Learning from Summer

Online Technical Appendixes

Catherine H. Augustine, Jennifer Sloan McCombs, John F. Pane,
Heather L. Schwartz, Jonathan Schweig, Andrew McEachin, Kyle Siler-Evans

Commissioned by
The Wallace Foundation
Supporting ideas.
Sharing solutions.
Expanding opportunities.

RAND
CORPORATION

For more information on this publication, visit www.rand.org/t/RR1557

Published by the RAND Corporation, Santa Monica, Calif.

Preface

Research has determined that low-income students lose ground to more affluent peers over the summer. Other research has shown that some summer learning programs can benefit students, but we know very little about whether large, district-run, voluntary programs can improve student outcomes among low-income students.

To fill this gap, The Wallace Foundation launched the National Summer Learning Project in 2011. This six-year study offers the first-ever assessment of the effectiveness of large-scale, voluntary, district-run, summer learning programs serving low-income elementary students. The study, conducted by the RAND Corporation, uses a randomized controlled trial to assess the effects of district-run voluntary summer programs on academic achievement, social and emotional skills, and behavior over the near and long term. All students in the study were in the third grade as of spring 2013 and enrolled in a public school in one of five urban districts: Boston; Dallas; Duval County, Florida; Pittsburgh; or Rochester, New York.

The study follows these students from third to seventh grade. Our primary focus is on academic outcomes, but we also examine students' social and emotional outcomes as well as behavior and attendance during the school year. We have also collected extensive data about the summer programs to help us examine how implementation is related to program effects.

This document includes the technical appendixes that accompany the third report resulting from the study, *Learning from Summer: Effects of Two Years of Voluntary Summer Learning Programs on Low-Income Urban Youth* (Augustine et al., 2016). These appendixes contain

- details of our randomization design (including treatment uptake, attrition, baseline equivalence of the treatment and control groups)
- details of our data collection processes and protocols (including characteristics of the participating students, descriptions of outcome measures, and a description of the observation process and protocol)
- a review of the extant literature on summer learning loss
- a description of the mediators and moderators included in analyses (including detailed information about how attendance and academic time on task were calculated)
- analytic models used for estimating program effects
- the complete results of all regression analyses.

This study is undertaken by RAND Education, a unit of the RAND Corporation that conducts research on prekindergarten, K–12, and higher education issues, such as preschool quality rating systems, assessment and accountability, teacher and leader effectiveness, school improvement, out-of-school time, educational technology, and higher education cost and completion.

This study is sponsored by The Wallace Foundation, which seeks to support and share effective ideas and practices to foster improvements in learning and enrichment for disadvantaged children and the vitality of the arts for everyone. Its current objectives are to improve the quality of schools, primarily by developing and placing effective principals in high-need schools; improve the quality of and access to after-school programs through coordinated city systems and by strengthening the financial management skills of providers; reimagine and expand learning time during the traditional school day and year, as well as during the summer months; expand access to arts learning; and develop audiences for the arts. For more information and research on these and other related topics, visit The Foundation's Knowledge Center at www.wallacefoundation.org.

Table of Contents

Figures

Tables

Abbreviations

CBO	community-based organizations
DESSA-RRE	Devereux Student Strengths Assessment–RAND Research Edition
ECLS-K	Early Childhood Longitudinal Study Kindergarten
ELL	English language learner
FRPL	free and reduced-price lunch
GMADE	Group Mathematics Assessment and Diagnostic Evaluation
GRADE	Group Reading Assessment and Diagnostic Evaluation
IEP	individualized education plan
ITT	intent-to-treat
LA	language arts
LPM	linear probability model
MDES	minimum detectable effect size
NWEA	Northwestern Evaluation Association
OLS	ordinary least squares
SD	standard deviation
SE	standard error
SES	socioeconomic status
SY	school year
TOT	treatment on the treated

Appendix A: Randomization Design and Implementation

This appendix discusses the details of how randomization was conducted, the statistical power for detecting treatment effects, the rates of attrition for each outcome, and the comparability of the treatment and control groups after attrition.

Randomization of Students to Treatment and Control Groups

In each district, parents submitted applications for their children to participate in the summer program along with their consent for children's participation in the study. Students admitted to the programs were assigned to a specific summer site, typically through geographic feeder patterns.

Stratification Plan

Our thinking about how to design the experiment was strongly influenced by Imbens (2011), who discusses the methodological considerations that should be made when designing a randomized controlled trial experiment. He shows that partitioning the sample into strata (groups of individuals with similar characteristics) and then randomizing within strata is preferable to randomizing without first creating strata, from the standpoint of maximizing statistical power, and that the benefits of randomization are strongest when (1) there are "relatively many and small strata" and (2) the strata are based on covariates that are strongly related to the outcome of interest.

With these considerations in mind, we constructed strata for the experiment based on the following variables:

- district
- third-grade school
- English language learner (ELL) status in third grade
- race/ethnicity: Hispanic, African American, other[1]
- eligibility for either free or reduced-price lunch (FRPL) in third grade
- prior achievement.

We chose these variables for several reasons. First, these are variables for which each district in the study maintains records. Second, these are all factors that are well known to be strong predictors of student achievement. Finally, such variables as ELL status are related to the type of test a student takes or whether a student is tested at all. As we will discuss, we conducted

---

[1] For the purposes of the stratification, Hispanic will refer to students of Hispanic origin of any race and African American will refer to non-Hispanic black students.

robustness checks where we limited the sample to particular student subgroups, including students tested only in English, since the testing conditions may not be comparable for ELL students. Since some researchers advocate stratifying on variables that will be used in subgroup analyses (Duflo, Glennerster, and Kremer, 2007), we thought it important to include these factors in the definition of the randomization strata.

Stratifying by prior achievement was important for our experiment, but it was unclear which achievement measure to use for this. In some districts, systematic testing begins in spring of the second grade, whereas other districts begin with benchmark assessments in the fall of third grade. Further, students are tested in multiple subjects. We stratified on the most-recent test scores that were available for all students. In districts where all students were tested in reading and mathematics, we used the average of these two subjects as the stratifying variable. A different approach was used in sites that administered some of their tests in Spanish to Spanish-speaking ELL students. For example, in Dallas, the stratification was based on mathematics tests, which are administered in English to all students.

While Imbens (2011) argues in favor of using strata with relatively few students, he also points out that there are analytic challenges associated with having few (e.g., two) students per strata.[2] A practical limitation of having strata with too few observations is that if students drop out of the sample because of attrition and only control or treatment students are left, the entire strata must be dropped from the analysis. Thus, we defined each stratum to have about 15–30 students. For example, with ten students assigned to treatment in a stratum, it was unlikely that all of the students would attrit from the study or be treatment no-shows.[3]

The basic algorithm used to generate the strata was as follows:

1. We fully stratified the summer program enrollees into cells defined by district-school-race-ELL-FRPL-achievement. For this step, achievement is a binary variable with half the enrollees in a district in a high-achievement group and the other half in a low-achievement group.[4] Since a goal of the study was to examine effects by higher- and lower-achieving students, it was important to stratify the sample in this way so that the high- and low-achievement subgroups coincided with the strata.
2. For cells defined in Step 1 with at least one but fewer than 15 students, we aggregated cells until each had at least 15 students. Cells were aggregated in the following order: (a) FRPL eligibility, (b) race, (c) ELL status, and (d) binary achievement. We did not collapse schools to ensure that within each school the proportion assigned to treatment

---

[2] Specifically, it was impossible to compute the variance of the within-strata treatment effect estimate. The estimated variance of the treatment effect in the case with just two observations per strata (i.e., a paired randomization design) would be biased upward for the true variance of the estimated treatment effect.

[3] Suppose that five students dropped out of the study (which would be higher than the long-run attrition rate of 30 percent that we assumed). If the no-show rate were 30 percent, then the probability that all five of the nondropouts were no-shows would be only about 0.002.

[4] Students with missing baseline achievement data were assigned to the low-achievement strata. See our discussion for additional sensitivity checks that were performed to handle missing baseline data.

was near the intended proportion (as close as possible considering rounding). To see how this worked, suppose that the cell for FRPL students who are African American, ELL, low-achievement, and in school A contained only five students. The first step in aggregating cells would be to pool across the FRPL category (essentially eliminating the FRPL criteria) to form a cell for students who were African American, ELL, low-achievement, and in school A. If this new cell still had fewer than 15 students, then the next step would be to form a cell for students who were in the African American or other race categories (i.e., pool across the smallest race categories), and if further pooling were necessary, to pool again by race, as necessary until all race categories are pooled, and then to pool by ELL, etc. This process was repeated until students were all assigned to cells with at least 15 students or to cells at the school level that could not be further aggregated.

The ordering of covariates for the aggregation reflected how important we thought each variable was in the stratification; we aggregated on less-important variables first. We placed the greatest importance on the sending school for programmatic reasons: Stratifying on the school ensured that no sending school would have a disproportionately large or small proportion of students assigned to treatment, helping to make clear that all stakeholders were being treated fairly by the randomization process. The second tier of importance was given to the achievement and ELL variables because we examined treatment effects by subgroups according to these variables. Finally, FRPL and race were included in the stratification plan because they were strongly associated with student outcomes; however, they received the lowest priority.

3. For cells that, at the end of Step 2, contained more than 30 students, we stratified them further by the achievement variable to form as many cells as possible that contained at least 15 students. For instance, if a cell had 65 students at the end of Step 2, we formed three cells with 16 students each and one with 17 students.[5] We used achievement to further stratify the larger cells because prior achievement was the strongest predictor of future achievement, so forming strata in this way maximized statistical power.

Writing the Computer Code for the Randomization

We used Stata.do files to carry out the stratified random assignment. We assigned percentage *P* of student applicants to the treatment group. *P* varied across districts and summer sites within a district, ranging between 50 percent and 60 percent. We capped *P* within any summer site at 70 percent.

Within each strata, *P* percent of students were assigned to the treatment group. For strata where the number of observations did not enable exactly *P* percent to be assigned to treatment,

---

[5] More formally, for a cell, c, with N_c students, we formed $k=int(N_c/15)$ subcells, with $int(N_c/k)$ students in k–mod(N_c,k) cells and $int(N_c/k)$+1 students in mod(N_c,k) cells.

the number of students assigned to treatment was equal to *round*($P*N_c$), where N_c was the number of students in strata *c*. In this way, whether there was slightly more or less than *P* percent of students assigned to treatment varied across strata but this variation was random.

To do the actual random assignment, we used the STATA pseudorandom number generator. Specifically, we assigned each student a random number drawn from a uniform distribution on the interval (0,1). Students were sorted according to this number so that the sort order of the students was random. After sorting the data in this way, within each strata, students whose sort order was less than or equal to N_{tc} were assigned to treatment, where N_{tc} is the number of students in strata *c* assigned to treatment. The strata identifier, *c*, was stored with each record for use in future analyses.

Siblings

It could be disruptive to families if one or more of their children were admitted to the program and one or more were not admitted. In all districts that requested that we account for siblings, we adopted procedures to keep together all siblings that made valid applications to the program.[6] Where one of the siblings was in third grade and the remaining siblings were in other grades, the admission decision for all of the children was based on the third-grader's randomly assigned admission status. Where there were multiple siblings in the third-grade sample, the siblings were randomly assigned as a group so that all received admission or all were denied admission.

Program Uptake

Next, we discuss estimates of the impact of the randomized treatment assignment on participation in the summer programs in both 2013 and 2014. We examine uptake during each of the two summers and examine uptake across the two years combined. Participation in the summer program is defined as attending at least one day of the relevant program offering period (summer 1, summer 2, or any time over the two summers). With perfect compliance to the experimental protocol, treatment assignment and program uptake would be the same. However, as Table A.1 indicates, not all students admitted to the summer programs actually attended, and some students assigned to the control group attended the program.[7] In the second year, slightly more than half the students who were admitted to summer programs actually attended. Overall, nearly 85 percent of admitted students attended at least one day of the summer program in either summer.

---

[6] Siblings were not considered when randomizing for Boston, by the decision of the district.

[7] The uptake rates by treatment assignment status were virtually identical for the sample of mathematics and LA non-attriters. In Dallas, some students assigned to the control group were nonetheless admitted to the program. There were a handful of other such "crossover" control group students in other districts as well.

Table A.1. Impact of Treatment Assignment on the Likelihood of Program Participation

Attendance Period	Program Uptake Among Students Assigned to Treatment Group	Program Uptake Among Students Assigned to Control Group	Difference in Uptake Between Treatment and Control Groups
Summer 1 (2013)	0.799	0.047	0.752
Summer 2 (2014)	0.518	0.008	0.510
Either summer	0.842	0.051	0.791

Table A.2 shows linear probability model estimates of the impact of treatment assignment on program uptake. Standard errors were calculated using the Eicker-Huber-White sandwich estimator (e.g., using Stata's "robust" command) that is robust to heteroskedasticity (Eicker, 1967; Huber, 1967; White, 1980). For Year 1, the results indicate that assignment to be eligible for the program increased the likelihood of attending the program for at least one day by 74 percentage points. For Year 2, the results indicated that assignment to be eligible for the program increased the likelihood of attending the program for at least one day by nearly 50 percentage points. Overall, across both years, assignment to be eligible for the program increased the likelihood of attending the program for at least one day by 79 percentage points. Where we present treatment-on-the-treated (TOT) effect estimates, we report instrumental variable estimates of the impact of summer program attendance for the set of students whose summer program attendance was affected by the experimental assignment, which are equal to the intent-to-treat (ITT) estimates scaled up by the inverse of the estimates in Table A.2.

Table A.2. Impact of Treatment Assignment on the Likelihood of Program Participation

Assignment	Estimate	Standard Error	*p* value
Year 1			
Only strata fixed effects	0.741	0.008	0.000
All student-level covariates	0.741	0.008	0.000
Year 2			
Only strata fixed effects	0.498	0.010	0.000
All student-level covariates	0.497	0.010	0.000
Both years			
Only strata fixed effects	0.791	0.008	0.000
All student-level covariates	0.791	0.008	0.000

NOTE: Student-level covariates are standardized mathematics and reading scores from the state's spring third-grade assessments and fall third-grade diagnostic tests; classroom average of these pretests; dummy variables for FRPL, African American, Hispanic, ELL, special education and male classifications; and dummy variables for missing pretest scores.

Minimum Detectable Effect Sizes

To estimate the statistical power of the study during the design phase, we applied formulas for experiments in which treatment students were clustered in classes, and calculated the minimum detectable effect size (MDES) the study would be able to identify with 80 percent probability using a two-tailed test and a 0.05 level of significance. To perform these calculations, we estimated several parameters using existing empirical data from pilot work in summers 2011 and 2012, as well as the published literature. These estimates were uncertain, and as a general rule, we chose conservative values that would produce higher, rather than lower, MDES estimates.

After the student sample and the proportion assigned to treatment were finalized, we used this information, along with the remaining assumptions from the earlier power calculations, to compute the MDES for near-term ITT outcomes. These MDES values are shown in Table A.3 for the overall study as well as some descriptive information about MDES for district-specific analyses. MDES vary by district, mainly because of the number of participating students, so some districts have a smaller MDES than others. The largest district-specific MDES is 0.23, the smallest is 0.13, the average is 0.19. These values are consistent with treatment effects that have been found in prior random assignment studies on summer programs, and our own review of the literature found that summer programs had, on average, effects of approximately 0.10 standard deviations (SD).[8]

Table A.3. Minimum Detectable Effect Sizes for Intent-to-Treat Analyses of Near-Term Outcomes

Analysis Sample	MDES
Overall	0.08
District	
High	0.23
Mean (SD)	0.19 (0.04)
Low	0.13

Attrition

Here, we discuss estimates of the impact of attrition on our inferences about program impacts. Typically, attrition is characterized by the rate at which participants withdraw from a study. However, generally speaking, attrition can also characterize the ways in which outcomes are not available for study participants for a variety of reasons. For example, for the fall (short-term) assessments, students may refuse to take these assessments, may have had prolonged school absences during the fall test administration windows, or may have changed schools or

---

[8] This literature review is described in Chapter Two of the main report and in more detail in Appendix G of this document.

school districts before the administration. For the spring outcomes (course grades, state assessments, school-year suspension, and school-year attendance), students may have moved out of district, or may have moved within the district to a school that does not report these data to local education agencies (for example, movement to a charter school). In the remainder of this section, we discuss attrition in this general way for each data collection time point (fall 2013, spring 2014, fall 2014, and spring 2015) and for each relevant outcome.

Fall 2013

As described in more detail in Appendix B, fall 2013 mathematics outcomes were available for 5,127 students, and reading outcomes were available for 5,101 students. These represent attrition rates of 9.1 percent and 9.5 percent, respectively.

To test whether there was differential attrition in the treatment and control groups, we ran linear probability models that predicted attrition based on the treatment indicator. The first model included fixed effects for random assignment strata but no other covariates; the second also included the student-level covariates already discussed. Standard errors were calculated using the Huber-Eicker-White sandwich estimator that is robust to heteroskedasticity. The results show that treatment group students and control group students do not have distinguishably differential tendencies to attrit (see Table A.4).

Table A.4. Assessment of Differential Attrition (Fall 2013 Outcomes)

Subject of Study	Only Strata Fixed Effects Estimate (SE)	All Student-Level Covariates Estimate (SE)
Mathematics	–0.012 (0.008)	–0.012 (0.008)
Reading	–0.010 (0.008)	–0.007 (0.008)

NOTE: SE = standard error. Student-level covariates are standardized mathematics and reading scores from the state's spring third-grade assessments and fall third-grade diagnostic tests; classroom average of these pretests; dummy variables for FRPL, African American, Hispanic, ELL, special education and male classifications; and dummy variables for missing pretest scores.

Spring 2014

Spring 2014 outcomes include state standardized tests (mathematics and language arts [LA]), end-of-year course grades (mathematics and LA), school-year suspensions, and school-year attendance rates. Mathematics state assessment data was available for 90.6 percent of students ($N = 5{,}138$); LA state assessment data was available for 90.8 percent of students ($N = 5{,}130$). Course grades were available for 89.8 percent and 89.9 percent of students in mathematics ($N = 5{,}062$) and LA ($N = 5{,}065$), respectively. Suspension data was available for 94.4 percent of students ($N = 5{,}331$), and attendance data was available for 94.3 percent of students ($N = 5{,}329$). The results in Table A.5 show that treatment group students and control group students do not have distinguishably differential tendencies to attrit.

Table A.5. Assessment of Differential Attrition (Spring 2014 Outcomes)

Assessment	Only Strata Fixed Effects Estimate (SE)	All Student-Level Covariates Estimate (SE)
Mathematics state test	–0.005 (0.008)	–0.007 (0.008)
LA state test	–0.005 (0.008)	–0.005 (0.008)
Mathematics grades	–0.013 (0.008)	–0.013 (0.008)
LA grades	–0.010 (0.008)	–0.009 (0.008)
Suspension in school year (SY) 2013–2014	–0.002 (0.006)	–0.001 (0.006)
Attendance in SY 2013–2014	–0.003 (0.006)	–0.003 (0.006)

NOTE: Student-level covariates are standardized mathematics and reading scores from the state's spring third-grade assessments and fall third-grade diagnostic tests; classroom average of these pretests; dummy variables for FRPL, African American, Hispanic, ELL, special education and male classifications; and dummy variables for missing pretest scores.

Fall 2014

Fall 2014 mathematics outcomes were available for 4,505 students, and reading outcomes were available for 4,484 students. These represent attrition rates of 20.1 percent and 20.5 percent, respectively. The results show that differences between treatment and control group students are indistinguishable from zero (see Table A.6).

Table A.6. Assessment of Differential Attrition (Fall 2014 Outcomes)

Subject of Study	Only Strata Fixed Effects Estimate (SE)	All Student-Level Covariates Estimate (SE)
Mathematics	–0.006 (0.011)	–0.006 (0.011)
Reading	–0.007 (0.011)	–0.005 (0.011)

NOTE: Student-level covariates are standardized mathematics and reading scores from the state's spring third-grade assessments and fall third-grade diagnostic tests; classroom average of these pretests; dummy variables for FRPL, African American, Hispanic, ELL, special education and male classifications; and dummy variables for missing pretest scores.

Spring 2015

Spring 2015 outcomes include state standardized tests (mathematics and LA), end-of-year course grades (mathematics and LA), school-year suspensions, and school-year attendance rates. Mathematics state assessment data was available for 62.3 percent of students ($N = 4{,}301$); LA state assessment data was available for 62.9 percent of students ($N = 4{,}337$). Course grades were available for 79.1 percent and 79.2 percent of students in mathematics ($N = 4{,}458$) and LA ($N = 4{,}464$), respectively. Suspension data was available for 85 percent of students ($N = 4{,}782$) and attendance data was available for 85 percent of students ($N = 4{,}782$). The results in Table A.7 show that treatment group students and control group students do not have distinguishably differential tendencies to attrit.

Table A.7. Assessment of Differential Attrition (Spring 2015 Outcomes)

Assessment Measure	Only Strata Fixed Effects Estimate (SE)	All Student-Level Covariates Estimate (SE)
Mathematics state test	–0.003 (0.010)	–0.002 (0.010)
LA state test	–0.004 (0.010)	–0.002 (0.010)
Mathematics grades	–0.006 (0.011)	–0.005 (0.010)
LA grades	–0.008 (0.011)	–0.005 (0.010)
Suspension in SY 2014–2015	–0.008 (0.009)	–0.007 (0.009)
Attendance in SY 2014–2015	–0.008 (0.010)	–0.007 (0.009)

NOTE: Student-level covariates are standardized mathematics and reading scores from the state's spring third-grade assessments and fall third-grade diagnostic tests; classroom average of these pretests; dummy variables for FRPL, African American, Hispanic, ELL, special education and male classifications; and dummy variables for missing pretest scores.

Balance of the Treatment and Control Groups After Attrition

Next, we assessed balance in the observable characteristics of the treatment and control groups that were retained in the analytic sample after attrition. As above, these results are presented for each data collection time point (fall 2013, spring 2014, fall 2014, and spring 2015) and for each relevant outcome.

Fall 2013

Table A.8 shows the results for mathematics from statistical models that predicted assignment to the treatment group based on each student-level achievement or demographic variable, fit one at a time, controlling for the strata used in random assignment. Table A.9 shows the corresponding results for reading.

Table A.8. Assessment of Treatment-Control Group Balance After Attrition (Fall 2013 Mathematics)

Characteristics	Estimate	Standard Error	*p* value
2012 benchmark mathematics assessment	0.009	0.009	0.353
2013 state mathematics assessment	0.015	0.008	0.061
2012 benchmark reading assessment	0.000	0.009	0.992
2013 state reading state assessment	0.007	0.008	0.341
Eligible for free or reduced-price lunch	0.002	0.026	0.938
Black	0.012	0.019	0.509
Hispanic	0.004	0.022	0.845
English-language learner	–0.007	0.020	0.721
Special education student (gifted excluded)	0.043	0.025	0.088
Student is male	–0.003	0.015	0.821

NOTE: Table shows results of univariate models (with strata fixed effects) using each covariate to predict treatment in the sample remaining after attrition. A likelihood ratio test of these variables' joint ability to predict treatment assignment in a multivariate model yielded a *p* value of 0.540.

Table A.9. Assessment of Treatment-Control Group Balance After Attrition (Fall 2013 Reading)

Characteristic	Estimate	Standard Error	*p* value
2012 benchmark mathematics assessment	0.011	0.009	0.228
2013 state mathematics assessment	0.018	0.008	0.025
2012 benchmark reading assessment	0.003	0.009	0.760
2013 state reading state assessment	0.010	0.008	0.200
Eligible for free or reduced-price lunch	–0.001	0.026	0.980
Black	0.017	0.019	0.372
Hispanic	–0.001	0.022	0.972
English-language learner	–0.013	0.020	0.505
Special education student (gifted excluded)	0.044	0.025	0.078
Student is male	–0.006	0.015	0.704

NOTE: Table shows results of univariate models (with strata fixed effects) using each covariate to predict treatment in the sample remaining after attrition. A likelihood ratio test of these variables' joint ability to predict treatment assignment in a multivariate model yielded a *p* value of 0.322.

In both cases, the differences between the retained treatment and control groups were generally small and not significant. An exception was that, in both mathematics and reading, the treatment group had slightly higher scores on the 2013 state assessment, by 0.015 and 0.018, respectively. The difference was marginally significant in mathematics and significant at the $p < 0.05$ level in reading. These differences were small with to what is considered acceptable in a valid experiment.[9]

Spring 2014

We then assessed balance in the observable characteristics of the treatment and control groups that were retained in the analytic sample for spring 2014 outcomes. Tables A.10–A.12 show the balance for state assessments and course grades in mathematics and LA, as well as for attendance and suspension outcomes, and help us to understand whether there is adequate evidence that the treatment and control groups are equivalent at baseline, after accounting for attrition. Again, balance was appraised using statistical models that predicted assignment to the treatment group based on each student-level achievement or demographic variable, fit one at a time, controlling for the strata used in random assignment.

---

[9] For example, the What Works Clearinghouse (U.S. Department of Education, 2014) sets a limit of 0.25 for pretreatment group differences when the variable will be used as a covariate in outcomes models, as we did here.

Table A.10. Assessment of Treatment-Control Group Balance After Attrition (Spring 2014 Mathematics)

Characteristic	Estimate	Standard Error	*p* value
Spring Standardized Assessments			
2012 benchmark mathematics assessment	0.005	0.009	0.557
2013 state mathematics assessment	0.012	0.008	0.628
2012 benchmark reading assessment	0.000	0.009	0.945
2013 state reading state assessment	0.004	0.008	0.628
Eligible for free or reduced-price lunch	0.006	0.025	0.828
Black	0.003	0.019	0.874
Hispanic	0.012	0.022	0.589
English-language learner	–0.009	0.020	0.647
Special education student (gifted excluded)	0.037	0.026	0.150
Student is male	0.002	0.015	0.904
Spring Course Grades			
2012 benchmark mathematics assessment	0.007	0.009	0.412
2013 state mathematics assessment	0.014	0.008	0.102
2012 benchmark reading assessment	–0.000	0.008	0.966
2013 state reading state assessment	0.004	0.008	0.637
Eligible for free or reduced-price lunch	0.011	0.026	0.681
Black	0.009	0.019	0.638
Hispanic	0.006	0.022	0.802
English-language learner	–0.009	0.020	0.659
Special education student (gifted excluded)	0.045	0.025	0.075
Student is male	0.002	0.015	0.879

NOTE: Table shows results of univariate models (with strata fixed effects) using each covariate to predict treatment in the sample remaining after attrition.

Table A.11. Assessment of Treatment-Control Group Balance After Attrition (Spring 2014 LA)

Characteristic	Estimate	Standard Error	*p* value
Spring Standardized Assessments			
2012 benchmark mathematics assessment	0.006	0.009	0.509
2013 state mathematics assessment	0.013	0.008	0.137
2012 benchmark reading assessment	0.001	0.009	0.949
2013 state reading state assessment	0.005	0.008	0.576
Eligible for free or reduced-price lunch	0.005	0.026	0.853
Black	0.006	0.019	0.760
Hispanic	0.011	0.022	0.607
English language learner	–0.008	0.020	0.704
Special education student (gifted excluded)	0.038	0.026	0.143
Student is male	0.001	0.015	0.940

NOTE: Table shows results of univariate models (with strata fixed effects) using each covariate to predict treatment in the sample remaining after attrition.

Table A.11—Continued

Characteristic	Estimate	Standard Error	*p* value
Spring Course Grades			
2012 benchmark mathematics assessment	0.005	0.009	0.569
2013 state mathematics assessment	0.011	0.008	0.200
2012 benchmark reading assessment	–0.002	0.009	0.855
2013 state reading state assessment	0.004	0.008	0.639
Eligible for free or reduced-price lunch	0.008	0.026	0.754
Black	0.008	0.019	0.683
Hispanic	0.008	0.022	0.726
English-language learner	–0.005	0.020	0.817
Special education student (gifted excluded)	0.040	0.025	0.113
Student is male	–0.001	0.015	0.949

NOTE: Table shows results of univariate models (with strata fixed effects) using each covariate to predict treatment in the sample remaining after attrition.

Table A.12. Assessment of Treatment-Control Group Balance After Attrition (Spring 2014 Attendance and Suspension)

Characteristic	Estimate	Standard Error	*p* value
School-Year Attendance			
2012 benchmark mathematics assessment	0.007	0.009	0.416
2013 state mathematics assessment	0.014	0.008	0.091
2012 benchmark reading assessment	0.000	0.008	0.970
2013 state reading state assessment	0.006	0.008	0.475
Eligible for free or reduced-price lunch	0.005	0.025	0.839
Black	0.008	0.018	0.672
Hispanic	0.004	0.021	0.844
English-language learner	–0.004	0.020	0.835
Special education student (gifted excluded)	0.037	0.024	0.132
Student is male	–0.005	0.014	0.738
School-Year Suspensions			
2012 benchmark mathematics assessment	0.007	0.009	0.441
2013 state mathematics assessment	0.014	0.008	0.094
2012 benchmark reading assessment	–0.001	0.008	0.930
2013 state reading state assessment	0.005	0.008	0.492
Eligible for free or reduced-price lunch	0.007	0.025	0.794
Black	0.007	0.018	0.688
Hispanic	0.005	0.021	0.832
English-language learner	–0.004	0.020	0.838
Special education student (gifted excluded)	0.037	0.024	0.130
Student is male	–0.005	0.014	0.710

NOTE: Table shows results of univariate models (with strata fixed effects) using each covariate to predict treatment in the sample remaining after attrition.

In both cases, the differences between the retained treatment and control groups were generally small and not significant.

Fall 2014

Table A.13 shows the results for mathematics from statistical models that predicted assignment to the treatment group based on each student-level achievement or demographic variable, fit one at a time, controlling for the strata used in random assignment. Table A.14 shows the corresponding results for reading.

Table A.13. Assessment of Treatment-Control Group Balance After Attrition (Fall 2014 Mathematics)

Characteristic	Estimate	Standard Error	*p* value
2012 benchmark mathematics assessment	0.006	0.010	0.518
2013 state mathematics assessment	0.012	0.009	0.512
2012 benchmark reading assessment	0.001	0.009	0.909
2013 state reading state assessment	0.060	0.009	0.512
Eligible for free or reduced-price lunch	–0.004	0.027	0.876
Black	0.014	0.021	0.490
Hispanic	0.004	0.024	0.862
English-language learner	0.001	0.022	0.980
Special education student (gifted excluded)	0.032	0.028	0.233
Student is male	0.005	0.016	0.751

NOTE: Table shows results of univariate models (with strata fixed effects) using each covariate to predict treatment in the sample remaining after attrition.

Table A.14. Assessment of Treatment-Control Group Balance After Attrition (Fall 2014 Reading)

Characteristic	Estimate	Standard Error	*p* value
2012 benchmark mathematics assessment	0.002	0.010	0.802
2013 state mathematics assessment	0.014	0.009	0.114
2012 benchmark reading assessment	0.003	0.009	0.761
2013 state reading state assessment	0.007	0.009	0.418
Eligible for free or reduced-price lunch	0.003	0.028	0.924
Black	0.018	0.021	0.377
Hispanic	–0.004	0.024	0.872
English-language learner	0.001	0.022	0.975
Special education student (gifted excluded)	0.045	0.028	0.105
Student is male	0.000	0.016	0.984

NOTE: Table shows results of univariate models (with strata fixed effects) using each covariate to predict treatment in the sample remaining after attrition. A likelihood ratio test of these variables' joint ability to predict treatment assignment in a multivariate model yielded a *p* value of 0.322.

In both cases, the differences between the retained treatment and control groups were generally small and not significant.

Spring 2015

We then assessed balance in the observable characteristics of the treatment and control groups that were retained in the analytic sample for spring 2015 outcomes. Tables A.15–A.17 show the balance for state assessments and course grades in mathematics and LA and for attendance and suspension outcomes. Again, balance was appraised using statistical models that predicted assignment to the treatment group based on each student-level achievement or demographic variable, fit one at a time, controlling for the strata used in random assignment.

Table A.15. Assessment of Treatment-Control Group Balance After Attrition (Spring 2015 Mathematics)

Characteristic	Estimate	Standard Error	*p* value
Spring Standardized Assessments			
2012 benchmark mathematics assessment	0.007	0.011	0.502
2013 state mathematics assessment	0.022	0.010	0.031
2012 benchmark reading assessment	0.000	0.010	0.985
2013 state reading state assessment	0.009	0.010	0.359
Eligible for free or reduced-price lunch	0.028	0.033	0.392
Black	0.009	0.024	0.718
Hispanic	0.000	0.026	0.991
English-language learner	0.005	0.023	0.831
Special education student (gifted excluded)	0.061	0.031	0.047
Student is male	0.007	0.018	0.699
Spring Course Grades			
2012 benchmark mathematics assessment	0.006	0.010	0.501
2013 state mathematics assessment	0.017	0.009	0.063
2012 benchmark reading assessment	–0.003	0.009	0.780
2013 state reading state assessment	0.006	0.009	0.516
Eligible for free or reduced-price lunch	0.026	0.028	0.341
Black	0.013	0.021	0.540
Hispanic	–0.004	0.024	0.876
English-language learner	–0.003	0.022	0.887
Special education student (gifted excluded)	0.051	0.027	0.059
Student is male	0.007	0.016	0.673

NOTE: Table shows results of univariate models (with strata fixed effects) using each covariate to predict treatment in the sample remaining after attrition.

Table A.16. Assessment of Treatment-Control Group Balance After Attrition (Spring 2015 LA)

Characteristic	Estimate	Standard Error	*p* value
Spring Standardized Assessments			
2012 benchmark mathematics assessment	0.006	0.011	0.593
2013 state mathematics assessment	0.021	0.010	0.039
2012 benchmark reading assessment	–0.002	0.010	0.851
2013 state reading state assessment	0.009	0.010	0.385
Eligible for free or reduced-price lunch	0.038	0.033	0.249
Black	0.014	0.024	0.563
Hispanic	–0.002	0.026	0.924
English-language learner	0.003	0.023	0.902
Special education student (gifted excluded)	0.062	0.030	0.041
Student is male	0.005	0.018	0.775
Spring Course Grades			
2012 benchmark mathematics assessment	0.008	0.010	0.428
2013 state mathematics assessment	0.018	0.009	0.050
2012 benchmark reading assessment	–0.003	0.009	0.765
2013 state reading state assessment	0.006	0.009	0.478
Eligible for free or reduced-price lunch	0.028	0.028	0.312
Black	0.014	0.021	0.503
Hispanic	–0.006	0.024	0.819
English-language learner	–0.002	0.022	0.918
Special education student (gifted excluded)	0.048	0.027	0.075
Student is male	0.007	0.016	0.667

NOTE: Table shows results of univariate models (with strata fixed effects) using each covariate to predict treatment in the sample remaining after attrition.

