multiple modified-calendar schools in only six districts.

In this procedure, each effect size associated with a comparison is first coded as if it were an independent estimate of the treatment's impact. For example, if a single school district permitted comparisons of first- and second-grade students' math and

reading scores, four separate *d* indexes were calculated. However, for estimating the overall effect of school calendar, these four *d* indexes were averaged prior to entry into the analysis, so that the district contributed only one effect size. To calculate the overall weighted mean and confidence interval, this one effect size would be weighted by the inverse of its variance. However, in an analysis that examined the effects of school calendar on math and reading scores separately, this school district would contribute two effect sizes, one to estimate each outcome.[2]

Tests for Moderators of Effects

We tested possible moderators of school calendar effects by using homogeneity analyses (Cooper & Hedges, 1994; Hedges & Olkin, 1985). Homogeneity analyses compare the amount of variance in an observed set of effect sizes with the amount of variance that would be expected from sampling error alone. The analyses can be carried out to determine whether (a) the variance in a group of individual effect sizes is greater than that predicted by sampling error, or (b) a group of average effect sizes varies more than would be predicted by sampling error. In the latter case, the strategy is analogous to testing for group mean differences in an analysis of variance or testing for linear effects in a multiple regression.

Fixed and Random Error

When an effect size is said to be fixed, it is assumed that sampling error is due solely to differences among participants in the study. However, it is also possible to view studies as containing other random influences, such as differences in teachers, facilities, and community economics. This view assumes that calendar realizations in districts or schools in our meta-analysis also constitute a random sample drawn from a (vaguely defined) population of calendar conditions. Because we believe that random variation in interventions is a significant component of error, we adopt a random-error model that takes into account this study-level variance in effect sizes (see Hedges & Vevea, 1998, for a discussion of fixed- and random-error models).

Rather than use a single model of error, we chose to apply both models to our data. We conducted all of our analyses twice, once with fixed-error assumptions and once with random-error assumptions. By using this sensitivity analysis (Greenhouse & Iyengar, 1994), we could examine the effects of differing assumptions on the outcomes of the synthesis. Differences in results based on which set of assumptions was used could then be part of our interpretation of results. For example, if an analysis revealed that a moderator variable was significant under fixed-error assumptions but not under random-error assumptions, that result suggested a limit on the generalizability of the moderator variable.

Statistical Adjustment for Methodological Factors

As is true for any data set composed of cases not under the control of the researcher, we confronted the possibility that significant associations could exist among some of our moderator variables. For example, studies that used pretest-posttest change scores might also be more likely to sample students from smaller school districts. This confounding of characteristics highlights the fact that plausible rival hypotheses will exist whenever a claim is made for a causal link between the effects of school calendars and achievement, if the calendar choice is associated with another educationally relevant variable.

12

Assertions about causality based on synthesis-generated evidence should always be taken as suggestive (see Cooper, 1998, for a discussion of study-generated and synthesis-generated evidence). However, in cases known to have correlated predictors, we can use statistical techniques to control for some rival hypotheses. We chose to use this strategy in our analyses. We used multiple regression procedures to adjust each d index to remove variation correlated with various methodological factors. Then we ran analyses to examine substantive moderators using both the unadjusted and adjusted d index as the predicted variable. If a substantive characteristic proved to be a significant moderator of school calendar effects when we used both effect size estimates, we could be more confident that the relationship was not the spurious result of a confound with a methodological variation. As was true of our decision to examine the data by using both fixed-error and random-error models, the use of two estimates of effect permitted us to interpret our findings under both liberal and conservative assumptions.

In sum, then, the test of each substantive moderator was conducted four times. The four analyses represent a full crossing of fixed-error and random-error models with unadjusted d indexes and d indexes adjusted to remove all variance associated with methodological factors.

Methods for Attitude-Related Data Synthesis

Literature Search Procedures

The search for surveys of the attitudes of students, parents, teachers, staff, and community members toward modified school calendars was conducted as part of the search for studies of student achievement. Thus the sources of surveys were identical to those of achievement studies. However, in only a few instances were studies and attitude surveys conducted in the same community at the same time. Therefore, the study and survey data sets were analyzed separately.

Criteria for Including Studies

To be included in the database, a study had to measure either attitudes toward modified school calendars or constructs related to attitudes (see below). In all cases, respondents were part of a school community that participated in a modified calendar program. In several cases, participants were surveyed regarding their attitudes before and after implementation of the modified calendar.

Information Retrieved From Surveys

In addition to the information retrieved from surveys, we retrieved several characteristics of each calendar program and its implementation that might have been related to attitudes. Specifically, if a modified calendar school used multiple tracks, we recorded whether parents were allowed to choose their children's vacation track. We recorded whether there were intersession programs available during the school year and, if so, the number of children attending them. We noted whether the survey participants were students, parents, teachers, administrators, community members, or staff.

First, we determined whether each survey asked respondents to consider their own beliefs and feelings about modified calendars or the beliefs and feelings held by the students in the program. Just as participants could vary from one study to

13

another, questions could address not only respondents' reactions but also their views on the reactions of other people.

A variety of attitude-related constructs were used in various studies to assess participants' feelings and beliefs about modified calendars. The dependent variables that we recorded included (a) general satisfaction, (b) perceptions of how well students were achieving, (c) enthusiasm about the program, (d) attitudes toward logistics issues, (e) impact on family issues, (f) satisfaction with intersession, and (g) impact on students other than achievement. We also recorded whether dependent variables had been measured before or after implementation of the modified calendar.

We recorded whether the wording of a question was favorable, unfavorable, or neutral toward modified school calendars. Negatively worded questions were then reverse scored.

Possible responses were presented to participants on a variety of numerical scales. We found responses to surveys using 3-, 4-, 6-, and 10-point scales, in addition to a percentage agreement format. To examine influences on attitudes, we needed to homogenize the numerical response scales. To accomplish that, we standardized the scores at the independent sample level. Specifically, we standardized each set of survey questions that used a particular numerical scale (e.g., we grouped 3-point scales separately from 5-point scales) so that (a) the average sample using that scale would have a mean of zero, and (b) the variance in average sample responses around that mean would be 1.

Standardization enabled us to use the average survey responses based on different raw numerical scales in analyses meant to investigate program and community characteristics that might influence attitudes toward modified calendars.[3] The sample was the unit of analysis. The survey, program, respondent, and measurement characteristics were the predictor variables.

Results for Achievement Outcomes

Direction-Only Analyses

Effect sizes for two-group comparison studies could not be derived for 19 school districts because the reports included insufficient statistical data. Most often, this was because information on the standard deviation of groups was missing and no information on the results of a pertinent inference test (i.e., t values or F values) was provided. Table 1 provides information on these reports, including the overall direction of effect in each case.

The 19 districts that yielded only information on the direction of comparison between a modified calendar and a traditional calendar were combined with the directional information from the 39 districts from which we were able to obtain an effect size. Thus, for the direction-only analyses, 58 districts contributed results. For the overall analysis, 36 of the districts produced results that showed positive effects of a modified calendar and 22 revealed negative effects.

Hedges and Olkin (1985) provided a technique by which the underlying magnitude of a treatment's effect can be estimated from the proportions of studies showing positive and negative directional effects. This approach requires that the vote counter know the direction of each test of the program and the sample size associated with each condition, treatment, and control. In addition, the procedure is dramatically simplified if the sample sizes of the treatment and control conditions are equal within

studies and also across all tests of the treatment. The vote counter enters these values in a table, provided by Hedges and Olkin (1985), to find the estimated effect size.

Our directional data meet only the first two requirements. However, to give us a first estimate of what the effect size associated with modified calendars might be, we used the vote-count estimating procedure by using some assumptions to simulate the "equal sample sizes" requirement. First, we devised an estimate for the "equal" sample sizes in our treatment and control conditions. We used the median overall sample size, which was 382, or about 191 in each condition. Next, we calculated the proportion of the studies that yielded a positive effect (approximately 62%). Then we used that proportion and the median sample size to estimate the overall effect size; this estimate was $d = .04$.

Effect Size Analyses

Overview of Comparisons in the Effect Size Database

The 47 reports from which d indexes could be obtained described 39 districts that compared a modified school calendar with a traditional calendar.[4] For this study, we considered the school district the unit of analysis. A total of 644 comparisons were made between students on a modified school calendar and those on a traditional calendar. The number of independent samples in each district ranged from 1 to 16. The sample sizes for comparisons ranged from 28 to 12,025. In all, about 44,000 students participated.

All of the documents that we used were published or prepared between 1973 and 2000. The median year of document appearance was 1994. Twenty-five of the reports were dissertations, 8 were journal articles, 9 were school reports, 1 was a study conducted by a private research firm, 1 was a paper presentation, and 3 were master's theses.

Table 2 contains some critical characteristics of each of the 39 school districts, including the average effect sizes across all district outcomes.

Measures of Central Tendency and Distribution

To minimize the impact of extreme values on our data analyses, we tested for the existence of statistical outliers in the distributions of both effect sizes and sample sizes. We used Tukey's (1977) definition of an outlier as any value more than 3 interquartile ranges from the 75th percentile (for high values) or 3 interquartile ranges less than the 25th percentile (for low values). For effect sizes, any value greater than 1.46 or less than −1.36 qualified as an outlier. Examining the 644 individual effect sizes revealed 10 effect sizes larger than 1.46 and 7 effect sizes smaller than −1.36. These effect sizes were set to values of 1.46 and −1.36, respectively. With respect to sample size, two districts with sample sizes larger than 3,056 were set to 3,056.

The average unweighted effect size for the 39 district-level comparisons was $d = .09$. When the 39 effect sizes were weighted by the inverse of their variance, the average d index was $d = .06$ with a 95% confidence interval of $\pm .02$. Table 3 presents a stem-and-leaf display of the district-level d indexes. The stems list the first two digits of each effect size value, and the leaves list the hundredths value for each effect size. For example, the +.5 row of Table 3 indicates that there were two d indexes with values of .53 and .54. Taken as a whole, the table reveals that 23 of the 39 effect sizes showed modified school calendars to have a positive effect.

Summary of studies providing evidence of direction of achievement effect

District	State	Author	Year	Calendar	Track	Number of students in analysis	Grade level	Outcome variable	Overall direction of effect
Alameda City Unified School District	CA	Kneese	1996b	60/15	Single-track	606	3–7	Reading, math	+
Brevard County Area II and Area III Schools	FL	Cason	1995	45/15	n/a	n/a	4	Reading, math, attendance	−
Buncombe County School District	NC	Gorsuch	1997	n/a	n/a	66	3–5	Reading, math	−
Cherry Creek School District No. 5	CO	Smith Glass	1975	45/15	Multi-track	1,697	3–6	Reading, language, math	−
Chula Vista Elementary School District	CA	Chula Vista Elementary School District	1991	Multiple calendars	Single- and multi-track	n/a	3–6	Reading, math	+
Clark County School District	NV	Costa	1987	n/a	n/a	n/a	2–6	Reading, language, math	−
Delphi District	UT	Shields	1996	n/a	n/a	n/a	5	Reading, language, math, other	+
Friendswood Independent School District	TX	McEwen, Johnson, & Owens	1995	Multiple calendars	Single-track	607	4–6	Language, math, other	+

District	State	Source	Year	Calendar	Track	N	Grades	Outcomes	Effect
Fulton County School District	GA	Russell	1992	45/15	Single-track	188	3-5	Language, math, other	+
Jordan School District	UT	Sorensen	1995	45/15	Multi-track	n/a	4-6	Reading, language, math	+
Laurel School District	MS	Cason	1995	45/15	n/a	n/a	4	Reading, math, attendance	=
Lebanon Community School Corporation	IN	Arnove et al.	1995	45/15	Single-track	n/a	2-3	Reading, math	+
Orange County Public Schools	FL	Fardig	1991	60/15	Single- and multi-track	n/a	K-6	Reading, math	+
Pajaro Valley Unified School District	CA	Pelavin	1979	45/15	Multi-track	951	2, 5, 7	Reading, math	+
Shelby County School District	AL	Cason	1995	45/15	n/a	n/a	4	Reading, math, attendance	=
South Bay Union School District	CA	Bechtel	1991	60/20	Multi-track	525	2-3	Reading	+
Sweetwater Union High School District	CA	Chen	1993	45/15	Single-track	n/a	9-12	Reading, language, math, other	+
Texarkana Independent School District	TX	Texarkana Independent School District	1992	n/a	Single-track	178	K-5	Reading, math	+

Note. n/a = information not available from the reports.