Table A.17. Assessment of Treatment-Control Group Balance After Attrition (Attendance and Suspension)

Characteristic	Estimate	Standard Error	*p* value
School-Year Attendance			
2012 benchmark mathematics assessment	0.007	0.009	0.449
2013 state mathematics assessment	0.015	0.009	0.094
2012 benchmark reading assessment	–0.002	0.009	0.814
2013 state reading state assessment	0.005	0.008	0.544
Eligible for free or reduced-price lunch	0.020	0.027	0.457
Black	0.011	0.020	0.586
Hispanic	–0.005	0.023	0.834
English-language learner	–0.001	0.021	0.973
Special education student (gifted excluded)	0.035	0.026	0.172
Student is male	0.007	0.015	0.651
School-Year Suspensions			
2012 benchmark mathematics assessment	0.007	0.009	0.449
2013 state mathematics assessment	0.015	0.009	0.094
2012 benchmark reading assessment	–0.002	0.009	0.814
2013 state reading state assessment	0.005	0.008	0.544
Eligible for free or reduced-price lunch	0.020	0.027	0.457
Black	0.011	0.020	0.586
Hispanic	–0.005	0.023	0.834
English-language learner	–0.001	0.021	0.973
Special education student (gifted excluded)	0.035	0.026	0.172
Student is male	0.007	0.015	0.651

NOTE: Table shows results of univariate models (with strata fixed effects) using each covariate to predict treatment in the sample remaining after attrition.

In both cases, the differences between the retained treatment and control groups were generally small and not significant.

Appendix B: Data Collection

Characteristics of Students in the Sample

Table B.1 shows the characteristics of students in the experiment according to whether they belong to the treatment or control group. These characteristics are descriptive of the students *as randomized* and do not account for attrition.[10] Treatment and control group students differed in the aggregate along some demographic characteristics because district demographics varied (for example, Dallas has a greater portion of Hispanic and ELL students) and because the percentage assigned to treatment also varied by district. In Dallas, 50 percent of eligible applicants were randomized to the treatment group, whereas it was 60 percent in each of the other four districts. The combination of these two factors resulted in group differences on race/ethnicity, FRPL eligibility, and ELL variables. Once the varying proportion assigned to treatment was properly accounted for by controlling for strata, we did not see any statistically significant imbalance between the treatment and control groups on observed pretreatment characteristics listed in Table B.1.

Table B.1. Characteristics of Students in the Experiment

Combined Sample	Treatment Group Mean	Treatment Group SD	Control Group Mean	Control Group SD	Standardized Difference	*p* value
Prior Achievement						
Standardized spring 2013 mathematics score	0.017	0.918	0.003	0.933	0.015	0.570
Standardized spring 2013 LA score	–0.013	0.913	–0.006	0.918	–0.008	0.768
Lower achieving	46.5%	0.499	46.2%	0.499	0.006	0.816
Spring 2013 mathematics score missing	8.3%	0.276	8.0%	0.271	0.012	0.661
Spring 2013 LA score missing	9.4%	0.292	9.3%	0.291	0.001	0.968
Demographic Characteristics						
ELL	29.3%	0.455	33.7%	0.473	–0.095	0.000
FRPL-eligible	86.2%	0.345	88.5%	0.319	–0.069	0.009
African American	49.2%	0.500	44.5%	0.497	0.094	0.000
Hispanic	38.0%	0.486	43.6%	0.496	–0.114	0.000
Other racial or ethnic category	12.7%	0.333	11.9%	0.323	0.027	0.317
Number	**3,194**	—	**2,445**	—	—	—

NOTES: Two students in the treatment group dropped out of the study after randomization, reducing the total treatment group to 3,192.

---

[10] For information about the baseline equivalence of treatment and control groups after accounting for attrition, refer to Appendix A.

Academic Achievement

A primary outcome of interest in this study was student performance on standardized assessments of their mathematics and reading achievement. Here, we examined student performance at four time points: fall 2013, spring 2014, fall 2014, and spring 2015. In addition, we examined spring 2014 and spring 2015 course grades, as well as school-year suspensions and attendance in SYs 2013–2014 and 2014–2015. We describe each of these types of data below.

Spring Tests

The two springtime points consisted of scale scores on statewide standardized mathematics and LA tests. We obtained these data from the participating school districts. For spring 2014 and 2015, these tests were the Massachusetts Comprehensive Assessment System, the Florida Comprehensive Assessment Test, the State of Texas Assessments of Academic Readiness, the Pennsylvania System of School Assessment, and the New York State English Language Arts and Mathematics Exams. For the analysis, we standardized the scale scores within each district based on the study sample to have a mean of 0 and standard deviation of 1, enabling model coefficients on the treatment indicator to be read as standardized effect sizes. Figures B.1–B.4 display the resulting standardized scores for mathematics and LA in spring 2014 and spring 2015. In each district, the score distributions are symmetric, which is highlighted by the density curves presented along with the histograms. In District D, in spring 2014 there are several outliers in the lower tail of the distribution for both LA and mathematics. These outliers have valid raw scores of 0 on these assessments.

Figure B.1. Histograms of Standardized Scale Scores on the State Spring 2014 Mathematics Assessments

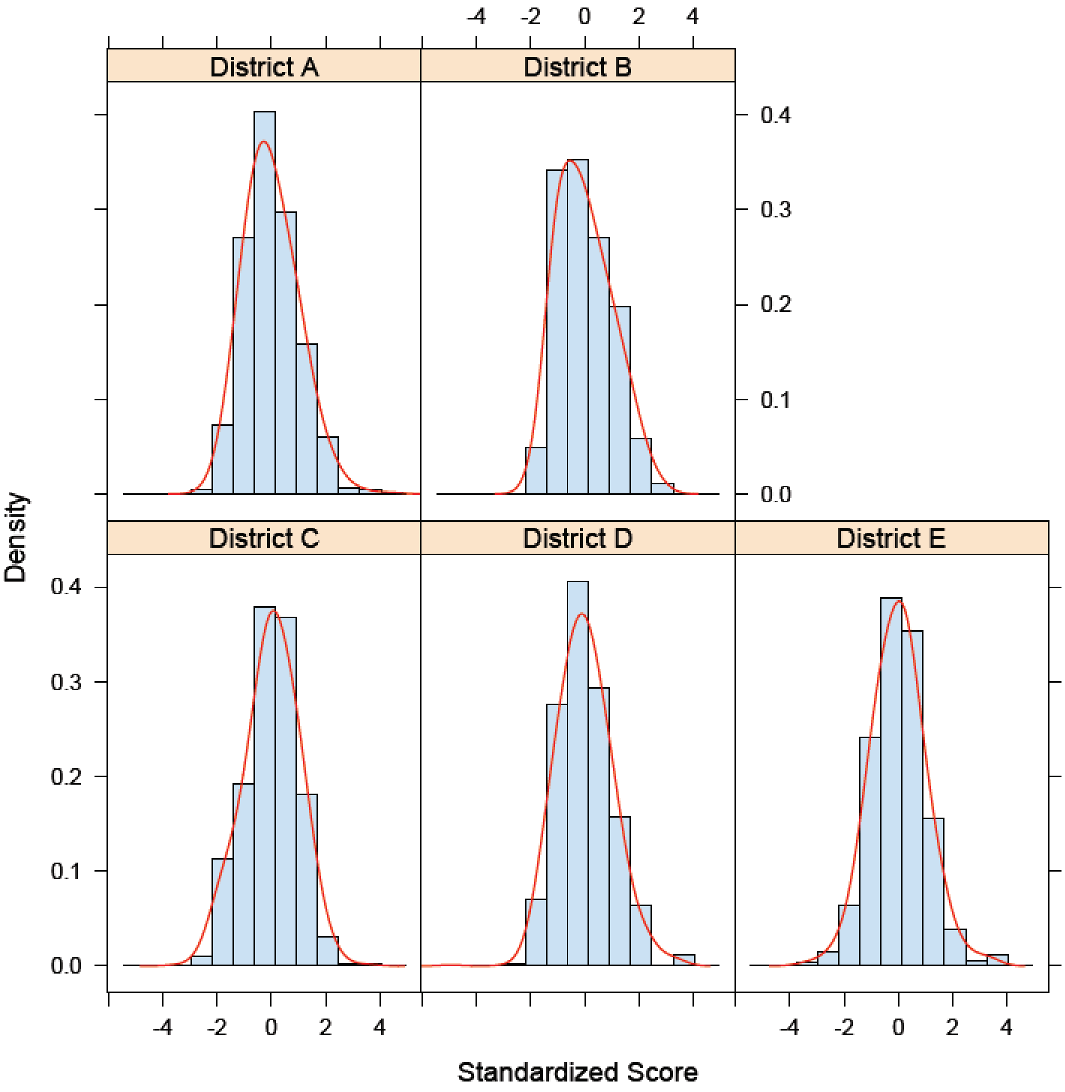


Figure B.2. Histograms of Standardized Scale Scores on the State Spring 2014 LA Assessments

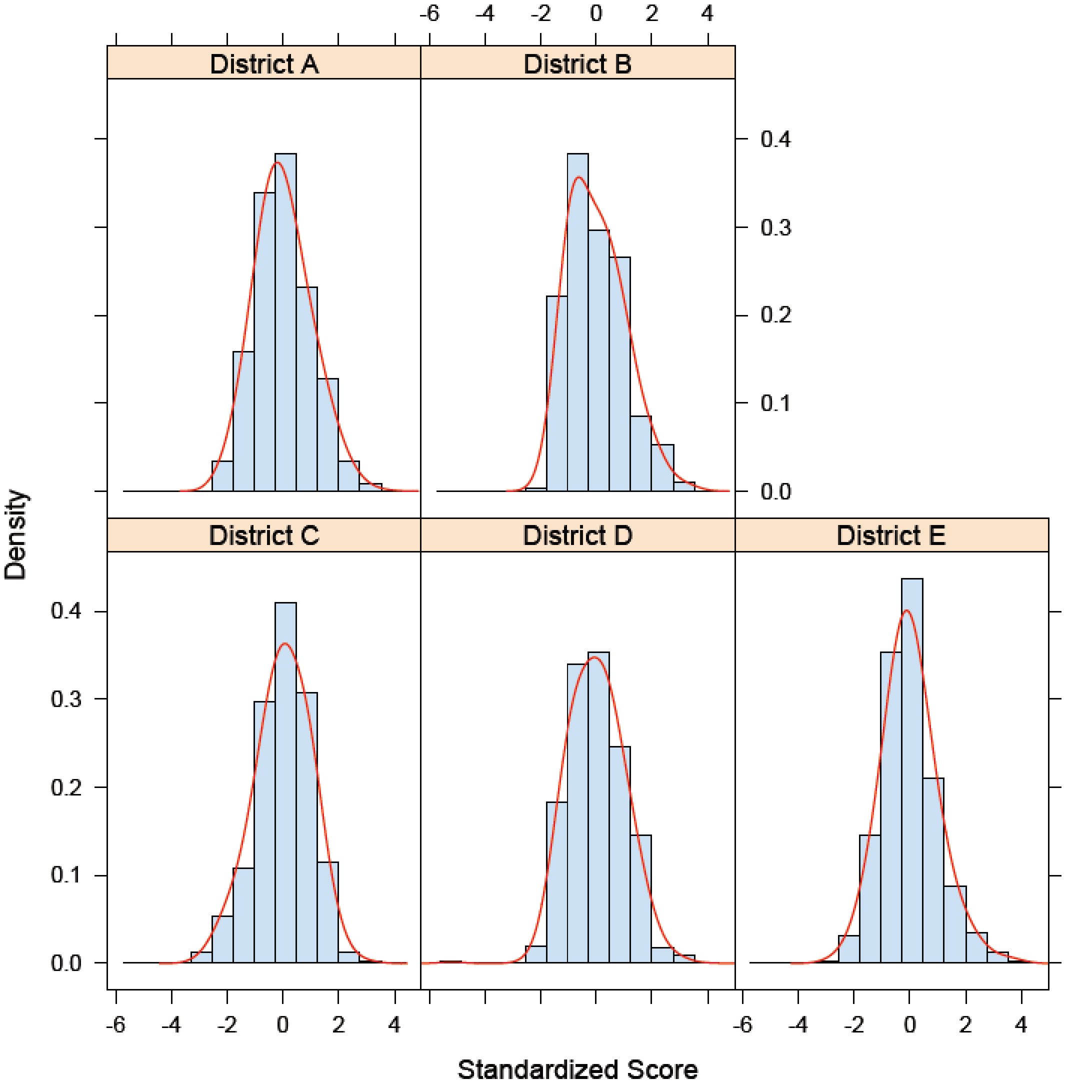


Figure B.3. Histograms of Standardized Scale Scores on the State Spring 2015 Mathematics Assessments

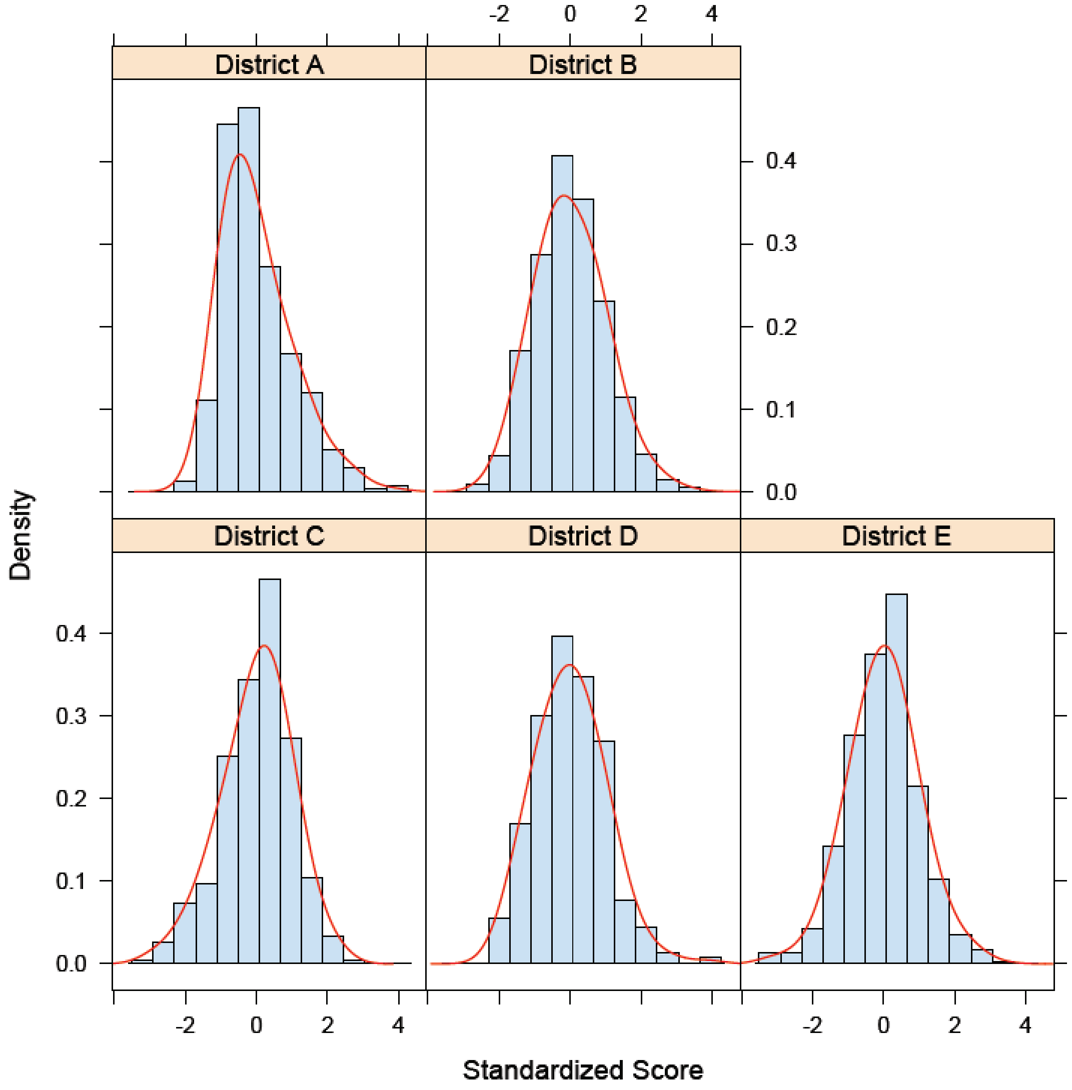
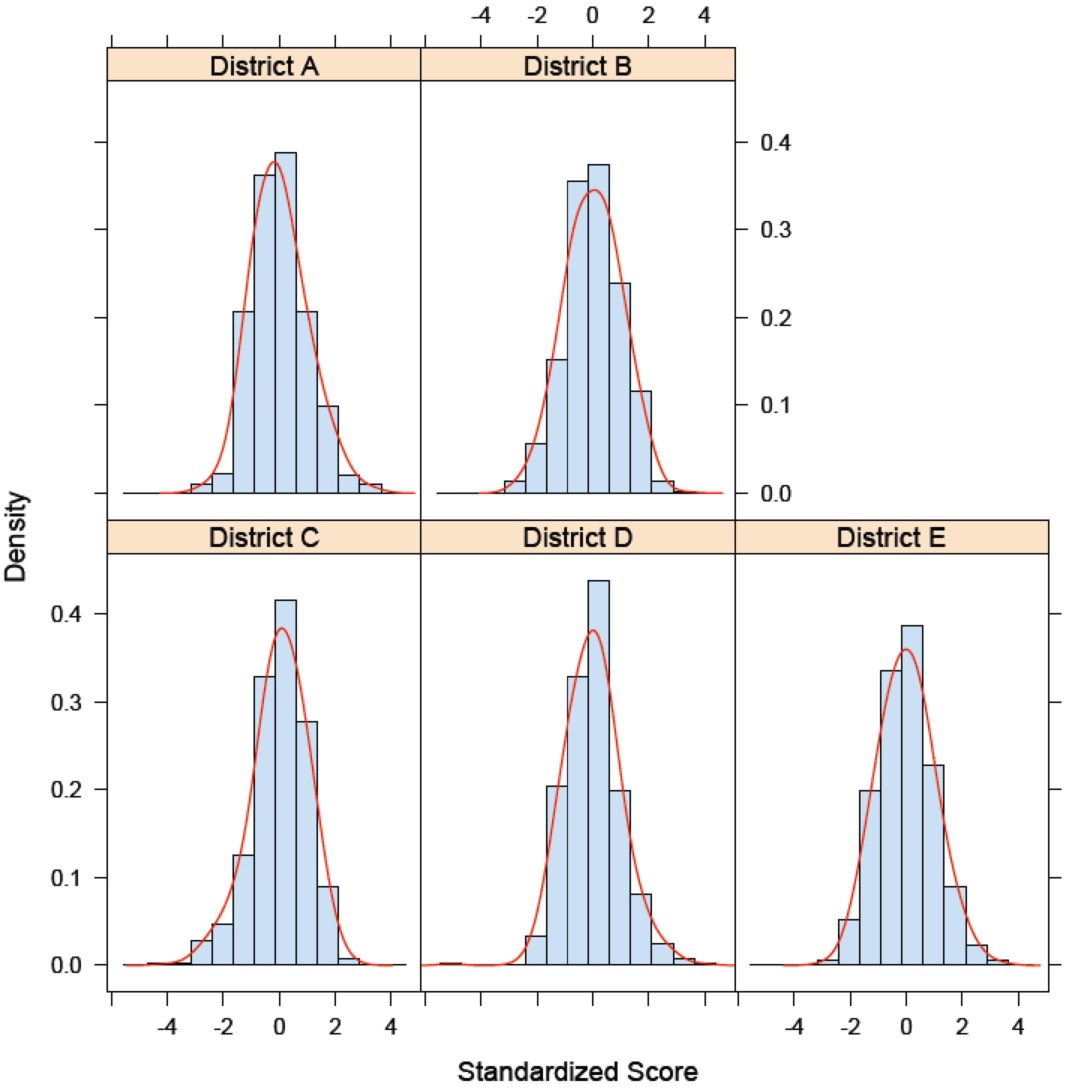


Figure B.4. Histograms of Standardized Scale Scores on the State Spring 2015 LA Assessments



Fall Tests

To assess if summer programs had an immediate, short-run effect, we administered mathematics and reading tests in fall 2013 and in fall 2014 to students from both the treatment and control groups. We selected broad, general-knowledge, standardized assessments similar to state assessments and appropriate for the study population. The majority of students took the Group Mathematics Assessment and Diagnostic Evaluation (GMADE) and Group Reading Assessment and Diagnostic Evaluation (GRADE), developed by Pearson Education. The GMADE is a 90-minute multiple-choice paper test that is aligned to content standards developed by the National Council of Teachers of Mathematics. The GMADE assesses skills in three specific domains: concepts and communication, operations and computation, and process and application. The GMADE is offered at various levels that roughly correspond to grade levels, but is designed with flexibility to administer the test above or below the grade level indicated. (For example, Level 3 is nominally for third-graders but is considered appropriate for second- or fourth-graders as well.) The GRADE is a 65-minute multiple choice paper test of reading comprehension, and like the GMADE, is offered at various levels with flexibility to administer the test above or below the grade level indicated. Students in the study were all fourth-graders in fall 2013 (with rare exceptions of grade retention or advancement). The project selected the Level 3 exam for fall 2013 and Level 4 exam for fall 2014 because the study students are generally low-performing and because the tests would be administered early in fourth and fifth grades, respectively.

There were two exceptions regarding which fall standardized assessments were administered to students in the study. The first occurred in District A, where students took a Level 4 district-administered GRADE assessment in fall 2013 and a Level 5 GRADE assessment in fall 2014 (which is one level higher than the project-administered exams at these two time points) because the district was already administering this exam to all fourth-graders in fall 2014 and to all fifth-graders in fall 2015 as part of a districtwide initiative. The second occurred in District D for students who took the spring 2013 or spring 2014 assessment in Spanish rather than in English. For these students, the project administered the reading comprehension subtest of the Spanish-language Logramos assessment from Riverside Publishing instead of the GRADE.

The project-administered assessments were given in fall 2013 and in fall 2014 during the third, fourth, and fifth weeks of the school year. The Wallace Foundation contracted the research firm Mathematica Policy Research to administer these assessments. Only participating students who were still enrolled in a public school within the five school districts were eligible for fall 2013 and fall 2014 testing. In total, we obtained fall 2013 mathematics and reading scores for 90.9 percent and 90.4 percent of the original sample. In fall 2014, we obtained mathematics and reading scores for 79.9 percent and 79.5 percent of the original sample. Descriptive information of the assessments' response rates for both assessment waves in Table B.2.

Table B.2. Response Rates for Fall Mathematics and Reading Assessments, 2013 and 2014

	Fall Mathematics			Fall Reading		
District	Type of Assessment Administered	Number of Students with Scorable Tests	Response Rate (%)	Type of Assessment Administered	Number of Students with Scorable Tests	Response Rate (%)
2013						
A	Level 3 GMADE	587	89.5	Level 4 GRADE administered by district	565	86.1
B	Level 3 GMADE	870	90.9	Level 3 GRADE	874	91.3
C	Level 3 GMADE	992	91.9	Level 3 GRADE	991	91.8
D	Level 3 GMADE	1,860	90.4	Level 3 GRADE for ELL, Logramos reading comprehension & vocabulary subtest	1,956 (889 GRADE & 967 Logramos)	95.1
E	Level 3 GMADE	818	92.0	Level 3 GRADE	813	91.5
Total		**5,127**	**90.9**		**5,099**	**90.4**
2014						
A	Level 4 GMADE	509	77.6	Level 5 GRADE administered by district	505	77.0
B	Level 4 GMADE	638	66.7	Level 4 GRADE	640	66.9
C	Level 4 GMADE	885	81.9	Level 4 GRADE	877	81.2
D	Level 4 GMADE	1,699	82.6	Level 4 GRADE for ELL, Logramos reading comprehension & vocabulary subtest	1,687 (866 GRADE & 821 Logramos)	82.1
E	Level 4 GMADE	774	87.2	Level 4 GRADE	775	87.3
Total		**4,505**	**79.9**		**4,484**	**79.5**

Figures B.5 and B.6 show score distributions on these assessments. There is no evidence of floor or ceiling effects, except for slight truncation of the distribution (floor effect) on the GRADE Level 4 and Level 5 exam that was administered in District A in fall 2013 and in fall 2014.

Figure B.5. Fall 2013 Score Distributions

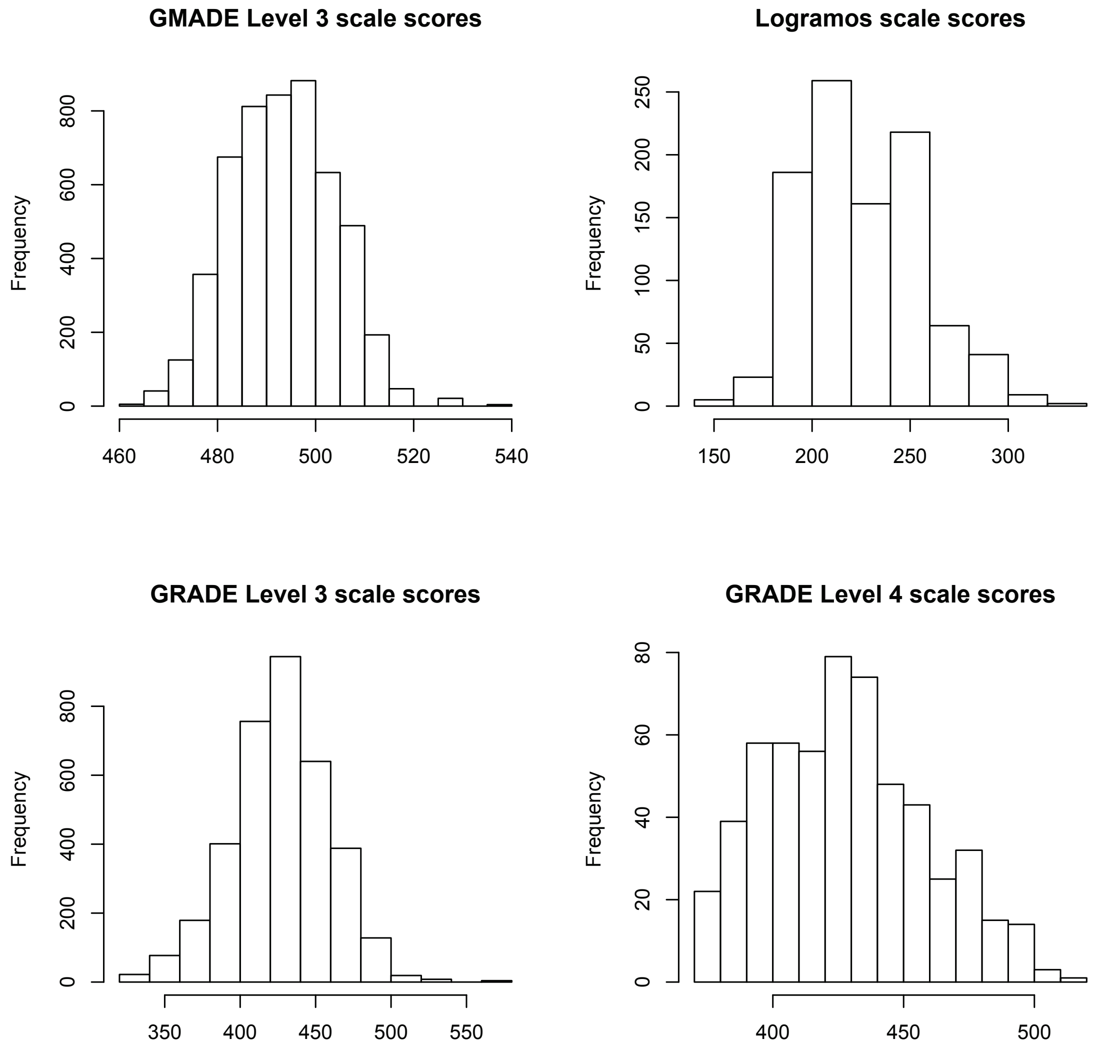
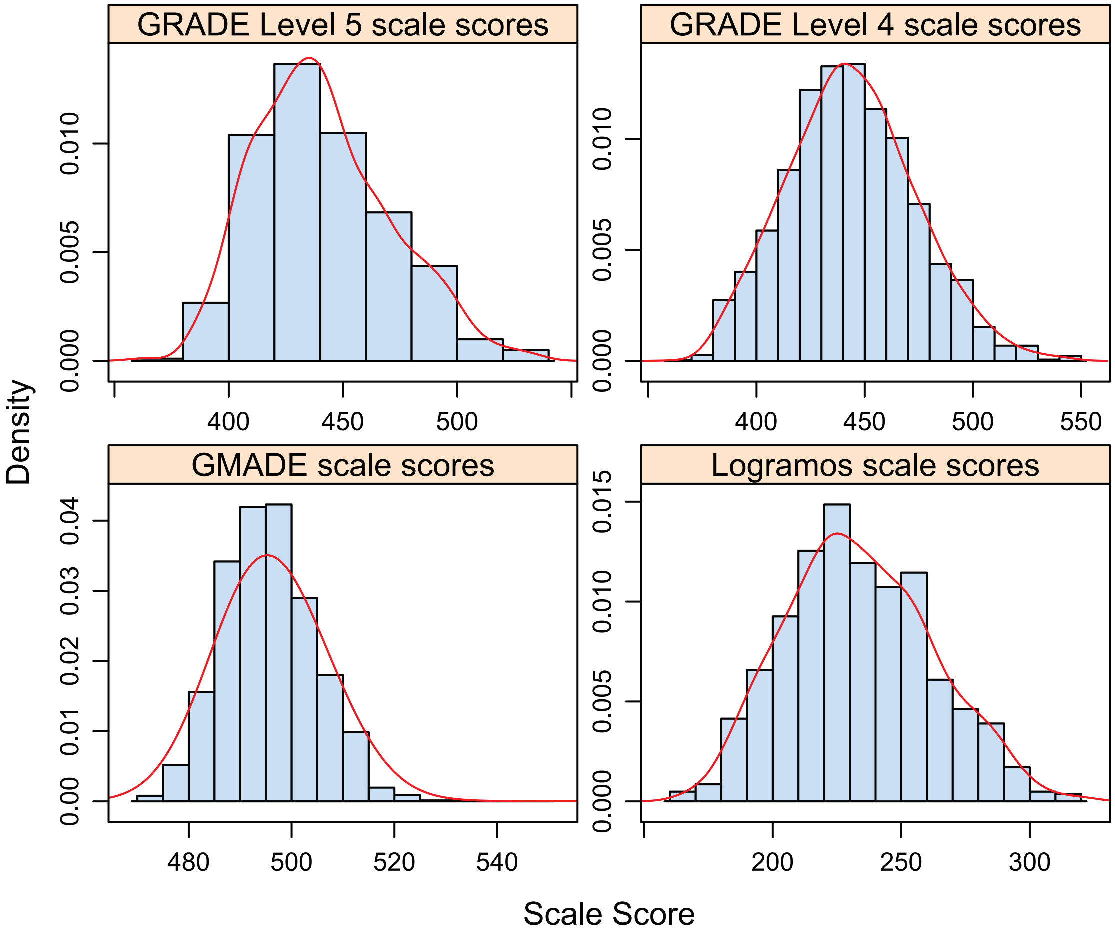


Figure B.6. Fall 2014 Score Distributions



End-of-Course Grades

The five districts varied in their course grade systems. A summary of the grade systems is presented in Tables B.3 and B.4. In spring 2014, District D reported both reading and LA grades, and we computed an average across domains. By spring 2015, this changed to only a reading grade. District B did not give overall course grades, but recorded between four and five domain-specific scores for each subject in each trimester (Table B.4). To maintain interpretable cross-district comparisons, we first rescaled course grades from the final marking period from each district on a 1–5 numeric scale, equivalent to an A–F letter grade system (i.e., 1 = F, 2 = D, 3 = C, 4 = B, and 5 = A). Then, for districts with separate grades in multiple LA domains (e.g., in District D), we created an overall LA grade by averaging across those domains. Similar steps were taken in District B: For mathematics, an overall grade was created by averaging across the four mathematics domain scores; for LA, an overall grade was created by averaging across the eight reading and writing domain scores. Figures B.7 and B.8 display the resulting grade distributions for mathematics and LA in spring 2014 and in spring 2015.

Table B.3. Description of Course Grading Systems

District	Grades	Marking Periods	Mathematics	LA	Reading	English	Writing
A	A–E	Quarter	X		X	X	
B	1–4	Trimester	X		X		X[1]
C	1–9	Quarter	X	X			
D	50–100	Semester	X	X[1]	X		
E	A–F	Quarter	X	X			

NOTE: Table applies to SYs 2013–2014 and 2014–2015.

[1] As of spring 2015, there were two changes: District B no longer had a separate writing grade, and District D no longer had an LA grade separate from reading.

Table B.4. Domains in District B's Language Arts and Mathematics Grading Systems

Subject	Description of Domains Within the Subject
Reading	Reads with fluency and accuracy Understands what is read Reads a variety of material on level Overall reading effort
Writing	Spelling and vocabulary Mechanics and usage Content and organization Style and voice Overall writing effort
Mathematics	Demonstrates fluency/accuracy in number sense Develops and explains strategies to solve problems Understands and applies mathematical thinking Overall math effort

Figure B.7. Histograms of Rescaled SY 2013–2014 Mathematics Course Grades for Each District

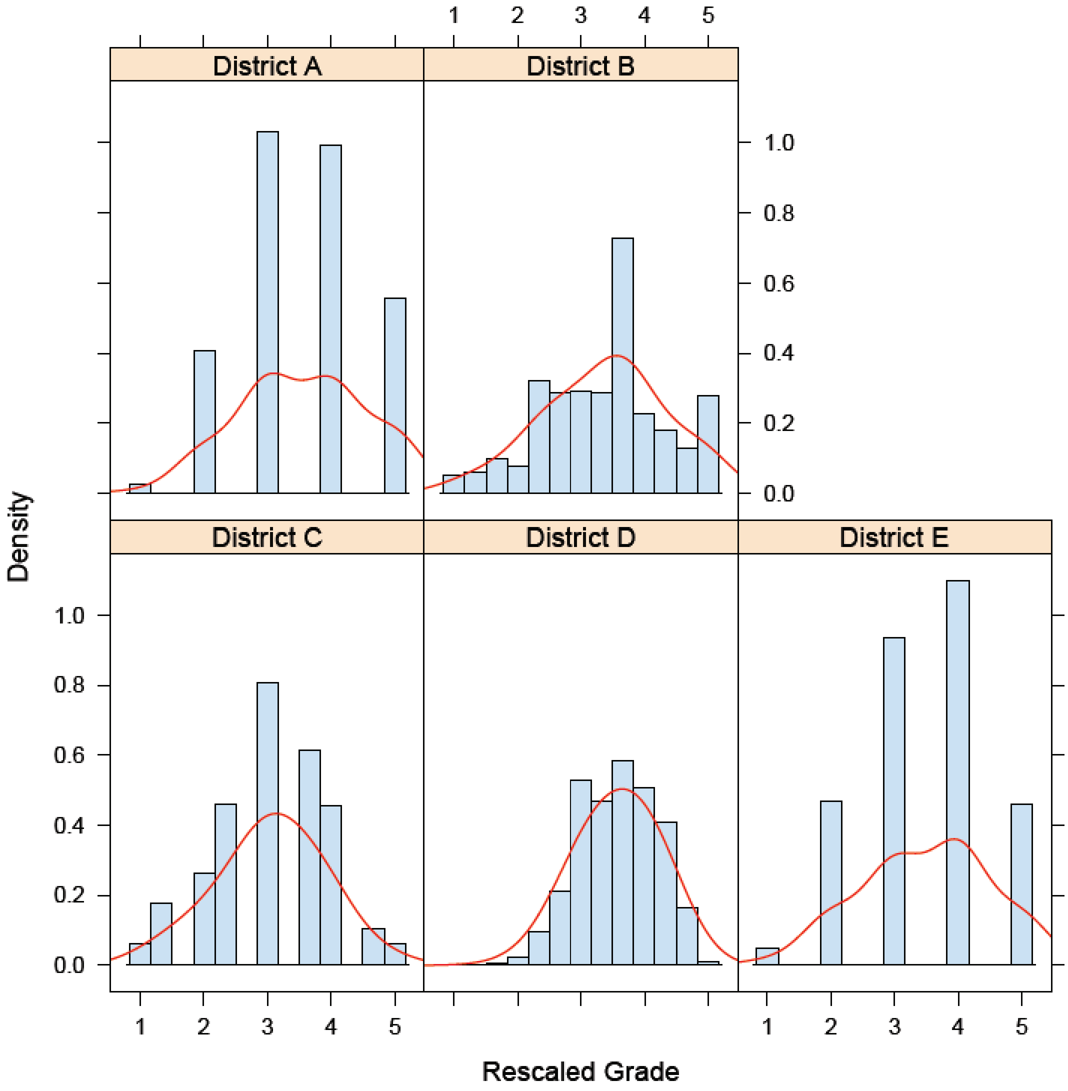
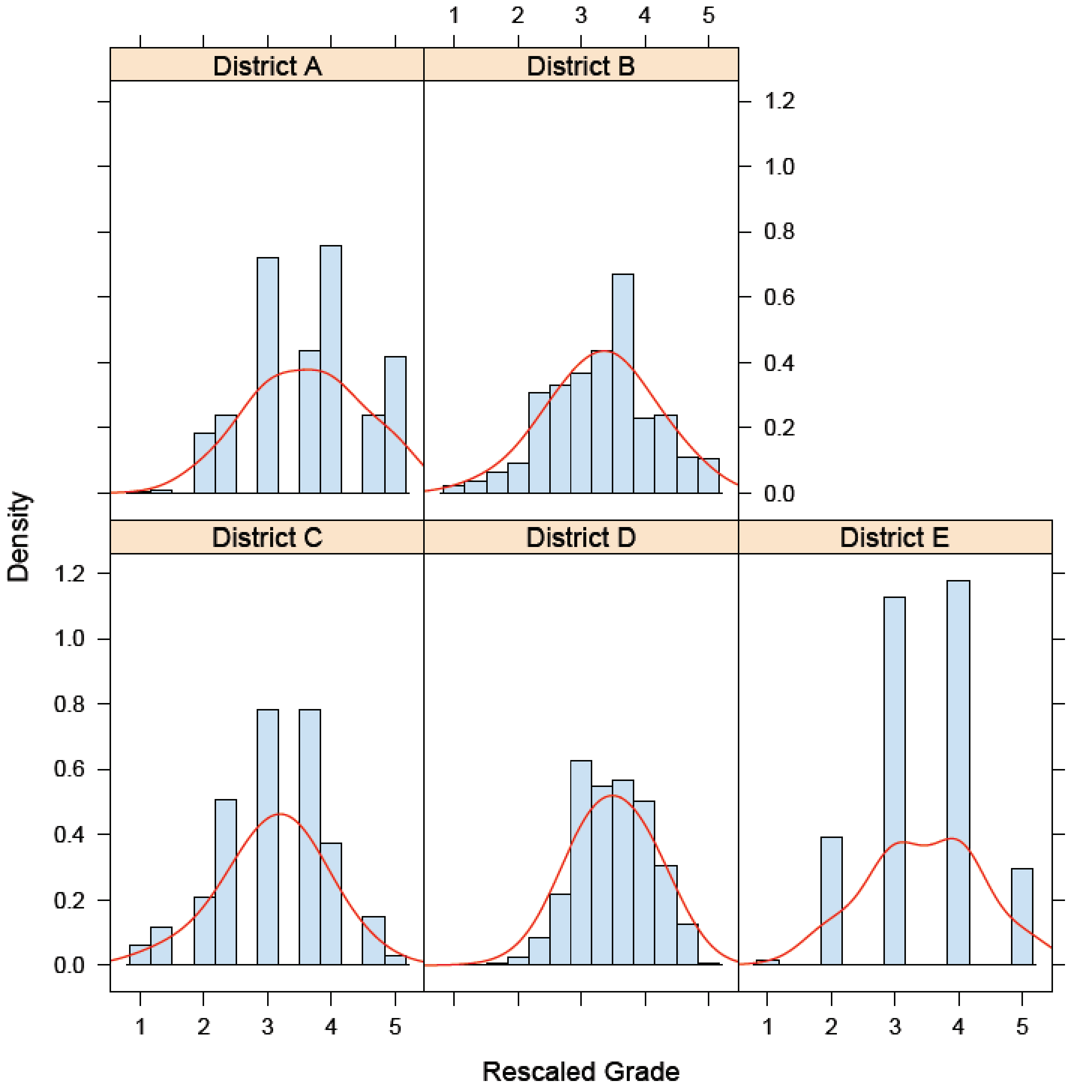


Figure B.8. Histograms of Rescaled SY 2013–2014 Language Arts Course Grades for Each District



Note that in Districts A and E, the grading systems for mathematics were already on a five-point scale, so there was no need for rescaling in these districts. This is why the histograms for those districts have fewer bins and the histograms appear more discrete. In District E, the grading system for LA was also already on a five-point scale, so there was no need for rescaling in this, either. District A also awards grades on a five-point scale; however, the LA grades represent an average of two separate grades: reading and English. This is why District A's LA grade distribution appears more continuous than its mathematics grade distribution.

Figure B.9. Histograms of Rescaled SY 2014–2015 Mathematics Course Grades for Each District

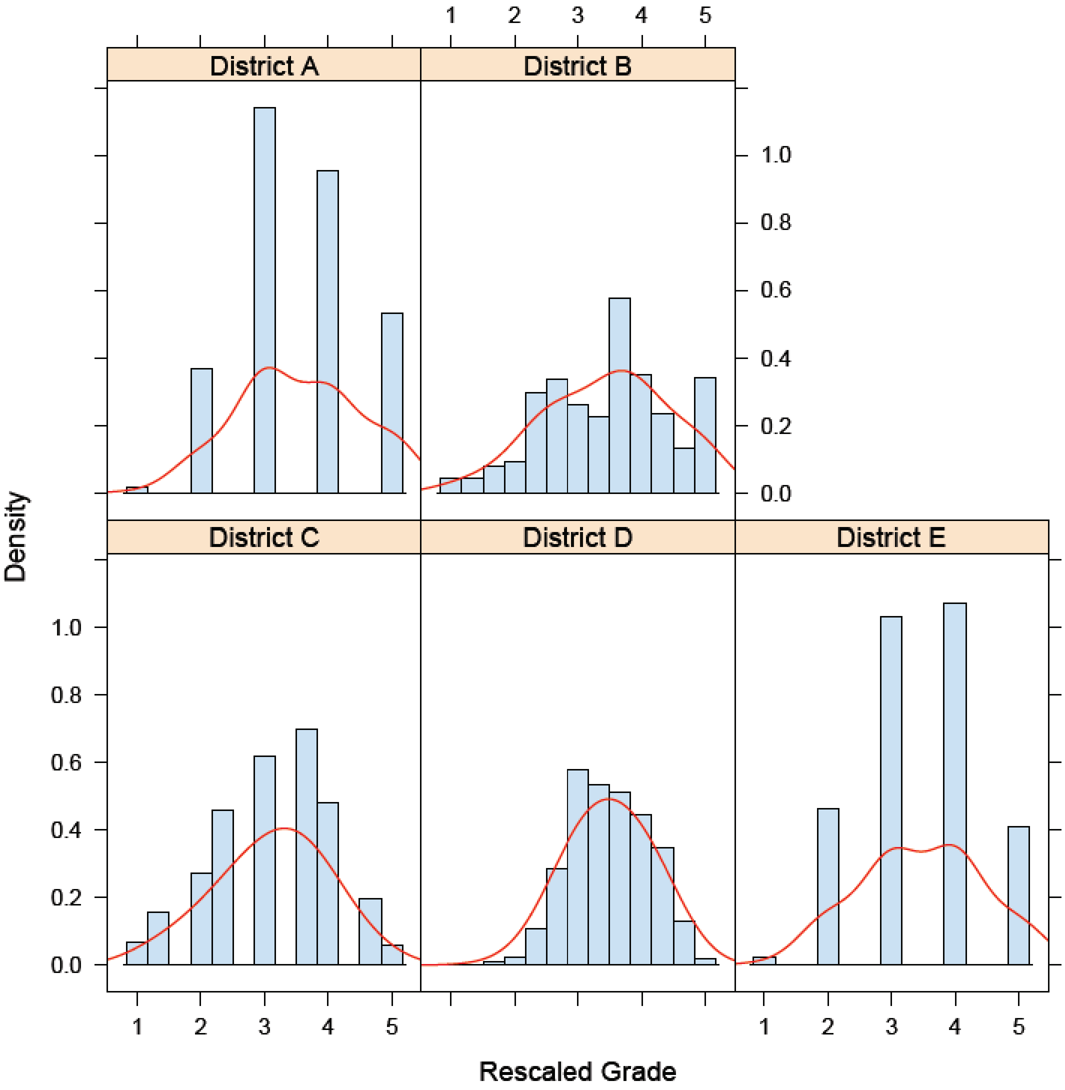
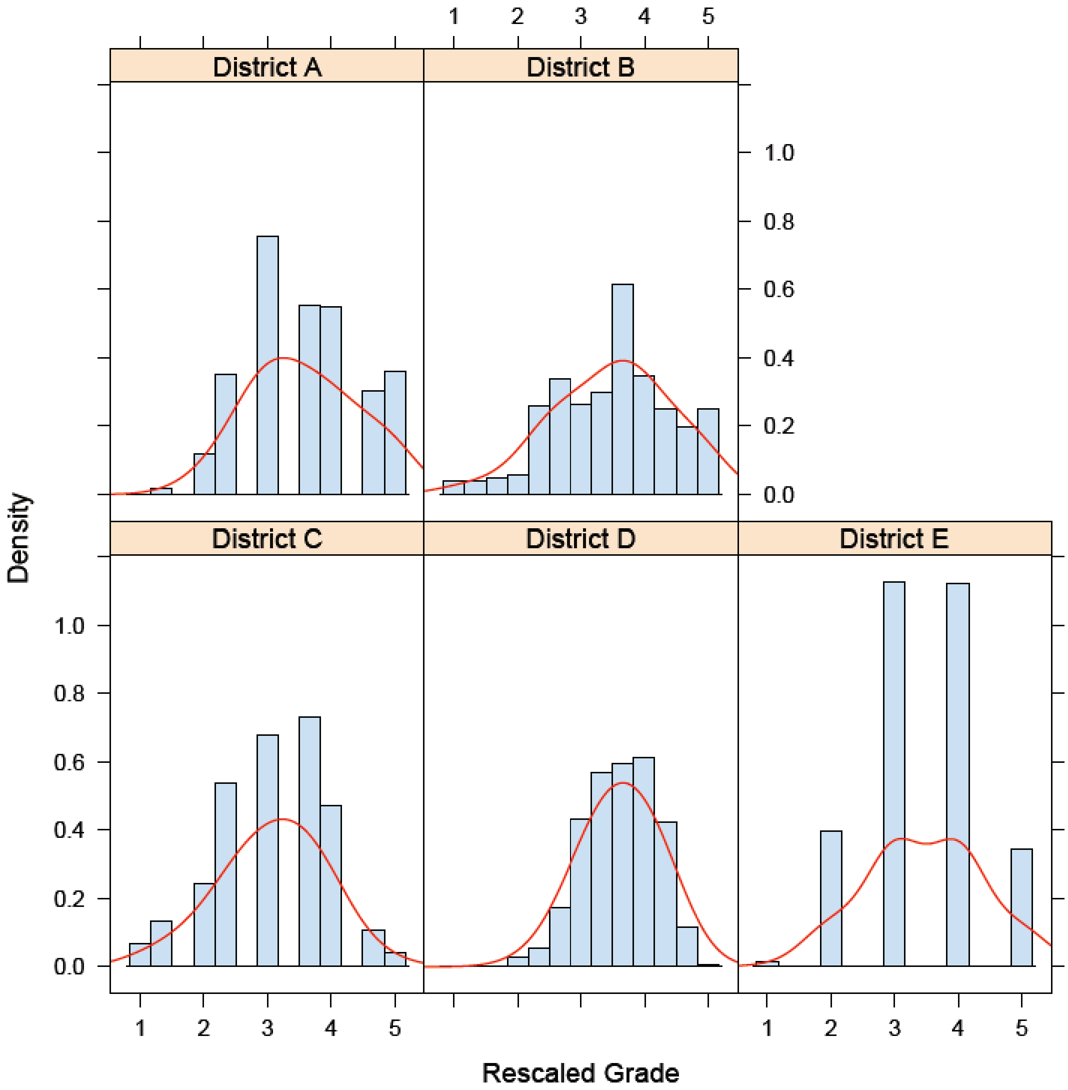


Figure B.10. Histograms of Rescaled SY 2014–2015 Language Arts Course Grades for Each District



Suspensions

As with course grades, the five school districts had different reporting standards for suspension information. In-school suspensions and out-of-school suspensions are reported separately in Districts B and C. Districts A, D, and E report the number of days suspended for each student, but not the type of suspension nor the number of suspensions. For our analysis, we created a variable (ever suspended) to indicate whether a student had ever been suspended (either in school or out of school) during the 2013–2014 academic year or the 2014–2015 academic year. For any students who were missing all other spring outcomes (i.e., course grades, spring standardized test scores, school year attendance), suspension data was assumed missing. Table B.5 displays the percentage of students who were suspended in each district.

Table B.5. Districtwide Suspension Rates for SYs 2013–2014 and 2014–2015

District	*N*	Out-of-School Suspensions (%)	In-School Suspensions (%)	Ever Suspended (%)
2013–2014				
A	857		11.67	
B	1,931	3.37	0.62	3.57
C	886	2.93	0.73	3.39
D	1,034		12.19	
E	612		17.48	
2014–2015				
A	833	5.88	4.92	9.12
B	1,780	4.55	1.24	5.45
C	722	3.74	0.83	4.57
D	874		10.53	
E	573		18.50	

NOTE: *N* = total number of students with available suspension data the sample from each district.

School-Year Attendance

School-year attendance indicates the percentage of total school days in academic year that the student was marked as being in attendance. These data came from school districts in summer following the completed school year and reflect their official attendance statistics. Table B.6 displays the summary statistics for each district.

Table B.6. School-Year Attendance Data for SYs 2013–2014 and 2014–2015 by District

District	*N*	Attendance (%)	SD (%)
2013–2014			
A	856	95.67	4.30
B	1,931	97.62	3.20
C	886	95.07	4.67
D	1,033	93.27	6.35
E	612	94.50	5.50
2014–2015			
A	833	95.80	4.71
B	1780	97.61	2.93
C	722	92.34	13.41
D	874	92.70	8.18
E	573	90.45	10.54

NOTE: *N* = total number of students with available attendance data the sample from each district.

Social-Emotional Outcomes

Broadly, social-emotional competence refers to the ability of students to successfully interact with other students and adults in a way that demonstrates an awareness of, and ability to manage, emotions in an age- and context-appropriate manner. To measure social and emotional well-being in the fall after the summer programs, RAND administered the Devereux Student Strengths Assessment–RAND Research Edition (DESSA-RRE) to school-year teachers who reported on the behaviors of individual study students. RAND administered the DESSA-RRE at two points: fall 2013 and fall 2014. The DESSA is a strength-based assessment that assesses only positive behaviors rather than maladaptive ones.[11]

The DESSA-RRE is one scale of 27 items that RAND staff selected from the original pool of 72 items on the DESSA (LeBuffe, Shapiro, and Naglieri, 2009). RAND selected these items based on their alignment with the school districts' stated goals for their summer programming. Drawing on student data from the DESSA national standardization sample, the developers determined that the pool of 27 items has a high degree of reliability; the 27 items and the corresponding coefficient *Alpha* are listed in Table B.7.

RAND administered the DESSA-RRE online to a treatment student's teacher of record beginning on the first day of the 11th week of the school year. We selected this timing because it was the first time point at which a large majority of students would have been assigned to their teacher of record for at least four weeks. (Each district had determined the teacher of record no later than the first day of the tenth week of the school year.) For each of the 27 items in the DESSA-RRE, the rater is asked to indicate on a five-point scale how often the student engaged in each behavior over the past four weeks.

The survey took approximately five minutes to complete per student, and teachers were given a $20 Amazon gift card per survey completed. Teachers were required to answer 26 of 27 items for a survey to be deemed complete. In fall 2013, we obtained responses from 84.0 percent of teachers of record and for 79.0 percent of the study students. Using district data for students who had left the district as of the time that we administered the DESSA, the effective response rate was 86.4 percent of the still-enrolled student sample. In fall 2014, we obtained responses from 77 percent of teachers of record and for 66.0 percent of the study students. Using district data for students who had left the district as of the time that we administered the DESSA, the effective response rate was 75.1 percent of the still-enrolled student sample.

With the fall 2013 DESSA-RRE results, we performed exploratory factor analyses and identified two subscales with high levels of internal consistency reliability. The items loading on each scale and the scales' reliability are also shown in Table B.7. We generated scores for the scales by averaging responses across the relevant items for each student. To these scales we assigned the names *self-regulation* (for items generally about students' ability to control their

---

[11] Development of the DESSA was led by Paul LeBuffe, Valerie Shapiro, and Jack Naglieri at the Devereux Center for Resilient Children and made publicly available in 2009.

behavior and interactions) and *self-motivation* (for items generally about students' academic focus and drive).

Table B.7. DESSA-RRE Social-Emotional Behavior Scales

Behavior	Overall Social-Emotional Behavior Scale	Self-Regulation Scale	Self-Motivation Scale
Coefficient *Alpha*	0.968	0.969	0.954
During the past four weeks, how often did the child…			
Carry herself/himself with confidence	✓	✓	
Keep trying when unsuccessful	✓	✓	
Say good things about herself/himself	✓		✓
Compliment or congratulate someone	✓		✓
Show good judgment	✓		✓
Pay attention	✓		✓
Wait for her/his turn	✓		✓
Act comfortable in a new situation	✓		✓
Do things independently	✓		✓
Respect another person's opinion	✓		✓
Contribute to group efforts	✓		✓
Do routine tasks or chores without being reminded	✓	✓	
Perform the steps of a task in order	✓		
Show creativity in completing a task	✓	✓	
Share with others	✓	✓	
Accept another choice when first choice was unavailable	✓		
Say good things about the future	✓	✓	
Stay calm when faced with a challenge	✓	✓	
Attract positive attention from adults	✓	✓	
Cooperate with peers or siblings	✓	✓	
Show care when doing a project or school work	✓	✓	
Make a suggestion or request in a polite way	✓	✓	
Learn from experience	✓	✓	
Work hard on projects	✓		✓
Follow rules	✓	✓	
Offer to help somebody	✓	✓	
Adjust well when going from one setting to another	✓	✓	

Student Survey Responses

To understand both the control and treatment groups' participation in any type of camp or summer school activity, the study included a short student survey about their activities during summer 2013 and in summer 2014. At the same time that Mathematica Policy Research administered the GMADE, GRADE, and/or Logramos to students in the study at fall 2013 and fall 2014, Mathematica Policy Research also administered a four- or five-question survey to students. The survey was also translated into Spanish, and students who took the Logramos instead of the GRADE took the Spanish version of the student survey. Given the wide variety of summer programming available to students in the five school districts where the study occurred, the primary purpose of the survey was to gauge the contrast between the treatment and control group's exposure to any type of summer programming during summer 2013 and summer 2014.

Table B.8 reports the number of students who completed the student survey and the proportions who answered each item in summer 2013. The table first summarizes the language in which students took the survey, and then the treatment and control group responses to each survey item. Table B.9 reports the same information and the slight deviations plus a new question added to the summer 2014 survey.

Table B.8. Summer 2013 Student Survey Responses

	Treatment Group Respondents		**Control Group Respondents**	
Survey Item	***N***	**Percentage**	***N***	**Percentage**
Language in which student took the survey				
English	2,429	82.9	1,737	78.8
Spanish	500	17.1	468	21.2
At home this last summer, I read a book or a magazine				
Never	370	12.7	321	14.7
A few times this summer	1,563	53.6	1,159	52.9
At least once a week	986	33.8	710	32.4
This last summer, I went to camp or summer school				
Did not go to camp or summer school	546	18.7	1,270	57.7
Went for a few days	238	8.1	125	5.7
Went for one week	154	5.3	111	5.0
Went for a few weeks	728	24.9	253	11.5
Went for at least a month	1,256	43.0	442	20.1
I did reading and writing at my camp or summer school this last summer				
Did not go to camp or summer school	544	18.8	1,269	58.6
No	168	5.8	230	10.6
Yes	2,179	75.4	665	30.7
I did math at my camp or summer school this last summer				
Did not go to camp or summer school	545	18.8	1,272	58.7
No	185	6.4	363	16.7
Yes	2,169	74.8	533	24.6

NOTE: Not all students answered each of the four survey questions. Thus, the sum of respondents for each item is not always equal.

Table B.9. Summer 2014 Student Survey Responses

	Treatment Group Respondents		Control Group Respondents	
Survey Item	***N***	**Percentage**	***N***	**Percentage**
Language in which student took the survey				
English	2,130	83.1	1,558	79.4
Spanish	432	16.9	405	20.6
At home this last summer, I read a book or a magazine				
Never	360	14.1	269	13.7
A few times this summer	874	34.1	725	36.9
At least once a week	646	25.2	531	27.0
Every day or almost every day	681	26.6	437	22.3
This last summer, I went to camp or summer school				
I did not go to camp or summer school	809	31.6	1,263	64.4
Went for a few days	124	4.9	83	4.2
For one week	69	2.7	74	3.4
For two weeks	96	3.7	60	3.1
For three weeks	208	8.1	91	4.6
For at least one month	1,253	49.0	391	19.9
I did reading at my camp or summer school this last summer almost every day				
Did not go to camp or summer school	787	30.8	1,219	62.5
No	283	11.1	265	13.6
Yes	1,486	58.1	466	23.9
I did math at my camp or summer school this last summer almost every day				
Did not go to camp or summer school	775	30.4	1,198	61.2
No	230	9.0	361	18.5
Yes	1,545	60.6	398	20.3
Which of these things did you do every day or almost every week last summer?				
Took care of a younger sibling or relative during the day	1,025	40.0	771	39.3
Went to a place like a pool, park, theme park, mall, or movie theater	1,966	76.7	1,535	78.2
Went to an educational place like a library or museum	811	31.6	572	29.1
Played with kids in my neighborhood	1,346	52.5	1,022	52.1
Stayed mostly in the house	803	31.3	697	35.5
I did not do any of these things	94	3.7	53	2.7

NOTE: Not all students answered each of the four survey questions. Thus, the sum of respondents for each item is not always equal.

Summer Teacher Survey

RAND administered a five-to-ten–minute survey to all teachers of mathematics and LA in summer 2013 and again in summer 2014. Table B.10 shows the response rates and aggregated responses to the survey items. In summer 2013, the number of mathematics and LA teacher respondents per district ranged from a minimum of 16 (out of 16 possible) to a maximum of 59 respondents (out of 66 possible) respondents per district. In summer 2014, the number of mathematics and LA teacher respondents per district ranged from a minimum of 12 (out of 12 possible) to a maximum of 40 respondents (out of 45 possible) respondents per district. As shown in Table B.10, we obtained response rates of 89 to 100 percent of summer teachers in each district in summer 2013 and 98 to 100 in each district in summer 2014. We offered a $20 Amazon gift card to teachers who completed the survey. In all districts except for Boston, eligible teachers were first emailed a link to an electronic version of the survey. For nonresponders, RAND disseminated paper copies. In Boston, only paper was administered because Internet access was not available at each of the summer sites. The teacher survey was administered during the third through fifth week of the summer program within each district. The survey followed the same format in each district, with minor customization to reflect site names within each district and different dates and format of professional development offered to summer teachers. At the end of this appendix is a sample of the full Boston survey summer 2013 and summer 2014.

Table B.10. Response Rates on Summers 2013 and 2014 Academic Teacher Surveys

	District				
Survey Item	**A**	**B**	**C**	**D**	**E**
Summer 2013					
Mathematics					
Total teachers	27	27	33	8	17
Total respondents	30	24	30	8	17
Language Arts					
Total teachers	29	27	36	8	22
Total respondents	30	26	32	8	20
Total survey respondents[a]	40	40	59	16	37
Overall response rate	100%	89%	89%	100%	95%
Summer 2014					
Mathematics					
Total teachers	25	19	19	6	30
Total respondents	25	19	19	6	30
Language Arts					
Total teachers	26	19	26	6	32
Total respondents	26	19	26	6	31
Total survey respondents[a]	37	36	40	12	53
Overall response rate	100%	100%	100%	100%	98%

[a] Some teachers teach both subjects. Totals may be less than the sum of mathematics and LA teachers because some teachers taught both subjects.

Table B.11. Summer 2013 Academic Teachers' Views of their Summer Program

	District				
Survey Item	**A**	**B**	**C**	**D**	**E**
Number of respondents (*N*)	40	40	59	16	37
Participation rate	100	89	89	100	95
Background					
Taught in summer program in 2012	40	35	25	63	32
Worked with same students during SY 2012–2013	46	38	23	69	46
Quality and Structure of Summer Program					
This program is well managed and well organized	93	80	72	69	95
Support staff (e.g., camp counselors, paraprofessionals, instructional aides, tutors) provide necessary support in my classroom	90	88	81	94	86
There is a clear procedure for handling student discipline problems	80	83	88	88	81
The procedure for handling student discipline problems is effective	83	80	83	63	73
Climate and culture of summer program					
Administrators in this program care about students and teachers	100	100	97	100	95
Teachers listen to students when they have a problem	100	95	98	94	97
Faculty and staff make students feel cared for	100	98	100	94	100
Faculty and staff treat students with respect	100	98	100	94	100
Teachers enjoy teaching here	93	97	97	81	94
Faculty and staff remind students to be friendly and respectful to each other	98	100	100	100	97
About students in summer program					
Due to student misbehavior, a great deal of learning time is wasted	38	18	34	56	57
Students enjoy this summer program	95	100	83	88	95
Students solve problems without fighting or saying mean things	75	85	90	75	64
Students feel safe traveling to and being in this school	100	100	98	100	95
Students treat adults in school with respect	88	85	95	88	84
Children get into physical fights with other students at school at least once a week	8	5	7	38	35
Children are bullied and harassed by other students at least once a week	21	5	15	50	57

NOTES: Unless otherwise noted, values shown in table are the percentage of respondents who agree or strongly agree. Sources: Data from RAND's academic teacher surveys administered in summer 2013.

Table B.12. Summer 2013 Academic Teachers' Views of PD and Program Curricula

	District				
Survey Item	**A**	**B**	**C**	**D**	**E**
Number of respondents (*N*)	59	37	40	40	16
Participation rate	89	95	89	100	100
Professional development					
Prepared me to teach the math curriculum well (among math teachers)	93	94	95	86	63
Prepared me to teach the LA curriculum well (among LA teachers)	96	100	58	83	63
Opinions about mathematics curriculum among mathematics teachers					
I was provided a written math curriculum	100	100	88	100	63
I was provided a pacing guide indicating which math topics are to be taught each week	100	100	96	97	50
I received lesson plans to use for my math classes	83	100	83	83	50
I obtained the math instructional materials (textbooks, curricular guides, lesson plans) with sufficient time to prepare for the first day of class	50	94	75	93	75
I received information about students' IEPs or special needs prior to the first day of class	27	44	25	17	25
I received school year data on my students' prior math performance to help inform my instruction	7	18	13	60	0
I received summer math pretest results for my students	55	59	92	86	75
The planned pacing of the curriculum was reasonable	90	88	96	73	75
Children's math skills are improving as a result of this program	83	100	96	93	63
The math curriculum is clear for me to follow	93	94	100	100	71
The math curriculum includes fun, interesting activities for children	87	100	88	77	13
The math curriculum content is too difficult for a majority of students in my class	17	59	17	30	38
The math curriculum content is too easy for a majority of students in my class	52	18	25	60	50
The math curriculum addresses gaps that many students have from last year	86	76	79	80	50
Opinions about LA curriculum among LA teachers					
My school/site grouped students by ability in LA for classroom assignment	41	41	23	40	75
I was provided a written LA curriculum	97	100	88	100	100
I was provided a pacing guide indicating which LA topics are to be taught each week	97	100	92	97	100
I received lesson plans to use for my LA classes	88	100	92	93	100
I received information about students' IEPs or special needs prior to the first day of class	13	29	15	20	13
I received school year data on my students' prior LA performance to help inform my instruction	3	6	0	67	0
I received summer LA pre-test data for my students	25	41	54	37	0
The planned pacing of the curriculum was reasonable	94	82	84	50	75
Children's LA skills are improving as a result of this program	94	100	88	97	100
The LA curriculum is clear for me to follow	97	100	88	77	100
The LA curriculum includes fun, interesting activities for children	69	100	69	90	75
The LA curriculum content is too difficult for a majority of students in my class	34	35	27	47	25
The LA curriculum content is too easy for a majority of students in my class	47	41	15	17	13
The LA curriculum addresses gaps that many students have from last year	74	75	69	67	38
The LA curriculum provides students texts that are appropriate for their reading level	94	76	69	87	88

NOTE: IEP = individualized education plan. Unless otherwise noted, values shown in table are the percentage of respondents who agree or strongly agree.

Table B.13. Summer 2014 Academic Teachers' Views of Their Summer Program by District

	District				
Survey Item	**A**	**B**	**C**	**D**	**E**
Number of respondents (*N*)	38	51	36	37	11
Background					
Taught in summer program in 2013	34	82	56	43	73
Worked with same students during SY 2013–2014	36	67	25	62	75
Quality and structure of summer program					
This program is well managed and well organized	93	100	85	100	75
Support staff (e.g., camp counselors, paraprofessionals, instructional aides, tutors) provides necessary support in my classroom	93	100	88	95	92
There is a clear procedure for handling student discipline problems	95	100	89	89	92
The procedure for handling student discipline problems is effective	90	98	91	86	83
Climate and culture of summer program					
Teachers listen to students when they have a problem	100	100	100	100	100
Faculty and staff make students feel cared for	100	100	97	100	100
Faculty and staff treat students with respect	100	100	97	100	100
Teachers enjoy teaching here	100	100	94	97	92
Faculty and staff remind students to be friendly and respectful to each other	100	100	100	97	100
About students in summer program					
Due to student misbehavior, a great deal of learning time is wasted	28	25	22	22	33
Students enjoy this summer program	100	100	89	94	83
Students solve problems without fighting, or saying mean things	93	89	92	83	58
Students feel safe travelling to and being in this school	100	100	97	100	100
Students treat adults in school with respect	92	98	97	94	100
Children get into physical fights with other students at school at least once a week	15	2	6	0	33
Children are bullied and harassed by other students at least once a week	15	19	14	8	42

NOTE: Unless otherwise noted, values shown in table are the percentage of respondents who agree or strongly agree.

Table B.14. Summer 2014 Academic Teachers' Views of the PD and Curriculum for Their Summer Program by District

	District				
Survey Item	**A**	**B**	**C**	**D**	**E**
Number of respondents (*N*)	44	35	11	34	36
Professional development					
Prepared me to teach the math curriculum well (among math teachers)	100	100	88	80	58
Prepared me to teach the LA curriculum well (among LA teachers)	100	93	83	72	76
Opinions about mathematics curriculum among mathematics teachers					
My school/site grouped students by ability in math for classroom assignment	60	26	50	29	40
By the first day of the program, I was provided a written math curriculum	100	100	100	100	84
By the first day of the program, I was provided a pacing guide indicating which math topics are to be taught each week	100	100	100	100	80
By the first day of the program, I received lesson plans to use for my math classes	100	100	100	79	84
I received information about students' IEPs or special needs	87	42	50	11	24
I received school year data on my students' prior math performance	30	5	67	5	8
I received summer math pre-test results for my students	77	5	0	17	67
The planned pacing of the curriculum was reasonable	100	89	83	95	76
Children's math skills are improving as a result of this program	100	84	83	100	100
The math curriculum is clear for me to follow	100	95	100	100	76
The math curriculum includes fun, interesting activities for children	100	100	50	95	68
The math curriculum is aligned with the curriculum students received in the past school year	100	95	83	84	88
The math curriculum is aligned with the curriculum students will receive in the next school year	100	89	50	95	79
The math curriculum content is too difficult for the Level I students in my class	66	25	50	12	70
The math curriculum content is too easy for the Level I students in my class	0	0	33	0	9
The math curriculum content is too difficult for the Level II students in my class	17	0	0	13	21
About students in summer program					
Due to student misbehavior, a great deal of learning time is wasted	25	28	33	22	22
Students enjoy this summer program	100	100	83	89	94
Students solve problems without fighting, or saying mean things	89	93	58	92	83
Students feel safe travelling to and being in this school	100	100	100	97	100
Students treat adults in school with respect	98	92	100	97	94
Children get into physical fights with other students at school at least once a week	2	15	33	6	0
Children are bullied and harassed by other students at least once a week	19	15	42	14	8

NOTE: Unless otherwise noted, values shown in table are the percentage of respondents who agree or strongly agree.