TABLE 2
Summary of studies providing evidence of magnitude of achievement effect

District	State	Author	Year	Calendar	Track	Number of students in analysis	Grade level	Outcome variable	Average effect size
Austin Independent School District	TX	Dunn	1996	n/a	n/a	48	K-6	Reading, math	+.03
Caddo Parish School District	LA	Caddo Parish School Board	1995	45/15	Single-track	671	3-5	Language, math	-.07
Cajon Valley Union School District	CA	Marr	1989	60/20	n/a	884	3-4	Reading	+.10
Chicago Public Schools	IL	Espinosa	2000	60/20	n/a	311	3-5	Reading, math	+.13
Conroe Independent School District	TX	(1) Ritter (2) Dunn	1992 1996	30/10	Single-track	112	K-6	Reading, math, other	-.21
Coweta County School System	GA	Heaberlin	2000	45/15	Single-track	135	1-5	Reading, math	-.06
Deer Park Independent School District and Pasadena Independent School District (combined analysis)	TX	(1) Owens & Waxman (2) Hasselmeier	1996 1997	60/20	Single-track	1,139	3-5	Reading, math	+.75
Denver Public School District	CO	Ricketts	1976	60/15	Multi-track	3,040	1-9	Reading, math, other	-.07

18

District	State	Source	Year	Calendar	Track	N	Grades	Subjects	Effect
Durham Public School District	NC	Haenn	1996	45/15	Single-track	304	K-5	Reading, math, other, attendance	-.03
Edenton-Chowan County School District	NC	Edenton-Chowan Schools	1997	45/15	n/a	2,572	3-7	Reading, math	+.13
Fort Worth Independent School District	TX	Coulter	1997	45/15	n/a	3,056	K-9	Reading, math	-.03
Francis Howell R-3 School District	MO	Schoewe	1993	n/a	n/a	530	12	Reading, math, attendance	-.10
Harlandale Independent School District	TX	Paloczy	1997	45/15	Single-track	148	3-5	Reading, math, attendance	+.43
Los Angeles local districts	CA	(1) Johnson (2) Mauzui (3) Mitchell	1984 1992 1999	n/a	Multi-track	3,056	2-6	Reading, math	-.09
Mooresville Graded School District	NC	(1) Boyles (2) Carroll	1993 1997	45/15	n/a	2,408	2-9	Reading, math	+.09
Natomas Unified School District	CA	American Lakes Year-Round School Steering Committee	1991	60/15	Multi-track	2,110	K-6	Attendance	-.05
Northville Public Schools	MI	Moorgai	1976	45/15	Single-track	320	K-6	Other, attendance	+.39

(continued)

19

TABLE 2
Summary of studies providing evidence of magnitude of achievement effect (Continued)

District	State	Author	Year	Calendar	Track	Number of students in analysis	Grade level	Outcome variable	Average effect size
Oroville City Elementary School District	CA	Veatch	1991	60/15	Multi-track	160	1-5	Other	+.22
Oxnard Elementary School District	CA	Brekke	1984	n/a	Multi-track	924	K-6	Attendance	+.04
Palmdale Unified School District	CA	Gandara & Fish	1994	60/15	Multi-track	281	K-6	Reading, math	+.22
Pinellas County School District	FL	Potter Johnson	1976	45/15	Multi-track	31	5	Reading, math	-.37
Rockingham County Consolidated School District	NC	Brady	1996	n/a	n/a	154	3-5	Reading, math	+.33
San Diego Unified School District	CA	Fass-Holmes & Gates	1994	n/a	Single-track	857	2-6	Reading, language, math, other, attendance	+.13
San Ysidro School District	CA	Barron	1993	60/20	Single-track	589	2-5	Reading, language, math, attendance	-.24
Sunnyside Unified School District No. 12	AZ	Marty	1978	n/a	n/a	144	9	Math	-.56
Virginia Beach City School District	VA	Powers	1974	45/15	Multi-track	291	4	Reading, math, other	+.23

20

District	State	Author	Year	Calendar	Track type	N	Grade	Measures	Effect
Waco Independent School District	TX	(1) Elsberry (2) Stripling (3) Stripling	1992 1994 1995	60/20	Single-track	289 776 2,344	3, 5 5 4-8	Reading, math, attendance	+.29
Waxahachie Independent School District	TX	Dunn	1996	n/a	n/a	52	K-6	Reading, math	+.12
West Carrollton School District	OH	(1) Campbell (2) Roby	1994 1995	45/15	Single-track	157	K-2, 6	Reading, math	+.12
n/a	TX	Kneese & Knight	1995	30/10	n/a	616	3-6	Reading, math	+.35
n/a	TX	DeLaro	1994	Multiple calendars	Single-and multi-track	270	3	Reading, language	–.01
n/a	CA	Miranda	1993	45/15	Single-track	193	9-12	Language, other	+.54
n/a	AZ	McCraley	1975	n/a	n/a	732	K-8	Reading, math	–.11
n/a	TX	Woolley	1996	60/20	n/a	766	5	Reading, attendance	–.05
n/a	WA	Eisele	1998	60/20	n/a	216	3-6	Reading, math	–.20
n/a	KY	Reece, Myers, & Notsinger	2000	45/10 and 45/15	Single-track	218	2	Reading, math, language	+.34
n/a	n/a	Kneese	2000	60/15	Single-track	297	3-7	Reading, math	+.25
n/a	IL	Kuner-Roth	1985	n/a	Multi-track	360	7	Reading, language	+.09
n/a	GA	Consolie	1999	45/15	Single-track	159	5	Reading, math	+.79

Note. n/a = information not available from the reports.

21

TABLE 3
Stem and leaf display of unweighted achievement effect sizes

Stem	Leaf
+.7	8
+.5	34
+.4	3
+.3	2448
+.2	22359
+.1	002223
+.0	3479
−.0	033556779
−.1	00
−.2	014
−.3	7
−.4	
−.5	6

Note. Effect sizes (*d* indexes) are based on each district as an independent sample (*n* = 39). The dotted line represents the value zero; the wavy line indicates a break in the distribution (there is no +.6 value in the stem column).

Tests for Moderators of Study Outcomes

We collected information on several features of studies, of school districts and schools, and of modified calendar variations that we could not examine as moderators because either (a) too few reports contained information on them, or (b) there was not enough variation across the categories of the feature. For example, most studies that attempted to equate the two calendar groups used a post hoc statistical control. No study used only prior matching of students, and just two studies used both statistical control and matching. Because of the lack of variation, we could not examine this study feature as a moderator of the calendar effect. Similarly, we hoped to compare effect sizes from studies that used a cohort design with those that used a static-groups design. However, only three reports used a cohort design. Similar limitations were experienced for student achievement level (which was infrequently reported by school districts), whether or not the study excluded students who fell below an established attendance level.

Study features. The first set of moderators of the effect of school calendar that we tested were three features of study design: (a) whether a study was conducted by an internal or external evaluating agent, (b) whether an attempt was made to equate the modified and traditional calendar groups by using either matching or statistical control, and (c) the size of the sample.

Table 4 presents the homogeneity test, average effect sizes, and confidence intervals for each of these features of studies. In using the table, the reader should note that, for example, if an average *d* index equals .20 and its associated confidence

TABLE 4

Average achievement effect sizes and homogeneity tests for evaluation features

Variable	Level	k	Fixed error		Random error	
			d	Qb	d	Qb
Evaluation agent				6.11*		.30
	External	23	.10 ± .03		.08 ± .09	
	Internal	21	.05 ± .03		.12 ± .10	
Matching or statistical control				11.95***		.40
	No	24	.09 ± .03		.07 ± .09	
	Yes	17	.01 ± .04		.11 ± .11	
Sample size				8.70***		2.05
	>884	9	.06 ± .03		.06 ± .14	
	160–884	20	.07 ± .04		.11 ± .09	
	<160	10	.12 ± .12		.10 ± .17	

Note. Sample size was tested continuously. k = number of independent samples. $d = d$ index. Qb = degree of between-groups homogeneity.
*$p < .05$. ***$p < .001$.

interval equals .05, then the 95% confidence interval for the average value is .15 to .25. If the confidence interval does not contain the value .00, then the effect size is statistically different from zero at $p < .05$.

The source of an evaluating agent (internal or external to the school district) was a significant moderator of effect sizes in the fixed-error model. Specifically, studies conducted by external evaluators were associated with larger effect sizes ($d = .10$) than were studies conducted by internal evaluators ($d = .05$), $Q(1, k = 44) = 6.11$, $p < .05$. For the random-error model, there was no effect size difference between internal and external evaluators.

Using a fixed-error model, studies using matching or statistical controls in an attempt to equate students had smaller effect sizes, $d = .01$, than did studies that made no attempt to equate students, $d = .09$, $Q(1, k = 41) = 11.95$, $p < .001$. However, the difference was not significant when we used a random-error model.

Sample size was a significant predictor of effect size in the fixed-error analysis, $Q(1, k = 39) = 8.70$, $p < .001$. The smallest sample sizes (< 160) were associated with the largest effect sizes ($d = .12$). Again however, this result did not hold in the random-error analysis.

To create effect size estimates that were uncorrelated with methodological variations in studies, we decided to control for the three features of studies related to design and analysis, that is, the evaluating agent, statistical control, and sample size. The unit of analysis used for purposes of adjusting effect sizes for the three methodological characteristics was the individual comparison or effect size ($n = 644$). The three variables were entered into a regression equation simultaneously. The residuals from the regression were then used in the moderator analyses described below;

23

however, because the residuals had an average value of zero, we first added .06, the average of the weighted effect sizes based on the 644 comparisons, to each residual. This procedure is called "fitting an average value to the regression."

Publication characteristics. Table 5 presents information about analyses examining effect sizes for moderators related to publication characteristics. It includes results from the fixed- and random-error Q statistics, the mean effect sizes, and the 95% confidence interval around the mean.

The relationship between the type of study report and the reported effect of modified calendars was tested in two ways. First, we compared master's theses and dissertations to all other documents. In the fixed-error analyses, theses and dissertations were associated with larger effect sizes than were other types of documents, $Q(1, k = 42) = 10.47, p < .001$, for the unadjusted analysis and $Q(1, k = 42) = 9.52$, $p < .001$, for the adjusted analysis. For theses and dissertations, effect sizes were $d = .11$ and $d = .09$ for the unadjusted and adjusted analyses, respectively; effect sizes for other documents were $d = .05$ and $d = .03$. Under random-error assumptions, there were no differences in effect sizes for theses and dissertations in comparison with other publications.

Next, we compared the effect sizes from studies published in journals with those from all other types of publications. There were no significant differences in any of the four analyses that we conducted.

Because we received some of our reports from an advocacy organization (NAYRE), we tested whether those reports differed systematically from the other reports in our database. Systematically larger effect sizes from reports obtained from NAYRE might be evidence of a bias resulting from the process that the organization used to obtain or disseminate the research reports. Under fixed-error assumptions, unadjusted effect sizes from reports obtained through NAYRE were not significantly different from reports that came from other sources. However, using adjusted effect sizes, reports obtained from NAYRE revealed on average *lower* effect sizes ($d = .02$) than did reports from other sources ($d = .07$), $Q(1, k = 48) = 3.99, p < .05$. Under random-error assumptions, effect sizes from reports obtained through NAYRE did not differ from those obtained through other searches using either unadjusted or adjusted effect sizes.

Community and student differences. Table 6 presents information about analyses examining effect sizes for moderators related to community and student characteristics. Studies were compared depending on whether they were conducted in large urban, small urban, suburban, or rural communities. First, we performed the analysis using the size of the community as a continuous variable. In both the unadjusted and adjusted analyses, a fixed-error model revealed that studies carried out in larger communities resulted in smaller effect sizes than studies carried out in smaller communities, $Q(3, k = 29) = 23.57, p < .001$, for the unadjusted analysis and $Q(3, k = 29) = 23.96, p < .001$, for the adjusted analysis. Average effect sizes ranged from .02 to .06 for large and small urban schools and from .14 to .21 for suburban and rural schools. However, using random-error assumptions, the size of the community moderator did not produce a significant result based on either unadjusted or adjusted effect sizes. Average effect size estimates ranged from $-.03$ to .07 for large and small urban schools and from .04 to .15 for suburban and rural schools. Next,

24

TABLE 5

Average achievement effect size and homogeneity tests for publication characteristics

Variable	Level	k	Unadjusted effect size				Adjusted effect size			
			Fixed error		Random error		Fixed error		Random error	
			d	Q_b	d	Q_b	d	Q_b	d	Q_b
Publication				10.47**		.20		9.52**		.16
	Dissertation/thesis	26	.11 ± .03		.10 ± .09		.09 ± .03		.09 ± .09	
	All other	18	.05 ± .03		.11 ± .10		.03 ± .03		.12 ± .10	
Publication				.05		1.40		1.16		1.79
	Journal	8	.06 ± .05		.18 ± .15		.07 ± .05		.15 ± .15	
	All other	33	.06 ± .02		.07 ± .08		.04 ± .02		.06 ± .08	

Note. k = number of independent samples. d = d index. Q_b = degree of between-groups homogeneity.
**p < .01.