Table B.14—Continued

Survey Item	District A	B	C	D	E
Opinions about LA curriculum among LA teachers					
My school/site grouped students by ability in LA for classroom assignment	48	31	67	16	42
By the first day of the program, I was provided a written LA curriculum	100	92	100	89	92
By the first day of the program, I was provided a pacing guide indicating which LA topics are to be taught each week	100	100	100	100	81
By the first day of the program, I received lesson plans to use for my LA classes	100	96	83	89	73
I received information about students' IEPs or special needs	87	42	83	21	31
I received school year data on my students' prior LA performance	33	16	83	11	35
I received summer LA pre-test data for my students	74	12	50	11	15
The planned pacing of the curriculum was reasonable	90	88	50	79	71
Children's LA skills are improving as a result of this program	100	100	83	95	92
The LA curriculum is clear for me to follow	100	96	100	100	81
The LA curriculum includes fun, interesting activities for children	81	96	50	63	81
The LA curriculum is aligned with the curriculum students received in the past school year	100	88	60	74	84
The LA curriculum is aligned with the curriculum students will receive in the next school year	100	96	60	95	92
The LA curriculum content is too difficult for the Level I students in my class	81	36	67	71	52
The LA curriculum content is too easy for the Level I students in my class	0	0	0	0	0
The LA curriculum content is too difficult for the Level II students in my class	29	4	50	50	27
The LA curriculum content is too easy for the Level II students in my class	0	4	0	0	0
The LA curriculum content is too difficult for the Level III students in my class	0	4	0	11	4
The LA curriculum content is too easy for the Level III students in my class	7	33	33	0	0
The LA curriculum content is too difficult for the Level IV students in my class	0	—	0	0	0
The LA curriculum content is too easy for the Level IV students in my class	25	—	50	16	21
The LA curriculum addresses gaps that many students have from last year	94	92	50	50	80
The LA curriculum provides students texts that are appropriate for their reading level	97	92	50	74	96

NOTE: Unless otherwise noted, values shown in table are the percentage of respondents who agree or strongly agree.

Classroom Observations

We conducted observations of academic and enrichment instruction in the five districts, using the same protocol for both. To create our observation protocol, we first reviewed some widely used validated instruments (such as the Classroom Assessment Scoring System measure developed at the University of Virginia [Teachstone Training, 2016] and the Framework for Teaching developed by Charlotte Danielson [Danielson Group, 2013]). These classroom observation instruments, however, were not necessarily designed to analyze aspects of the classroom that research about *summer programming* indicates are the most important features linked to improvements in student achievement.

Consequently, RAND developed its own classroom observation protocol designed specifically to measure certain key aspects of our theoretical framework about how summer programs might lead to gains in student learning. The classroom observation protocol was intended to gather information on the quality of instruction, time on academic task, and other aspects of the classroom, such as opportunities for social and emotional development. We developed our own classroom observation protocol to pilot in summer 2011, designed specifically to measure certain key aspects of the theoretical framework about how summer programs might lead to gains in student learning. Based on our experience piloting the 2011 protocol, we further refined it for summer 2012 and used the revised observation protocol for both academic and enrichment classroom observations. We again refined it in summer 2013 and finally in summer 2014. The summer 2014 protocol is provided at the end of this appendix. Table B.15 indicates the number of classroom observations that RAND performed in each of the two summers.

The core domains of the classroom are described below:

- **Time on task.** The amount of productive time on task is positively linked to student achievement (Harnischfeger and Wiley, 1976; Lomax and Cooley, 1979; Fisher et al., 1980; Karweit and Slavin, 1982; Hawley et al., 1984; Karweit, 1985). The classroom observation protocol has a running time log of minutes spent on instruction versus noninstruction and observers categorize the types of instruction occurring throughout the class.
- **Student engagement**. Student engagement in tasks leads to greater academic achievement (Skinner, Kindermann, and Furrer, 2009). We therefore asked observers to rate in various ways how engaged students appeared.
- **Indicators of instructional quality**. At the end of each observed class period, the rater completed items relating to the safety of the instructional space, adequacy of materials, teacher/student interactions, student/student interactions, class routines, behavior management, instruction clarity, content clarity, factual accuracy of instructional content, provision of time for student independent practice, and student/student discussion of class

content. Raters also recorded final impressions, and described the kind of questions the teacher posed to students throughout the class.

Especially to measure time on task, RAND observers stayed in and coded the entire class, whether it was 30 or 120 minutes to code the amount of intended time spent on instruction.

Table B.15. Number of Classroom Observations Each Summer

	District					
Subject Area	**A**	**B**	**C**	**D**	**E**	**Total**
Summer 2013						
LA	57	56	46	16	36	**211**
LA—Writing	NA	NA	NA	NA	24	**24**
Walk to Intervention (LA—WTI)	NA	NA	NA	NA	47	**47**
Enrichment	66	71	70	11	22	**240**
Mathematics	34	56	38	16	34	**178**
Science	NA	NA	26	NA	NA	**26**
Success Maker	NA	NA	22	NA	NA	**22**
4go5 LA	NA	4	11	4	NA	**19**
4go5 Math	NA	3	9	4	NA	**16**
Total	**157**	**190**	**222**	**51**	**163**	**783**
Summer 2014						
LA	46	36	32	12	37	**163**
Enrichment	46	53	76	15	17	**207**
Walk to Intervention (LA—WTI)	NA	NA	NA	NA	45	**45**
Mathematics	36	34	33	12	32	**147**
Science	NA	NA	28	NA	NA	**28**
Social Studies	NA	NA	3	NA	NA	**3**
iReady	NA	NA	14	NA	NA	**14**
Total	**128**	**123**	**186**	**39**	**132**	**608**

Tables B.16–B.25 provide descriptive statistics derived from the summer 2014 classroom observations.

Table B.16. Overview of Instructional Time by District in Summer 2014

	District					
Subject Area	**A**	**B**	**C**	**D**	**E**	**Total**
Mathematics						
Average number of students per classroom	8	10	13	9	10	8
Average intended class minutes	60	77	88	96	118	60
Average actual class minutes as percentage of intended (%)	100	99	100	96	93	100
Percentage of actual class minutes that were off task (%)	16	10	13	22	14	16
Percentage of actual class minutes devoted to independent practice (%)	26	31	40	28	38	26
Language arts						
Average number of students per classroom	10	10	12	9	9	10
Average intended class minutes	79	86	88	96	114	79
Average actual class minutes as percentage of intended (%)	109	110	98	97	95	109
Percentage of actual class minutes that were off task (%)	19	14	20	24	10	19
Percentage of actual class minutes devoted to independent practice (%)	17	49	19	35	36	17
Enrichment						
Average number of students per classroom	10	10	12	9	9	10
Average intended class minutes	79	86	88	96	144	79
Average actual class minutes as percentage of intended (%)	109	110	98	97	95	109
Percentage of actual class minutes that were off task (%)	19	14	20	24	10	19
Percentage of actual class minutes devoted to independent practice (%)	17	49	19	35	36	17

NOTE: Values shown in the total column are the weighted average of the classrooms RAND observed.

Table B.17. Summer 2014 Mathematics Practices That Support Student Engagement by District

	District				
Description	**A (*N* = 34)**	**B (*N* = 36)**	**C (*N* = 33)**	**D (*N* = 12)**	**E (*N* = 32)**
Prior to students doing independent practice, the teacher explained or wrote down what students would do or what skills they would cover during the overall session.	94	92	82	92	97
Prior to students doing independent practice, the teacher states the purpose for what they will do (i.e., why students would learn the skill in terms of real world relevance)	6	11	6	8	0
Large majority of students are on-task throughout class period	76	67	82	73	88
Teacher: (1) performs ongoing assessment throughout the whole class period by checking for students' understanding of content, and (2) addresses misunderstanding if and as they arise	59	72	24	75	56
Little to no time is wasted; pacing is efficient. The class resembles a "well-oiled machine" where a majority of students know what is expected of them and how to go about doing it throughout the whole class	53	58	52	42	69

NOTE: Values shown are the percentage of classroom observations rated yes out of total rated observations.

Table B.18. Summer 2014 Desirable Mathematics Classroom Practices by District

	District				
Description	**A (*N* = 34)**	**B (*N* = 36)**	**C (*N* = 33)**	**D (*N* = 12)**	**E (*N* = 32)**
Students respect one another	100	100	91	83	97
All or almost all students exhibited obvious signs of enthusiasm for the class throughout the class period (e.g., jumping out of seat, quickly and enthusiastically answering teacher's questions)	3	0	12	17	3
The teacher (a) explicitly encouraged at least one student struggling with a particular task to persist at academic/ content-related tasks that were difficult for them or (b) explicitly taught students strategies to persist at tasks	9	28	39	25	41
There was a helpful adult other than the teacher in the classroom (Helpful means the adult worked directly with students, engaged in the material, or supporting students learning while they were in the room for a majority of the class time)	9	42	21	92	38
Students verbally encourage each other, are overtly friendly and supportive	15	0	9	8	3
Teacher shows explicit signs of caring and positive affect toward youth	21	19	45	25	59
Students show explicit signs they have warm, positive affect to teacher	12	8	24	8	41
Lesson is characterized by appropriately challenging, rigorous tasks that engage critical thinking skills	27	39	18	33	13
The teacher *explicitly* taught social skills such as respecting, listening, cooperating with, or helping others or teaching of politeness	0	0	6	0	3

NOTE: Values shown are the percentage of classroom observations rated yes out of total rated observations.

Table B.19. Summer 2014 Undesirable Mathematics Classroom Practices by District

	District				
Description	**A (*N* = 34)**	**B (*N* = 36)**	**C (*N* = 33)**	**D (*N* = 12)**	**E (*N* = 32)**
The teacher provided or failed to correct factually inaccurate information that would confuse students about the content/skills they were to learn	9	0	24	0	0
The explanation of the instructional content was unclear, hard to follow, incomplete, or inconsistent	15	6	21	8	6
In at least one instance, the teacher was disrespectful to students	0	0	9	8	16
There was one or more flagrant instance of student misbehavior	0	0	0	0	3
When the teacher disciplined students, the majority of the class was interrupted for a long period	12	0	3	8	6
The teacher responsible for the activity was disengaged in the classroom because of distractions by factors that were within her control	9	8	6	0	9
There were adults other than the teacher in the classroom who engaged in activities that distracted from learning (e.g., checking cell phone, interrupting the lesson, asking off-topic questions)	6	3	6	8	6

NOTE: Values shown are the percentage of classroom observations rated yes out of total rated observations.

Table B.20. Summer 2014 LA Practices That Support Student Engagement by District

	District				
Description	A (*N* = 37)	B (*N* = 36)	C (*N* = 32)	D (*N* = 12)	E (*N* = 46)
Prior to students doing independent practice, the teacher explained or wrote down what students would do or what skills they would cover during the overall session	100	94	88	67	96
Prior to students doing independent practice, the teacher states the purpose for what they will do (i.e., why students would learn the skill in terms of real world relevance)	0	8	6	17	4
Large majority of students are on-task throughout class period.	65	86	72	50	65
Teacher: (1) performs ongoing assessment throughout the whole class period by checking for students' understanding of content, and (2) addresses misunderstanding if and as they arise	8	58	22	58	50
Little to no time is wasted; pacing is efficient. The class resembles a “well-oiled machine” where a majority of students know what is expected of them and how to go about doing it throughout the whole class	43	83	41	25	26

NOTE: Values shown are the percentage of classroom observations rated yes out of total rated observations.

Table B.21. Summer 2014 Desirable LA Classroom Practices by District

	District				
Description	A (*N* = 37)	B (*N* = 36)	C (*N* = 32)	D (*N* = 12)	E (*N* = 46)
Students respect one another	92	100	84	75	98
All or almost all students exhibited obvious signs of enthusiasm for the class throughout the class period (e.g., jumping out of seat, quickly & enthusiastically answering teacher’s questions)	3	17	9	0	2
The teacher (a) explicitly encouraged at least one student struggling with a particular task to persist at academic/content-related tasks that were difficult for them or (b) explicitly taught students strategies to persist at tasks	27	11	16	0	17
There was a helpful adult other than the teacher in the classroom. Helpful means the adult worked directly with students, engaged in the material, or supporting students learning while they were in the room for a majority of the class time	49	11	13	100	63
Students verbally encourage each other, are overtly friendly and supportive	8	11	19	8	4
Teacher shows explicit signs of caring and positive affect toward youth	24	28	38	25	20
Students show explicit signs they have warm, positive affect to teacher	16	14	13	17	20
Lesson is characterized by appropriately challenging, rigorous tasks that engage critical thinking skills	5	25	9	0	15
The teacher *explicitly* taught social skills such as respecting, listening, cooperating with, or helping others or teaching of politeness	5	6	3	17	7

NOTE: Values shown are the percentage of classroom observations rated yes out of total rated observations.

Table B.22. Summer 2014 Undesirable LA Classroom Practices by District

	District				
Description	A (*N* = 37)	B (*N* = 36)	C (*N* = 32)	D (*N* = 12)	E (*N* = 46)
The teacher provided or failed to correct factually inaccurate information that would confuse students about the content/skills they were to learn	0	3	13	17	0
The explanation of the instructional content was unclear, hard to follow, incomplete, or inconsistent	11	0	3	25	4
In at least one instance, the teacher was disrespectful to students	22	0	0	17	0
There was one or more flagrant instance of student misbehavior	5	0	0	0	2
When the teacher disciplined students, the majority of the class was interrupted for a long period	16	19	3	0	2
The teacher responsible for the activity was disengaged in the classroom because of distractions by factors that were within her control	22	0	9	0	2
There were adults other than the teacher in the classroom who engaged in activities that distracted from learning (e.g. checking cell phone, interrupting the lesson, asking off-topic questions)	11	8	0	0	4

NOTE: Values shown are the percentage of classroom observations rated yes out of total rated observations.

Table B.23. Summer 2014 Enrichment Practices That Support Student Engagement by District

	District				
Description	A (*N* = 46)	B (*N* = 15)	C (*N* = 76)	D (*N* = 53)	E (*N* = 17)
Prior to students doing independent practice, the teacher explained or wrote down what students would do or what skills they would cover during the overall session	82	67	70	85	100
Prior to students doing independent practice, the teacher states the purpose for what they will do (i.e., why students would learn the skill in terms of real world relevance)	4	0	7	4	0
Large majority of students are on task throughout class period	84	73	89	70	82
Teacher: (1) performs ongoing assessment throughout the whole class period by checking for students' understanding of content, and (2) addresses misunderstanding if and as they arise	96	100	86	60	94
Little to no time is wasted; pacing is efficient. The class resembles a "well-oiled machine" where a majority of students know what is expected of them and how to go about doing it throughout the whole class	45	47	53	32	71

NOTE: Values shown are the percentage of classroom observations rated yes out of total rated observations.

Table B.24. Summer 2014 Desirable Enrichment Classroom Practices by District

	District				
Description	**A (*N* = 46)**	**B (*N* = 15)**	**C (*N* = 76)**	**D (*N* = 53)**	**E (*N* = 17)**
Students respect one another	96	73	96	89	94
All or almost all students exhibited obvious signs of enthusiasm for the class throughout the class period (e.g., jumping out of seat, quickly and enthusiastically answering teacher's questions)	65	40	55	36	53
The teacher (a) explicitly encouraged at least one student struggling with a particular task to persist at academic/content-related tasks that were difficult for them or (b) explicitly taught students strategies to persist at tasks	9	20	28	4	29
There was a helpful adult other than the teacher in the classroom. Helpful means the adult worked directly with students, engaged in the material, or supporting students learning while they were in the room for a majority of the class time	85	100	26	8	71
Students verbally encourage each other, are overtly friendly and supportive	13	13	25	9	41
Teacher shows explicit signs of caring and positive affect toward youth	22	27	36	9	41
Students show explicit signs that they have warm, positive affect to teacher	22	27	22	13	41
The teacher *explicitly* taught social skills such as respecting, listening, cooperating with, or helping others or teaching of politeness	11	13	5	11	6

NOTE: Values shown are the percentage of classroom observations rated yes out of total rated observations.

Table B.25. Summer 2014 Undesirable Enrichment Classroom Practices by District

	District				
Description	A (*N* = 46)	B (*N* = 15)	C (*N* = 76)	D (*N* = 53)	E (*N* = 17)
The teacher provided or failed to correct factually inaccurate information that would confuse students about the content/skills they were to learn	0	0	3	2	0
The explanation of the instructional content was unclear, hard to follow, incomplete, or inconsistent	2	0	0	8	6
In at least one instance, the teacher was disrespectful to students	0	0	7	2	35
All or almost all students in the class appeared bored throughout the class	7	0	8	10	25
There was one or more flagrant instance of student misbehavior	0	0	0	8	24
When the teacher disciplined students, the majority of the class was interrupted for a long period	2	7	1	21	29
The teacher responsible for the activity was disengaged in the classroom because of distractions by factors that were within her control	0	0	8	13	12
There were adults other than the teacher in the classroom who engaged in activities that distracted from learning (e.g., checking cell phone, interrupting the lesson, asking off-topic questions)	9	0	4	4	12

NOTE: Values shown are the percentage of classroom observations rated yes out of total rated observations.

Inter-Rater Agreement

We strove to ensure interrater agreement on the academic and enrichment instruction observation protocols. All observers across the five districts attended three days of training on how to use the instruments prior to summer 2013 and again for four days of training in summer 2014. The two trainings followed the same format, so only summer 2013 is described here. At this training, observers watched and rated between eight and 12 videos per day of LA, mathematics, and enrichment classrooms at elementary grade levels, completed the full observation protocols individually, and then assessed the degree of agreement on each item on the observation protocols to calibrate the observers’ scoring of the classroom instruction. The group then extensively discussed rating disparities and recoded additional videos to further calibrate ratings. Following the three-day training, four lead RAND researchers then established their own consistency in rating through pairwise correlations from ratings of additional classroom videos. The four lead researchers then participated in co-observations with the RAND staff responsible for field observations within each of the five school districts. They co-observed ten to 12 classroom segments (each of at least 15 minutes in duration) in the field during the first week of the summer program in each of the five districts. The lead researcher and the RAND co-observer collected their ratings on each of the items on the observation protocol and their ratings were compared across the ten to 12 classroom segments within each item.

During summer 2013, RAND observed a total of 783 classroom sessions, and during summer 2014, RAND observed a total of 608 classroom sessions. Although this is a substantial number of observations, they represent only about 10 percent of all instruction in LA and mathematics occurring in each summer. In so doing, RAND followed each classroom cohort for one entire day. For example, if a summer site had a red group with 12 students, a green group with 11 students, and a yellow group with 13 students, RAND visited the site for three full days, observing the red group on the first visit, the green group during the second visit, and the yellow group during the third. (Table B.15 displays the number of observations and subjects.) For each classroom observed, the RAND staff person observed the entire class session from start to finish, coding a minute-by-minute time log during the class session, and then, at the end of class, answered 24 yes/no items plus six open-ended response items.

Daily Site Observation

As of summer 2014, at the end of each day in which an observer followed a cohort of students through the day, the RAND observer would then fill out a short online survey with the yes-or-no questions shown in Table B.26. This survey was completed in addition to the classroom observations and interviews that the observer conducted that day. The observers were trained on this instrument during the weeklong training in spring 2014.The items were used to develop a site-level climate index for summer 2014 only. Table B.26 reports the average ratings on each item on the five-point scale.

Table B.26. RAND Daily Site Observation by District

	District				
Survey Item	**A**	**B**	**C**	**D**	**E**
Number of site observations within the district (*N*)	35	33	39	13	34
Adults at the site address student behavior consistently and appropriately when misbehavior occurs	3.7	4.1	3.2	2.9	3.4
Almost none or no student misbehavior at this site	3.9	3.9	3.0	2.7	3.8
Site is well organized	3.6	4.1	3.3	2.8	3.3
Students appeared to have enjoyable day	4.0	4.1	3.3	3.6	3.3
Staff are overtly friendly toward students	3.5	4.4	3.4	3.5	3.6
Students are overtly friendly toward and supportive of one another	3.5	3.5	3.1	2.9	3.4

NOTE: Unless otherwise noted, values shown are average ratings on a 1–5 scale.

Summer Student Attendance

Given known inaccuracies in the summer 2012 data collection, RAND collected and audited each week of the five- to six-week summer program student-level daily attendance data from each of the five school districts in both subsequent summers (2013 and 2014). These checks were to ensure that control group students were not attending the summer program and to ensure that attendance data was being routinely entered. We further checked for suspicious patterns, such as 100-percent attendance or absence on a given day. We used these checks to communicate with district leaders to fix software glitches and remind site leaders to submit daily or weekly attendance records (depending on the district). In one district, RAND also collected paper copies of the attendance logs to cross-check them later with that district's electronic attendance records.

Summer Program Costs

To assess the revenue and monetary costs of implementing a summer program, in fall 2014, we collected data about the prior full year of costs, including the preparation for and the execution of the summer 2014 program. We applied a cost-ingredient approach, where we created a customized spreadsheet for each district that identified all of the known cost ingredients for the given summer program. For example, we included curriculum coordinators for districts that had hired them to write the summer reading curriculum. After developing the spreadsheets, we held calls in spring 2013 with each of the relevant sources of cost data (e.g., summer program directors, facilities services directors, food service coordinators, transportation coordinators, and—in some cases—representatives from community-based organizations [CBOs]) to preview the data request, set deadlines, and ensure the cost categories were collectable. A sample spreadsheet is included at the end of this appendix.

The data request covered the revenue sources, the number of staff positions and corresponding salaries, and line item costs for the various components of each summer program. In cases where personnel split responsibilities between the summer program and other activities, they were asked to prorate their salaries based on the percentage of time spent on the summer program. They were also asked to denote which costs were paid for out of the summer budget and which were in-kind contributions from the district. Where possible, the cost data were validated against other sources, including budgets submitted by the districts to The Wallace Foundation, publicly available salary schedules, and reimbursement rates for the federal meals program. When necessary, we used email and phone correspondence to clarify and refine the cost data and correct discrepancies.

Cost Data Collection and Cleaning

Cost data were gathered from the five participating summer programs after identifying key personnel mentioned above. Beginning in November 2014, the relevant personnel were sent a template spreadsheet that requested cost data broken out into various categories. As shown in Figure 2.5 of the main report, costs were categorized as district and site management, curriculum, enrichment, academics, food, transportation, and professional development. Table B.27 shows how we categorized each line-item cost. Not all districts reported values for each line item.

Table B.27. Line Item Expenses Included in Each of the Six Cost Categories

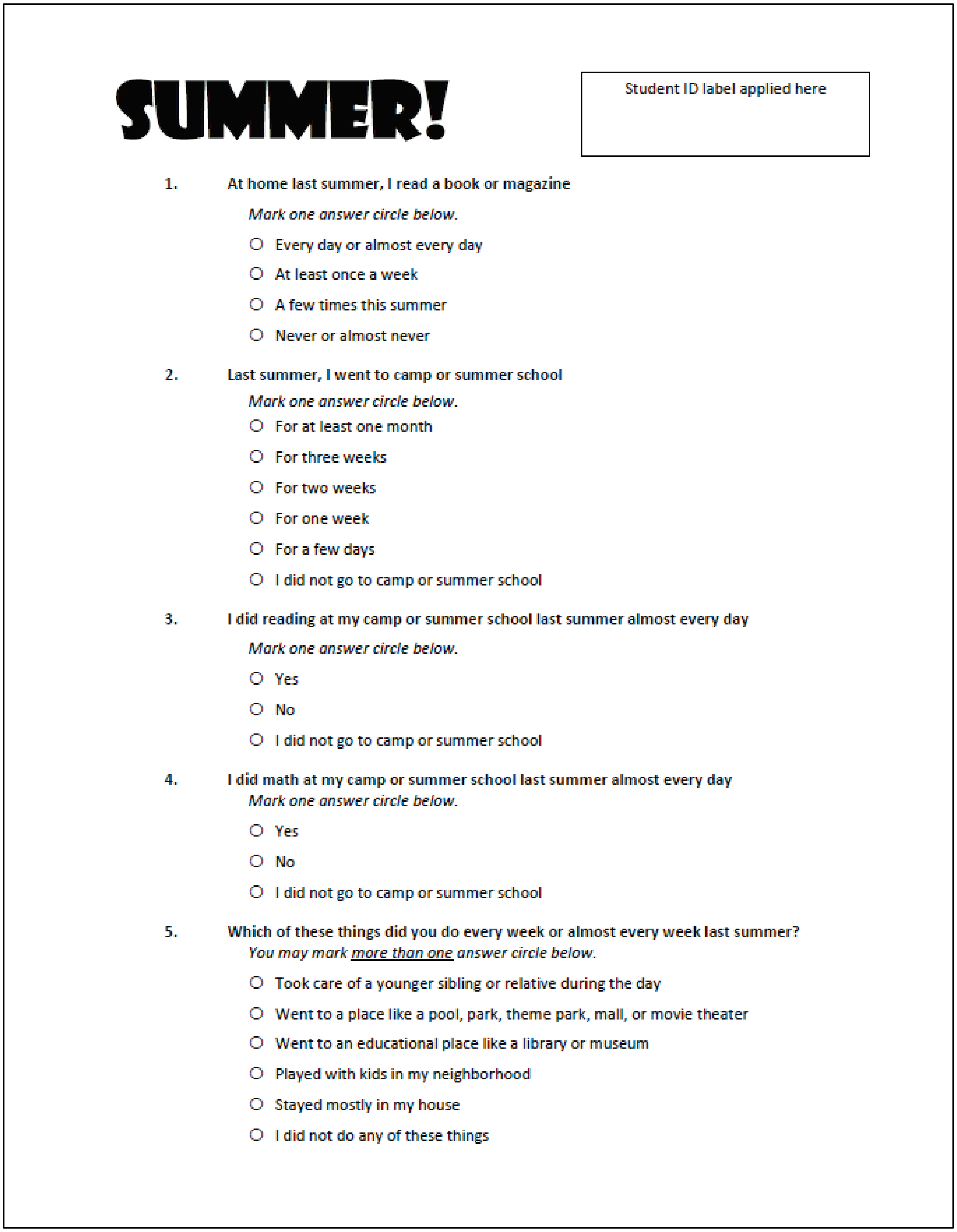
Cost Category	Line-Item Costs
District and site management	Program coordinator
	Summer learning director
	Data manager*
	Director of operations, student support services
	Evaluation staff
	Information technology staff*
	Accounts payable clerk*
	Legal clerk*
	Interns
	Office supplies/materials
	District administrator travel to sites
	Marketing of summer program
	Mileage reimbursement
	Fingerprinting
	Printing
	Nurses and nurse supplies
	Custodial staff and supplies
	Security personnel
	Site managers/administrators
	Behavioral coaches
	Utilities*
Curriculum	Curriculum writers and coaches
	Curricular materials
	Classroom materials
Enrichment	CBO enrichment providers/partners contracts
	CBO partner instructors
	CBO partner administrative staff
	CBO partner curricular materials/development
	Evaluation
	Other classroom materials
	District-employed music/art/special resource teachers
	District-employed physical education teachers
	Field trips
Academics	Scholastic Book Fair
	District-employed regular classroom teachers
	District-employed ESOL teachers
	District-employed special education
	District-employed teacher assistants
	Interns
	District-Employed Paraeducators
	Substitute teachers
Food	Food service staff
	Food service coordinator
	Food
	Snacks
Student transportation	Transportation coordinator and routing staff
	Transportation contract (bus, driver, garage, maintenance)
Professional development	Professional development programming for teachers
	Professional development days for teachers

NOTE: * In-kind contributions from the district, which were not paid for out of the summer budget.

Our goal was to produce estimates that reflect the cost of sustaining a district-run, voluntary summer program offered to multiple grade levels. Over the course of the analysis, we had to make a number of decisions on which costs to include, how to categorize costs, and how to resolve ambiguities in the data. The list below summarizes the assumptions and decisions made in our cost analysis:

- In cases where personnel split responsibilities between the summer program and other activities, they were asked to prorate their salaries based on the percentage of time spent on the summer program.
- Costs that were strictly related to the study were excluded. This included travel to The Wallace Professional Learning Community conference, as well as incentives that were targeted at boosting enrollment of treatment group students for the study.
- Districts were asked to denote which costs were paid for out of the summer budget and which were in-kind contributions from the district (e.g., data managers or information technology personnel who supported the summer program but were not paid for out of the summer budget). Generally, in-kind costs were excluded from the cost estimates. One exception was a district that used assistant principals, who were on a 12-month contract and were therefore not paid out of the summer budget, as site leaders for the summer program. Because site leaders are a core element of a summer program, we include the relevant portion of those assistant principals' salaries.
- Generally CBO services for enrichment activities were provided under fixed-price contracts, which may include organizational overhead or administrative costs. In most cases, the costs of CBO services were entirely attributed to the enrichment category (Figure 2.5 in the main report). However, in one district, the CBO played an active role in the overall program administration. In this case, after reviewing the roles and responsibilities of each of the CBO personnel, those that were deemed part of the general program administration were moved to the district and site management category, while those that were focused on enrichment services were included in the enrichment category. A line-item cost for administrative overhead for the CBO was also categorized under district and site management.
- When calculating the cost per filled seat, we used the summer program attendance rate for the full program (i.e., all grade levels) when available. In cases when the full program attendance was not available, we used the attendance rate for the treatment-group cohort, which was collected as part of the study.

Student Summer Activities Questionnaire (Fall 2014)



SUMMER!

Student ID label applied here

1. **At home last summer, I read a book or magazine**

   Mark one answer circle below.

   ○ Every day or almost every day

   ○ At least once a week

   ○ A few times this summer

   ○ Never or almost never

2. **Last summer, I went to camp or summer school**

   Mark one answer circle below.

   ○ For at least one month

   ○ For three weeks

   ○ For two weeks

   ○ For one week

   ○ For a few days

   ○ I did not go to camp or summer school

3. **I did reading at my camp or summer school last summer almost every day**

   Mark one answer circle below.

   ○ Yes

   ○ No

   ○ I did not go to camp or summer school

4. **I did math at my camp or summer school last summer almost every day**

   Mark one answer circle below.

   ○ Yes

   ○ No

   ○ I did not go to camp or summer school

5. **Which of these things did you do every week or almost every week last summer?**

   You may mark more than one answer circle below.

   ○ Took care of a younger sibling or relative during the day

   ○ Went to a place like a pool, park, theme park, mall, or movie theater

   ○ Went to an educational place like a library or museum

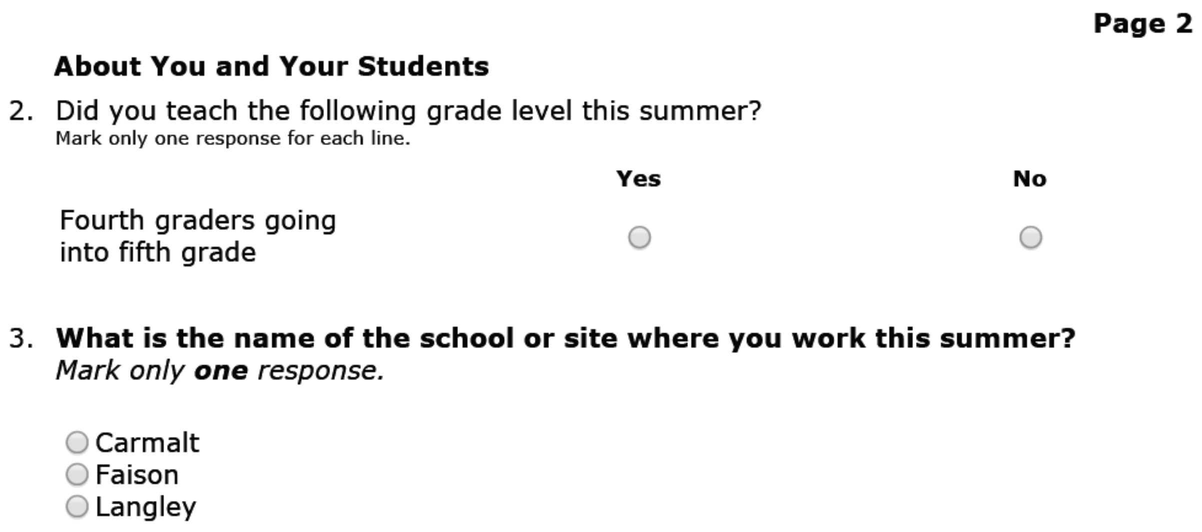
   ○ Played with kids in my neighborhood

   ○ Stayed mostly in my house

   ○ I did not do any of these things

Summer 2014 Academic Teacher Survey

We use the survey customized for Pittsburgh as our sample. We have intentionally omitted the first question.



Page 2

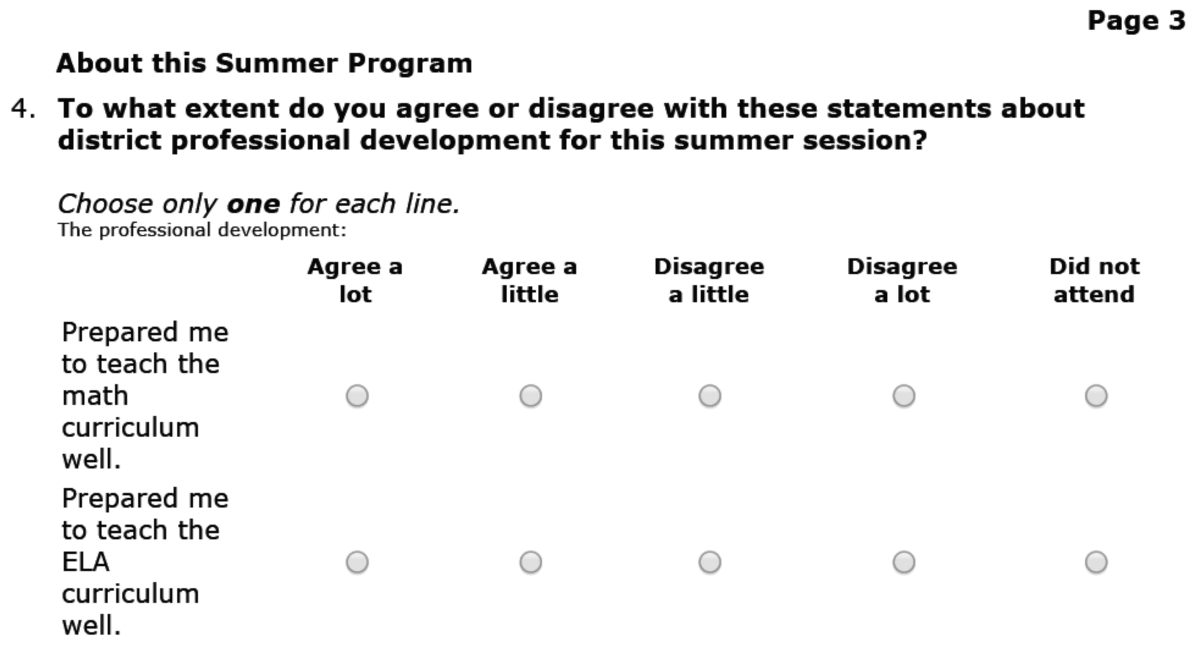
About You and Your Students

2. Did you teach the following grade level this summer?
Mark only one response for each line.

	Yes	**No**
Fourth graders going into fifth grade	○	○

3. **What is the name of the school or site where you work this summer?**
*Mark only **one** response.*

○ Carmalt
○ Faison
○ Langley



Page 3

About this Summer Program

4. **To what extent do you agree or disagree with these statements about district professional development for this summer session?**

*Choose only **one** for each line.*
The professional development:

	Agree a lot	**Agree a little**	**Disagree a little**	**Disagree a lot**	**Did not attend**
Prepared me to teach the math curriculum well.	○	○	○	○	○
Prepared me to teach the ELA curriculum well.	○	○	○	○	○

Page 4

5. **Did you teach math this summer?**

   ○ Yes
   ○ No

Page 5

About this Summer Program

6. **Thinking about only your *math class* for students in your summer program, are the following statements true?**
   *Mark only **one** response for each line.*

	Yes	No
My school/site grouped students by ability in math for classroom assignments.	○	○
By the first day of the program, I was provided a written math curriculum.	○	○
By the first day of the program, I was provided a pacing guide indicating which math topics are to be taught each day.	○	○
By the first day of the program, I received lesson plans to use for my math classes.	○	○
I received information about students' IEPs or special needs.	○	○
I received school year data on my students' prior math performance.	○	○
I received summer math pre-test results for my students.	○	○

7. **Thinking about only the *math curriculum* for students in your summer program, how much do you agree with the following statements?**
*Choose only **one** for each line.*

	Agree a lot	**Agree a little**	**Disagree a little**	**Disagree a lot**
The planned pacing of the curriculum was reasonable.	○	○	○	○
Children’s math skills are improving as a result of this program.	○	○	○	○
The math curriculum is clear for me to follow	○	○	○	○
The math curriculum includes fun, interesting activities for children.	○	○	○	○
The math curriculum addresses gaps that many students have from last year.	○	○	○	○
The math curriculum is aligned with the curriculum students received in the past school year.	○	○	○	○
The math curriculum is aligned with the curriculum students will receive in the next school year.	○	○	○	○

8. Please indicate how well the math curriculum matched the ability levels of students in your class(es).
Mark only one response for each line.