25

TABLE 6
Average achievement effect size and homogeneity tests for community and student differences

| | | | Unadjusted effect size | | | | Adjusted effect size | | | |
| | | | Fixed error | | Random error | | Fixed error | | Random error | |
Variable	Level	k	d	Qb	d	Qb	d	Qb	d	Qb
City size				23.57***		1.26		23.96***		1.72
	Large urban	12	.06 ± .03		.07 ± .13		.07 ± .03		.07 ± .13	
	Small urban	6	.01 ± .05		.00 ± .18		.02 ± .05		-.03 ± .18	
	Suburban	6	.21 ± .07		.15 ± .19		.18 ± .07		.13 ± .19	
	Rural	5	.14 ± .14		.06 ± .23		.11 ± .14		.04 ± .23	
SES				83.15***		6.00*		79.05***		4.28*
	Low	18	.24 ± .04		.22 ± .11		.21 ± .04		.19 ± .11	
	Moderate, middle, mixed	25	.02 ± .03		.05 ± .09		.00 ± .03		.05 ± .09	
Grade level				14.22***		2.37		9.32**		1.43
	Elementary	23	.08 ± .03		.12 ± .09		.08 ± .03		.10 ± .09	
	Secondary	9	-.01 ± .04		-.01 ± .14		.01 ± .04		.00 ± .14	
% White				4.64*		.01		11.27***		.02
	<2/3 of the sample	16	.09 ± .03		.12 ± .12		.10 ± .03		.10 ± .12	
	>2/3 of the sample	14	.00 ± .05		.02 ± .13		-.04 ± .05		.00 ± .13	

Note. k = number of independent samples. d = d index. Qb = degree of between-groups homogeneity.
*p < .05. **p < .01. ***p < .001.

we compared urban school districts (large and small) with suburban and rural districts. We obtained similar results related to community size for the unadjusted effect sizes, fixed-error analysis, $Q(1, k = 29) = 21.06$, $p < .001$, and for the adjusted effect sizes, fixed-error analysis, $Q(1, k = 29) = 15.92$, $p < .001$. Again, no differences emerged when we used random-error assumptions.

We examined SES by comparing school districts serving poorer communities with those serving lower-middle- and middle-income communities. We found a significant difference for both adjusted and unadjusted effect sizes when using the fixed-error model, $Q(1, k = 43) = 83.15$, $p < .001$, and $Q(1, k = 43) = 79.05$, $p < .001$, respectively. In addition, we found a significant relationship between community SES and unadjusted effect sizes in the random-error analysis, $Q(1, k = 43) = 6.00$, $p < .05$, and a significant relationship in the adjusted effect sizes in the random-error analysis, $Q(1, k = 43) = 4.28$, $p < .05$. In all cases, the effects of the modified school calendar were greater in districts classified as low SES (average effect sizes ranged from .19 to .24) than in districts classified as mixed, moderate, or middle SES (average effect sizes ranged from .00 to .05).

Samples of students drawn from elementary schools were compared with samples drawn from secondary schools. In the unadjusted effect size, fixed-error analysis, the grade level of the sample was significantly related to effect size, $Q(1, k = 32) = 14.22$, $p < .001$. The grade level of the sample was also significant in the adjusted effect size, fixed-error analysis, $Q(1, k = 32) = 9.32$, $p < .01$. In the random-error analyses, the grade level of the sample was not related to effect size, whether unadjusted or adjusted effect sizes were used. Average effect sizes ranged from .04 to .11 for samples based on elementary school students and from −.02 to .02 for samples based on secondary students.

Of the 39 school district reports, 30 provided information regarding the ethnic distribution of the students in the sample. Unfortunately, there were not enough individual cases in the African American, Hispanic, or Asian American categories to permit independent testing to determine whether these minority groups might have responded differently to modified calendars. Instead, we categorized each district by the average percentage of White students served and examined the impact of the variation on effect size. For the fixed-error analysis, the percentage of White students in the sample was related to both unadjusted effect sizes, $Q(1, k = 30) = 4.64$, $p < .05$, and adjusted effect sizes, $Q(1, k = 30) = 11.27$, $p < .001$. In both cases, the samples with more minority students had higher effect sizes. For illustrative purposes, we split the percentage of White students into two groups. Districts where more than two-thirds of students were White revealed average effect sizes ranging from −.04 to .02; districts where less than two-thirds of students were White revealed average effect sizes of .09 to .12. For the random-error model, the percentage of White students in the sample was not related to effect size, whether unadjusted or adjusted effect sizes were analyzed.

Only seven reports gave information concerning the gender distribution of their samples. Of these, three provided separate effect size estimates for male and female participants. The fact that these three studies were matched on all other characteristics (e.g., male and female students attended the same schools and took the same tests) allowed us to compare the relationship between gender and effect size by using these three districts only. To conduct the analysis, we first created a difference score

27

for each of the three districts by subtracting the effect size for female participants from that for male participants. This effect size difference was an effect size in its own right. Then we calculated the weighted average of the differences in effect size and their confidence intervals using both fixed- and random-error assumptions. For the fixed-error model, the average difference between male and female effects was .22, with larger effects appearing for male participants; the 95% confidence interval for the effect size ranged from −.03 to +.47. For the random-error model, the average effect was also .22, and the 95% confidence interval ranged from −.11 to +.55. Because the confidence interval contains .00, the null hypothesis that the difference between the male and female effect sizes is zero cannot be rejected.

Program characteristics. Table 7 contains the results of analyses examining the influence of characteristics of the modified calendar program on outcome measures. We used the year in which a study was published or appeared as a proxy for how recently a program had been instituted (which was rarely stated explicitly in reports). The year of a study was not a significant moderator of effect size in the fixed-error models for either the unadjusted or the adjusted effect sizes. The random-error analyses also revealed no statistically significant difference related to the year of a study. Average effect size estimates ranged from .05 to .09 for studies appearing before 1990 and from .04 to .12 for studies appearing in 1990 or later.

Relatively few reports (25 of 39) mentioned the number of years that schools had operated on the modified calendar. Therefore, we were able to examine only studies of schools in their 1st year of operation, as compared with schools with more than 1 year of operation. The number of years that a school had been using the modified calendar was not significantly related to effect size in the fixed-error analysis, $Q(1, k = 25) = 1.93$, *ns,* for the unadjusted effect sizes. For the adjusted effect sizes, number of years of program operation revealed a nonsignificant trend related to effect size, $Q(1, k = 25) = 2.84$, $p < .10$, with programs in operation for more than 1 year revealing larger effect sizes ($d = .03$) than programs in their 1st year of operation ($d = -.02$). In the random-error analysis, there was no relationship between number of years of program operation and effect size in either the unadjusted effect sizes, $Q(1, k = 25) = .21$, *ns,* or the adjusted effect sizes, $Q(1, k = 25) = .53$, *ns.*

We compared effect sizes from districts using two different types of modified calendars, which we roughly categorized as "shorter breaks" and "longer breaks": Children might go to school for 9 weeks and then have 3 weeks off (called a 45/15 calendar) or go for 12 weeks and have 4 weeks off (called a 60/20 calendar). The length of the vacation break was not related to effect size in the unadjusted effect size, fixed-error analysis but was statistically significant in the adjusted effect size, fixed-error analysis, $Q(1, k = 28) = 5.53$, $p < .05$. In the random-error analyses, the length of the break was not related to either the unadjusted effect sizes or the adjusted effect sizes. Average effect sizes from 45/15, 30/10, and 60/15 calendars ranged from .08 to .15; average effect sizes from 60/20 calendars ranged from .02 to .06.

Districts that used a single-track modified calendar were compared with those that used a multi-track arrangement. In the fixed-error analyses, schools on single-track modified calendars yielded larger effect sizes than schools using multi-track calendars. The fixed-error homogeneity tests were $Q(1, k = 23) = 31.76$, $p < .001$, for the

TABLE 7
Average achievement effect sizes and homogeneity tests for characteristics of modified calendar program

			Unadjusted effect size				Adjusted effect size			
			Fixed error		Random error		Fixed error		Random error	
Variable	Level	k	d	Qb	d	Qb	d	Qb	d	Qb
Year of publication				2.41		.53		2.53		.66
	<1990	12	.09 ± .03		.06 ± .13		.08 ± .03		.05 ± .13	
	≥1990	30	.06 ± .03		.12 ± .08		.04 ± .03		.11 ± .08	
Number of years of operation				1.93		.21		2.84†		.53
	1	8	.02 ± .05		.07 ± .16		-.02 ± .05		.03 ± .16	
	>1	17	.06 ± .03		.12 ± .10		.03 ± .03		.10 ± .10	
Calendar				1.12		.93		5.53*		1.36
	45/15, 30/10, 60/15	20	.08 ± .03		.15 ± .10		.08 ± .03		.15 ± .10	
	60/20	8	.05 ± .04		.06 ± .15		.02 ± .04		.05 ± .15	
Tracking				31.76***		2.67		30.66***		2.20
	Single-track	15	.16 ± .04		.22 ± .12		.11 ± .04		.19 ± .12	
	Multi-track	8	-.01 ± .05		.06 ± .16		-.06 ± .05		.04 ± .16	
Intersession				36.34***		2.35		.54		.12
	Mentioned	15	.16 ± .04		.16 ± .11		.06 ± .04		.10 ± .11	
	Not mentioned	24	.01 ± .03		.05 ± .09		.05 ± .03		.08 ± .09	

Note. k = number of independent samples. *d* = *d* index. *Qb* = degree of between-groups homogeneity.
†*p* < .10. **p* < .05. ****p* < .001.

29

unadjusted effect sizes and $Q(1, k = 23) = 30.66, p < .001$, for the adjusted effect sizes. Under random-error assumptions, there was no difference between the types of tracking. The average effect sizes ranged from .11 to .22 for single-track calendar schools and from −.06 to .06 for multi-track calendar schools.

Reports from 15 of 39 school districts clearly mentioned that intersession activities were made available to students on the modified calendar. In the unadjusted effect size, fixed-error analysis, districts in which intersessions were available revealed effect sizes that were higher than for districts whose reports made no explicit mention of intersessions, $Q(1, k = 39) = 36.34, p < .001$. However, this effect did not hold in the adjusted effect size, fixed-error analysis or in either of the random-error analyses. Average effect sizes ranged from .06 to .16 for districts mentioning that intersessions were available and from .01 to .08 for districts stating that no intersessions were available.

Outcome characteristics. We examined the relationship between a study's effect size and two characteristics of how outcomes were measured: (a) the subject matter of the test, and (b) whether the measure was a standardized testing instrument. Table 8 presents information on the results of these analyses.

Subject matter measures were divided into three categories: mathematics (e.g., scores on standardized math tests, math grades), reading (e.g., scores on standardized reading and language tests, vocabulary), and other (e.g., social science grades). First, an omnibus test of subject matter differences revealed nearly significant differences for the unadjusted effect size, fixed-error analysis, $Q(2, k = 78) = 5.80, p < .06$, but not for the adjusted effect size, fixed-error analysis, or either of the two random-error analyses. Next, we compared modified calendar effects on math with effects on reading. No significant differences were found for any of the four analyses. Average effect sizes associated with math ranged from .04 to .05; average effect sizes associated with reading ranged from .06 to .12.

Next, we compared the influence of modified calendars on math and reading test scores as compared with all other subjects. Effect sizes were marginally related to the outcome measure in unadjusted effect size, fixed-error analysis, $Q(1, k = 48) = 3.36, p < .07$, and for the adjusted effect size, fixed-error analysis, $Q(1, k = 48) = 2.99, p < .09$. However, for the random-error analyses, there was no difference in either the unadjusted effect size analysis or the adjusted effect size analysis. For reading and math scores, effect sizes ranged from $d = .03$ to $d = .15$; effect sizes based on other content areas ranged from .07 to .13.