	Curriculum was too difficult	**Curriculum was just right**	**Curriculum was too easy**	**None of these children were in my class**
Students far below grade level in mathematics (Below Basic)	○	○	○	○
Students somewhat below grade level in mathematics (Basic)	○	○	○	○
Students at grade level in mathematics (Proficient)	○	○	○	○
Students above grade level in mathematics (Advanced)	○	○	○	○

Page 6

About this Summer Program

9. **Did you teach ELA this summer?**
   ○ Yes
   ○ No

Page 7

About this Summer Program

10. **Thinking about only the *English Language Arts class* for students in your summer program, are the following statements true?**
*Mark only **one** response for each line.*

	Yes	**No**
My school/site grouped students by ability in ELA for classroom assignments.	○	○

By the first day of the program, I was provided a written ELA curriculum.	○	○
By the first day of the program, I was provided a pacing guide indicating which ELA topics are to be taught each day.	○	○
By the first day of the program, I received lesson plans to use for my ELA classes.	○	○
I received information about students' IEPs or special needs.	○	○
I received school year data on my students' prior ELA performance.	○	○
I received summer ELA pre-test data for my students.	○	○

11. **Thinking about only the *ELA curriculum* for students in your program this summer, how much do you agree with the following statements?**
Mark only ***one*** *response for each line.*

	Agree a lot	**Agree a little**	**Disagree a little**	**Disagree a lot**
The planned pacing of the curriculum was reasonable.	○	○	○	○
Children's ELA skills are improving as a result of this program.	○	○	○	○
The ELA curriculum is clear for me to follow.	○	○	○	○

The ELA curriculum includes fun, interesting activities for children.	○	○	○	○
The ELA curriculum addresses gaps that many students have from last year.	○	○	○	○
The ELA curriculum provides students texts that are appropriate for their reading level.	○	○	○	○
The ELA curriculum is aligned with the curriculum students received in the past school year.	○	○	○	○
The ELA curriculum is aligned with the curriculum students will receive in the next school year.	○	○	○	○

12. Please indicate how well the ELA curriculum matched the ability levels of students in your class(es).
Mark only one response for each line.

	Curriculum was too difficult	**Curriculum was just right**	**Curriculum was too easy**	**None of these children were in my class**
Students far below grade level in ELA (Below Basic)	○	○	○	○
Students somewhat below grade level in ELA (Basic)	○	○	○	○
Students at grade level in ELA (Proficient)	○	○	○	○
Students above grade level in ELA (Advanced)	○	○	○	○

Page 8

About this Summer Program

13. **How much do you agree or disagree with these statements about the *quality and structure* of the summer program?**

Choose *only* ***one*** *for each line.*

	Agree a lot	**Agree a little**	**Disagree a little**	**Disagree a lot**
This program is				

well managed and well organized.	○	○	○	○
Support staff (e.g., camp counselors, paraprofessionals, instructional aides, tutors) provide necessary support in my classroom.	○	○	○	○
There is a clear procedure for handling student discipline problems.	○	○	○	○
The procedure for handling student discipline problems is effective.	○	○	○	○

14. **How much do you agree or disagree with these statements about the *climate and culture* of the summer program?**

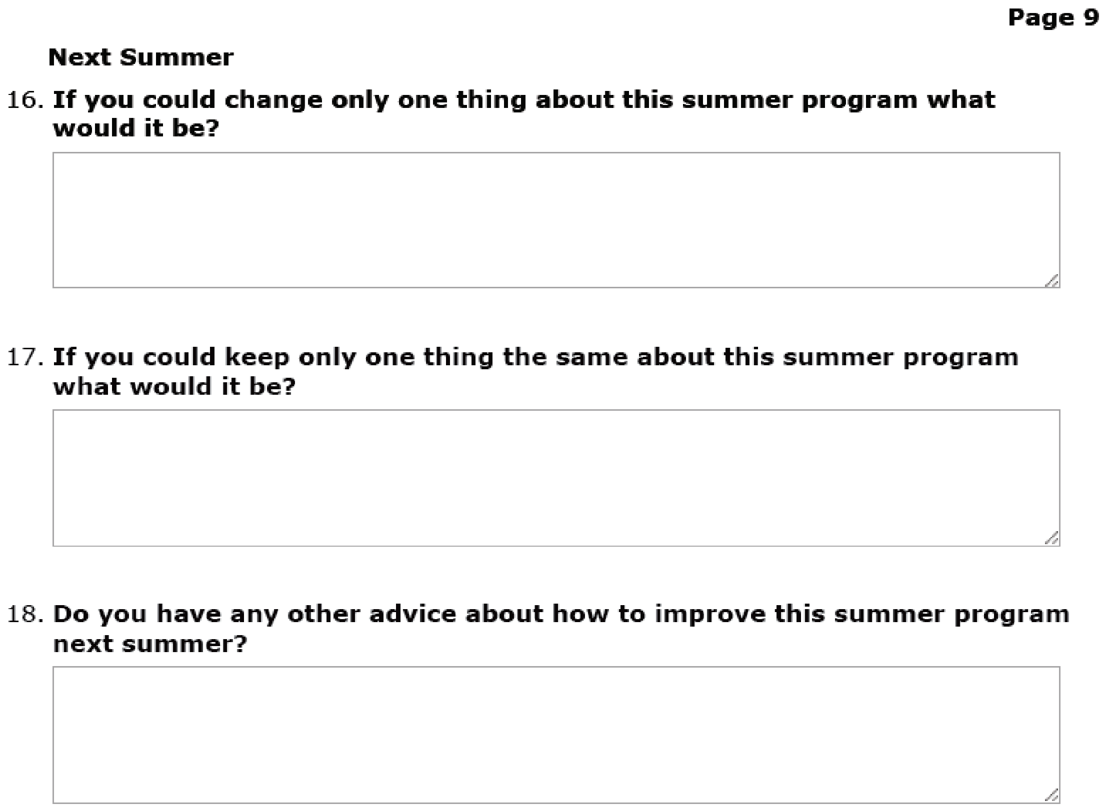
Choose *only* ***one*** *for each line.*

	Agree a lot	**Agree a little**	**Disagree a little**	**Disagree a lot**
Teachers listen to students when they have a problem.	○	○	○	○
Faculty and staff make students feel cared for.	○	○	○	○
Faculty and staff treat students with respect.	○	○	○	○
Teachers enjoy teaching here.	○	○	○	○
Faculty and staff remind students to be friendly and respectful to each other.	○	○	○	○

15. **How much do you agree or disagree with these statements about the *students* in the summer program?**

Choose *only **one** for each line.*

	Agree a lot	**Agree a little**	**Disagree a little**	**Disagree a lot**
Due to student misbehavior, a great deal of learning time is wasted.	○	○	○	○
Students enjoy this summer program.	○	○	○	○
Students solve problems without fighting, or saying mean things.	○	○	○	○
Students feel safe travelling to and being in this school.	○	○	○	○
Students treat adults in school with respect.	○	○	○	○
Children get into physical fights with other students at school at least once a week.	○	○	○	○
Children are bullied, or harassed by other students at least once a week.	○	○	○	○



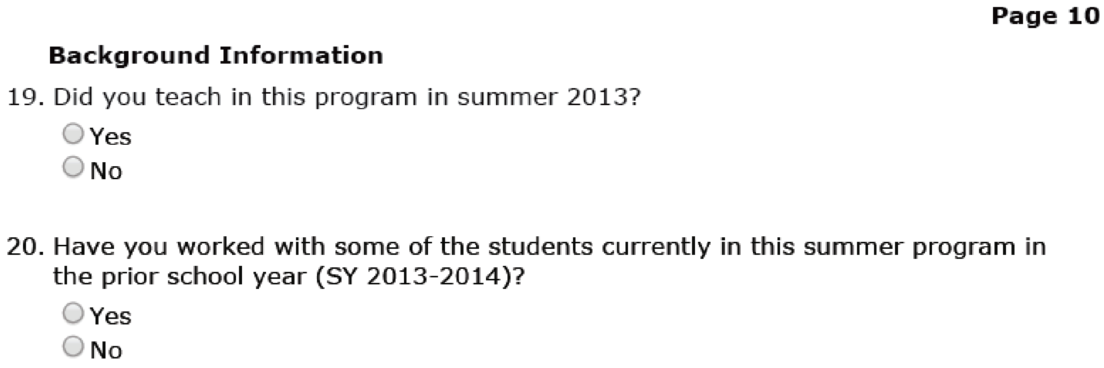
Page 9

Next Summer

16. **If you could change only one thing about this summer program what would it be?**

17. **If you could keep only one thing the same about this summer program what would it be?**

18. **Do you have any other advice about how to improve this summer program next summer?**



Page 10

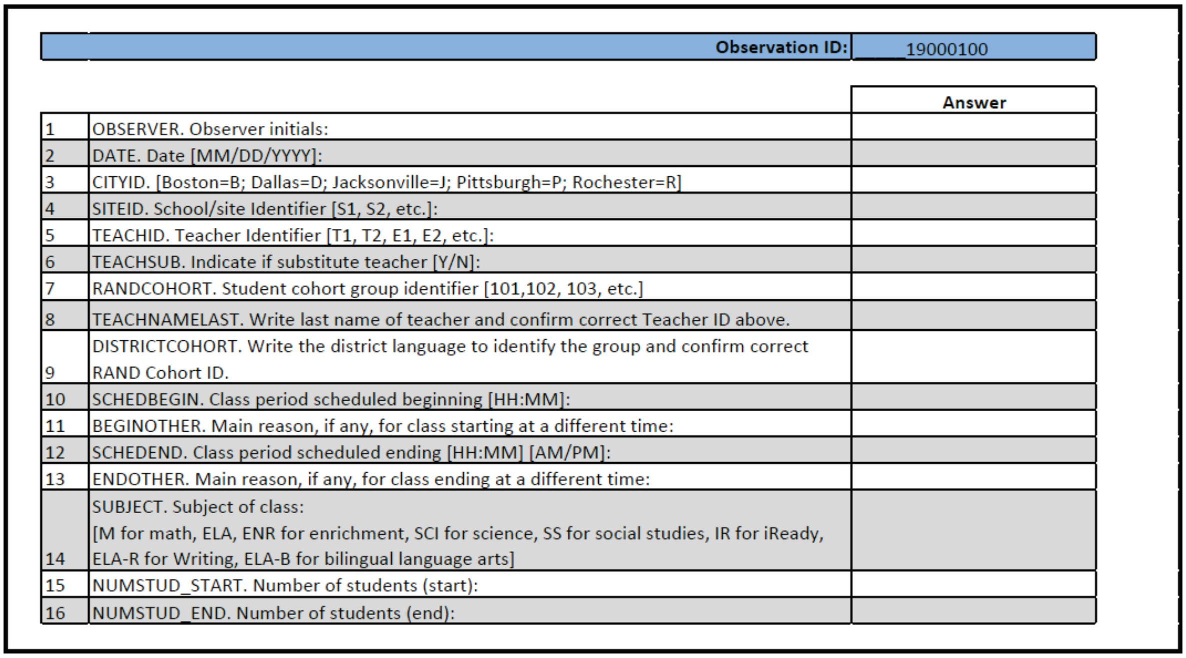
Background Information

19. Did you teach in this program in summer 2013?
    - ○ Yes
    - ○ No

20. Have you worked with some of the students currently in this summer program in the prior school year (SY 2013-2014)?
    - ○ Yes
    - ○ No

Summer 2014 Classroom Observation Protocols

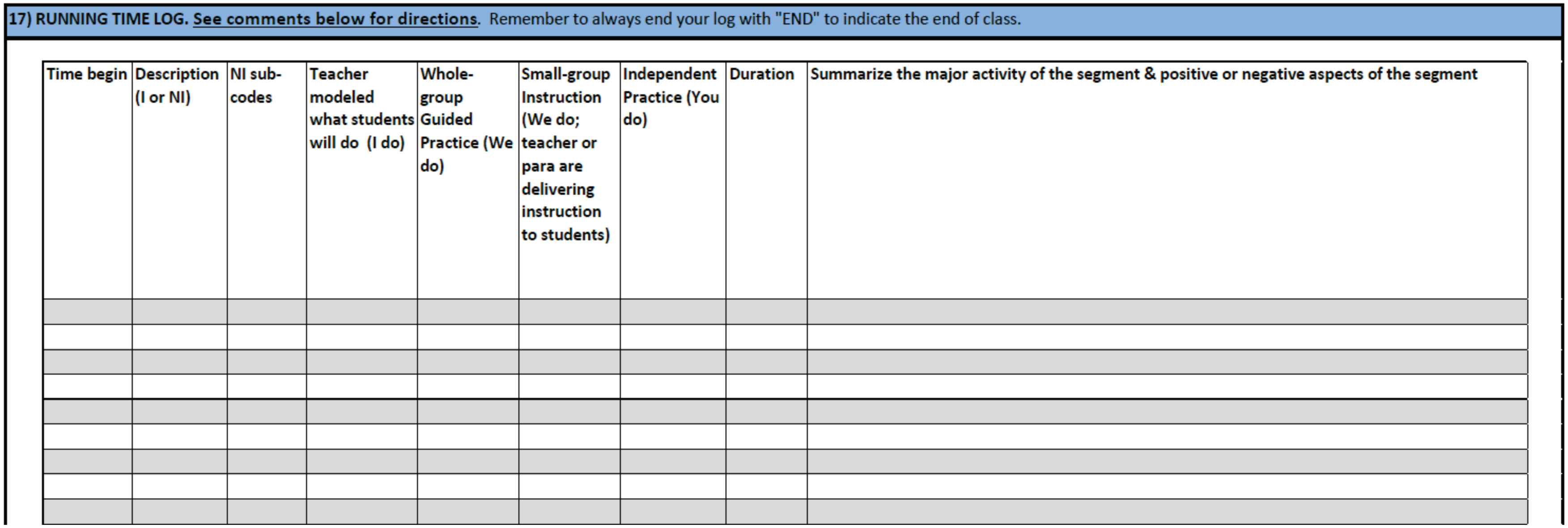
Overview



Observation ID: ____19000100

		Answer
1	OBSERVER. Observer initials:	
2	DATE. Date [MM/DD/YYYY]:	
3	CITYID. [Boston=B; Dallas=D; Jacksonville=J; Pittsburgh=P; Rochester=R]	
4	SITEID. School/site Identifier [S1, S2, etc.]:	
5	TEACHID. Teacher Identifier [T1, T2, E1, E2, etc.]:	
6	TEACHSUB. Indicate if substitute teacher [Y/N]:	
7	RANDCOHORT. Student cohort group identifier [101,102, 103, etc.]	
8	TEACHNAMELAST. Write last name of teacher and confirm correct Teacher ID above.	
9	DISTRICTCOHORT. Write the district language to identify the group and confirm correct RAND Cohort ID.	
10	SCHEDBEGIN. Class period scheduled beginning [HH:MM]:	
11	BEGINOTHER. Main reason, if any, for class starting at a different time:	
12	SCHEDEND. Class period scheduled ending [HH:MM] [AM/PM]:	
13	ENDOTHER. Main reason, if any, for class ending at a different time:	
14	SUBJECT. Subject of class: [M for math, ELA, ENR for enrichment, SCI for science, SS for social studies, IR for iReady, ELA-R for Writing, ELA-B for bilingual language arts]	
15	NUMSTUD_START. Number of students (start):	
16	NUMSTUD_END. Number of students (end):	

Academic Class Segments



17) RUNNING TIME LOG. See comments below for directions. Remember to always end your log with "END" to indicate the end of class.

Time begin	Description (I or NI)	NI sub-codes	Teacher modeled what students will do (I do)	Whole-group Guided Practice (We do)	Small-group Instruction (We do; teacher or para are delivering instruction to students)	Independent Practice (You do)	Duration	Summarize the major activity of the segment & positive or negative aspects of the segment

NOTE: Duration: Minute value is automatically calculated by the time entries.

Directions: Start a new row for each new activity. Segments are at least 60 seconds long. Your time log should begin when a majority of students are in the room, regardless of whether the teacher has launched the lesson. The log should end when the majority of students leave the room. You should watch and record the entire class period.

Time begin:
Time of the start of the class and subsequent class segments is needed to calculate actual time, time on and off task, and time of independent practice. Start a new row for each new activity. Segments must be at least 60 seconds long to initiate new row. Your time log should begin when a majority of students are in the room, regardless of whether the teacher has launched the lesson.

Description:
I indicates that majority of students are engaged in an instructional activity.
NI indicates a majority of students not engaged in an subject-related instructional activity for more than 60seconds, e.g., off-topic conversation, class started late or ended early, transition to the next activity, teacher involved in management activities, break in class.
End indicates the end of the class period.

Sub-codes for non-instruction:
Teacher sets out classroom/behavior rules (R). Includes activities like teacher explaining what good behavior means in this classroom and what she expects. It does not include "get in a line" or disciplinary time , which should be coded as T.
Teacher- initiated interruption (**T**) includes administrative activities such as teacher taking attendance, passing out materials, or moving desks, transitions between class activities, teacher addressing behavior, bathroom breaks, and snack breaks;
Externally-initiated interruption (**E**) principal visit or loudspeaker announcement that stops teaching, fire-drill, or other unscheduled interruption out of teacher's control;
Pause for scheduled break in class (**P**), for example lunch and recess occur between part 1 and 2 of an ELA lesson. This code allows us to pause the class segments timer.

Teacher modeled what students will do (I do): Teacher explicitly models what students will do. The teacher is delivering direct instruction that builds students understanding of ELA or mathematics. Teacher models step-by-step how students will do an academic task; there is little to no student participation during the teacher modeling.

Whole-group Guided Practice (We do): Yes indicates that the teacher facilitates in a structured or semi-structured way a whole group activity where the kids demonstrate or practice a skill as a whole group. Some students might practice or demonstrate a skill or strategy in front of the entire class, share their thinking about how or why they used the skill or strategy, and received feedback. Although only some students may answers or solutions aloud, all students have an opportunity to hear how to practice the targeted skill or strategy. All students might complete portions of an activity before reviewing the concepts as a class. *Guided practice sets students up to successfully complete an application activity of the skill or strategy independently. Guided practice provides teachers an opportunity to understand if students have a misconception and where the misconception or misunderstanding may be occurring.*
I-R-E that asks only for the correct answer and does not require students to share their thinking or approach to completing the activity is not guided practice. A student who doesn't understand concept would benefit from seeing guided practice. Teacher question: What is the vocabulary word that means low cost? Student response: Inexpensive. What is 3+5? Or what is the solution to number 5? Does not count as guided practice. Reviewing solutions or answers without conceptual discussion does not count as guided practice.

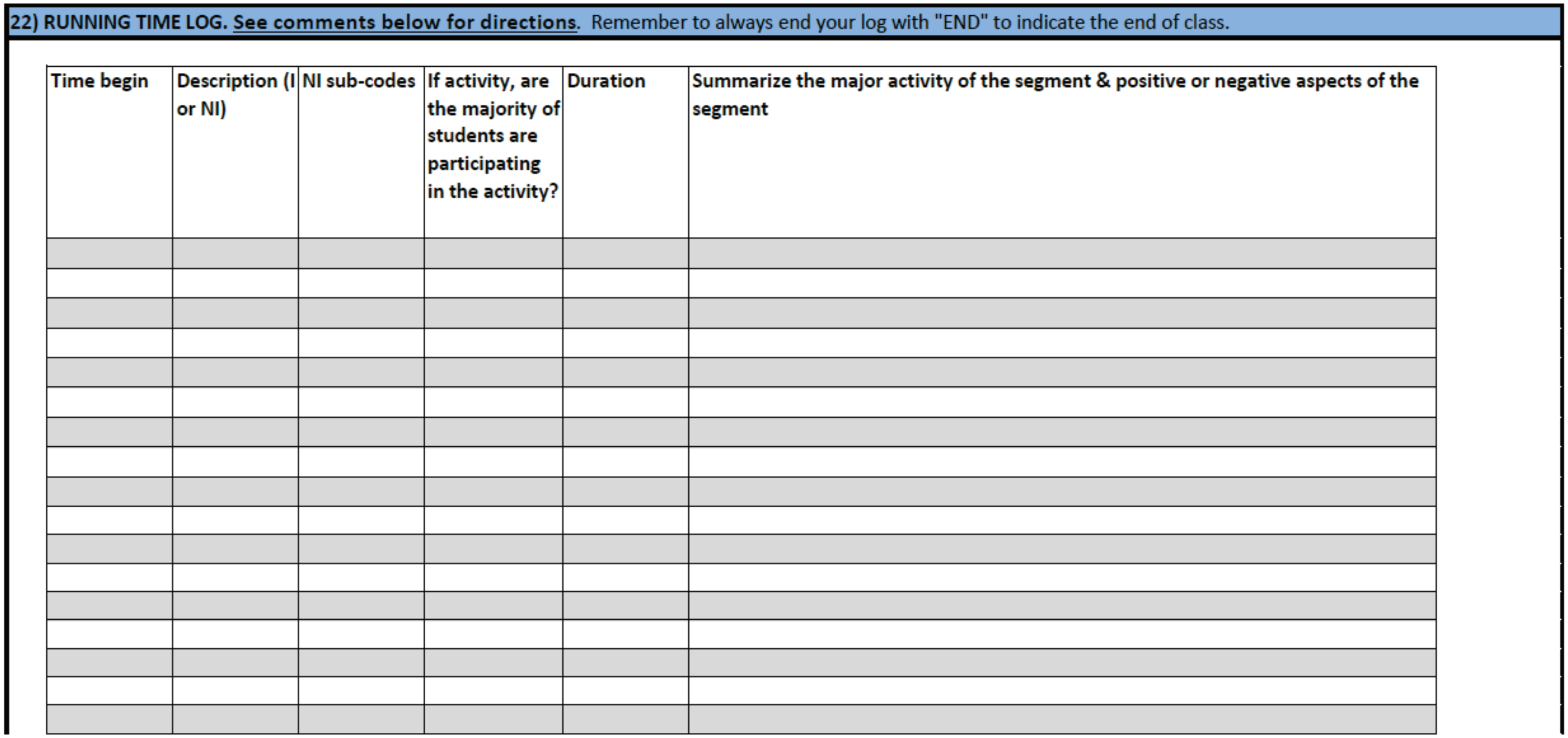
> Example of whole group guided practice in mathematics: What is the first step to solving the problem? How do you know? Is there another way we would have started this problem? What do we do next? Teacher facilitates a discussion where students solve a fraction equation aloud is an example of whole group guided practice. All students might write the steps on worksheets while they solve the problem or steps might be written on the board as a reference for students.
>
> Example of whole group guided practice in ELA: Teacher reads a passage aloud and asks students to summarize the passage. Student shares summary and teacher asks questions of other students about why details were excluded from the summary and others were included in the summary. Asks students for other variations of the summary. Teacher may distribute four different passages to students. Asks students to develop a summary for the passage as a team, present the summary, and explain rationale for what was included or excluded in the summary. As the independent practice, students would summarize passages in their independent reading books or a worksheet for a sustained period of time. In a mini-lesson, students may be asked to edit the passage from the teacher's writer's notebook. Students and teachers provide feedback and discuss editing choices before students edit text passages independently.

Small-group Guided Practice (We do): Yes indicates that the teacher facilitates in a structured or semi-structured way an activity that provides insights into the existence of misconceptions in students and where the misconception or misunderstanding may occur. Guided practice sets the small group of students up to successfully complete an application activity of the skill or strategy independently. The teacher could also reteach a mini lesson to a small group of students if the teacher determines only a group experiences a misconception or misunderstanding that prevents successful independent practice.

Independent Practice (You do): Yes indicates that students have independent practice opportunity with subject content for that time segment
Independent Practice (YES if it occurs): Students have independent practice, whether in small groups or independent work. Do not count pair and shares or brief (< 2 min) activities. Students completes activities without consistent support from the teacher. E.g., reading a book and filling out a worksheet.

Duration:
Minute value is automatically calculated by the time entries.

Enrichment Class Segments

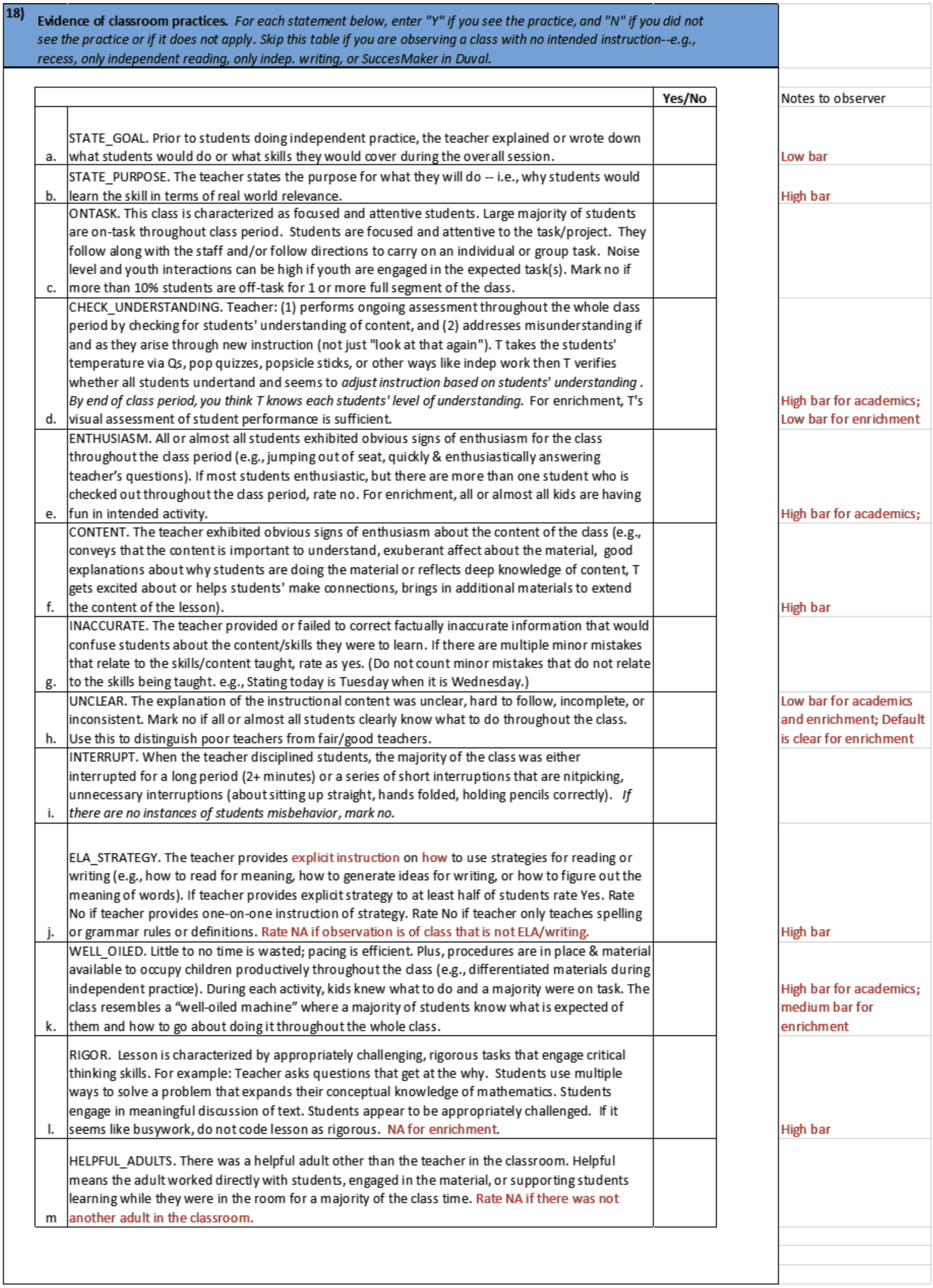


22) RUNNING TIME LOG. See comments below for directions. Remember to always end your log with "END" to indicate the end of class.

Time begin	**Description (I or NI)**	**NI sub-codes**	**If activity, are the majority of students are participating in the activity?**	**Duration**	**Summarize the major activity of the segment & positive or negative aspects of the segment**

NOTE: When kids start an activity on their own or do an activity, start a new segment.

Evidence of Classroom Practices



18) **Evidence of classroom practices.** *For each statement below, enter "Y" if you see the practice, and "N" if you did not see the practice or if it does not apply. Skip this table if you are observing a class with no intended instruction--e.g., recess, only independent reading, only indep. writing, or SuccesMaker in Duval.*

		Yes/No	Notes to observer
a.	STATE_GOAL. Prior to students doing independent practice, the teacher explained or wrote down what students would do or what skills they would cover during the overall session.		Low bar
b.	STATE_PURPOSE. The teacher states the purpose for what they will do -- i.e., why students would learn the skill in terms of real world relevance.		High bar
c.	ONTASK. This class is characterized as focused and attentive students. Large majority of students are on-task throughout class period. Students are focused and attentive to the task/project. They follow along with the staff and/or follow directions to carry on an individual or group task. Noise level and youth interactions can be high if youth are engaged in the expected task(s). Mark no if more than 10% students are off-task for 1 or more full segment of the class.		
d.	CHECK_UNDERSTANDING. Teacher: (1) performs ongoing assessment throughout the whole class period by checking for students' understanding of content, and (2) addresses misunderstanding if and as they arise through new instruction (not just "look at that again"). T takes the students' temperature via Qs, pop quizzes, popsicle sticks, or other ways like indep work then T verifies whether all students undertand and seems to *adjust instruction based on students' understanding*. *By end of class period, you think T knows each students' level of understanding.* For enrichment, T's visual assessment of student performance is sufficient.		High bar for academics; Low bar for enrichment
e.	ENTHUSIASM. All or almost all students exhibited obvious signs of enthusiasm for the class throughout the class period (e.g., jumping out of seat, quickly & enthusiastically answering teacher's questions). If most students enthusiastic, but there are more than one student who is checked out throughout the class period, rate no. For enrichment, all or almost all kids are having fun in intended activity.		High bar for academics;
f.	CONTENT. The teacher exhibited obvious signs of enthusiasm about the content of the class (e.g., conveys that the content is important to understand, exuberant affect about the material, good explanations about why students are doing the material or reflects deep knowledge of content, T gets excited about or helps students' make connections, brings in additional materials to extend the content of the lesson).		High bar
g.	INACCURATE. The teacher provided or failed to correct factually inaccurate information that would confuse students about the content/skills they were to learn. If there are multiple minor mistakes that relate to the skills/content taught, rate as yes. (Do not count minor mistakes that do not relate to the skills being taught. e.g., Stating today is Tuesday when it is Wednesday.)		
h.	UNCLEAR. The explanation of the instructional content was unclear, hard to follow, incomplete, or inconsistent. Mark no if all or almost all students clearly know what to do throughout the class. Use this to distinguish poor teachers from fair/good teachers.		Low bar for academics and enrichment; Default is clear for enrichment
i.	INTERRUPT. When the teacher disciplined students, the majority of the class was either interrupted for a long period (2+ minutes) or a series of short interruptions that are nitpicking, unnecessary interruptions (about sitting up straight, hands folded, holding pencils correctly). *If there are no instances of students misbehavior, mark no.*		
j.	ELA_STRATEGY. The teacher provides explicit instruction on how to use strategies for reading or writing (e.g., how to read for meaning, how to generate ideas for writing, or how to figure out the meaning of words). If teacher provides explicit strategy to at least half of students rate Yes. Rate No if teacher provides one-on-one instruction of strategy. Rate No if teacher only teaches spelling or grammar rules or definitions. Rate NA if observation is of class that is not ELA/writing.		High bar
k.	WELL_OILED. Little to no time is wasted; pacing is efficient. Plus, procedures are in place & material available to occupy children productively throughout the class (e.g., differentiated materials during independent practice). During each activity, kids knew what to do and a majority were on task. The class resembles a "well-oiled machine" where a majority of students know what is expected of them and how to go about doing it throughout the whole class.		High bar for academics; medium bar for enrichment
l.	RIGOR. Lesson is characterized by appropriately challenging, rigorous tasks that engage critical thinking skills. For example: Teacher asks questions that get at the why. Students use multiple ways to solve a problem that expands their conceptual knowledge of mathematics. Students engage in meaningful discussion of text. Students appear to be appropriately challenged. If it seems like busywork, do not code lesson as rigorous. NA for enrichment.		High bar
m	HELPFUL_ADULTS. There was a helpful adult other than the teacher in the classroom. Helpful means the adult worked directly with students, engaged in the material, or supporting students learning while they were in the room for a majority of the class time. Rate NA if there was not another adult in the classroom.		

Evidence of Summer School Climate

19) **Evidence of summer school climate.** *For each statement below, enter "y" if you see the practice, and "n" if you did not see the practice or if it does not apply. Skip this table if you are observing a class with no intended instruction--e.g., recess, only independent reading, only indep. writing, or SuccessMaker in Duval.*

		Yes/No	Notes to observer
a.	RESPECT. Students respect one another. They refrain from derogatory comments or actions about an individual person and the work s/he is doing; if disagreements occur, they are handled constructively.		Low bar
b.	FRIENDLY. Students verbally encourage each other, are overtly friendly and supportive.		High bar
c.	LIKE_TEACHER. Students show explicit signs that they have warm, positive affect to teacher (not just respect for teachers). For example, throughout the class they may smile at teacher, laugh with them, and/or share good-natured jokes.		High bar
d.	LIKE_STUDENTS. Teacher shows explicit signs of positive affect toward youth. Mark no if teacher is simply respectful toward students. Teacher tone is warm and caring. He or she uses positive language, smiles, laughs, or shares good-natured jokes throughout class. If no verbal interaction is necessary, teacher demonstrates a positive and caring affect toward youth.		High bar
e.	DISRESPECTFUL. In at least one instance, the teacher was disrespectful to students. This includes yelling at one or more students, intimidating or being rude or dismissive to students, using physical aggression, intentionally humiliating or ignoring a student, using discriminatory acts or derogatory language to students.		
f.	MISBEHAVIOR. There was one or more flagrant instance of student misbehavior. This includes a physical fight or persistent bullying or persistent use of discriminatory or derogatory language.		
g.	PERSIST. The teacher (a) explicitly encouraged at least one student struggling with a particular tasks to persist at academic/content-related tasks that were difficult for them (e.g., exhortations to keep trying, you know you can do it, helping students stick with rather than quit a task, to stretch to a higher level than the one student currently performs at), or (b) explicitly taught students strategies to persist at tasks.		
h.	SOCIALSKILLS. The teacher *explicitly* taught social skills such as respecting, listening, cooperating with, or helping others or teaching of politeness. Do not check if these skills were implicitly involved.		
i.	TDISENGAGED. The teacher responsible for the activity was disengaged in the classroom because of an apathetic, flat affect, or by going through the motions, or exerting extremely low effort (e.g., reading off script without deviation) or because of distractions by factors that were within her control (i.e., a teacher stopping by to have a conversation about the weekend, the teacher checking his/her cell phone, texting, or taking or making a personal call that was not related to an emergency, personal chat with co-teacher or paraprofessional while students are working).		
J.	BORED. All or almost all students in the class appeared bored throughout the class. Boredom characterized the class period, even if students complied with teachers' requests. NA for academics.		

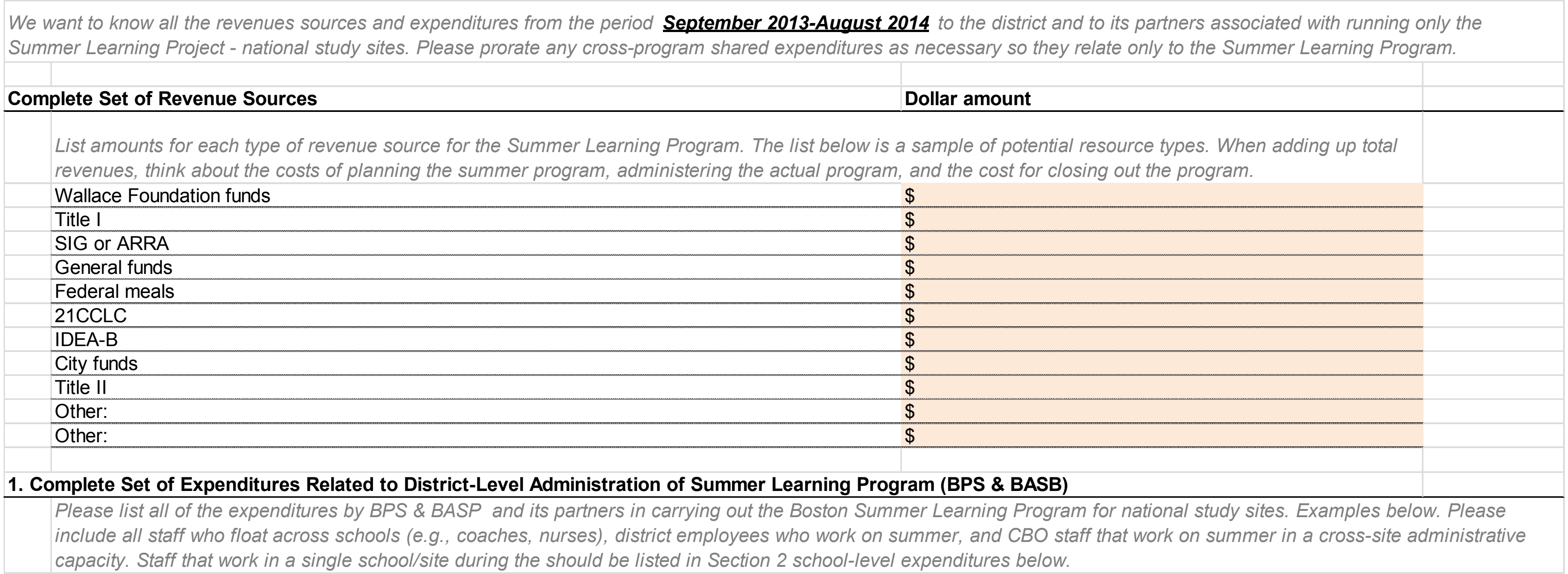
Overall Reactions

OVERALL REACTIONS. Type in white cells below your overall impressions. Row height will expand as you type.

20)	LEARNED. Based on evidence of student demonstrations, what did students learn?
21)	BARRIERS. What, if any, were the main impediments or barriers to learning in this class? Note, please give examples of factual inaccuracies or shortage of materials.
22)	RATING. Rate this class terrible, mixed, good, or outstanding.

Data Request Form for Summer 2014 District-Level Costs

We present the form customized for Boston as our sample.



We want to know all the revenues sources and expenditures from the period ***September 2013-August 2014*** *to the district and to its partners associated with running only the Summer Learning Project - national study sites. Please prorate any cross-program shared expenditures as necessary so they relate only to the Summer Learning Program.*

Complete Set of Revenue Sources	**Dollar amount**
List amounts for each type of revenue source for the Summer Learning Program. The list below is a sample of potential resource types. When adding up total revenues, think about the costs of planning the summer program, administering the actual program, and the cost for closing out the program.	
Wallace Foundation funds	$
Title I	$
SIG or ARRA	$
General funds	$
Federal meals	$
21CCLC	$
IDEA-B	$
City funds	$
Title II	$
Other:	$
Other:	$

1. Complete Set of Expenditures Related to District-Level Administration of Summer Learning Program (BPS & BASB)

Please list all of the expenditures by BPS & BASP and its partners in carrying out the Boston Summer Learning Program for national study sites. Examples below. Please include all staff who float across schools (e.g., coaches, nurses), district employees who work on summer, and CBO staff that work on summer in a cross-site administrative capacity. Staff that work in a single school/site during the should be listed in Section 2 school-level expenditures below.

1A. District administrators who worked on the Summer Program and the time they spent doing so	**Dollar amount**	**Paid out of summer budget (Y/N)?**	**Number of staff in this category**
Ex: curriculum designer each @ 10% FTE	*list 10% of salary + benefits for BOTH curric designers here*	*Y*	*2*
BPS Summer Learning Coordinator (Ariana)	$		
Chief Academic Officer	$		
Director of Extended Learning Services (Melissa)	$		
Katy Hogue (Data)	$		
Curriculum Specialists (Kerri & Chris)	$		2
BPS Coaches	$		6
Denise Jordan? (payroll assistance)	$		
Other:	$		
Other:	$		
Sum of Section 1A expenditures for district-level staff	*If it is too sensitive to list individuals' salary plus benefits line by line immediately above, please fill in column B as shown in the examples for each staff person but leave their corresponding cells in column C blank and only report sum of these expenditures*		

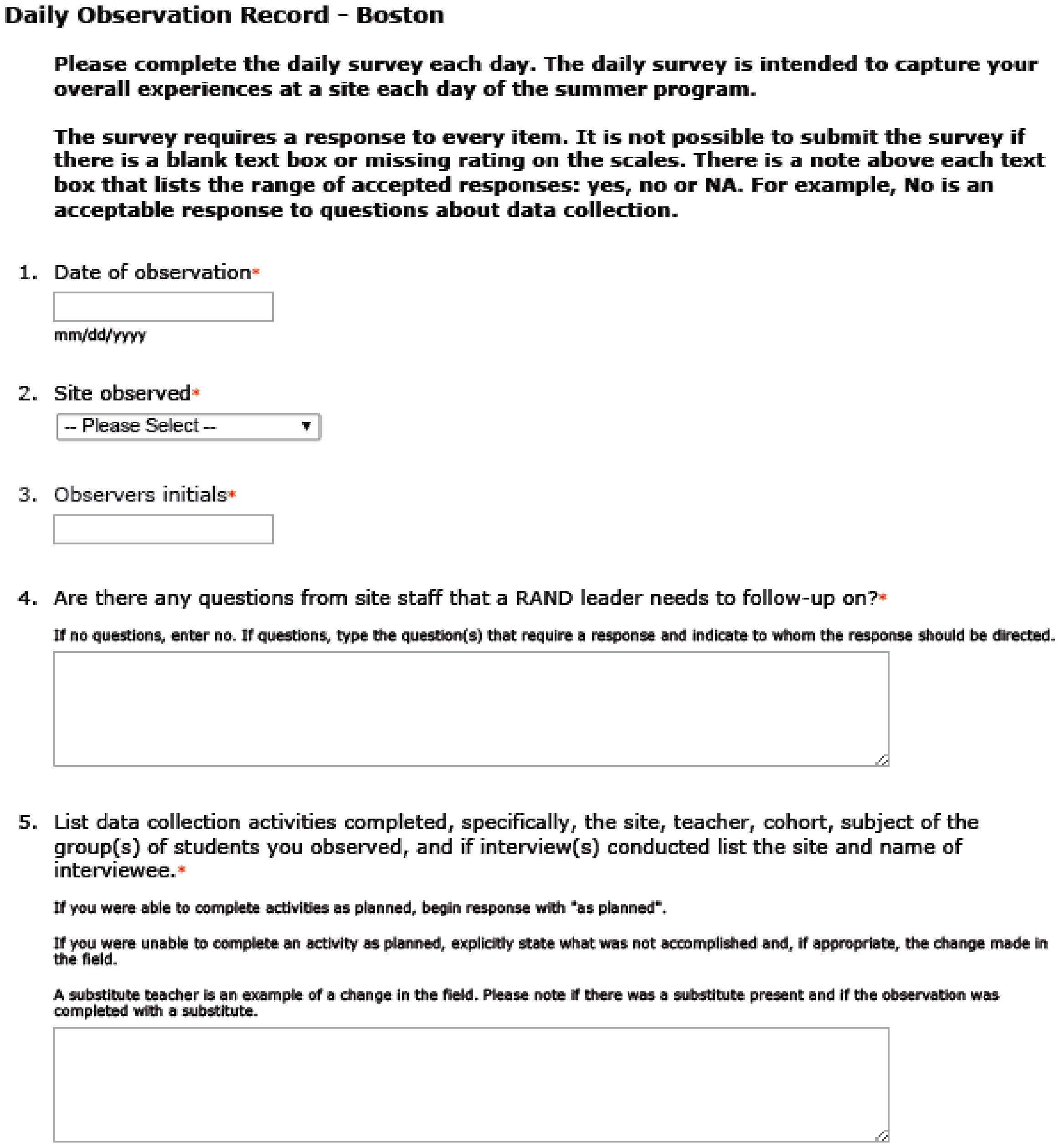
1B. Other district-level Expenditures here incurred by the district (BPS & BASB)	**Dollar amount**	**Paid out of summer budget (Y/N)?**
Food (breakfasts, lunch, snacks, water, food safety training, delivery)	$	
Curricular materials & Support from Curriculum Companies (storage and distribution, reordering/refreshing, Voyager, Coach training and PD etc)	$	
Consulting Expenditures	$	
BASB & BPS Office supplies & materials (printer, mailings, phone, paper, scanner, computers, etc.)	$	
Adults' travel to meetings during summer 2014 for coordination/oversight	$	
BASB/BPS incurred costs for training(room rental, materials, social-emotional training expenditures, etc.)	$	
Marketing and Communication of summer programming (engagement grants, parent mailers, translations)	$	
District PD (travel to Wallace PLC, NSLA expenditures, etc.)	$	
Evaluation expenditures (e.g., district evaluation services to analyse pre- post- district assessments)	$	
PEAR Workshops	$	
Planning Grant	$	
Indirect costs not captured above	$	
Per-Pupil Payments to non-profit partners ($1,350 max payment per student)	$	
Control Group Scholarships ($500 per kid)	$	
Other:	$	
Other:	$	
Sum of Section 1B expenditures for other district-level costs	$	

1C. BASB employees who worked on Summer Learning Program National Study Sites in a coordinating capacity across sites within the district	Dollar amount	Paid out of summer budget (Y/N)?
Ex: Project manager at partner agency A @ 60% FTE	*$*	*Y*
Project Manager of Boston After School & Beyond	$	
Project Assistant (Alissa, Erika)	$	
BASB Executive Director (Chris)	$	
Project Coordinator	$	
Other:	$	
Other:	$	
Sum of Section 1C. Expenditures	*If it is too sensitive to list individuals' salary plus benefits line by line immediately above, please fill in column B as shown in the examples for each staff person but leave their corresponding cells in column C blank and only report sum of these employees' costs here.*	

2. Complete Set of Expenditures Related to Summer Sites (BPS and BASB expenditures)	Dollar amount	Paid out of summer budget (Y/N)?	Number of staff in this category
Please compile both BASB & BPS expenditures at the school level. Please add up the expenditures for all the relevant summer sites here; we do not need expenditures broken out site-by-site.			
School administrator salaries & benefits & taxes (Site Coordinators)	$		
BPS regular classroom teachers salaries, benefits, taxes	$		
BPS Paraeducators salaries, benefits, and taxes	$		
Substitute teachers salaries, benefits, taxes	$		
Engagement Coordinators salaries', benefits', taxes'	$		
Other:	$		
Other:	$		

Summer 2014 Daily Site Observation Protocol

The Boston form is our sample.



Daily Observation Record - Boston

Please complete the daily survey each day. The daily survey is intended to capture your overall experiences at a site each day of the summer program.

The survey requires a response to every item. It is not possible to submit the survey if there is a blank text box or missing rating on the scales. There is a note above each text box that lists the range of accepted responses: yes, no or NA. For example, No is an acceptable response to questions about data collection.

1. Date of observation*

   mm/dd/yyyy

2. Site observed*

   -- Please Select -- ▾

3. Observers initials*

4. Are there any questions from site staff that a RAND leader needs to follow-up on?*

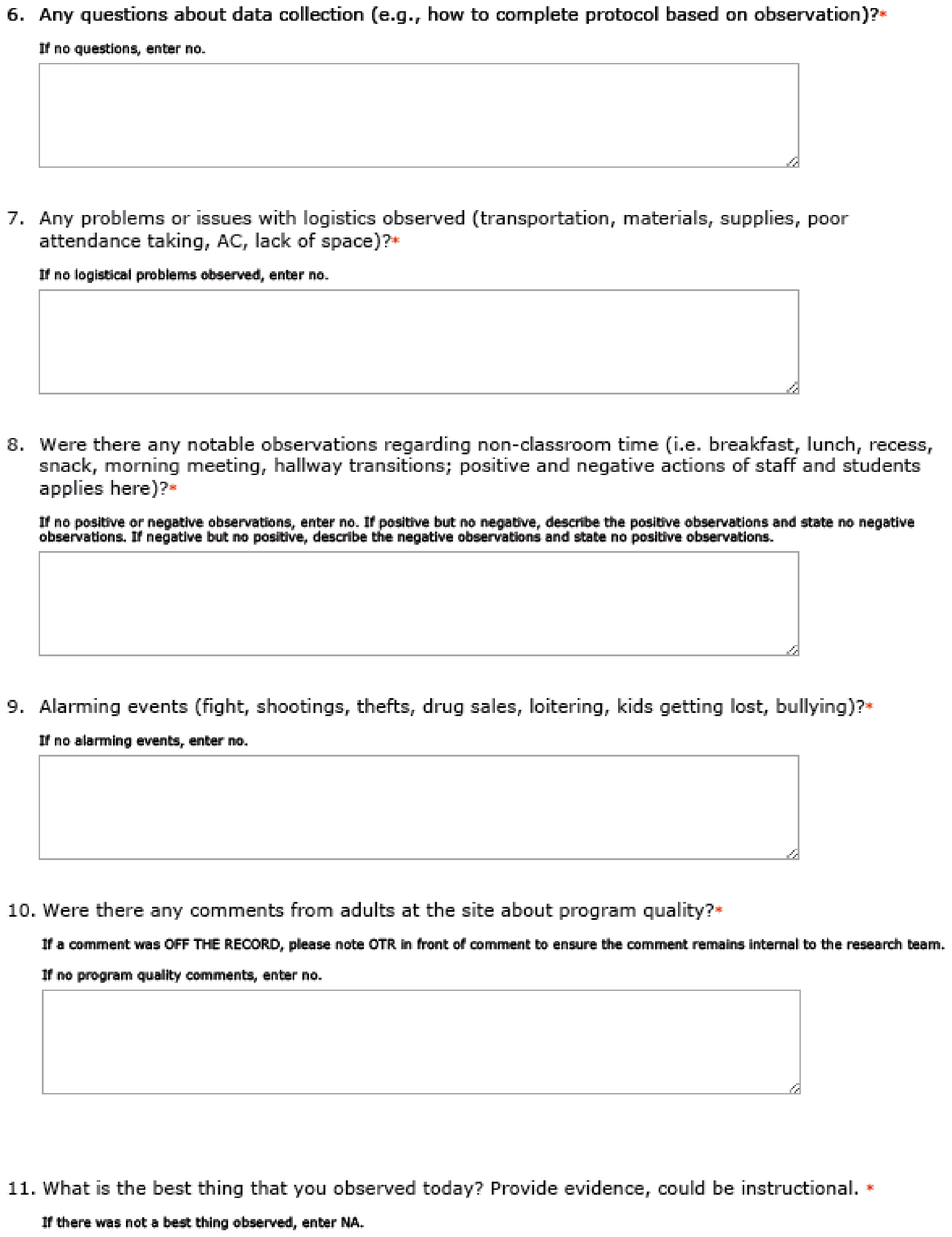
   If no questions, enter no. If questions, type the question(s) that require a response and indicate to whom the response should be directed.

5. List data collection activities completed, specifically, the site, teacher, cohort, subject of the group(s) of students you observed, and if interview(s) conducted list the site and name of interviewee.*

   If you were able to complete activities as planned, begin response with "as planned".

   If you were unable to complete an activity as planned, explicitly state what was not accomplished and, if appropriate, the change made in the field.

   A substitute teacher is an example of a change in the field. Please note if there was a substitute present and if the observation was completed with a substitute.



6. Any questions about data collection (e.g., how to complete protocol based on observation)?*

If no questions, enter no.

7. Any problems or issues with logistics observed (transportation, materials, supplies, poor attendance taking, AC, lack of space)?*

If no logistical problems observed, enter no.

8. Were there any notable observations regarding non-classroom time (i.e. breakfast, lunch, recess, snack, morning meeting, hallway transitions; positive and negative actions of staff and students applies here)?*

If no positive or negative observations, enter no. If positive but no negative, describe the positive observations and state no negative observations. If negative but no positive, describe the negative observations and state no positive observations.

9. Alarming events (fight, shootings, thefts, drug sales, loitering, kids getting lost, bullying)?*

If no alarming events, enter no.

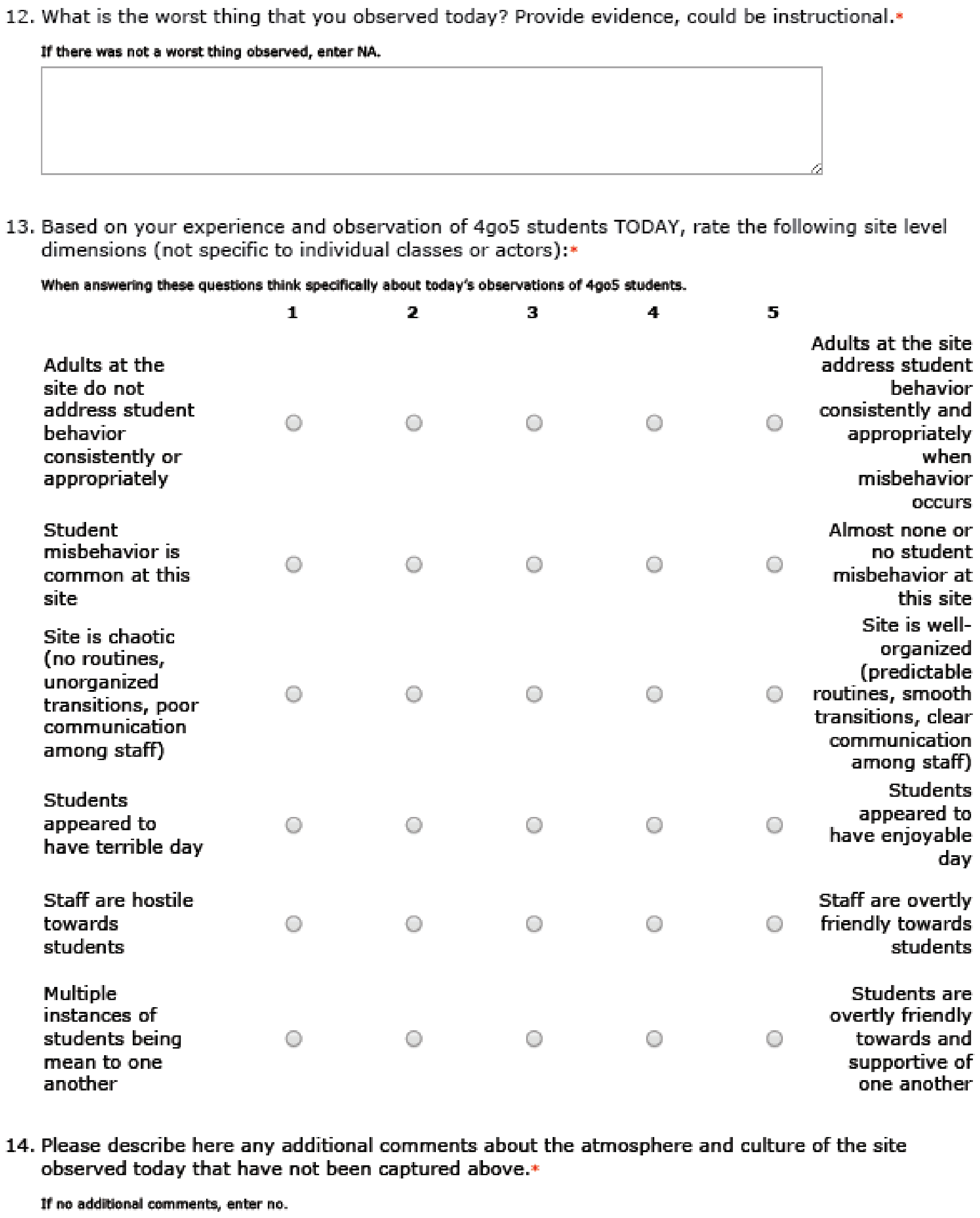
10. Were there any comments from adults at the site about program quality?*

If a comment was OFF THE RECORD, please note OTR in front of comment to ensure the comment remains internal to the research team.

If no program quality comments, enter no.

11. What is the best thing that you observed today? Provide evidence, could be instructional. *

If there was not a best thing observed, enter NA.



12. What is the worst thing that you observed today? Provide evidence, could be instructional.*

If there was not a worst thing observed, enter NA.

13. Based on your experience and observation of 4go5 students TODAY, rate the following site level dimensions (not specific to individual classes or actors):*

When answering these questions think specifically about today's observations of 4go5 students.

	1	2	3	4	5	
Adults at the site do not address student behavior consistently or appropriately	○	○	○	○	○	Adults at the site address student behavior consistently and appropriately when misbehavior occurs
Student misbehavior is common at this site	○	○	○	○	○	Almost none or no student misbehavior at this site
Site is chaotic (no routines, unorganized transitions, poor communication among staff)	○	○	○	○	○	Site is well-organized (predictable routines, smooth transitions, clear communication among staff)
Students appeared to have terrible day	○	○	○	○	○	Students appeared to have enjoyable day
Staff are hostile towards students	○	○	○	○	○	Staff are overtly friendly towards students
Multiple instances of students being mean to one another	○	○	○	○	○	Students are overtly friendly towards and supportive of one another

14. Please describe here any additional comments about the atmosphere and culture of the site observed today that have not been captured above.*

If no additional comments, enter no.

Appendix C: Recent Studies on Summer Learning

Making Summer Count: How Summer Programs Can Boost Children's Learning, published in 2011, synthesized the existing research base on summer learning loss and summer learning program effectiveness (McCombs et al., 2011). That synthesis influenced the research questions and programs addressed in this study. Since 2011, there have been several new evaluations of summer programs and trends in student achievement over the summer months. This appendix provides an update on this research to better understand what happens to student learning during the summer and how summer affects learning trajectories throughout the year.

Summer Learning Loss

Since the release of the influential Coleman report (Coleman et al., 1966), there has been an increased appreciation of the influence of students' out-of-school time on achievement and educational attainment and the role these influences play in the development of persistent racial and socioeconomic achievement and attainment gaps. Even before students enter kindergarten, substantial achievement gaps exist along racial and socioeconomic lines (Reardon, Robinson-Cimpian, and Weathers, 2015).

Once enrolled in school, students' out of school experiences continue to affect their educational outcomes. For example, absences from school because of illness, truancy, and weather, among other resaons, have a negative impact on student achievement, and the negative effect is even greater for low-income students (Marcotte and Hemelt, 2008; Ready, 2010; Gottfried, 2009, 2011; Gershenson, Jacknowitz, and Brannegan, 2015). Given the negative impact of absenteeism on students' achievement, it is reasonable to ask whether students' summer vacation, a time most often spent away from school and that makes up roughly one-quarter of the calendar year, also affects student achievement and persistent achievement gaps.

To date, the dominant understanding of student learning over the summer derives from a meta-analysis of 13 studies from 1975 to 1995. Cooper et al. (1996) found that students lost mathematics and reading knowledge over the summer, although the loss in mathematics knowledge was generally greater than in reading. This evidence also suggested losses were larger for low-income students and for all students in upper elementary and middle school grades. However, the studies in this meta-analysis are dated.[12] Schools operate in a different policy environment now, and arguably that environment is more focused on enhancing student outcomes. The changed policy environment merits revisiting this research.

---

[12] For example, the two most commonly cited studies are from the 1970s and 1980s. The first analyzed data from 42 schools in Atlanta in the early 1970s (Heyns, 1979). The second analyzed data from 750 students in Baltimore in the 1980s (Entwisle and Alexander, 1990, 1992; Entwisle, Alexander, and Olson, 2001; Alexander, Entwisle, and Olson, 2007).

Over the past decade, a few data sources have allowed researchers to extend our understanding of the role of summer break in student achievement and the development of persistent race and income achievement gaps. These studies either take advantage of seasonal testing in the Early Childhood Longitudinal Study Kindergarten (ECLS-K) class or the Northwestern Evaluation Association (NWEA) Measures of Academic Progress assessment, or they examine specific summer interventions.

Throughout the main report, we cite relevant current literature on summer learning loss and summer program effectiveness. While these reflect our current understanding of summer learning loss and provide important context for the current study, it is also important to acknowledge the ways in which the summer learning loss literature of the past 20 years is limited.

Summer learning loss literature has not kept pace with other research topics, such as school choice or accountability, partly because of the common practice of testing students only once during the school year. Although the ECLS-K data set includes a rich description of student and family characteristics, including summer activities, it only enables measurement of achievement over one summer for one nationally representative cohort of students—kindergarten and first grade in 2001.

The NWEA data set contains more recent test scores from nonrandom samples of students across the 50 states, but it too suffers from limitations. In most cases, the research using NWEA data does not include student socioeconomic descriptors; therefore, NWEA studies cannot evaluate the impact of student-level poverty on students' summer learning loss as other studies have done. Further, the NWEA data do not contain school calendar information, which prevents researchers from knowing how much school time elapses after spring and before fall testing.

Researchers also differ in preferred methods of measuring students' summer learning and its relationship with socioeconomic status. For example, while some include linear predictors for the relationship between summer learning and socioeconomic status, others model the relationship more flexibly. We think the flexible specification better captures the true relationship between poverty and summer learning, where the key differences are likely between students at the tails of the socioeconomic distribution (e.g., Reardon's [2011] work on the widening achievement gap between students in the 10th and 90th percentiles in the income distribution).

Related to the lack of school calendar information in the NWEA data, researchers also differ in whether they adjust for the timing of fall and spring assessments. Schools do not typically assess students on the first and last day of school. As such, simply measuring the gap between two test points usually conflates school year and summer time periods. Most ECLS-K studies separate the amount of time students have spent in schools and summer breaks between testing periods using both the test administration date and school calendar information. The only publications on summer learning using NWEA data and accounting for test timing are forthcoming or working papers (Von Hippel, Hamrock, and Kumar, 2016; Atteberry, McEachin, and Bloodworth, forthcoming). Atteberry, McEachin, and Bloodworth follow Quinn's (2014) method of preprocessing the data to project students' learning to what it would have been at the start and end of the school year, and then use standard growth model methods. Von Hippel, Hamrock, and Kumar (2016) estimated seasonal learning growth models that use school calendar data to separate school year learning from summer learning. Other NWEA studies (e.g., Rambo-Hernandez and McCoach, 2015) do not have access to school calendar data.

Finally, researchers also differ in how they estimate students' growth trajectories over time. The common method in ECLS-K studies uses four assessments from fall of kindergarten to the spring of first grade to fit multilevel growth models (Downey, Von Hippel, and Broh, 2004; McCoach et al., 2006; Benson and Borman, 2010; Ready, 2010; Von Hippel, Hamrock, and Kumar, 2016). This data set and approach not only allow researchers to estimate the amount of summer learning (gain or loss), and how this varies by student demographics, they also allow researchers to compare how summer learning rates compare with school year learning in kindergarten and first grade. Hayes and Gershenson (2014) and Burkham et al. (2004) take a different approach, however, and only estimate students' summer learning between the spring of kindergarten and the fall of first grade. In their analyses, they do attempt to back-out school year learning rates from the time between the administration of the spring kindergarten assessment and the end of the school year, and the start of first grade in the fall and the administration of the first-grade assessment. Although this is a plausible method to measure summer learning, it produces counterintuitive findings: Students' summer learning rates are not only positive but also larger than school-year learning during kindergarten. Researchers using a growth model over all four assessments do not find this pattern (Downey, Von Hippel, and Broh, 2004; McCoach et al., 2006; Benson and Borman, 2010; Ready, 2010; Von Hippel, Hamrock, and Kumar, 2016).

In summary, we think the best evidence for summer learning patterns comes from the ECLS-K studies that adjust for assessment timing and use all four assessments to measure school year and summer learning (Downey, Von Hippel, and Broh, 2004; McCoach et al., 2006; Benson and Borman, 2010; Ready, 2010; Von Hippel, Hamrock, and Kumar, 2016), the two NWEA studies that adjusts for assessment timing (Von Hippel, Hamrock, and Kumar, 2016; Atteberry, McEachin, and Bloodworth, forthcoming), and the handful of experimental and quasi-experimental studies that include summer learning estimates (Borman and Dowling, 2006; White et al., 2014; Zvoch and Stevens, 2013, 2015). We use these studies as the core of our review.

Income-Based and Race-Based Gaps in Learning over the Summer

While recent studies are inconclusive on the absolute loss of achievement over summer breaks, they show evidence that low-income students experienced setbacks over the summer relative to their wealthier peers. On average, most studies found that low-income students learned less *relative* to their wealthier peers even if they did not experience *absolute* losses over the summer (Downey, Von Hippel, and Broh, 2004; McCoach et al., 2006; Benson and Borman, 2010; Ready, 2010; Von Hippel, Hamrock, and Kumar, 2016). This point is also supported in recent summer intervention evaluations (Kim, 2004; Benson and Borman, 2010; White et al., 2014). Likewise, students in low-income neighborhoods (Benson and Borman, 2010) and schools serving larger shares of low-income students (White et al., 2014; Atteberry, McEachin, and Bloodworth, forthcoming) experienced larger losses over the summer relative to peers in wealthier neighborhoods or schools.

It is unclear what causes students of different backgrounds to have different summer experiences. Some research suggests that summer learning loss for low-income students could be related to students' opportunities to practice academic skills over the summer (Heyns, 1979; Cooper et al., 1996; Downey, Von Hippel, and Broh, 2004). For example, Gershenson (2013) found that low-income students were more likely to watch two or more hours of television per day during the summer, on average, than were students from wealthier backgrounds.

Recent research does not find consistent evidence that African American or Hispanic students experience different summer-learning trajectories than white students regardless of whether family income is included in the analysis. When researchers included the socioeconomic status (SES) data from the ECLS-K class in their statistical models, the gap between African American and Hispanic students and their white peers did not widen over the summer between kindergarten and first grade (Burkham et al., 2004; Downey, Von Hippel, and Broh, 2004; McCoach et al., 2006; Benson and Borman, 2010; Von Hippel, Hamrock, and Kumar, 2016). Two recent studies using different samples of NWEA data came to different conclusions about the effect of summer breaks on racial achievement gaps (Von Hippel, Hamrock, and Kumar, 2016; Atteberry, McEachin, and Bloodworth, forthcoming). The former found no change in the gap between African American and Hispanic students and white students, while the latter found a widening of these achievement gaps. Quinn (2014) and Von Hippel, Hamrock, and Kumar (2016) show that both the magnitude and direction of changes in the achievement gap are sensitive to a number of statistical considerations, and that results vary from negative to no difference to positive between African American and white children over the summer.

Extent of Summer Learning Loss as Children Age

Recent research, on the whole, suggests that students experience summer learning loss starting in the summer prior to second grade (Atteberry, McEachin, and Bloodworth, forthcoming). In the early primary grades, by contrast, results from the ECLS-K studies suggest that, at worst, summer learning is flat, and in some cases students actually *gain* ground over the summer between kindergarten and first grade (Downey, Von Hippel, and Broh, 2004; McCoach et al., 2006; Benson and Borman, 2010; Ready, 2010). Taken together, these studies comport with results from Cooper et al. (1996) that find summer learning losses to be greater in later grades, and that losses do not show up in reading until second grade.

Summer Program Effectiveness

Here, we review evidence on the effectiveness of summer learning programs from Cooper et al. (2000) and 25 experimental or quasiexperimental research studies conducted since 2000. We include only studies that used rigorous research methods to limit the possibility that the effects reported were driven by students' backgrounds and prior achievement rather than the summer programs. The programs covered in our review include voluntary classroom-based summer programs, mandatory summer programs that students must attend to avoid retention in grade, and voluntary at-home summer reading programs.

Most of the extant literature focuses on ITT effects. These effects capture the average impact of an intervention as it was designed. For example, ITT effects treat every student in the treatment and control group equally, regardless of their participation in the program (attendance) or fidelity of treatment assignment. However, as with school-year learning, it is also important to understand how effective the treatment is for students who actively engage with the intervention (e.g., high attenders). Researchers often call the latter the TOT effects. For this reason, we describe both the overall effects of summer learning programs on students' academic and nonacademic outcomes (ITT effects), how these effects vary over time, and how subject, student demographics, grade level, and attendance influence them (TOT). We also review descriptive results that correlate characteristics associated with effective summer learning programs with student outcomes.