Finally, we compared effect sizes that were based on standardized achievement measures with those based on all other achievement measures (e.g., grades). In the fixed-error analysis, the type of measure was related to effect size in both the unadjusted analyses, $Q(1, k = 42) = 10.61, p < .001$, and the adjusted analyses, $Q(1, k = 42) = 12.65, p < .001$. However, the random-error analyses revealed no significant relation whether unadjusted or adjusted effect sizes were used. The average effect sizes ranged from .07 to .08 for standardized tests and from .09 to .16 for other measures.

Statewide Assessments

We found three studies of the impact of modified school calendars on academic achievement that used state-level archival records as data and schools (rather than

TABLE 8

Average achievement effect sizes and homogeneity tests for outcome characteristics

Variable	Level	k	Unadjusted effect size				Adjusted effect size			
			Fixed error		Random error		Fixed error		Random error	
			d	Qb	d	Qb	d	Qb	d	Qb
DV				5.80†		2.07		4.31		1.54
	Math	31	.05 ± .03		.05 ± .08		.04 ± .03		.05 ± .08	
	Reading	35	.08 ± .02		.12 ± .08		.06 ± .02		.12 ± .08	
	Other	12	.11 ± .04		.15 ± .13		.09 ± .04		.13 ± .13	
Measure				10.61***		.25		12.65***		.03
	Standardized tests	34	.07 ± .02		.08 ± .08		.07 ± .02		.08 ± .08	
	Other	8	.16 ± .05		.12 ± .15		.16 ± .05		.09 ± .15	

Note. k = number of independent samples. d = d index. Qb = degree of between-groups homogeneity. DV = dependent variable.
†$p < .10$. ***$p < .001$.

31

students) as the units of analysis. The first study (Quinlan, George, & Emmett, 1987) used California Assessment Program (CAP) results for the years 1982–1983 and 1984–1985. Data were obtained from 387 modified calendar schools and 7,806 traditional calendar schools for Grades 3 and 6 in reading and mathematics. Results suggested that modified calendar schools served lower-SES communities (as measured by occupation of parents and proportion of families receiving economic assistance from the federal government) and about twice as many limited- and non-English-speaking students. Although the data were not presented in a fashion that allowed us to estimate the size of the effect, an unadjusted examination of achievement test scores indicated that students in year-round schools consistently performed below their traditional calendar counterparts. However, a multiple regression analysis adjusting for SES and native language differences indicated that "year-round schools are narrowing the gap between year-round and traditional calendar schools" (p. 33). Although year-round schools were improving, they still performed "below the level predicted on the basis of their background characteristics" (p. 33). A comparison of the adjusted achievement scores of single-track as compared with multi-track modified calendar schools revealed that single-track schools were performing at a level similar to that of traditional calendar schools but that multi-track schools were performing below predicted levels.

A second statewide study was conducted using end-of-grade assessments in reading and math for students attending Grades 3–8 in North Carolina (McMillen, 2001). About 350,000 students attending 1,364 schools on a traditional calendar and about 17,000 students attending 106 schools on a modified calendar provided test scores. McMillen used these data in a hierarchical linear model analysis. The students' previous end-of-grade scores, sex, ethnicity, and parents' education levels served as control variables. Four separate analyses were run distinguishing (a) reading and math crossed with (b) schoolwide programs and programs in which some students in a school were on modified calendars while others were on traditional calendars, called "school-within-a-school" programs.

The effect of school calendars was tested as a between-schools factor after controlling for the student characteristics as within-school factors. In the schoolwide analyses, the beta weight associated with the calendar variation was .10 for reading and .23 for math, both favoring modified calendars. However, the calendar difference was not statistically significant. For the school-within-a-school analyses, the effect of the calendar was the opposite, favoring traditional calendars; for reading, the beta was equal to −.11, and for math the beta was equal to −.22. Again, the differences were not significant.

Beta weights are measures of effect, but they are not combinable or directly comparable with d indexes. However, McMillen (personal communication, November 21, 2001) did share with us the information needed to derive student-level d indexes, controlling for the student-level covariates. For schoolwide programs, students attending modified calendar schools scored higher in both reading, approximate $d = .024$, and math, approximate $d = .05$. For school-within-a-school programs, students attending traditional calendar schools scored higher in both reading, approximate $d = −.01$, and math, approximate $d = −.03$.

McMillen also conducted analyses testing for interacting effects of the calendar program and student characteristics. For these, he did report several associated d indexes. In the schoolwide analyses, he found that lower-achieving students in

modified calendar schools had significantly higher achievement scores than lower-achieving students in traditional calendar schools. For both reading and math, the *d* index was approximately .05. Also, McMillen found that White students scored about .04 standard deviation higher on math tests in modified calendar schools than in traditional calendar schools, but he found no difference among White students in reading or among non-White students in either reading or math. No interaction was found involving parent education level.

For school-within-a-school data, there were no statistically significant interactions involving prior achievement or ethnicity. Students whose parents had some postsecondary education demonstrated higher reading scores in traditional calendar programs, with the *d* index equal to approximately .03; but no difference appeared in math.

None of the analyses for math or reading within either program type revealed significant interactions involving the sex of the student. Scores were standardized within grade levels, which therefore could not be examined for interacting effects with the type of calendar.

Stenvall and Stenvall (2001), the latter being the executive director of NAYRE, conducted an analysis of California statewide achievement data for the years 1999 and 2000. About 4.1 million students attending approximately 5,300 traditional calendar schools and 1,300 modified calendar schools provided data. It appears that the school was the unit of analysis, although that decision is not stated explicitly.

The report also is unclear regarding exactly what served as the measure of achievement. Initially, the measure is referred to as the "Advanced Placement Index" (API); later it is called the "Academic Performance Index" (p. 2). That index first is cited as based on a statewide administration of the Stanford 9 standardized test of basic skills (preface) and later as based on the API 9 test (p. 2). In either case, it appears that a statewide assessment of achievement served as the outcome variable.

Initial multiple regression analyses were conducted separately on elementary, middle, and high school achievement scores. For example, for elementary school students, 10 variables were used to predict achievement. Five of these were discarded for being "not significant in formulating an acceptable regression equation" (p. 3). The students' ethnicity was discarded because it "proved highly biased" (p. 3), and student mobility, parent education, and teachers' credentialing were removed because they "distorted the multiple regression models" (p. 3). Operational definitions for the exclusion criteria were not provided in the report.

Regrettably, the authors did not report having included the type of school calendar in the regression equation. They did present some information relevant to the impact of the school calendar, although formal statistical tests were not conducted on those data and effect sizes cannot be derived from them. First, the authors found that elementary school students who attended modified calendar schools in 1999 had lower achievement test scores than students who attended regular traditional calendar schools but higher scores than students in the single-track California traditional calendar schools that used double sessions (as a means to reduce overcrowding). In 1999, middle and high school students on traditional calendars outperformed students on modified calendars. The authors suggest that these differences were due to higher percentages of limited-English-proficient or economically poor students in modified calendar schools. However, the analysis included no controls for any community, school, or student factors that might be confounded with the calendar

program; and, as noted above, the authors did not pursue that possibility in their regression analysis.

Second, the authors calculated the percentage change in achievement scores by dividing scores obtained in 2000 by those obtained in 1999. They found that elementary school students on single-track, three-track, and five-track modified calendars experienced larger percentage gains than did students in traditional calendar schools; but students in four-track calendars did not. Middle schools with traditional calendars experienced smaller average percentage gains than did modified calendar schools with single tracks or three tracks but equal gains in comparison with four-track schools and greater gains than five-track schools. Again, the authors invoke other school characteristics, such as overcrowding and concentration of disadvantaged students, to explain the lack of evidence favoring modified calendar schools; but no statistical test of these hypotheses is offered. High schools on traditional calendars experienced lower percentage gains in achievement than did those on modified calendars.

Results for Attitude Surveys

Descriptive Statistics

Our literature search uncovered 63 reports containing attitude surveys.[5] The reports originated in 55 school districts located in 20 U.S. states and in Canada. Statistics were reported for a total of 180 independent samples of respondents. The number of questions asked of each respondent ranged from 1 to 33. Surveys were taken between the years 1972 and 1998.

The unstandardized means for the response scales indicate that, in general, survey respondents felt more positive than negative about modified school calendars. For districts that asked questions with a 3-point response scale (having a midpoint of 2), the mean response was 2.42 ($SD = .35$); for those with a 4-point scale (having a midpoint of 2.5), the mean response was 2.85 ($SD = .22$); for those with a 5-point scale (having a midpoint of 3), the mean was 3.59 ($SD = .62$); and the one district with a 10-point scale (having a midpoint of 5.5) had a mean response of 9.51. Thus it appears that regardless of the scale used, the average value of responses was approximately one standard deviation above the scale midpoint. This suggests that more than 80% of responses were positive toward the modified calendar. For questions worded as "agree/disagree," the mean percentage of agreement with a statement that was positive toward modified calendars (or disagreement with a statement negative toward modified calendars) was 67.74% ($SD = 17.57$).

Moderator Analyses

As previously described, we standardized the various response scales and included them together in analyses to provide an indication of factors that influenced participants' attitudes toward modified school calendars. However, because of missing data, only a handful of moderator variables had more than a dozen data points. We confined our analyses to these variables.

Response Rate

The first moderator that we examined was the response rate to the survey. In the 21 independent samples for which response rates were reported, we found no relationship between how positive the average attitude was and the percentage of contacted people who responded, $r(19) = .07$, *ns*.

34

Evaluating Agent

A trend was found between how positive respondents were toward modified calendars and whether the evaluating agent was internal or external to the school district conducting the survey. More positive attitudes toward modified calendars tended to be expressed when the evaluator worked for the school district, $r(56) = .26, p < .06$.

Response Dimension

We examined whether the type of response dimension being assessed had an impact on participants' expressed feelings or beliefs about modified school calendars. We included in this analysis pre-implementation attitudes, typically measured by using a general assessment of (a) a favorable or opposing view of modified calendars, or (b) a positive or negative impression of modified calendars. As with the achievement meta-analysis, each independent sample could provide one data point to each dimension. If a sample had more than one question that tapped the same dimension, we averaged the answers before conducting the analysis. Thus the 180 independent samples resulted in 277 data points.

We found a significant difference between the eight response dimensions being assessed, $F(7, 269) = 12.79, p < .05$. Post hoc LSD (least significant difference) t tests suggested that pre-implementation attitudes ($M = -.41, SD = .82$) were significantly more negative than post-implementation assessments of satisfaction ($M = .40$, $SD = .79$), achievement-related attitudes ($M = .22, SD = .84$), enthusiasm ($M = .17$, $SD = 1.08$), family concerns ($M = .11, SD = .68$), and views on intersession issues ($M = .12, SD = .90$). Pre-implementation attitudes were more negative but not significantly more negative than those on logistical concerns ($M = .004, SD = .67$) and student issues ($M = -.15, SD = .82$). Among the post-implementation responses, those regarding satisfaction were significantly more positive than those regarding logistical concerns and student issues. Because the pre-implementation attitudes were theoretically distinct from the other categories of responses (because the participants had not yet experienced the modified calendar), those attitudes were removed from the remaining analyses.

Participant and Question Referent

We examined whether (a) the participant filling out the survey, and (b) the person the survey question referred to influenced attitudes toward modified calendars. Neither the participant, $F(6, 109) = 1.15, ns$, nor the question referent, $F(5, 147) = .88, ns$, was significantly related to attitudes.

Program Size

We examined the relationship between attitudes toward modified calendars and three indexes of the size of the calendar program. Correlations indicated that modified calendar programs that were implemented with more students, $r(35) = -.36, p < .03$, and more schools, $r(37) = -.37, p < .03$, were associated with less positive attitudes toward the calendar. A negative but nonsignificant relationship was also found between attitudes and the number of schools involved in the program, $r(45) = -.26$, $p < .08$.

Community Size

There was no relationship between the size of a community offering the modified school calendar and respondents' attitudes toward the program, $r(44) = -.01, ns$.

Program History

We found no relationship between the number of years that a modified calendar had been in operation and respondent attitudes, $r(37) = -.11$, ns. However, respondents expressed more positive attitudes toward programs that had more recently begun operating, $r(38) = .32$, $p < .05$.

Program Characteristics

Although more positive attitudes were expressed by respondents from districts using single-track, as opposed to multi-track, modified calendars, the difference proved nonsignificant, $r(40) = -.20$, ns. Likewise, more positive attitudes were expressed by respondents in multi-tracked districts where parents were able to choose which track their child would attend than in districts not providing that option, but the relationship was not significant, $r(12) = .30$, ns. The type of calendar schedule, or number of vacations, was unrelated to attitudes, $r(40) = .02$, ns.