Academic Outcomes

In general, similar to the random assignment studies reviewed by Cooper et al. (2000), our review found that summer programs had effects of around 0.10 SD—which is equivalent to moving a student from the 50th percentile of an achievement distribution to the 54th percentile—although some studies found no effect and others showed larger effects.

We reviewed a number of voluntary summer learning programs that invited students to attend site-based interventions but did not require attendance for grade promotion.[13] Across these studies, the average treatment effects varied from zero (Borman, Benson, and Overman, 2005; Arbreton et al., 2011; McCombs et al., 2014 [reading]; Somers et al., 2015), to small-to-moderate (i.e., 0.05 to 0.15 SD) positive effects in mathematics and/or reading achievement (McCombs et al., 2014 [mathematics]; Zvoch and Stevens, 2015), to large effects in mathematics and/or reading (0.2 to 1.0 SD) (Schacter and Jo, 2005; Borman, Goetz, and Dowling, 2009; Zvoch and Stevens, 2011, 2013; Snipes et al., 2015 [mathematics]). It should be noted that McCombs et al. (2014) was the only study to correct for multiple hypothesis tests. Consistent with more general literature on education studies, the effects tended to be larger in studies with

---

[13] These studies are Borman, Benson, and Overman, 2005; Schacter and Jo, 2005; Borman and Dowling, 2006; Chaplin and Capizzano, 2006; Borman, Goetz, and Dowling, 2009; Arbreton et al., 2011; Zvoch and Stevens, 2011; Zvoch and Stevens, 2013; McCombs et al., 2014; Mac Iver and Mac Iver, 2014; Snipes et al., 2015; Somers et al., 2015; and Zvoch and Stevens, 2015.

smaller sample sizes (e.g., a few hundred students in one location) or that used curriculum-based assessments closely aligned to the design of the intervention or those studying lower grade levels.

We also reviewed four studies of mandatory education summer learning programs that met our research design criteria. All four found positive impacts on student achievement ranging from approximately 0.1 SD in mathematics and reading achievement (Jacob and Lefgren, 2004; Matsudaira, 2008) to similar effect sizes but only in reading (Mariano and Martorell, 2013).

Evaluations of reading-at-home programs also sometimes show that they are effective. Some recent studies of reading-at-home programs using randomized controlled trial designs found promising or significant short-term effects (Kim, 2006; Kim and White, 2008; Allington et al., 2010; Guryan, Kim, and Park, 2015) and cumulative effects over time (Allington et al., 2010). Other studies found no overall reading effects among students who were part of the treatment group (Kim, 2004; Borman, Goetz, and Dowling, 2009; Kim and Guryan, 2010).

Nonacademic Outcomes

Evaluation results are thus far not promising in terms of summer programs boosting social and emotional competencies such as self-regulation and motivation. An early evaluation of Building Educated Leaders for Life found positive effects on the degree to which parents encouraged their children to read, but no influence on students' academic self-perceptions or social behaviors (Chaplin and Capizzano, 2006). Similarly, Snipes et al. (2015) found that while a short summer algebra readiness program improved student achievement, it did not have a significant effect on students' interest or sense of competence in mathematics. Furthermore, Somers et al. (2015) and McCombs et al. (2014) found no effect on social-emotional competencies as measured at the end of the summer program or at the beginning of the fall semester.

Long-Term Effects

No rigorous research study has examined outcomes in the near term and then again beyond two years after summer program participation. The studies that did look one to two years out found that positive effects, if any, decayed by approximately one-third to one-half within just one year after the end of the summer program (Schacter and Jo, 2005; Kim and Quinn, 2013). For example, Schacter and Jo (2005) found a statistically significant, positive effect on students' decoding and reading comprehension immediately after the summer intervention; however, the decoding effects were not statistically significant nine months later and the reading compression effects decreased by 65 percent (although remained statistically significant).

Variation in Effectiveness of Summer Learning Programs

We also reviewed the literature to examine how the subject of assessments, student demographic subgroups, and grade levels moderate the causal effects of summer interventions.

We did not find the consistent evidence reported in Cooper et al. (2000) that these variables moderate the effect of summer programs. We also reviewed the literature for noncausal descriptive evidence of attendance as a mediator for summer interventions.[14]

Subject

We found mixed evidence that summer programs focusing on both mathematics and reading were more effective in raising students' mathematics achievement than reading achievement. This examination excluded results from single-subject programs. The mandatory summer programs we reviewed found larger effects in mathematics than reading, although the differences were small (Jacob and Lefgren, 2004; Matsudaira, 2008; McCombs et al., 2009). Of the three voluntary studies that included multiple subjects, McCombs et al. (2014) found positive effects only in mathematics, but Arbreton et al. (2011) and Somers et al. (2015) found no statistically significant effects in either subject.

Demographic Groups

Recent studies either found no relationship between SES and summer program effectiveness (Arbreton et al., 2011; McCombs et al., 2014), or that lower-income students, or students attending schools with larger shares of lower-income students, gained more than their higher-income peers from these programs (Allington et al., 2010; Kim and Quinn, 2013; White et al., 2014). This evidence stands in contrast to results found by Cooper et al. (2000) that middle SES students had larger positive effects than lower SES students.

The evidence for race/ethnicity as a moderating variable is less extensive. Kim (2006) found larger treatment effects for African American students, but there is not much corroborating evidence because many other studies did not test this moderator (e.g., Cooper et al., 2000; McCombs et al., 2014).

Grade

We did not find consistent variation in program effectiveness across grade levels. Matsudaira's examination (2008) of the effects of a mandatory summer school program in an urban district found that students in higher grades benefited more from summer school than did students in third grade. However, other studies found no differences by grade level (Roderick, Engel, and Nagaoka, 2003; Jacob and Lefgren, 2004; McCombs et al., 2009).

---

[14] We define a *statistical moderator* as a characteristic unrelated to the design of the intervention (e.g., race). We are interested in whether these characteristics influence the efficacy of the intervention. We define a *statistical mediator* as a characteristic that is directly related to either the design or fidelity of the intervention (e.g., student attendance), and we are interested in the role this characteristic has in the efficacy of the intervention. Although assignment to summer interventions is random, students' decision to attend the intervention on any given day is not random. Therefore the relationship between attendance behavior and student outcomes for the treatment students is correlational and not causal.

Attendance

Similar to Cooper et al. (2000), we found evidence of positive, noncausal relationships between positive effects of summer interventions and attendance, hours of instruction, and years of participation (Borman, Benson, and Overman, 2005; Benson and Dowling, 2006; McCombs et al., 2009, 2014). Each additional week of summer attendance is related to an approximate 0.02 to 0.05 SD in achievement (Borman, Benson, and Overman, 2005 [reading]; McCombs et al., 2014 [mathematics]). Similar to the findings discussed in Chapter Six in the report, Borman and Dowling (2006) found that students who had high attendance in each of two summers outperformed their peers who participated less frequently in the summer program.

Components of Quality Summer Learning Programs

We now turn to whether the structural components of the programs—such as the duration, nature of the curricula, and teacher characteristics—contributed to the variation in program effectiveness. As with the attendance results, structural components of summer programs were not randomly assigned to students and should be treated as exploratory and not causal. In this section, we explore the program characteristics in the 25 studies in our literature review of summer interventions as well as those in Cooper et al. (2000).

Small Class Sizes

Small class sizes might provide teachers with more time to work individually with students and to create greater opportunities to differentiate instruction based on student needs. Research has found that small class size is associated with summer program effectiveness; Cooper et al. (2000) found that summer programs in which class size was capped at 20 students were more effective than others in producing achievement gains. Although Kim and Quinn (2013) found no statistically significant relationship between class size and program quality, they did observe effects from combining class size with dosage (defined as class sizes of no more than 13 students, at least four hours of participation per day, and at least 70 hours of total participation). However, McCombs et al. (2014) did not find that class size strengthened the positive relationship between instructional hours and student achievement. This may be because of insufficient variation in class sizes; prevailing class sizes were small in all five studied districts—ranging from an average of eight to 14 students per teacher.

Aligned to Student Needs

Learning science recommends that to maximize the benefit of academic experiences, especially in literacy, students' assignments should be well aligned to their interests and needs (e.g., Wright and Stone, 2004). It is recommended that summer learning programs align instruction to school-year activities, and instruction should be tightly focused on addressing student needs with high-quality instruction (e.g., Zvoch and Stevens, 2013, 2015). The findings from the many replications of Project READS (Kim, 2006; Kim and White, 2008; Kim and Guryan, 2010; Guryan, Kim, and Park, 2015), an at-home summer literacy program, clearly show that students were not only more likely to read over the summer when books were aligned to their interests and matched to their reading levels, they were also more likely to comprehend what they were reading, and these comprehension effects persisted into the following school year.

Qualified Teachers

McCombs et al. (2014) found a positive, statistically significant association between prior teaching experience and reading outcomes. Specifically, they found that students who had summer teachers who had just taught either their sending or receiving grade performed better than other students on a fall reading assessment. To recruit and hire the right teachers, Augustine et al. (2013) recommended developing rigorous selection processes to recruit motivated teachers and, to the extent possible, taking teachers' school-year performance into consideration. Augustine et al. also stressed the importance of hiring teachers with not only grade-level but also subject matter experience and, if possible, familiarity with the students.

High-Quality Instruction

In addition to the importance of recruiting qualified teachers, their instruction of the curriculum is important. McCombs et al. (2014) observed and evaluated instructional quality for each classroom in their study. Their analysis found a positive association between quality of instruction and better student performance in reading. They did not find a relationship between quality of instruction and student performance in mathematics. Furthermore, Kim and Quinn (2013) examined voluntary and at-home literacy programs that used research-based instruction (as operationalized by the National Reading Panel in 2000), such as guided repeated oral reading, relating readings to students' prior experiences, explicitly modeling strategies for students, and so on. Programs that included these practices had significantly larger positive effects on students' reading outcomes than programs that did not use them.

Site Culture

McCombs et al. (2014) expected that students in more orderly sites would have better outcomes because they and their teachers would be less likely to be distracted by misbehavior. To evaluate student discipline and order in the district programs they studied, they created a scale for each site within each district based on teacher survey data. On the survey, teachers were asked for their observations of student bullying, physical fighting, and other indicators of orderliness. They found that students who attended more orderly sites outperformed other students on the fall reading assessment.

Sufficient Duration

Researchers generally distinguish between allocated time (the time on the school calendar for a given content area) and academic learning time (the amount of time students spend working on rigorous tasks at the appropriate level of difficulty). Academic learning time is more predictive of student achievement (Harnischfeger and Wiley, 1976; Fisher et al., 1980; Karweit and Slavin, 1982; Hawley et al., 1984; Karweit, 1985). Furthermore, research also suggests that spaced practice (once a day for several days) as opposed to one long, concentrated lesson (all day long for just one day) appears to be more effective in facilitating learning (Walberg, 1988; Rohrer and Taylor, 2006; Rohrer and Pashler, 2010). When focusing on boosting students' literacy skills, Zvoch and Stevens (2013) recommend that students receive at least two hours of teacher-directed daily instruction blended between whole group and lessons in small groups (three to five students) and that the program meet regularly during the week (four to five times) for at least five weeks.

McCombs et al. (2014) found a relationship between academic time on task and mathematics outcomes, and recommended that to maximize benefits for students, school districts may want to plan for programs that run at least five weeks and schedule 60 to 90 minutes of mathematics per day. Because instructional time on task is reduced because of student absences and inefficient use of time inside the day, McCombs et al. (2014) suggested special efforts to promote consistent attendance, maintain daily schedules, and ensure teachers maximize instructional time inside the classroom.

Conclusions

Generally, recent research confirms what we understood in 2011, with some new nuances:

- Low-income students experience setbacks over the summer relative to their wealthier peers. Students in upper elementary grades and middle grades experience summer learning loss. However, evidence is mixed about the overall learning trajectories of lower elementary grade students.
- Summer learning programs can, but do not necessarily, benefit students. Some studies find positive benefits from these programs, while others do not.
- In the limited number of studies that measure both mathematics and reading outcomes, some evidence indicates that mathematics effects are either more likely to be statistically significant than reading or are larger than reading when both are significant; however, several studies do not find significant effects in either subject.
- Mandatory programs are most likely to consistently produce positive academic gains for students. This may be because of the built-in incentive for students to attend consistently (i.e., the threat of grade retention).
- Although academic benefits are often observed, the few studies that have examined nonacademic benefits have found no effect.
- Academic benefits fade over time, as is true for most academic interventions that have been tracked over time.
- There is little connection between students' backgrounds and grade level and the effectiveness of a summer program, suggesting that there might not be a "best" target population for summer programming.

Appendix D: Hypothesized Mediators and Moderators of Summer Program Effects

Attendance and Academic Time on Task

Our primary hypothesis is that the amount of instructional time a student receives mediates (is part of the causal pathway for) the effect of the summer program on that student's outcomes. Therefore, a primary objective of the analysis is to estimate as accurately as possible the amount of instruction students received. We measure the amount of instruction using information about both program attendance (the number of days of the summer program an individual student attended) and program academic time on task (the number of instructional hours an attender received).

Attendance

We collected and audited each week of the five- to six-week summer program student-level daily attendance data from each of the five school districts. This helped us to increase the accuracy of the attendance data.

Once we had collected daily attendance data, we performed a series of analyses to identify discrete cut points in the relationship between days of program attendance and estimated treatment effects. Specifically, we used splines (compositions of polynomials) to allow for nonlinear relationships. For example, if 15 or fewer instructional days are not associated with any increase in achievement, but there are incremental increases beyond those days, then the spline curve might be relatively flat until 15 days, with an increasing slope thereafter. Interpreting these curves was guided by practical considerations and statistical criteria.[15] Although the curves varied across years and outcomes, there was a consistent tendency for them to increase for higher levels of attendance and to become significantly positive in the range of 15–25 days or more of attendance. To define a consistent threshold for use across all outcomes, we thus defined high attendance to be 20 or more days.

Because students self-select into their level of participation in summer programming, the spline analyses represent an exploratory technique that examines the relationship between

---

[15] At the encouragement of a peer reviewer, we explored a variety of ways of using splines in this analysis. In McCombs et al. (2014), we used another spline-based strategy for determining attendance categories: Generalized Additive Mixed Models analysis. The reviewer encouraged us to revisit this analysis to see if the spline curves would be useful for direct presentation of results instead of creating bins. However, the raw curves across years and outcomes were difficult to interpret, and the reviewer conceded that using the splines to define attendance categories was the preferred method for this study.

intensity of treatment and a given outcome, as well as any nonlinearities that may exist in that relationship.[16]

Academic Time on Task

To observe instructional time that students received within the program day, RAND staff followed each student classroom cohort for at least one entire day during the summer sessions. For example, if a site enrolled three classrooms of students—group A, group B, and group C—RAND visited the site for three full days in in both summer 2013 and 2014 to follow each classroom cohort for a school day. Thus, RAND observed each student cohort's mathematics and LA class at least once, and a majority of their enrichment programming courses at least once during the program using the classroom observation tool described in Appendix B. [17]

As we observed a sample of classrooms, in addition to observing for instructional quality and other features of instruction as described, we kept a time log recording when classes were scheduled to begin and end, the minute the majority of students were in the room and the teacher launched or ended the session, and minute-by-minute notes on class segments to track instructional and noninstructional time during the enacted class period (see Summer 2014 Classroom Observation Protocol in Appendix B). For example, we recorded that a class was scheduled to begin at 10:00 a.m., actually launched at 10:11 a.m., lost a combined total of six minutes to noninstructional activities such as a bathroom break, ended at 10:59 a.m., and was scheduled to end at 11:00 a.m.

With these classroom observation data linked to student classroom rosters, we created student-level mathematics/LA academic time-on-task indicators that equal the product of the following three measures: (1) the number of days a given student attended the summer program, multiplied by (2) the average number of hours that observed mathematics/LA classes lasted (meaning the enacted time from class launch to class wrap-up, regardless of scheduled class time), averaged across the subject-relevant classes RAND observed within a given site in summer 2013, multiplied by (3) the average percentage of enacted class time that was devoted to instruction. Table D.1 summarizes the average and the maximum of the number of instructional hours that treatment group students (including no-shows) received in mathematics and in LA across districts.

To increase the reliability of (2) and (3), we first averaged to the classroom (when classrooms were observed more than once), then to teachers (when teachers were observed more than once), and then to site. These are important components of our calculation, particularly because our

---

[16] We also explored the use of principal stratification to examine the relationship between attendance and outcomes. However, the results of these analyses were not trustworthy.

[17] Because of both the simultaneous enrichment activity rotations at some sites and the conducting of teacher interviews during some enrichment sessions in the second half of the summer session, RAND did not observe all classroom cohort-enrichment activity combinations at least once.

observations represent only about 10 percent of the total instruction provided over the course of each summer.

Table D.1. Distribution of Instructional Hours That Treatment Group Students Received

	Language Arts		Mathematics	
District	Average Number of Instructional Hours	Maximum Number of Instructional Hours	Average Number of Instructional Hours	Maximum Number of Instructional Hours
Summer 2013				
District A	23.6	52.7	18.2	42.1
District B	24.3	55.2	21.4	44.1
District C	30.1	60.2	14.9	32.8
District D	21.4	34.3	15.8	25.1
District E	13.3	27.5	15.3	34.6
Overall	**23.0**	**60.2**	**17.2**	**44.1**
Summer 2014				
District A	18.1	54.9	10.9	27.9
District B	10.1	40.1	9.4	46.7
District C	16.0	49.1	14.8	46.3
District D	12.3	36.5	13.5	38.8
District E	16.7	38.7	11.1	26.0
Overall	**14.2**	**54.9**	**11.5**	**46.7**

Once we developed estimates of the average number of instructional hours an attender received during summer 2013, we then created academic time-on-task categories that related instructional hours to daily attendance data. Specifically, we estimated that, averaging across classes, students received 1.7 hours of LA instruction per summer school day, and 1.275 hours of mathematics instruction per summer school day. This implies that high attenders (those who attend at least 20 days) would have received an average of at least 34 hours of LA instruction, and high attenders would have received at least 25.5 hours of mathematics instruction. Of course, the actual hours of instruction estimated for any particular individual varied as a function of days attended and the hours of instruction estimated for their specific classes.

Table D.2 displays the resulting ranges for levels of academic time on task and attendance.

Table D.2. Thresholds for Attendance and Academic Time-on-Task Categories

	Mathematics		Reading	
Threshold	Attendance (Days)	Dosage (Hours)	Attendance (Days)	Dosage (Hours)
Low	< 20	< 25.5	< 20	< 34
High	≥20	≥25.5	≥20	≥34

Cumulative Measures of Attendance and Academic Time on Task

Because the summer program is two years long, it is also possible to explore the cumulative effects of attendance and academic time on task. We define high attendance in both years as attending 20 or more days in each year of the summer program. High academic time on task is, correspondingly, defined as receiving 25.5 instructional hours in mathematics in each year, or 34 hours of reading instruction in each year.

Creation of Relative Opportunity for Individual Attention

In Kim and Quinn's (2013) meta-analysis, they observed that programs that had small class sizes and high academic time on task appeared to be associated with positive outcomes. To explore whether this relationship held for the National Summer Learning Study, we used our data on academic time on task and class size to create a synthetic variable we term students' "relative opportunity for individual attention." We hypothesized that students' outcomes would be mediated by not only the total amount of instructional time received in the summer session, but also by the number of students over which a lead teacher's attention was spread. In other words, we hypothesized that smaller class sizes would enhance the effective "dose" of instructional time received by a focal student as compared with another student receiving the same amount of instructional hours but within a larger class. Consequently, we developed a measure of dosage-by-class size, which was simply the division of a student's mathematics/LA instructional hours by that student's average mathematics/LA class size. "Relative opportunity for individual attention" is intended as a proxy for, rather than a direct measure of, individualized attention. We interpreted this variable as the relative amount of instructional attention that a teacher theoretically could have been able to provide each student over the entire summer.

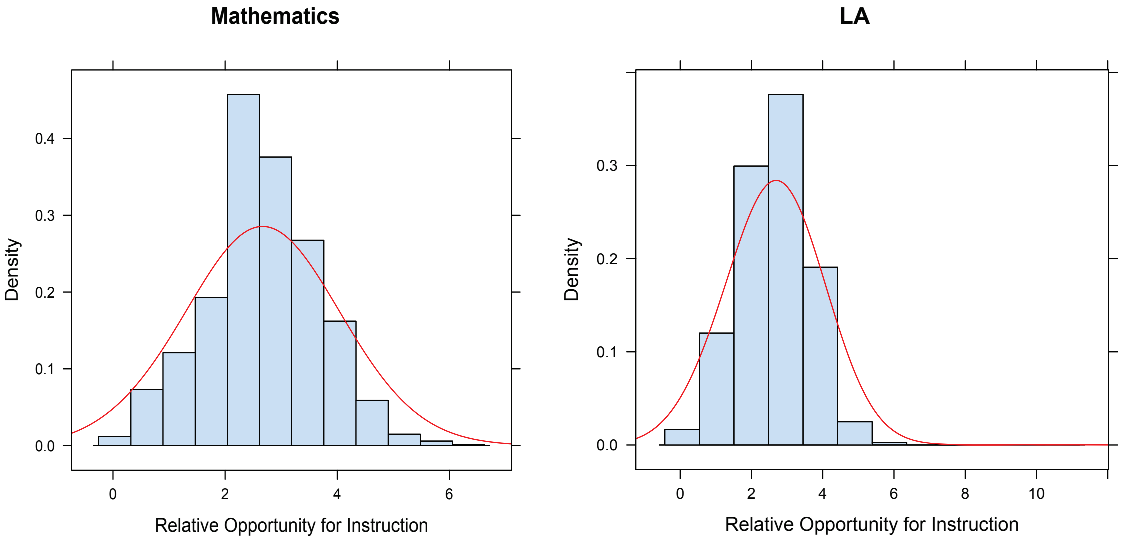
To create this measure, we first calculated the average number of students present in each LA/mathematics class (shown in Table D.3) by applying districts' student-level attendance data to both summer 2013 and 2014 LA/mathematics classroom rosters. Each student in the treatment group who attended one or more days of the summer program was associated with his assigned LA/mathematics classroom size.

Table D.3. Distribution of Average Language Arts/Mathematics Class Sizes

	Language Arts			Mathematics		
District	**Average Class Size**	**Minimum Class Size**	**Maximum Class Size**	**Average Class Size**	**Minimum Class Size**	**Maximum Class Size**
Summer 2013						
District A	9.3	4.2	14.7	9.3	4.2	14.7
District B	8.0	3.0	13.2	8.0	3.0	13.2
District C	11.3	6.9	17.1	11.3	6.9	17.1
District D	12.2	7.9	16.9	12.2	7.9	16.9
District E	14.4	9.9	17.1	14.4	9.9	17.1
Total	**10.4**	**3.0**	**17.1**	**10.4**	**3.0**	**17.1**
Summer 2014						
District A	9.1	5.5	12.4	9.1	5.5	12.4
District B	8.4	0.4	15.8	8.4	0.4	15.8
District C	9.7	4.3	14.0	9.7	4.3	14.0
District D	13.2	1.0	16.9	13.2	1.0	16.9
District E	8.7	3.4	13.2	8.7	3.4	13.2
Total	**9.5**	**0.4**	**16.9**	**9.5**	**0.4**	**16.9**

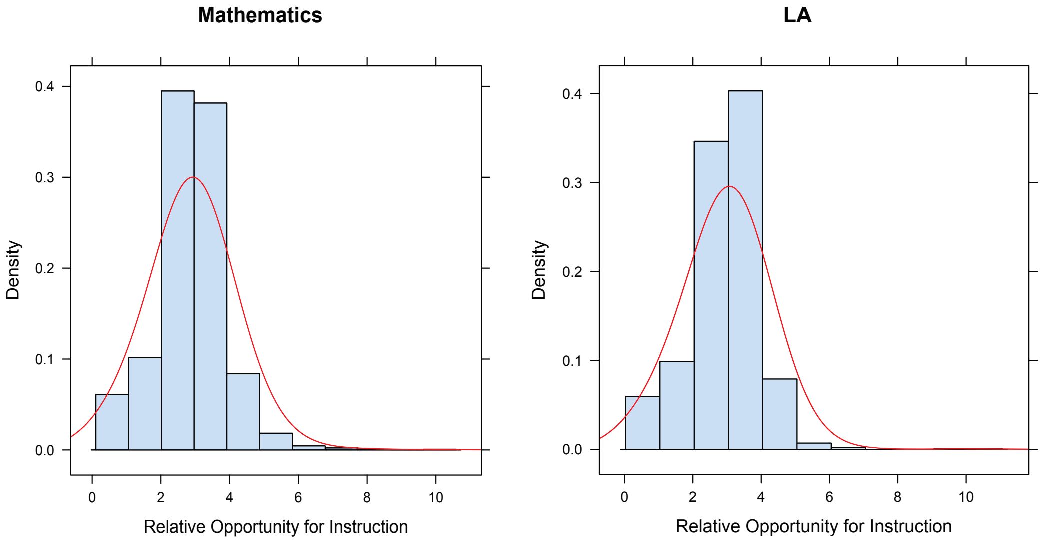
After dividing a focal student's total instructional hours by his or her average class size, we then applied a mathematical transformation (square root) to obtain a more normal distribution, and then normalized the values to have a standard deviation of one. Finally, we assigned a value of zero for this "relative opportunity for individual attention" measure to students in the treatment group who never attended the summer program. Figures D.1 and D.2 show the distributions of this variable for mathematics and LA students (excluding no-shows) for the 2013 and 2014 summer programs. These distributions are mostly symmetric and do not show evidence of floor or ceiling effects.

Figure D.1. Distributions of the Relative Opportunity for Individual Attention Variable (Fall 2013)


Mathematics

LA

Figure D.2. Distributions of the Relative Opportunity for Individual Attention Variable (Fall 2014)


Mathematics

LA

Scales Created from Teacher Survey and Classroom Observation Data

We further hypothesized that, in addition to the amount of instructional time students received, other factors may moderate the effects of the summer program. These factors include

- quality of instruction
- appropriateness of the mathematics/LA curriculum
- match between student grade level and a teacher’s prior year grade level assignment
- aspects of site climate, including the general degree of safety and order within summer sites.

To test these hypotheses, we identified individual items or sets of items from classroom observation and/or teacher survey data to serve as measures for these constructs.

We generated scales by first averaging item-level responses for each scale across the relevant classroom or survey items for each classroom observation or respondent. In addition to these six scales, we also tested whether teaching the sending or receiving grade level of students moderated the summer programming effects on students. Summary statistics for these scales are shown in Table D.4.

Table D.4. Summary Statistics of Hypothesized Mediators

Mediator	Level at Which Mediator Measured (data source)	*N*	Mean	Minimum	Maximum
Fall 2013					
Quality of LA instruction scale	Classroom (RAND classroom observations)	199	5.0	2.7	7.1
Quality of mathematics instruction scale	Classroom (RAND classroom observations)	195	5.2	2.6	7.0
Opportunity for social-emotional development scale[a]	Site (RAND classroom observations)	37	4.6	3.2	6.2
Appropriateness of LA curriculum[a]	Classroom (teacher survey)	139	16.0	8.0	20.0
Appropriateness of mathematics curriculum[v]	Classroom (teacher survey)	145	13.0	5.0	16.0
Student discipline and order scale	Site (teacher survey)	37	16.7	11.4	20.0
Student's LA teacher taught third or fourth grade in SY 2012–2013	Classroom (staff rosters)	155	0.56	0	1
Student's mathematics teacher taught third or fourth grade in SY 2012–2013	Classroom (staff rosters)	153	0.66	0	1
Fall 2014					
Quality of LA instruction scale	Classroom (RAND classroom observations)	140	5.8	1.8	9.0
Quality of mathematics instruction scale	Classroom (RAND classroom observations)	135	6.2	1.7	9.0
Good fit mathematics curriculum	Student	5,620	0.74	0	1
Good fit LA curriculum[b]	Student	5,617	0.68	0	1
Student discipline and order scale	Site	32	17.4	14.0	20.0
Student's LA teacher taught fourth or fifth grade in SY 2013–2014	Classroom (staff rosters)	146	0.60	0	1
Student's mathematics teacher taught fourth or fifth grade in SY 2013–2014	Classroom (staff rosters)	146	0.54	0	1
Daily climate scale[b]	Site	32	21.8	15.3	27.0
Positive school climate[b]	Site	32	4.4	3.9	6.0

NOTES: For student level, *N* = number of students; for summer teacher level, *N* = number of teachers; for summer site level, *N* = number of sites.

[a] Included in fall 2013 analyses, but not included in subsequent analyses.

[b] Used in all analyses beginning with the analyses of spring 2014 results.

The survey or classroom observation items that were included in each scale and the estimates of internal consistency reliability (coefficient Alpha) are provided in Tables D.5–D.9. Note that some of the scales shown in these tables had coefficient Alpha values of less than 0.70, meaning that there are high levels of measurement error in the scales that might have influenced the findings reported. However, this error would result in overly conservative, rather than upwardly biased, estimates of the moderating effect of these constructs.

Quality of Instruction

This scale is derived from RAND classroom observation data and calculated at the student's LA/mathematics classroom level by first summing the items in the scale (listed in Table D.5) and then averaging up to the classroom level.[18]

Table D.5. Mathematics/Language Arts Instructional Quality Items and Internal Consistency Reliability Estimates

Scale Items
Coefficient Alpha for Mathematics: 0.632
Coefficient Alpha for Language Arts: 0.672
1. Range: 0–1 point = observed % of mathematics/LA class time that was spent on instruction. Scaled so 0 = min observed percent on instruction, and 1 = max observed % on task.
2. 1 point if "The teacher exhibited obvious signs of enthusiasm about the content of the class."
3. 1 point if "Large majority of students are on task throughout the class. Students are focused and attentive to the task/project."
4. 1 point if there were no incidences of "The teacher provided or failed to correct factually inaccurate information that would confuse students about the content/skills they were to learn."
5. 1 point if "Teacher explained purpose of class in terms of real-world relevance."
6. 1 point if there were no incidences of "Teacher's explanation of the instructional content was unclear or hard to follow."
7. 1 point if "Teacher (1) performs ongoing assessment throughout the whole class period by checking for students' understanding of content, and (2) addresses misunderstanding if and as they arise.
8. 1 point if rated no: "When the teacher disciplined students, the majority of the class was interrupted for a long period."
9. 1 point if rated no: "The teacher responsible for the activity was disengaged in the classroom because of distractions by factors that were within her control."
10. 1 point if yes: "All or almost all students exhibited obvious signs of enthusiasm for the class throughout the class period (e.g., jumping out of seat, quickly and enthusiastically answering teacher's questions)."

---

[18] For our initial analyses, we explored methods for improving the accuracy of the quality measures by applying a simplified version of small-area estimation shrinkage (McCaffrey, Han, and Lockwood, 2013). However, reviewers expressed concerns that these shrunken estimates could produce biased or inconsistent parameter estimates in regression models. Our investigations into this issue suggested that the raw and shrunken estimates of instructional quality produce the same inferences when tested for relationships with treatment effects. We report all results for this mediator using the raw estimates of instructional quality.

Appropriateness of the (Mathematics/Language Arts) Curriculum

We hypothesized that curriculum deemed appropriate by teachers for their students—which we define as a combination of perceptions about reasonable pacing, clarity of curriculum, addressing the right gaps in student knowledge and skills, and being fun for students—would enhance the effectiveness of summer programming in boosting student achievement. This scale is thus derived from the academic teacher survey and is a teacher-level construct associated with the treatment students assigned to that mathematics/LA teacher. In the survey, teachers who reported teaching mathematics during summer 2013 were prompted to answer mathematics curriculum questions, with a parallel structure for LA teachers. Teachers who taught both subjects were asked to complete both sets of curriculum questions. The mathematics curriculum scale includes four items and the LA curriculum scale includes five.

Table D.6. Appropriateness of Mathematics/Language Arts Curriculum Scale Items and Internal Consistency Reliability Estimates

Scale Items
Coefficient Alpha for Language Arts: 0.749
Coefficient Alpha for Mathematics: 0.741
• 1–4 points on Likert scale. The planned pacing of the curriculum was reasonable.
• 1–4 points on Likert scale. The mathematics curriculum is clear for me to follow.
• 1–4 points on Likert scale. The mathematics curriculum addresses gaps that many students have from last year.
• 1–4 points on Likert scale. The mathematics curriculum includes fun, interesting activities for children.
• 1–4 points on Likert scale: [for LA only]: The LA curriculum provides students texts that are appropriate for their reading level.

SOURCE: RAND's summer 2014 academic teacher survey.
NOTES: 1 = Disagree a lot, 2 = Disagree a little, 3 = Agree a little, 4 = Agree a lot.

Good Fit of the (Mathematics/Language Arts) Curriculum

Within each district, we used teacher survey responses to determine whether the summer mathematics and LA curricula were a good fit for students. For each performance level on the spring 2014 or 2015 state assessments, if the majority of teachers said the curriculum was a good fit for students at that performance level, all students in that district who scored at that level received a 1 for good fit; alternatively, if the majority of teachers said the curriculum was not a good fit for a particular performance level, all students in that district who scored at that level received a 0 for good fit.[19]

---

[19] If students lacked a level score for spring 2014 or spring 2015 we used their level score from spring 2013. In the rare cases where students lacked level scores for both years, students were dropped from the analysis.

Student Discipline and Order Scale

The student discipline and order scale is a site-level scale, derived from teacher survey data within sites (see Appendix B). The working hypothesis here was that attendance at sites that teachers deemed safe (free of bullying and fighting) and that teachers deemed to have a clear set of procedures for student discipline would enhance the effect of summer programming on student achievement. Items in the scale were first summed within a respondent, and then an unweighted average of respondents was calculated to develop a site-level scale score.

Table D.7. Student Discipline and Order Scale Items, and Internal Consistency Reliability Estimates

Scale Items
Coefficient Alpha for Language Arts: 0.811
1. 1–4 points on Likert scale. “Children are bullied and harassed by other students at least once a week.”
2. 1–4 points on Likert scale. “Children get into physical fights with other students at school at least once a week.”
3. 1–4 points on Likert scale. “The procedure for handling student discipline problems is effective.”
4. 1–4 points on Likert scale. “There is a clear procedure for handling student discipline problems.”
5. 1–4 points on Likert scale. Reverse coded. “Due to student misbehavior, a great deal of learning time is wasted.”

SOURCE: Summer 2014 Academic Teacher Survey.

NOTES: 1 = Disagree a lot, 2 = Disagree a little, 3 = Agree a little, 4 = Agree a lot.

Positive School Climate

This scale is also derived from summer classroom observation data and initially calculated at the classroom level. To develop scale scores, we first averaged to the classroom (when classrooms were observed more than once), then the teachers (when teachers were observed more than once), then the site. This site-level estimate is then attributed to each student who attended that site.