Respondents in districts that had intersession activities available for students expressed more positive attitudes toward modified calendars than respondents in districts where no intersessions were available, but the difference was not significant, $r(56) = .09$, ns.

It has been argued that a salient parental concern about modified calendars is availability of intersession activities for students. To test this notion, we divided samples so that we could test whether pre- and post-implementation attitudes were influenced differently by the availability of intersession activities. We conducted a multiple regression analysis that included both main effects and their interaction. As noted above, we found a significant main effect indicating that post-implementation attitudes toward modified calendars were more positive than pre-implementation attitudes, and we found a nonsignificant main effect for the availability of intersessions. A significant interaction effect also emerged, $F(1, 71) = 4.73$, $p < .04$. The underlying means suggested that the pre-implementation attitudes expressed by respondents in districts that had no intersession programs ($M = -.68$, $n = 13$) were considerably less positive than the pre-implementation attitudes of respondents in districts that had intersession programs ($M = .18$, $n = 6$) and that the latter differed little from post-implementation attitudes, whether from districts without ($M = .19$, $n = 28$) or with ($M = .26$, $n = 28$) intersession programs.

Student Characteristics

We assessed whether the grade level of students made a difference in respondents' attitudes. We separated samples that referred to Grade 5 or lower from samples that referred to Grade 6 or higher. The analysis revealed that participants' attitudes for the lower-grade samples were significantly more positive ($M = .30$, $SD = .58$) than those for the higher-grade samples ($M = -.74$, $SD = .69$), $t(45) = 4.02$, $p < .001$.

Finally, school districts that had a higher percentage of White students expressed more positive attitudes toward modified calendar schools than districts with higher percentages of minority students, $r(17) = .52$, $p < .04$.

Discussion

The Methodological Quality of School Calendar Studies

Before we summarize and discuss our findings, it is important to point out that the quality of evidence available on modified school calendars leaves much to be desired.

Most notably, it is virtually impossible to conduct an experimental study of calendar variations. To do so would require that students be randomly assigned to schools operating on modified and traditional calendars. Only a single circumstance would lend itself to the ethical use of random assignment: If a school district found itself with many more families wishing to enroll their children in schools on a modified calendar than could be accommodated, then a random lottery could be used to select students for enrollment in those schools. Students who wished to attend but were not chosen would serve as the control group. This type of natural experiment has never occurred in modified calendar research.

Of the studies that we reviewed, 59% made no attempt to improve the similarity of students who attended modified calendar schools and their traditional calendar counterparts, beyond choosing a similar school in the same school district. In these studies there is no way to determine whether achievement differences were due to the calendar variation, to differences among students that existed before the calendar intervention, to school characteristics unrelated to the calendar, or to interactions among these variables.

In a minority of the studies, matching or statistical techniques were used to help improve the similarity of modified and traditional calendar students. Among the studies that used a control to improve the equivalence of students, 14 of 17 controlled for more than one variable. Although patterns of control variables varied widely from study to study, student SES and ethnicity were the variables most commonly controlled, followed by student IQ and prior achievement.

Matching and statistical control can never completely eliminate concerns about differential selection into treatment groups. The most obvious concern is that groups may be undermatched. If important differences between groups are omitted from the matching characteristics, rival hypotheses remain plausible to explain effects otherwise attributable to calendar differences. Most important is the concern that undermatching can cause an intervention to appear more efficacious than it actually is. However, it is also possible that matching students by choosing them from groups that are initially very different can lead to *underestimation* of intervention effects. For example, let us assume that a school with generally low-achieving students is placed on a modified calendar and then an attempt is made to find matches among traditional calendar students from a high-achieving school. If students are matched on, say, prior achievement and SES, it is likely that students chosen from the low-achieving school will be drawn from the very top of that school's achievement distribution. Students from the high-achieving school will be drawn from the bottom of that school's distribution. Because the controlled variables are measured with error and not perfectly correlated with the outcome, statistical regression can occur that might make the modified calendar intervention look detrimental to student achievement (see Shadish, Cook, & Campbell, 2002).

Because of the weak research designs, it is simply not possible to make strong inferences about the effects of modified calendars. The question may arise, then, whether it makes sense to assay the research evidence at all. Our rationale for doing so is twofold. First, although the synthesis of results across studies will not cancel out consistent and pervasive design flaws, nevertheless, clear patterns that emerge across flawed bodies of evidence can be informative to the extent that the strengths of some studies compensate for the weaknesses of others. Second, we believe that poor data, if properly placed in context and carefully qualified, can be better than

no data at all. Every day, policymakers decide whether to institute modified calendars. The lack of evidence, tentative or otherwise, will not delay their need to make choices.

The Impact of School Calendar Variations on Achievement

Even within this weak inferential framework, the evidence from the meta-analysis revealed ambiguous results. We examined the data on modified calendars in three different ways. First, we did a vote count of 58 districts from which we were able to discern the direction of findings. Speaking generally, 62% of the districts reported that students in modified calendar programs outperformed students in traditional calendar programs. The estimated effect size from the vote count was $d = .04$.

Second, we calculated average effect sizes for 39 school districts that provided enough information to permit this analysis. The average unweighted effect size for the district-level comparisons was $d = .09$. The average d index was $d = .06$ when the effect sizes were weighted by the inverse of their variance.

Third, we found 17 school districts that attempted some statistical or matching control of pre-existing student differences and then compared student achievement in modified and traditional calendar schools. The average weighted effect size was very close to zero, $d = .01$, when a fixed-error model was used; it was somewhat higher, $d = .11$, when a random-error model was used. The higher estimate was significantly different from zero.

These most general results of our analyses are consistent with the findings of McMillen (2001), who examined statewide data collected on North Carolina students. If his results are weighted by sample size and averaged across program types and subject matters, his values also suggest effect sizes favoring modified calendars that are slightly lower than $d = .05$.

Comparing our results to Stenvall and Stenvall's (2001) analysis of California data is more problematic. These authors drew contradictory conclusions about their own data (in the preface and on p. 2). Furthermore, although they suggested that the poorer achievement of students in modified calendar schools in 1999 and 2000 might be due to confounded school characteristics, such as overcrowding and higher concentrations of poverty, the authors did not test those hypotheses. Again, although the percentage gain in achievement scores from 1999 to 2000 was higher for modified calendar schools than for traditional calendar schools, we assume that, in most instances, this was not the 1st year of operation for the modified calendar programs. Without more complete data, then, it is impossible to discern why the achievement differences persisted. They could have been the result of (a) initially larger but diminishing differences between schools on the different calendars, or (b) anomalous data on, or overestimation of, differences in achievement gains from 1999 to 2000.

What, then, might we infer from research on the effects of modified school calendars on achievement? First, school districts have slightly better than a 50% chance of finding that students in modified calendar schools outperform their counterparts in other schools. Second, the improvement in achievement scores is unlikely to be greater than .10 standard deviation, relative to the scores that would be expected had the students attended traditional calendar schools. Point estimates of this relative improvement, measured in multiple ways, tend to center around $d = .05$, or one twentieth of a standard deviation. However, the possibility that the improvement may be nonexistent cannot be ruled out.

38

Beyond the statistical vagaries of estimation, both proponents and opponents of modified school calendars will point to more substantive factors that suggest that the studies may misestimate the impact of this innovation. For example, proponents of modified calendars might suggest that studies underestimate the innovation's effects on achievement because students on modified calendars often have fewer days in school prior to testing. Because there are more vacation days for modified calendar schools during fall, winter, and spring, an achievement test given to all students on the same day in May or June will mean that modified calendar students have received less instruction during that academic year. This contention has some legitimacy but only in cases where all students begin the school year on the same day in the fall. For example, Stripling (1994) reported that students on the modified calendar in the Waco (Texas) Independent School District had 14 fewer days of instruction at the time of testing than the comparison group of students attending traditional calendar schools. In some school districts, by contrast, the modified calendar schools begin the year before traditional schools do; this circumstance may serve either to equalize the instructional time of the two groups of students or to lend an instructional-time advantage to students attending schools on a modified calendar. In any case, we found no study that explicitly controlled for the number of days that school had been in session before achievement outcomes were measured.

Proponents also might assert that most studies looked at the impact of school calendars for only a single academic year. If the causal mechanism underlying the impact of the modified calendar is that it mitigates summer learning loss, then it is reasonable to expect that the effect would be cumulative over multiple years of exposure. For example, students in elementary schools will be exposed to the modified calendar for 5 or 6 years. Therefore, over the course of an elementary school education the impact of the modified calendar might be .25 standard deviation or more.

We found two studies that permitted the assessment of cumulative effects. Kneese (2000b) compared the fall and spring scores of students in modified and traditional calendar schools during the students' 2nd and 3rd years in the modified calendar program. She found significantly greater gains favoring the modified calendar during students' 2nd year in the program but not during the 3rd year. Stripling (1995) compared achievement in reading and mathematics for students on a modified calendar with that for students on a traditional calendar. She provided an examination of cumulative effects by further breaking out modified calendar students into two groups: those who had been on the modified calendar for only 1 year and those who had been on the modified calendar for 2 years. As in Stripling's study from the year before (1994), students on the modified calendar received less instruction than did their traditional calendar counterparts (13 days less).[6] The results suggested that, in comparison with traditional calendar students, students who had been on the modified calendar for 1 year had slightly more favorable achievement levels ($d = +.07$), and those who had been on the modified calendar for 2 years had slightly less favorable achievement levels ($d = -.11$). Two more studies addressed the issue of the accumulation of effects but did not permit an effect size estimate. Bechtel (1991) compared yearly gain scores for students who had been on modified calendars for 1, 2, or 3 years with the scores of students who had been on traditional calendars. The results suggested diminishing gains over time. That is, students who had been on a modified calendar for only 1 year showed larger gains than students who had been on a modified calendar for 2 years, and students who had been on modified

calendars for 2 years showed larger gains than students who had been on modified calendars for 3 years. The gain scores of students who had been on modified calendars for 3 years were not significantly different from the scores of traditional calendar students. Cason (1995) found an opposite effect. She compared standardized achievement test scores for three modified calendar schools and three traditional schools 1 and 2 years after implementation of the modified calendar. Results were based on an analysis of school-level data (and thus were excluded from our effect size database, which contained only student-level data). The data revealed that reading and math scores of modified calendar schools improved significantly after the 2nd year on the modified calendar, relative to traditional calendar schools.

We examined whether the number of years that a school had been operating under a modified calendar had an influence on the calendar effect. This measure might also be related to the issue of cumulative effects. It is not a precise test because we must assume that students in programs operating for multiple years have, in fact, been exposed to the modified calendar for more than 1 year. Clearly, this would not always be the case. However, it is certainly the case that schools testing the effect of modified calendars after multiple years of operation include more students with longer exposures than schools testing after just 1 year of operation. We found no difference in effects on achievement related to years of operation. Inspection of the average effect sizes suggested that effects were larger in schools operating the program for more years. Of course, this (nonsignificant) finding might also be interpreted to mean that experience improves programs, in which case the finding reveals little (if anything) about accumulating effects.

Opponents of modified calendars have argued that the availability of intersessions, rather than multiple short vacations, could be the causal mechanism underlying any apparent calendar effect. They might claim that added instructional time, rather than elimination of the long summer break, drives the marginally positive impact of the modified calendar. This argument is difficult to assess because students on traditional calendars have access to summer school programs just as students on modified calendars have access to added instruction during intersessions. Summer school can have a significant impact on achievement test scores (Cooper et al., 2000). We know of no study that measured or controlled for students attending intersessions or summer school.

In our meta-analysis, 15 studies did explicitly indicate that intersessions were available. These were compared with studies that did not mention intersessions and may or may not have included schools that offered them. This rough test revealed a significant difference favoring districts that offered intersessions under fixed-error assumptions when adjustments for methodology were not made. In all other analyses, districts offering intersessions reported slightly higher effect sizes, although those effects were not statistically significant.

District, Student, and Program Variations That Might Influence Calendar Effects

Generally speaking, few moderators of the overall calendar effect revealed influences that were robust across our four tests. The most reliable moderator was the socioeconomic makeup of the community served by a modified calendar school. On average, students from poorer communities attending modified calendar schools outperformed their traditional calendar counterparts by about .20 standard deviation.

This difference was statistically significant for both adjusted and unadjusted effect sizes under both fixed- and random-error assumptions. We could not test whether a student's achievement level was a moderator of calendar effects, but McMillen (2001) found that lower-achieving students in modified calendar schools outperformed their traditional calendar counterparts, though his estimate of the effect was smaller than our SES effect. Taken together, these findings strongly suggest, as proponents have argued, that the modified calendar has its greatest impact on students struggling in school or students from disadvantaged homes.

The positive impact of modified calendars for disadvantaged students appears real, but it is important to point out that in many instances the adoption of modified calendars in poorer school districts has not been based on that achievement effect. Rather, modified calendars have been adopted primarily to accommodate demographic changes. The evidence suggests that modified calendars are applied disproportionately among some disadvantaged populations, for example, the poor and the English language learners (see Quinlan, George, & Emmett, 1987). If this is so, the argument could be made that modified calendars are being used as a substitute for provision of equal-quality facilities to some populations (see Orellana & Thorne, 1998).

Two moderating variables revealed significant effects for both adjusted and unadjusted effect sizes when fixed-error assumptions were used and random-error assumptions were not used. These effects are worth noting but are of limited generalizability and therefore should not be taken as strong indicators of what might be revealed in future studies. First, suburban and rural modified calendar programs revealed larger effects than urban programs. Programs implemented in small communities may have been more successful because patrons had more input into their implementation. This explanation is suggested by the results of attitude surveys, to be discussed shortly. Second, effect sizes from single-track schools were larger than those from multi-track schools. There are few reasons to expect that the instruction received by students in single- and multi-tracked systems would be very different. Multi-tracking does add some uncertainty to school life, as classrooms often shift location. Multi-tracking also can strain teaching specialists' energies because they often are needed to teach year-round. Multi-track systems are typically more controversial, perhaps diminishing community and parent support. However, one clear difference between single- and multi-track schools is the justification for adopting their particular calendars. We could discern a rationale for adopting a modified school calendar in 19 of the 39 school districts in our database. In each case, there was a one-to-one relationship between the reasons given for adopting the modified calendar and the type of calendar adopted. Specifically, if the district was concerned about overcrowding, a multi-track calendar was adopted. However, if district personnel believed modified calendars to be beneficial academically, a single-track calendar was adopted. Thus, with respect to the differential effects obtained for single-track schools in comparison with multi-track schools, there is no way to tease apart selection effects that may be associated with district rationales for calendar adoption and the "true" academic effects of the calendars (if any).

Finally, the analyses examining the impact of student grade levels on calendar effects revealed significantly better achievement for elementary than for secondary students on modified calendars only for unadjusted effect sizes when fixed-error assumptions were used. However, in three of the four analyses, the positive impact of the modified calendar was significantly different from zero for elementary school

students, centering around $d = .09$. For secondary students, the effect was not significantly different from zero in any of the analyses, and the four estimates centered around $d = .00$. Thus, even though research suggests that summer learning loss is greater in later grades (Cooper et. al., 1996), it may be the case that younger students respond more positively to multiple, shorter breaks.

Attitudes Toward Modified Calendars

We found more than 50 school districts that had surveyed teachers, parents, students, administrators, and staff about their reactions to living with a modified school calendar. Results from these surveys can help policymakers to understand how the implementation process and features of modified calendars might influence public acceptance of the programs.

First, it is clear that respondents overwhelmingly described the experience of a modified calendar as positive, with average responses about one standard deviation above the scale midpoint. In other words, more than 80% of responses were on the positive side of the scale. Respondents not only expressed general satisfaction but also felt that the modified calendar had a positive effect on student achievement. Furthermore, there was no evidence that the positive attitude was a function of sampling bias.

It was clear that post-implementation attitudes were more positive than pre-implementation attitudes. However, much of the pre-implementation concern disappeared if preplanning included making available intersession activities for students out of school. Also, in districts where multi-tracking was used, attitudes were more positive when parents could choose their child's track. These results suggest that policymakers will find greater initial public acceptance of the modified calendar if they preplan to ensure that parents have options regarding when vacations will occur and how their children can spend vacations. The importance of flexibility and community input may underlie the findings suggesting that small programs (serving fewer students in fewer schools) were associated with more positive attitudes.

Attitudes were also more positive, but not significantly so, in single-tracked districts. This finding could be explained by the concern, often voiced by opponents of modified calendars, regarding placement of siblings and friends on different vacation schedules. Similarly, participants' attitudes in the elementary school samples were significantly more positive than in the secondary school samples. This may relate to concerns about the greater disruption that after-school activities under modified calendars can create for older students, many of whom participate in extracurricular activities or have after-school jobs.

Although our results suggested very positive attitudes toward modified calendars among participants, two findings might cause concern for proponents of modified calendars. Specifically, respondents expressed more positive attitudes toward programs that had more recently begun operating. No relationship was found between respondent attitudes and the number of years that a modified calendar had been in operation, but the nonsignificant relationship was negative ($r = -.11$). This finding may indicate that enthusiasm for some programs lessens as communities become more familiar with them. However, it is also possible that programs implemented more recently have incorporated lessons learned by their predecessors that permit them to avoid some negative participant reactions. It will take focused longitudinal research to critically test these two explanations.

Conclusion

Perhaps the clearest conclusion to be drawn from this synthesis is that a truly credible study of modified calendar effects has yet to be conducted. It would be difficult to argue with policymakers who choose to ignore the existent database because they feel that the research designs have been simply too flawed to be trusted. Furthermore, the cumulative result of past studies is so close to a chance outcome that the argument that poor designs have led to random findings remains plausible. As noted above, the question of how school calendars affect learning cries out for a longitudinal study involving a natural lottery, with controls for testing date and availability of intersessions and summer school, among other experimental and measured controls.

If the results of our synthesis are informative, they suggest a modified calendar effect on achievement that is quite small (approximately .05 standard deviation) relative to other effects associated with educational interventions. For example, Lipsey and Wilson (1993) compiled the results of 302 meta-analyses across the fields of education, mental health, and organizational psychology. The mean d index across all of these meta-analyses was $d = .50$. Of 180 education meta-analyses, only 12 revealed effects lower than $d = .10$. Lipsey and Wilson concluded that, "in assessing meta-analytic estimates of the effects of psychological, educational, and behavioral treatment, we cannot arbitrarily dismiss statistically modest values (even 0.10 or 0.20 SDs) as obviously trivial" (p. 1199). Clearly, Lipsey and Wilson would place the revealed effect of modified calendars in the "trivial" range. Furthermore, Cooper et al. (2000) found larger effects for summer school programs, with an overall d index of .20, than we found here for modified calendars. Their finding would imply that districts seeking effective remedial programs as well as solutions to summer learning loss should consider all possible interventions. However, direct comparisons between studies of any two specific interventions need to be interpreted in light of confounded differences in comparison groups and program designs.

Thus it would be inappropriate to suggest that the current evidence indicates that modified calendars have a significantly positive impact on achievement, in the practical sense. With hindsight, however, such a finding should not be surprising. When Cooper et al. (1996) performed a meta-analysis on the studies of summer learning loss, they concluded that achievement test scores in fall were approximately .10 standard deviation lower than test scores from the preceding spring. Thus, assuming that modified calendars operate primarily to diminish summer learning loss, .10 standard deviation would be the largest possible effect.

Proponents of modified calendars can find encouragement in a few findings of our synthesis. First, a case can still be made that the effect of calendar modification on achievement is cumulative. A well-designed longitudinal study is needed to test this hypothesis. Second, there is evidence that modified calendar programs do noticeably improve achievement for economically disadvantaged or poor-achieving students. Third, programs implemented more recently may be showing improved results. And, finally, it is clear that the students, parents, and staffs that participate in modified calendar programs are overwhelmingly positive about the experience. There are also specific actions that policymakers can take—such as involving the community in planning a program and providing quality intersession activities—that can improve community acceptance.

Elsewhere, we have argued that the adoption of school calendars in the past has been driven primarily by economic factors rather than by educational implications

(Cooper, 2002). There is no reason to believe that this circumstance will change in the future. As we noted earlier, today's calendar became dominant when the livelihoods of most Americans were tied to the farming cycle. This is no longer the case. American families and occupations are now characterized by enormous variation. Eventually, the shift in business and family economics evident in the late 20th and early 21st centuries will lead to a shift in the school calendar that fits better with the way most Americans live and work. Thus, modified school calendars with educational activities offered during intersessions are also likely to grow in popularity, however slowly. As Cooper (in press) observed, "The history of school calendars in the United States suggests that only innovations that consider local and national economics and the politics of family time along with the education of children will have a chance to succeed."

Notes

[1]Variance estimates for d indexes were calculated with formulas given in Cooper (1998), using as a base the total number of students in the analysis, including both traditional and modified calendar schools. When we calculated d indexes associated with categories of moderator variables, we calculated only weighted averages. The weighting procedure gives greater weight to effect sizes based on larger samples and is the generally preferred alternative among researchers.

[2]The use of shifting units of analysis retains as many data as possible from each study while holding to a minimum any violations of the assumption that data points are independent. Also, because effect sizes are weighted by sample size in the calculation of average d indexes, a study with many separate samples containing just a few students each will not have a larger effect on average d indexes than a study with only a single, or a few, large separate samples.

[3]This approach requires us to assume that the scale chosen for a survey by the researchers was unrelated to the attitudes of respondents.

[4]One study (Herman, 1991) did not identify the school district under study. The district was described by the author as large and urban, and the issuing institution was located in a school district that was already represented in our database. Therefore, we did not include the study in our analysis. The results of the study were mixed: California Test of Basic Skills (CTBS) reading scores favored traditional calendar students, CTBS math scores favored modified calendar students, and the California Assessment Program results favored traditional calendar students in both reading and math.

[5]One study, by Powers (1974), included measures of school attitudes that permitted the calculation of effect sizes but was not included in our analysis of the achievement-related data. Powers compared changes in school-related attitudes among 291 students during 1 year at a school-within-a-school program in Virginia Beach, Virginia. He found that attitude toward school did not change among students in the school-within-a-school

modified calendar program and that attitude deteriorated among students in the same school on the traditional calendar ($d = .38$). Powers also measured students' attitude toward learning and found no differences in the rate of change between the two calendar types ($d = .02$).

[6]Stripling (1995) notes that "principals of all but one of the elementary campuses reported an average of 10 days of informal instruction on . . . reading and mathematics objectives during the October intersession, resulting in a net loss of 3 days instruction for YRE" (p. 3). However, this statement would be valid only if all students in the modified calendar schools received the extra instruction. The extent of intersession participation cannot be gleaned from the report.

References

In the following list, * = study included in the achievement meta-analysis, # = study included in the attitudes meta-analysis, and † = study included in both the achievement and the attitudes meta-analyses.

†Abney, E., & Blasik, K. (1996). *Year Round Schools*. Lauderdale Lakes, FL: School Board of Broward County.

Alexander, P., Entwisle, D. R., & Olson, L. S. (in press). Schools, achievement, and inequality: A seasonal perspective. In G. Borman & M. Boulay (Eds.), *Summer learning: Research, policies, and programs*. Mahwah, NJ: Erlbaum.

#Allen Independent School District. (1992). *Results of a teacher survey on alternative calendars*. Allen, TX: Author.

#Allred, C. E. (1995). *Attitudes toward implementing a year-round school calendar for the Anderson County schools*. Unpublished bachelor's thesis, Tusculum College.

†American Lakes Year-Round School Steering Committee. (1991, January). *Year-round school status*. Unpublished manuscript. Sacramento, CA: Natomas Union School District.

#Antioch Unified School District. (1993). *Parent and Teacher Survey: Year-Round Education*. Antioch, CA: Author.

†Arnove, R. F., Ettinger, L., Franz, S., Rolle, A., Schonemann, N., & Vesper, N. (1995). *An independent evaluation of the ABC at the Harney Elementary School: A final report to the School Board of the Lebanon Community School Corporation*. Bloomington: Indiana University, School of Education.

#Ashburn, E. W. (1974). *Survey of student and teacher attitudes toward the 45-15 pilot program*. Virginia Beach, VA: Office of Research, Planning and Development of the Virginia Beach City Public Schools. (ERIC Document Reproduction Service No. ED109160)

Association of California School Administrators. (1988). *A primer on year-round education*. Sacramento, CA: Author.

#Audubon Elementary. (1991). *Staff Opinion Survey*. Unpublished school report.

†Baker, G. (1990). *Parent satisfaction with year-round and traditional school calendars in Conroe Independent School District*. Unpublished master's thesis, Sam Houston State University.

#Barrett, T., Ferrett, R., & Beaty, C. L. (1992). *Results of the year-round education parent, staff and student surveys*. Riverside, CA: Riverside United School District. (ERIC Document Reproduction Service No. ED358562)

*Barron, R. (1993). The effects of year-round education on achievement, attendance and teacher attendance in bilingual schools (Doctoral dissertation, Northern Arizona University, 1993). *Dissertation Abstracts International, 54-11*, 3935.

*Bechtel, R. E. (1991). A study of academic growth in third-grade students and its relationship to year-round education (Doctoral dissertation, Pepperdine University, 1991). *Dissertation Abstracts International, 52-07*, 2404.

45

#Berger, E. (1975). Some 45-15 drawbacks: The teacher's view. *Phi Delta Kappan, 56*, 420–421.

*Boyles, B. (1993, March). [Achievement levels in Mooresville City Schools, Grades 3–5]. Unpublished raw data.

Bradford, J. C., Jr. (1995, January). *Year-round education: Impact on support services, transportation, operation, facilities, and maintenance.* Paper presented at the annual meeting of the Association of School Business Officials of Maryland and Washington, DC, in Arnold, MD.

Brady, A. (1996). *Year-round vs. traditional third- to fifth-grade growth study.* Rockingham County, NC: Rockingham County Consolidated School District.

*Brekke, N. R. (1984). Year-round education: Cost saving and educationally effective. *ERS Spectrum, 2*(3), 25–30.

#Brekke, N. R. (1986). *A cost analysis of year-round education in the Oxnard School District.* Paper presented at the annual meeting of the National Council on Year-Round Education, Oxnard, CA. (ERIC Document Reproduction Service No. ED272973)

*Caddo Parish School Board. (1995, April). *Evaluation of the year round education program at Newton Smith and Shreve Island elementary schools.* Shreveport, LA: Author.

#Calgary Board of Education. (1997). *Modified school year calendar survey results.* Calgary, Alberta, Canada: Author.

†Campbell, W. D. (1994). Year-round schooling for academically at-risk students: Outcomes and perceptions of participants in an elementary program. *ERS Spectrum* (3), 20–24.

*Carroll, C. (1997, March). [Achievement levels in Mooresville City Schools, Grades 3–5]. Unpublished raw data.

*Cason, C. B. (1995). The impact of year-round school on student achievement, student/teacher attendance, and discipline (Doctoral dissertation, University of Alabama at Birmingham, 1995). *Dissertation Abstracts International, 56-11*, 4262.

#Cecchetti, A. (1990). *Lodi Unified School District program description.* Lodi, CA: Lodi Unified School District.

*Chen, Z. (1993). Year-round education: High school student achievement and teacher/administrator attitudes (Doctoral dissertation, United States International University, 1993). *Dissertation Abstracts International, 55-02*, 0260.

#Cherry Creek Schools. (1989). *Four-track year-round school survey.* Englewood, CO: Cherry Creek School District.

#Christie, S. (1989). A report on opinion surveys of parents, students, and staff of four-track year-round schools in Cajon Valley. El Cajon, CA: Cajon Valley Union School District (ERIC Document Reproduction Service No. ED303888)

*Chula Vista Elementary School District. (1991, March). *Comparison of test scores: Traditional and year-round schools, Grade 3 and 6 C.A.P. and S.A.T. results.* Chula Vista, CA: Author.

Cohen, J. (1988). *Statistical power analysis in the behavioral sciences.* Hillsdale, NJ: Erlbaum.

Coleman, R. W., & Freehorn, C. L. (1993). *A comparative study of multi-track year-round education and the use of relocatables.* Paper presented at the annual meeting of NAYRE, San Diego, CA.

#Collins, B. (1992). *Evaluation report of Mueller school multi-track program.* Chula Vista, CA: Chula Vista Elementary School District.

*Consolie, P. G. (1999). Achievement, attendance, and discipline in a year round elementary school (Doctoral dissertation, University of Georgia). *Dissertation Abstracts International, 60-11*, 5830.

Cooper, G., & Sweller, J. (1987). Effects of schema acquisition and rule automation on mathematical problem-solving transfer. *Journal of Educational Psychology, 79*, 347–362.

Cooper, H. (1998). *Synthesizing research: A guide for literature reviews* (3rd ed.). Thousand Oaks, CA: Sage.

Cooper, H. (in press). Is the school calendar dated? Education, economics, and the politics of time. In G. Borman & M. Boulay (Eds.), *Summer learning: Research, policies and programs.* Mahwah, NJ: Erlbaum.

Cooper, H., Charlton, K., Valentine, J. C., & Muhlenbruck, L. (2000). *Making the most of summer school.* Malden, MA: Blackwell.

Cooper, H., & Hedges, L. V. (1994). *Handbook of research synthesis.* New York: Russell Sage.

Cooper, H., Nye, B., Charlton, K., Lindsey, J. J., & Greathouse, S. (1996). The effects of summer vacation on achievement test scores: A narrative and meta-analytic review. *Review of Educational Research, 66*(3), 227–268.

*Costa, J. S. (1987). Comparative outcomes of the Clark County School District year-round and nine-month schools (Doctoral dissertation, University of Nevada at Las Vegas, 1987). *Dissertation Abstracts International, 48-10,* 2495.

*Coulter, S. (1997, December). *Year-round schools report, 1996–1997* (Report No. 12197). Fort Worth, TX: Fort Worth Independent School District, Department of Research and Evaluation.

#Crane Elementary School District. (1991). *Summary of year-round school survey.* Yuma, AZ: Author.

#Deason, D. R. (1975). A study of teacher attitudes in secondary 45-15 year-round school programs (Doctoral dissertation, United States International University, San Diego, 1975). *Dissertation Abstracts International, 36-06,* 3271.

*DeLaro, F. P. (1994). *An analysis of student achievement in year-round and traditional calendar elementary schools.* Unpublished doctoral dissertation, University of Houston.

*Dunn, E. R. (1996). The effect of calendar configuration on elementary students' achievement gains (Doctoral dissertation, Baylor University, 1996). *Dissertation Abstracts International, 57-10,* 4200.

†Edenton-Chowan Schools. (1997, October). *Assessment of year-round and traditional school models.* (Available from Edenton-Chowan Schools, 113 East King Street, Suite 300, Edenton, NC 27932)

*Eisele, J. G. (1998). *A program evaluation of two Washington elementary schools following a modified school calendar.* Unpublished doctoral dissertation, Seattle Pacific University.

*Elsberry, J. S. (1992). An evaluation of the implementation of year-round education (Doctoral dissertation, University of Texas at Austin, 1992). *Dissertation Abstracts International, 53-12,* 4146.

#Ernster, L. (1997). *Longfellow elementary school comment survey and calendar.* Rochester, MN: Rochester School District 535.

*Espinosa, D. (2000). *A comparison of Iowa Test of Basic Skills scores of students in year-round schools versus non-year-round schools.* Unpublished doctoral dissertation, University of Illinois at Urbana-Champaign.

*Färdig, D. (1991). *Year-round education program evaluation.* Orlando, FL: Orange County Public Schools.

Farley, R. (1996). *The New American Reality.* New York: Russell Sage Foundation.

#Farmington Public Schools. (1992). Year-round education: Another educational choice for Farmington. Farmington Hills, MI: Author.

*Fass-Holmes, B., & Gates, K. E. (1994, July). *Report of single-track year round education in San Diego Unified School District* (Report No. 689). San Diego, CA: San Diego City Schools Planning, Assessment, and Accountability Division.

#Fort Craig School of Dynamic Learning. (1996). *Results of Fort Craig School of Dynamic Learning parent survey.* Maryville, TN: Author.

#Fort Knox Community Schools. (1995). *Fort Knox Schools Survey Data.* Fort Knox, KY: Author.

#Gandara, P. (1992). Extended year, extended contracts: Increasing teacher salary options. *Urban Education, 27,* 229–247.

*Gandara, P., & Fish, J. (1994, April). Year-round schooling as an avenue to major structural reform. *Educational Evaluation and Policy Analysis, 16,* 67–85.

#Gardiner, L. (1988). *Year-round update.* Salt Lake City, UT: Salt Lake City School District.

Gold, K. M. (2002). *School's in: The history of summer education in American public schools.* New York: Peter Lang.

#Goldsboro Elementary School. (1992). *Staff survey: YRE/modified school calendar.* Sanford, FL: Author.

*Gorsuch, J. W. (1997). *A comparison of growth in student achievement for students enrolled in year-round and traditional school year programs at Black Mountain primary and elementary schools.* Asheville, NC: Buncombe County Schools Testing Office.

Greenhouse, J. B., & Iyengar, S. (1994). Sensitivity analysis and diagnostics. In H. Cooper & L. V. Hedges (Eds.), *Handbook of research synthesis* (pp. 383–398). New York: Russell Sage.

#Grossmont Union High School District. (1972). *Year-round school articulation and compatibility study: Vol. 2. Research reports conducted for the project.* San Diego County Department of Education, La Mesa, CA. (ERIC Document Reproduction Service No. ED113826)

*Haenn, J. F. (1996, April). *Evaluating the promise of single-track year-round schools.* Paper presented at the annual meeting of the American Educational Research Association, New York.

#Haldeman, J., Harney, K., & Kadlub, C. (1991). *A phase II report by the Superintendent's Study Group.* Las Vegas, NV: Clark County School District.

*Hasselmeier, J. (1997). *Traditional vs. year-round education on academic achievement.* Unpublished master's thesis, University of Houston, Clear Lake.

*Heaberlin, R. L. (2000). The effects of year-round education on elementary school students (Doctoral dissertation, University of Georgia, 2000). *Dissertation Abstracts International, 61–08,* 3009.

Hedges, L. V., & Olkin, I. (1985). *Statistical methods for meta-analysis.* Orlando, FL: Academic Press.

Hedges, L. V., & Vevea, J. L. (1998). Fixed and random effects models in meta-analysis. *Psychological Methods, 3,* 486–504.

#Heisinger, K. (1994). *Year-round education in California: Problems, practices and promises.* Unpublished master's thesis, San Jose State University.

Herman, J. L. (1991). Novel approaches to relieve overcrowding: The effects of Concept 6 year-round schools. *Urban Education, 26,* 195–213.

#Hill, B. (1980). The attitude of patrons toward year-round school in the Hillsboro, Oregon, Union High School District (Doctoral dissertation, Brigham Young University, 1980). *Dissertation Abstracts International, 41-05,* 1864.

#Hoffman, C., Wallace, D. L., & Reglin, G. (1991). *Attitudes of teachers concerning year-round school programs.* Unpublished report. (ERIC Document Reproduction Service No. ED335335)

#Housden, T., & Holmes, L. (1981). *Mesa Verde—A year-round high school: A descriptive report of 1980–81.* Carmichael, CA: San Juan Unified School District. (ERIC Document Reproduction Service No. ED218727)

Jamar, I. (1994). *Fall testing: Are some students differentially disadvantaged?* Pittsburgh, PA: University of Pittsburgh Learning Research and Development Center.

48

*Johnson, N. P. (1984). The effects of year-round school programs on pupil achievement in selected schools in the Los Angeles Unified School District (Doctoral dissertation, Pepperdine University, 1984). *Dissertation Abstracts International, 45-10*, 3601.

Karweit, N. (1985). Should we lengthen the school term? *Educational Researcher, 14*, 9–15.

Katsiyannis, A. (1991). Extended school year policies: An established necessity. *Remedial and Special Education, 12*, 24–28.

*Kneese, C. (1996a). Review of research on student learning in year-round education. *Journal of Research and Development in Education, 29*, 60–67.

*Kneese, C. (1996b). *Impact of the year-round calendar on student achievement in Alameda Unified School District.* Unpublished manuscript.

*Kneese, C. (2000a). *Year-round learning: A research synthesis relating to student achievement.* San Diego, CA: NAYRE.

*Kneese, C. (2000b). The impact of year-round education on student learning: A study of six elementary schools. *ERS Spectrum, 18*(1), 20–26.

Kneese, C., & Knight, S. (1996). Evaluating the achievement of at-risk students in year round education. *Planning and Changing: An Educational Leadership and Policy Journal, 26*, 71–90.

*Kuner-Roth, B. (1985). A comparison of academic achievement of students in a year round school district with a conventional school district (Doctoral dissertation, Temple University, 1985). *Dissertation Abstracts International, 47-03*, 0776.

#Leon County Schools. (1996). *Spring 1996 year-round-education survey: Student, parent and teacher results.* Tallahassee, FL: Author.

#Levy, E. (1997). *Year-round education program review.* Dry Creek Joint Elementary School District. Roseville, CA: California Department of Education.

Lipsey, M. W., & Wilson, D. B. (1993). The efficacy of psychological, educational, and behavioral treatment: Confirmation from meta-analysis. *American Psychologist, 48*, 1181–1209.

†Lomax, V., McNelis, M., & Wallace, B. (1996). *Evaluation of the changes at Caldwell Elementary.* Memphis, TN: Memphis City Schools, Office of Research and Evaluation.

†Management and Evaluation Associates, Inc. (1996). *Year-round education program, Year 1: 1995–1996.* Hightstown, NJ: Author.

*Marr, C. (1989). Year-round school and student achievement (Doctoral dissertation, Northern Arizona University, 1989). *Dissertation Abstracts International, 51-01*, 0124.

Matsui, B. I. (1992). Comparison of year round vs. traditional sequences on selected achievement variables (Doctoral dissertation, University of Southern California, 1992). *Dissertation Abstracts International, 53-12*, 4156.

*Matty, E. J. (1978). The 45-15 year-round school: An evaluation of first-year algebra achievement of selected ninth grade students (Doctoral dissertation, University of Arizona, 1978). *Dissertation Abstracts International, 39-02*, 0804.

Mazzarella, J. A. (1984). Longer day, longer year: Will they make a difference? *Principal, 63*, 14–20.

*McCraley, T. L. (1975). An experimental study of the 45-15 year-round school organizational plan and the traditional nine-month school organizational plan (Doctoral dissertation, Northern Arizona University, 1975). *Dissertation Abstracts International, 37-01*, 0074.

#McDaniel, W. J. (1976). Study of the attitudes of selected groups of citizens in Onondaga County toward the year-round school concept (Doctoral dissertation, Syracuse University, 1976). *Dissertation Abstracts International, 37-11*, 6879.

*McEwen, M., Johnson, H., & Owens, M. (1995, June). *Alternative school calendar and improved student performance: An evaluation performed at Friendswood ISD*

for Grades 4–6. Paper presented at the Fifth Annual Conference of the Texas Chapter of NAYRE, Houston, TX.

McMillen, B. J. (2001). A statewide evaluation of academic achievement in year-round schools. *Journal of Educational Research, 95,* 67–74.

#Merrell, R. G. (1980). *Some alternatives to school building construction.* Salt Lake City, UT: Utah State Board of Education. (ERIC Document Reproduction Service No. ED197484)

*Miranda, F. A. (1993). The effects of year round education on limited English proficient (LEP) students in high schools (Doctoral dissertation, University of La Verne, 1993). *Dissertation Abstracts International, 54-05,* 1750.

*Mitchell, R. E., & Mitchell, D. E. (1999). *Student segregation and achievement tracking in year-round schools.* Manuscript submitted for publication.

*Moortgat, L. R. (1976). Year-round school: Feasibility and effects of the 45-15 plan (Doctoral dissertation, Wayne State University, 1976). *Dissertation Abstracts International, 37-11,* 6856.

#Myers, C. G. (1996). *Report on year-round education.* Ukiah, CA: Ukiah Unified School District.

National Association for Year-Round Education. (2000). *Twenty-seventh reference directory of year-round education programs for the 2000–2001 School Year.* San Diego, CA: Author.

National Association for Year-Round Education. (2002). Specializing in time and learning. Retrieved July 19, 2002, from *http://www.nayre.org*

National Center for Education Statistics. (1996). *Digest of Education Statistics, 1996.* Washington, DC: U.S. Department of Education.

National Education Commission on Time and Learning. (1994). *Prisoners of time.* Washington, DC: Government Printing Office. (ERIC Document Reproduction Service No. ED 366115)

†O'Neal, S., Alexander, B., Seidenwurm, J., & Weil, K. (1991). *Year-round education: The second year.* Albuquerque, NM: Albuquerque Public Schools.

Orellana, M. F., & Thorne, B. (1998). Year-round schools and the politics of time. *Anthropology and Education Quarterly, 29*(4), 446–472.

*Owens, E. W., & Waxman, H. C. (1996). *Evaluating the effectiveness of year-round education for working-class, suburban communities.* Paper presented at the annual meeting of the American Educational Research Association, New York.

#Owens, M. (1993). *Interim report, year-round education pilot program.* Friendswood, TX: Friendswood Independent School District.

Palmer, E. A., & Bemis, A. E. (1999). *Year-round education.* Retrieved October 2, 2001, from *http://www.extension.umn.edu*

*Paloczy, S. T. (1997). The impact of the elementary school year-round education (YRE) pilot program on student achievement and behavior in the Harlandale Independent School District (Doctoral dissertation, Texas A&M University, 1997). *Dissertation Abstracts International, 58-07,* 2539.

†Pelavin, S. H. (1979). *A study of year-round schools: Vol. 1. Final report.* Menlo Park, CA: Stanford Research Institute International. (ERIC Document Reproduction Service No. ED170926)

†Philo Middle School. (1998). *School within a school.* Winston-Salem, NC: Author.

*Potter Johnson, G. A. (1976). *A preliminary study of academic performance change of fifth grade students attending two different summer vacation patterns.* Unpublished doctoral dissertation, University of South Florida.

*Powers, D. (1974). *The Virginia Beach extended school year program and its effects on student achievement and attitudes: First year report* (Report No. ETS-PE-74-24). Princeton, NJ: Educational Testing Service. (ERIC Document Reproduction Service No. ED109184)

Quality Education Data. (2000). *School profiles.* Retrieved April 20, 2000, from *http://survey.qeddata.com/locator/*

Quinlan, C., George, C., & Emmett, T. (1987). *Year-round education, year-round opportunities: A study of year-round education in California.* Sacramento, CA: California State Department of Education, Special Studies and Evaluation Reports Unit. (ERIC Document Reproduction Service No. ED282272)

*Reece, J. L., Myers, C. L., & Nofsinger, C. O. (2000). Retention of academic skills over the summer months in alternative and traditional calendar schools. *Journal of Research and Development in Education, 33,* 166–174.

*Ricketts, A. R. (1976). An examination of year-round school in District #11 (Doctoral dissertation, University of Colorado, 1976). *Dissertation Abstracts International, 34-07,* 1887.

*Ritter, C. (1992). Effects of the year round school calendar on gifted and talented students. Master's thesis, Sam Houston State University, Huntsville, TX. (ERIC Document Reproduction Service No. ED350739)

*Roby, D. E. (1995). Comparison of a year-round school and a traditional school: Reading and mathematics achievement. *ERS Spectrum, 13,* 7–10.

Rosenthal, R. (1994). Parametric measures of effect size. In H. Cooper & L. V. Hedges (Eds.), *Handbook of research synthesis* (pp. 231–244). New York: Russell Sage.

†Russell, C. (1992). *College Park Elementary School: Year-round school evaluation.* College Park, GA: Fulton County Board of Education, Office of Planning, Research, and Development.

#Sardo-Brown, D., & Rooney, M. (1992, July). The vote on all-year schools. *The American School Board Journal,* 25–30.

Sargent, L. R., & Fidler, D. A. (1987). Extended school year programs: In support of the concept. *Education and Training in Mental Retardation, 22,* 3–9.

#Schechty Associates. (1973). *A survey of parent attitudes toward and perceptions about the Virginia Beach 45-15 pilot program.* Chapel Hill, NC: Virginia Beach City Public Schools. (ERIC Document Reproduction Service No. ED109159)

*Schoewe, R. K. (1993). Effects of elementary calendar experience on academic achievement and rate of attendance of high school graduates (Doctoral dissertation, University of Missouri, St. Louis, 1993). *Dissertation Abstracts International, 54-05,* 1750.

#Schwer, R. W., & Daneshvary, R. (1991). *Year-round schools: Issues and concerns of Clark County residents.* Las Vegas, NV: Clark County School District.

#Serow, R., Banks, K., & Reese, A. (1992). *Year-round education program: Evaluation report.* Raleigh, NC: Wake County Public School System. (ERIC Document Reproduction Service No. ED350073)

Shadish, W. R., Cook, T. D., & Campbell, D. T. (2002). *Experimental and quasi-experimental designs for generalized causal inference.* Boston: Houghton Mifflin.

*Shields, C. M. (1996, January). *Year-round education: Is it worth the hassle?* Paper presented at the University of British Columbia Robson Square Lecture Series, Vancouver.

Shields, C. M., & Oberg, S. L. (2000). *Year-round schooling: Promises and pitfalls.* Lanham, MD: Scarecrow Press.

#Smith, K. (1992). *A parent and teacher survey of attitudes toward year-round education in the Preston, Idaho, School District.* Unpublished master's thesis, Utah State University.

†Smith, M. L., & Glass, G. V. (1975). *Evaluation of year-round schools, Cherry Creek District 5: Evaluation report.* Boulder: Colorado University Bureau of Educational Field Services. (ERIC Document Reproduction Service No. ED145537)

*Sorensen, P. A. (1995). A study comparing 22 modified 45-15 year-round and traditional schools in Jordan School District on Stanford Achievement Test scores for years 1991–1993 (Doctoral dissertation, Brigham Young University, 1995). *Dissertation Abstracts International, 56-07,* 2550.

Stenvall, J. T., & Stenvall, M. J. (2001). *An analysis of 2000 API scores for California public schools on traditional and year-round calendars at elementary, middle and high school levels.* San Diego, CA: NAYRE.

Stenvall, M. (1999). *A checklist for success: A guide to implementing year-round schools.* San Diego, CA: NAYRE.

#Stetson and Associates. (1993). *A review of the alternative calendar education (ACE) program.* Deer Park, TX: Deer Park Independent School District.

*Stripling, R. (1994). The effectiveness and efficiency of year-round education in the Waco Independent School District, 1993–1994. Waco, TX: Waco Independent School District.

*Stripling, R. (1995). The effectiveness and efficiency of year-round education in the Waco Independent School District, 1994–1995. Waco, TX: Waco Independent School District.

SummerMatters!! (2001). *What research says.* Retrieved October 2, 2001, from *http://www.summermatters.com/reviews.htm*

*Texarkana Independent School District. (1992, October). *Year-round scores significantly higher than conventional* (TISD News). Texarkana, TX: Author.

#Thomas, J. D. (1995). *Attitudes on year-round school by fourth and fifth grade students* (Fundamentals of Educational Research, EDF 6481). Cocoa, FL: Brevard County Public School System.

Time To Learn. (1996). *Year-round schools may not be the answer.* San Antonio, TX: Author.

#Tompkins, L., & Weaver, K. (1994). *Project BEST: An evaluation. Special report to the Emporia USD 253 Board of Education.* Emporia, KS: Kansas State Department of Education.

Tukey, J. W. (1977). *Exploratory data analysis.* Reading, MA: Addison-Wesley.

Veatch, N. A. (1991). *An evaluation of the Orchard plan: A year-round efficient educational program.* Unpublished master's thesis, University of California, Davis.

*Woolley, K. (1996). *The effects of year-round school on the achievement and attendance of elementary school students.* Unpublished doctoral dissertation, Baylor University.

Worsnop, R. L. (1996). Year-round schools. *CQ Researcher, 6*(19), 433–456.

Zykowski, J. L., Mitchell, D. E., Hough, D., & Gavin, S. E. (1991). *A review of year-round education research.* Riverside, CA: California Educational Research Cooperative. (ERIC Document Reproduction Service No. ED330040)

Authors

HARRIS COOPER is a Professor in the Department of Psychological Sciences, University of Missouri, Columbia, 217 McAlester Hall, Columbia, MO 65211; e-mail *cooperh@ missouri.edu.* His areas of specialization are research synthesis methodology, homework, variations in school calendars, and how after-school activities relate to school achievement.

JEFFREY C. VALENTINE is a Research Assistant Professor in the Department of Psychological Sciences, University of Missouri, Columbia, 217 McAlester Hall, Columbia, MO 65211; e-mail *valentinejc@missouri.edu.* His areas of specialization are research synthesis methodology and the influences of affect, cognition, and use of nonschool time on achievement.

KELLY CHARLTON is an Assistant Professor of Psychology at the University of North Carolina, Pembroke, P.O. Box 1510, Pembroke, NC 28372; e-mail *kelly.charlton@uncp.edu.* Her areas of specialization are the social psychology of education and intergroup/intragroup processes.

APRIL MELSON graduated with honors from the University of Missouri, Columbia. She is the Education Coordinator for the orthopedic residency and fellowship programs at the Nemours Children's Clinic, 83 W. Columbia Street, Orlando, FL 32806.