Table D.8. Site Climate Scale Items and Internal Consistency Reliability Estimates

Scale Items
Coefficient Alpha: 0.644
1. Average score of 0–1 point, where 1 point assigned if teacher shows explicit signs of positive affect toward youth. Mark no if teacher is simply respectful toward students. Teacher tone is warm and caring. He or she uses positive language, smiles, laughs, or shares good-natured jokes throughout class. If no verbal interaction is necessary, teacher demonstrates a positive and caring affect toward youth.
2. Average score of 0–1 point, where 1 point assigned if students show explicit signs that they have warm, positive affect to teacher (not just respect for teachers). For example, throughout the class they may smile at teacher, laugh with the teacher, or share good-natured jokes.
3. Average score of 0–1 point, where 1 point assigned if students verbally encourage one another, are overtly friendly and supportive.
4. Average score of 0–1 point, where 1 point assigned if students show respect for one another. They refrain from derogatory comments or actions about an individual person and the work he or she is doing; if disagreements occur, they are handled constructively.
5. Average score of 0–1 point, where 1 point assigned if there were no instances where the teacher was disrespectful to students. This includes yelling at one or more students, intimidating or being rude or dismissive to students, using physical aggression, intentionally humiliating or ignoring a student, using discriminatory acts or derogatory language to students.
6. Average score of 0–1 point, where 1 point assigned if there were no instances of flagrant student misbehavior. This includes a physical fight or persistent bullying or persistent use of discriminatory or derogatory language.
7. Average score of 0–1 point, where 1 point assigned if there were no instances where the teacher responsible for the activity was disengaged in the classroom because of distractions by factors that were within her control (i.e., a teacher stopping by to have a conversation about the weekend, the teacher checking his/her cell phone, texting, or taking or making a personal call that was not related to an emergency, personal chat with co-teacher or paraprofessional while students are working).

The Daily Climate Scale

This scale draws on a daily survey filled out by a RAND observer at the end of the summer program day. The daily climate scale is the average of the following six RAND observer–rated items (each item is rated at the end of the given observation day), and then averaged over the total number of observation days at the given summer site. The rating for each item was on a five-point scale from 1 (strongly disagree) to 5 (strongly agree). The middle point on the scale represented a neutral response. The items are presented in Table D.9.

Table D.9. Daily Climate Scale items

Scale Items
1. 1–5 points on Likert scale. Adults at the site address student behavior consistently and appropriately when misbehavior occurs
2. 1–5 points on Likert scale. Almost none or no student misbehavior at this site
3. 1–5 points on Likert scale. Site is well-organized (predictable routines, smooth transitions, clear communication among staff)
4. 1–5 points on Likert scale. Students appeared to have enjoyable day
5. 1–5 points on Likert scale. Staff are overtly friendly toward students
6. 1–5 points on Likert scale. Students are overtly friendly toward and supportive of one another

SOURCE: Summer 2014 daily climate observation survey.

NOTES: 1 = Strongly disagree, 2 = Disagree, 3 = Neutral, 4 = Agree, and 5 = Strongly agree.

Stand-Alone Moderator

Finally, we hypothesized that having a teacher who worked in a proximate grade level during the prior school year (either third or fourth grade in Year 1, or fourth or fifth grade in Year 2) would moderate the effect of summer programming on students' mathematics and reading achievement. We reasoned that those teachers would theoretically be versed in the school-year academic standards that applied either in the year preceding or the year following the students' summer session, and they would be familiar with the most-common gaps between students' knowledge and these standards. For this item, we simply associated the dichotomous indicator with each treatment group attender via classroom rosters.

Appendix E: Statistical Analysis

Analysis Plan

Preferred Random-Effects Model

Our preferred model for ITT analysis of the impact of the summer learning programs is a random-effects model:

$$Y_{qispc} \ = \ \alpha T_{ispc} + \beta X_{ispc} + \delta PreTestMean_{qc} + \gamma_s + \pi_p * Z_{ispc} + \ \mu_c * Z_{ispc} + \varepsilon_{qispc}$$

where:

- Y_{qispc} is the standardized post-test score in subject q for student i in strata s in summer site p in summer classroom c, where p and c are defined to be 0 for students who did not attend the summer program.
- T_{ispc} is a indicator of assignment to the treatment group.
- X_{ispc} is a vector of baseline covariates (see below).
- $PreTestMean_{qc}$ is a vector of mean pretest values of all students who were assigned to the same summer classroom in subject q. This is zero for all students who did not attend the summer program. There are four classroom means, one for each of the four pretests (spring 2013 mathematics and LA, and the earlier assessments in mathematics and LA that were used for stratification).
- γ_s are strata fixed-effects (dummy variables).
- Z_{ispc} is an indicator variable taking a value of 1 if a student is a member of class c in site p, used to define random effects. Every student in summer site p and classroom c is associated with a random effect, including those students assigned to treatment who take up the program and those students assigned to control who take up the program (i.e., "crossovers"). This is 0 for the rest of the control-group students and all treatment group students that do not take up treatment ("no-shows").[20]
- $\pi_p * Z_{ispc}$ is a random-effect common to all students in summer site p.
- $\mu_c * Z_{ispc}$ is a random-effect common to all students in summer classroom c.
- ε_{qispc} is a residual, the variance of which is allowed to vary by pattern of available pretests.

The baseline variables in the model are:

- spring 2013 assessment scores (third grade) in mathematics and LA, standardized within district; interacted with district dummies; missing scores equal to 0

---

[20] This represents a change in the random-effects specification from McCombs et al. (2014). In that report, random-effects were specified based on treatment assignment (rather than class membership). This has the effect of giving random effects to all treatment students, even those that are no-shows, and suppressing random effects for all control students, even those that are cross-overs. These current random effects more accurately reflect the random effects structure with treatment noncompliance. Resulting parameter estimates have shifted slightly from those reported in McCombs et al. (2014), although the general patterns of substantive findings have not changed.

- spring 2012 or fall 2012 benchmark assessment scores in mathematics and LA, standardized within district; interacted with district dummies; missing scores equal to 0
- dummy variables for patterns of missing values of the pretests by district
- dummy for FRPL
- dummy for African American
- dummy for Hispanic
- dummy for ELL
- dummy for special education (exclusive of gifted and talented designation)
- dummy for male.
- SY 2012–2013 attendance (percentage of school days attended)
- dummy for SY 2012–2013 suspension.[21]

For all results presented throughout this report, we estimated this model using R's "lme" command. We made the assumption that the variance of the random effects was constant. We allowed the variance of the residual error to vary across the patterns of missingness in the pretests by district, because the prior scores were likely strong predictors of outcomes, and so the residual variance in outcomes should depend on which and how many prior scores were available.

The R command was:

```
lme( [outcome var] ~ [treatment var] + [baseline variables in X] + [pre-test
classroom means] + [strata dummies] + [missing data pattern indicators],
random = list(dumid = [summer site and classroom dummies]),[22]
weights = varIdent(form = ~1 | [missing data pattern indicators]))[23]
```

Secondary analyses used simple extensions to this model. In the case of attendance and academic time-on-task models, the treatment assignment indicator was replaced with continuous or categorical variables for these mediators. These models are detailed below. When testing other mediators or moderators (such as student characteristics, or class/site characteristics), the variable of interest was interacted with the treatment indicator.

---

[21] These covariates became available and were added after the publication of McCombs et al. (2014).

[22] The random effect specification in the R command is adapted from Lockwood, Doran, and McCaffrey (2003), which describes a flexible approach to specifying groupings, including the partial nesting of only treatment students in summer sites and classes, and the additional complexity of cross-classification that will results from students having different site and class assignments in the two summers of this experiment. With complete compliance, this specification is equivalent to the specifications outlined in Lohr, Schochet and Sanders (2014) used to analyze partially nested experimental designs.

[23] For the fall 2013 analyses, models were also estimated using STATA's "xtmixed" command using the code:

```
xtmixed [outcome var] [treatment var] [baseline variables in X] [strata dummies]
[pre-test classroom means] || [summer site and classroom ID variables]: [summer
site and classroom dummies] , nocons covariance(identity) , reml
```

Attendance and Academic Time-on-Task Analysis

Our preferred model for attendance and academic time on task analysis is a modification of the random-effects model used for the ITT analysis:

$$Y_{qispc} \ = \ \alpha W_{ispc} * T_{ispc} + \beta X_{ispc} + \delta PreTestMean_{qc} + \gamma_s + \pi_p * Z_{ispc} + \ \mu_c * Z_{ispc} + \varepsilon_{qispc}$$

where variables are defined as above, and

- W_{ispc} is a categorical variable indicating academic time on task or attendance category membership ("no show," "low" or "high")

Robustness Checks

In addition to our preferred model, we estimated three additional models to test the robustness of our ITT estimates on the fall 2013 outcomes.

1. random effects, no baseline covariates
2. ordinary least squares (OLS) regression models with bootstrap-based inference, baseline covariates
3. OLS with bootstrap-based inference, no baseline covariates.

Model 1 acted as a check that the inclusion of baseline controls was not driving the estimate of the ITT. We expected the model with baseline covariates would have a similar point estimate but a much smaller standard error because of the variance reduction from controlling for the pretests. We found that the resulting point estimates were similar to the preferred model because covariates were well balanced across the experimental groups because of randomization. The statistical significance of the estimates in this model were not used to judge whether the summer programs had an effect.

Models 2 and 3 were estimated to address the potential concern that our preferred model requires parametric assumptions about the form of the clustering induced by the program being delivered in summer classrooms (and assumes there is no clustering between any students in the control group). Model 2 contains the same covariates as the random-effects model, but uses ordinary least squares estimation. We conducted statistical inferences making fewer parametric assumptions and also allowed a more general form of clustering. Model 3 contains no covariates and is a bit more conservative than Model 2, in that it can produce slightly larger standard errors.

In order to estimate Models 2 and 3, we defined clusters corresponding to each summer site. We then considered each sending school to be a member of exactly one of these summer site clusters, defined as the summer site to which the majority of the school's treatment group students were sent for summer 2013. Once the clusters were determined in this manner, all treatment students were assigned to the cluster where they actually attended during summer 2013. All control group students (as well as nonattending treatment group students) were assigned to clusters on the basis of their sending school at the time of randomization in spring

2013. This resulted in all students in a summer site or classroom being defined to be in the same cluster, regardless of their actual regular-year schools, thus accounting for any clustering generated at the summer site and classroom levels.

The number of resulting clusters was less than 40 (because the number of summer sites was fewer than 40), so we were concerned about using the usual Huber-Eicker-White sandwich estimator cluster adjustment, which requires a large number of clusters to be valid. Instead, we used the bootstrap procedure discussed by Cameron, Gelbach, and Miller (2008) to calculate p values for the null hypothesis that the treatment had zero effect. Our analyses confirmed that results from the OLS and random-effects models were substantively the same.[24]

Analysis of Treatment Effect on the Treated

To estimate the effect of attendance in the summer program (i.e., the TOT),[25] we had to account for the fact that selection into program attendance in the treatment group was endogenous. Therefore, we used randomization status as an instrumental variable for program attendance. Specifically, we estimated via two-stage least squares models of the form:

$$\begin{aligned} Y_{ic} &= \theta T_{ic} + d_c + X^{'}_{ic}\beta + \varepsilon_{ic} \\ T_{ic} &= \alpha Z_{ic} + d_c + X^{'}_{ic}\varphi + \varepsilon_{ic} \end{aligned}$$

where T_{ic} is a binary indicator of whether the student attended any days of the summer program.[26] Now, the parameter of interest is θ, the coefficient on T_{ic} in the outcome equation. This parameter captured the average effect of the intervention for the subgroup of students who participated in the program (Angrist, Imbens, and Rubin, 1996; Bloom, 2006). P values were derived using the wild cluster bootstrap-t procedure proposed by Cameron, Gelbach, and Miller (2008) for situations where the number of clusters is small. In this application, clusters were determined by summer site. For students in the control group, we used the summer site that most treated students in a student's regular-year school attended.

Multiple Hypotheses Testing

When performing multiple hypothesis tests on a data set, the chance of erroneously finding statistically significant results (i.e., Type I errors) increases as the number of tests increase. As a consequence, we adopted a standard corrective measure, which was to apply more-stringent

---

[24] As an additional robustness check, we calculated ITT using the approaches outlined in Jo, Asparouhov, and Muthen (2008). The results of these analyses were also substantively the same as our preferred model.

[25] Technically, given the fact that there are both "no-shows" and "crossovers", these TOT estimates actually represent complier average causal effects or local average treatment effects. See Bloom and Weiland (2014) for details.

[26] For fall 2014 and spring 2015 analyses, T_{ic} is a binary indicator of whether the student attended any days of the summer program in either year.

criteria for determining statistical significance. In practice, this translated into lowering the critical *p* value for determining statistical significance to something less than 0.05. Exactly how much lower than 0.05 was determined by the number of tested hypotheses that pertain to a given domain of outcomes and the specific correction method used.

The exact methods and decisions to make when employing these corrections are a matter of debate among statisticians. After reviewing the spectrum of most conservative to most liberal options, we adopted a middle-ground position that adheres to the most detailed guidance that IES has released on this topic (Schochet, 2008).

Consistent with WWC standards, we used the Benjamini-Hochberg method of controlling the false discovery rate (Benjamini and Hochberg, 1995). Following Schochet's (2008) guidance, we defined seven student outcome domains pertinent to the summer learning demonstration:

1. mathematics outcomes
2. reading outcomes
3. end of course mathematics grades
4. end of course LA grades
5. social-emotional outcomes
6. school-year behavior (e.g., suspensions)
7. school-year attendance.

Only those hypotheses that belonged to a confirmatory category of an outcome domain were subject to corrections for multiple hypotheses testing within that domain. Exploratory hypotheses were not subject to multiple hypotheses corrections. For domain-specific confirmatory analyses in each district summer learning demonstration report, we adjusted downward the critical *p* value for determining statistical significance according to the number of total hypotheses tests belonging to that domain for the period from 2013 to 2017. Table E.1 enumerates the confirmatory hypotheses tests conducted in each domain at each time point.

Table E.1. Number of Hypothesis Test for Each Outcome Domain for Each Study Time Point

	Year					
Domain	**2013**	**2014**	**2015**	**2016**	**2017**	**Total**
Mathematics assessments	15	30	9	0	9	**63**
Reading assessments	15	30	9	0	9	**63**
End-of-course mathematics grades	0	7	7	0	7	**21**
End-of-course LA grades	0	7	7	0	7	**21**
Social-emotional outcomes	15	15	0	0	9	**39**
School-year behavior	0	7	7	0	7	**21**
School-year attendance	0	7	7	0	7	**21**

Meta-Analysis

While the reading, mathematics, and social-emotional outcomes are collected using the same instruments in all districts in fall 2013 and in fall 2014, the spring outcomes are not uniformly captured across districts. For example, different states administer different standardized assessments, and districts use different scales for course grades, and track suspensions differently. In order to obtain ITT and TOT estimates for outcomes that use disparate measures across districts, we use a meta-analytic approach. Meta-analysis pools results from individual studies to obtain a summary estimate of effects (Glass, 1976; Nordmann, Kasenda, and Briel, 2012). In this case, we treat each district as a separate study, and then obtain an overall ITT or TOT estimate by synthesizing district-specific estimates. Because meta-analysis uses effect sizes to describe outcome variables, the outcomes are on a common metric, and are not sensitive to differences in assessments or other outcomes across sites (e.g., Yin, Schmidt, and Besag, 2006).

The current study applies a fixed-effect model to meta-analyze the data across the five study districts. First, a treatment effect is estimated in each district, along with a standard error for that effect. Then, the following model is employed:

$$T_i = \mu + \varepsilon_i$$

where T_i is the observed treatment effect (ITT or TOT estimate) in district i, μ is the overall effect, and ε_i is within-district error. An estimate of the overall treatment effect (across k districts) is given by:

$$\bar{T}. = \frac{\sum_{i=1}^{k} \frac{1}{v_i} T_i}{\sum_{i=1}^{k} \frac{1}{v_i}}$$

where v_i describes the within-district variance. Greater within-district variance means that there is more measurement error and, as such, effect estimates are less precise. $\bar{T}.$ is essentially a precision-weighted average of a treatment effect across the k districts. Districts with less measurement error are given more weight in the calculation of this average than districts with more measurement error (Borenstein, Rothstein, and Hedges, 2007).

We also employed techniques from meta-analysis (Hedges, 1982; Rosenthal and Rubin, 1982) to formally investigate heterogeneity in the magnitude of district specific effects.

We estimate these models in R using the “rmeta” software package (Lumley, 2012).

Linear Probability Models

Student suspension data analysis involved a binary outcome (i.e., 1 if suspended once or more during SY 2013–2014, 0 otherwise). We opted to analyze suspension data using a linear probability model (LPM), which substitutes a binary outcome into the existing random effects modeling framework outlined above.

Some researchers recommend using nonlinear response models (e.g., generalized linear models with logistic or probit link functions) with binary outcomes, and note that LPMs can result in predicted probabilities that are out of range or have inappropriate standard errors (Mood, 2010). However, under general conditions (and when the true data-generating mechanism is unknown),[27] the linear probability model may be a better choice than a logit model, particularly if the coefficients of the linear probability model are interpreted as average marginal effects on probabilities (Mood, 2010; Pischke, 2012). Additionally, omitted variables in logistic regression can influence parameter estimation even when variables are not correlated with variables already included in the model (Mood, 2010), and adding covariates into a logit model can actually decrease precision and increase standard errors (Robinson and Jewell, 1991).[28]

---

[27] The data-generating mechanism describes the underlying process by which observed data are believed to have originated. An important aspect of statistical inference is selecting a model that is compatible with the generating mechanism, what is sometimes called the selection problem (Spanos, 2006). With real-world observed data (as opposed to simulated data) that true process is often unknown.

[28] A secondary advantage of LPM in this situation is that the models for binary outcomes will have the same specification as our preferred model for continuous outcomes—that is, a complex hierarchical model with clustering only in the experimental arm and which allows variance of the residual error to vary across the patterns of missing data in pretest covariates. It likely would have been difficult or impossible to retain these features in a logistic regression framework.

Appendix F: Results from Regression Models

In this appendix, we report in tabular format the results narratively described in Chapters Four and Five of the main report. As described in Appendix E, all models included a rich set of covariates; however, here, we only report the regression coefficients of interest.

Table F.1. ITT Results, Overall

Analyses	Fall 2013 Estimate (SE)	Spring 2014 Estimate (SE)	Fall 2014 Estimate (SE)	Spring 2015 Estimate (SE)
Mathematics Analyses				
Achievement	0.08* (0.02)	0.04 (0.02)	0.03 (0.02)	0.04 (0.02)
End-of-year grades		0.01 (0.02)		0.00 (0.02)
Reading Analyses				
Achievement	0.00 (0.02)	0.01 (0.02)	0.04 (0.02)	0.03 (0.02)
End-of-year grades		0.01 (0.01)		0.00 (0.02)
Social-Emotional and Behavioral Analyses				
Social-emotional assessment	0.02 (0.02)		0.05 (0.03)	
Suspensions		0.00 (0.01)		–0.01 (0.01)
Attendance		0.00 (0.00)		0.00 (0.00)

* Indicates statistical significance at the $p < 0.05$ level after applying the Benjamini-Hochberg correction for multiple hypothesis tests.

Table F.2. Counts of Students Participating in ITT Analyses

	Fall 2013		Spring 2014		Fall 2014		Spring 2015	
Analysis	**Control**	**Treat**	**Control**	**Treat**	**Control**	**Treat**	**Control**	**Treat**
Mathematics Analyses								
Achievement	2,205	2,921	2,218	2,920	1,952	2,553	1,870	2,431
End-of-year grades			2,173	2,889			1,931	2,527
Reading Analyses								
Achievement	2,196	2,902	2,215	2,915	1,940	2,544	1,885	2,452
End-of-year grades			2,179	2,886			1,930	2,534
Social-Emotional and Behavioral Analyses								
Social-emotional assessment	1,903	2,542			1,579	2,144		
Suspensions			2,308	3,023			2,067	2,715
Attendance			2,306	3,023			2,067	2,715

Table F.3. Counts of Students Participating in Subgroup Analyses (Fall Analyses)

		Mathematics		Reading		Social-Emotional	
Subgroups		*N* Control	*N* Treat	*N* Control	*N* Treat	*N* Control	*N* Treat
Fall 2013							
ELL	No	1,447	2,057	1,435	2,045	1,268	1,810
	Yes	758	864	761	857	635	732
FRPL-eligible (excl. Boston)	No	182	258	179	255	162	233
	Yes	1,677	2,139	1,670	2,120	1,466	1,898
Below median on prior achievement	No	1,100	1,460	1,092	1,459	946	1,256
	Yes	1,105	1,461	1,104	1,443	957	1,286
Fall 2014							
ELL	No	1,255	1,768	1,251	1,772	1,044	1,507
	Yes	697	785	689	772	535	637
FRPL-eligible (excl. Boston)	No	182	258	179	255	162	233
	Yes	1,677	2,139	1,670	2,120	1,466	1,898
Level 1 in either subject	No	1,134	1,495	1,129	1,499	917	1,245
	Yes	818	1,058	811	1,045	662	899
Spring 2014							
ELL	No	1,455	2,064	1,455	2,062		
	Yes	763	856	760	853		
FRPL-eligible (excl. Boston)	No	185	257	184	256		
	Yes	1,691	2,137	1,691	2,135		
Level 1 in either subject	No	1,301	1,723	1,297	1,718		
	Yes	917	1,197	918	1,197		
Spring 2015							
ELL	No	1,183	1,666	1,198	1,688		
	Yes	687	765	687	764		
Level 1 in either subject	No	1,104	1,473	1,110	1,480		
	Yes	766	958	775	972		

NOTE: To calculate students' prior achievement, we used the control group's performance on the common GRADE/GMADE posttests to scale each district's pretests. The posttests were scaled to have a mean of zero, a standard deviation of one, then we calculated the mean and standard deviation of the pretests for the control group subsample in each district, excluding crossovers. Then, each district's pretests for the whole sample were scaled to have the district's calculated means and standard deviations.

Table F.4. Results of Subgroup Analyses

Subgroup	Fall 2013 Estimate (SE)	Spring 2014 Estimate (SE)	Fall 2014 Estimate (SE)	Spring 2015 Estimate (SE)
Mathematics Achievement				
ELL	0.00 (0.04)	–0.05 (0.05)	0.01 (0.04)	0.02 (0.05)
FRPL-eligible (excl. Boston)	0.04 (0.06)	0.03 (0.06)	–0.02 (0.07)	
Below median on prior achievement	–0.05 (0.03)			
Level 1 in mathematics		0.02 (0.04)	–0.02 (0.04)	0.00 (0.05)
Reading Achievement				
ELL	–0.01 (0.04)	–0.06 (0.05)	–0.01 (0.04)	0.09 (0.06)
FRPL-eligible (excl. Boston)	0.05 (0.05)	0.07 (0.07)	–0.02 (0.07)	
Below median on prior achievement	0.02 (0.03)			
Level 1 in reading		–0.01 (0.05)	0.02 (0.04)	–0.03 (0.04)
	Social-Emotional Assessment			
ELL	0.02 (0.05)		–0.04 (0.06)	
FRPL-eligible (excl. Boston)	0.05 (0.09)		0.11 (0.11)	
Below median on prior achievement	0.11 (0.05)			
Level 1 in either subject			–0.07 (0.06)	

Table F.5. Results of Subscale Analyses

Subgroup	Fall 2013 Estimate (SE)	Fall 2014 Estimate (SE)
Mathematics		
Math operations subscale	0.10* (0.02)	0.03 (0.02)
Math concepts subscale	0.08* (0.02)	0.04 (0.02)
Math process subscale	0.04 (0.02)	0.01 (0.02)
Reading		
Reading comprehension subscale	0.01 (0.02)	0.03 (0.02)
Reading vocabulary subscale	0.01 (0.02)	0.06 (0.02)
Reading listening subscale	–0.04 (0.03)	0.02 (0.04)
Social-Emotional		
Self-regulation	0.01 (0.03)	0.04 (0.03)
Self-motivation	0.03 (0.02)	0.07 (0.03)

* Indicates statistical significance at the $p < 0.05$ level after applying the Benjamini-Hochberg correction for multiple hypothesis tests.

Table F.6. Results of Treatment-on-the-Treated Analyses

Analyses	Fall 2013 Estimate (SE)	Spring 2014 Estimate (SE)	Fall 2014 Estimate (SE)	Spring 2015 Estimate (SE)
Mathematics Analyses				
Achievement	0.11* (0.03)	0.06 (0.02)	0.04 (0.03)	0.04 (0.02)
End-of-Year Grades		0.01 (0.01)		–0.01 (0.02)
Reading Analyses				
Achievement	0.02 (0.02)	0.01 (0.02)	0.05 (0.02)	0.03 (0.02)
End-of-Year Grades		0.01 (0.01)		0.00 (0.01)
Social-Emotional and Behavioral Analyses				
Social-Emotional Assessment	0.02 (0.02)		0.07 (0.04)	
Suspensions		–0.79% (0.01)		–1.21% (0.01)
Attendance		0.00% (0.00)		0.00% (0.00)

* Indicates statistical significance at the $p < 0.05$ level after applying the Benjamini-Hochberg correction for multiple hypothesis tests.

Table F.7. Nonexperimental Linear Effect of Attendance and Academic Time on Task

Characteristic	Fall 2013 Estimate (SE)	Fall 2014 Estimate (SE)
Mathematics		
Attendance	0.04* (0.02)	0.03 (0.02)
Academic time on task (instructional hours)	0.04* (0.01)	0.04 (0.02)
Reading		
Attendance	0.02 (0.02)	0.02 (0.02)
Academic time on task (instructional hours)	0.02 (0.01)	0.03 (0.02)
Social-Emotional		
Attendance	0.02 (0.03)	0.04 (0.03)

* Indicates statistical significance at the $p < 0.05$ level.

Table F.8a. Nonexperimental Effect of Attendance Categories

Characteristic	Fall 2013 Estimate (SE)	Spring 2014 Estimate (SE)	Fall 2014 Estimate (SE)	Spring 2015 Estimate (SE)
Mathematics Achievement				
No-show	0.00 (0.03)	0.00 (0.03)	–0.01 (0.02)	0.01 (0.03)
Low	0.07* (0.02)	0.02 (0.03)	0.02 (0.03)	–0.01 (0.04)
High	0.13* (0.02)	0.07* (0.02)	0.11* (0.03)	0.14* (0.03)
Attended summer 2013 only			–0.01 (0.03)	0.00 (0.03)
Attended summer 2014			0.08* (0.02)	0.08* (0.03)
Attended summer 2014 only			–0.03 (0.05)	0.05 (0.05)
Attended both summers			0.09* (0.03)	0.08* (0.03)
High attendance both summers			0.10* (0.03)	0.12* (0.03)
Students who attended Year 1 only	0.08* (0.03)		–0.01 (0.03)	

* Indicates statistical significance at the $p < 0.05$ level.

Table F.8a—Continued

Characteristic	Fall 2013 Estimate (SE)	Spring 2014 Estimate (SE)	Fall 2014 Estimate (SE)	Spring 2015 Estimate (SE)
Reading Achievement				
No-show	–0.03 (0.03)	0.02 (0.03)	0.01 (0.02)	0.01 (0.03)
Low	–0.01 (0.02)	–0.05 (0.03)	0.05 (0.03)	0.03 (0.04)
High	0.02 (0.02)	0.03 (0.02)	0.08* (0.03)	0.09* (0.03)
Attended summer 2013 only			–0.01 (0.03)	–0.01 (0.03)
Attended summer 2014			0.07* (0.02)	0.07* (0.03)
Attended summer 2014 only			0.01 (0.05)	0.07 (0.06)
Attended both summers			0.08* (0.02)	0.07* (0.03)
High attendance both summers			0.10* (0.03)	0.10* (0.03)
Students who attended Year 1 only	0.01 (0.02)		–0.02 (0.03)	
Social-Emotional Assessment				
No-show	0.05 (0.04)		0.04 (0.04)	
Low	–0.02 (0.04)		–0.02 (0.05)	
High	0.02 (0.03)		0.12* (0.04)	
Attended summer 2013 only			0.02 (0.04)	
Attended summer 2014			0.07 (0.04)	
Attended summer 2014 only			0.13 (0.08)	
Attended both summers			0.06 (0.04)	
High attendance both summers			0.14* (0.04)	
Students who attended Year 1 only	0.03 (0.04)		0.04 (0.05)	
Mathematics Grades				
No-show		–0.01 (0.02)		0.00 (0.02)
Low		0.02 (0.02)		–0.03 (0.03)
High		0.02 (0.02)		0.03 (0.03)
Attended summer 2013 only				–0.02 (0.03)
Attended summer 2014				0.01 (0.02)
Attended summer 2014 only				–0.03 (0.05)
Attended both summers				0.01 (0.03)
High attendance both summers				0.04 (0.03)
LA Grades				
No-show		0.00 (0.02)		0.01 (0.02)
Low		0.01 (0.02)		0.00 (0.03)
High		0.01 (0.02)		–0.01 (0.02)
Attended summer 2013 only				–0.01 (0.02)
Attended summer 2014				–0.01 (0.02)
Attended summer 2014 only				0.01 (0.04)
Attended both summers				–0.01 (0.02)
High attendance both summers				0.01 (0.03)
Suspensions				
No-show		0.1% (0.01)		–1.2% (0.01)
Low		–0.3% (0.01)		–0.1% (0.01)
High		–0.1% (0.01)		–1.5% (0.01)
Attended summer 2013 only				–1.0% (0.01)
Attended summer 2014				–1.3% (0.01)
Attended summer 2014 only				–0.7% (0.02)
Attended both summers				–1.3% (0.01)
High attendance both summers				–2.1% (0.01)

* Indicates statistical significance at the $p < 0.05$ level.

Table F.8b. Nonexperimental Effect of Academic Time on Task Categories

Characteristic	Fall 2013 Estimate (SE)	Spring 2014 Estimate (SE)	Fall 2014 Estimate (SE)	Spring 2015 Estimate (SE)
Mathematics Achievement				
No-show	0.00 (0.03)	0.00 (0.03)	–0.01 (0.02)	0.01 (0.03)
Low (< 25.5 instructional hours)	0.08* (0.02)	0.04 (0.02)	0.03 (0.03)	0.06 (0.03)
High (> = 25.5 instructional hours)	0.16* (0.03)	0.05 (0.03)	0.13* (0.03)	0.11* (0.03)
High academic time on task both summers			0.13* (0.04)	0.09* (0.04)
Reading Achievement				
No-show	–0.03 (0.03)	0.02 (0.03)	0.01 (0.02)	0.01 (0.03)
Low (< 34 instructional hours)	–0.01 (0.02)	–0.01 (0.02)	0.06* (0.03)	0.05 (0.03)
High (> = 34 instructional hours)	0.05* (0.02)	0.03 (0.03)	0.09* (0.03)	0.13* (0.04)
High academic time on task both summers			0.07 (0.04)	0.12* (0.05)
Mathematics Grades				
No-show		–0.01 (0.02)		0.00 (0.02)
Low (< 25.5 instructional hours)		0.02 (0.02)		–0.01 (0.03)
High (> = 25.5 instructional hours)		0.02 (0.02)		0.03 (0.03)
ELA Grades				
No-show		0.00 (0.02)		0.01 (0.02)
Low (< 34 instructional hours)		0.01 (0.02)		–0.01 (0.03)
High (> = 34 instructional hours)		0.02 (0.02)		–0.01 (0.03)

* Indicates statistical significance at the *p* < 0.05 level.

Table F.9. Nonexperimental Effects of Camp Attendance According to Student Survey

Characteristic	Fall 2013 Estimate (SE)	Fall 2014 Estimate (SE)
Mathematics		
Attended camp w/mathematics > = a few weeks, control	0.02 (0.03)	–0.12* (0.05)
Did not attend camp w/mathematics ≥ a few weeks, treatment	0.00 (0.02)	0.03 (0.02)
Attended camp w/mathematics ≥ a few weeks, treatment	0.14* (0.02)	–0.12 (0.07)
Reading		
Attended camp w/reading ≥ a few weeks, control	0.01 (0.03)	0.04 (0.06)
Did not attend camp w/reading ≥ a few weeks, treatment	–0.04* (0.02)	0.04 (0.02)
Attended camp w/reading ≥ a few weeks, treatment	0.04* (0.02)	0.02 (0.07)
Social-Emotional		
Attended camp ≥ a few weeks, control	–0.06 (0.04)	0.06 (0.05)
Did not attend camp ≥ a few weeks, treatment	0.01 (0.04)	0.09 (0.05)
Attended camp ≥ a few weeks, treatment	–0.01 (0.03)	0.12* (0.06)

* Indicates statistical significance at the *p* < 0.05 level.

Table F.10. Nonexperimental Estimates of the Effect of Relative Opportunity for Instruction

Characteristic	Fall 2013 Estimate (SE)	Fall 2014 Estimate (SE)
Mathematics		
Relative opportunity for instruction	0.01 (0.02)	0.04 (0.03)
Reading		
Relative opportunity for instruction	0.00 (0.02)	0.01 (0.02)

Table F.11. Nonexperimental Estimates of Moderation

Characteristic	Fall 2013 Estimate (SE)	Spring 2014 Estimate (SE)	Fall 2014 Estimate (SE)	Spring 2015 Estimate (SE)
Mathematics				
Mathematics classroom instructional quality scale	0.00 (0.01)	0.01 (0.01)	0.02 (0.02)	–0.02 (0.02)
Positive school climate	0.01 (0.02)		0.02 (0.02)	
Daily climate scale			0.01 (0.02)	
Good fit mathematics curriculum	0.00 (0.02)		0.03 (0.04)	
Student discipline and order	0.00 (0.02)	–0.03 (0.03)	–0.02 (0.02)	0.04 (0.03)
Mathematics teacher taught subject in adjacent grade prior school year	0.03 (0.03)	0.03 (0.03)	–0.03 (0.04)	–0.04 (0.05)
Reading				
LA classroom instructional quality scale	0.05* (0.01)	0.01 (0.02)	0.01 (0.02)	0.02 (0.02)
Positive school climate	–0.01 (0.01)		0.00 (0.02)	
Daily climate scale			0.00 (0.02)	
Good fit LA curriculum	0.00 (0.01)		–0.01 (0.04)	
Student discipline and order	0.04* (0.02)	0.04 (0.03)	0.00 (0.02)	–0.02 (0.03)
LA teacher taught subject in adjacent grade prior school year	0.09* (0.03)	0.04 (0.03)	0.00 (0.04)	–0.02 (0.05)
Social-Emotional				
Positive school climate	0.00 (0.02)		0.01 (0.03)	
Daily climate scale			–0.04 (0.03)	
Student discipline and order	0.04 (0.02)		0.04 (0.03)	
Mathematics Grades				
Mathematics classroom instructional quality scale		0.01 (0.01)		–0.03 (0.02)
Student discipline and order		0.00 (0.02)		0.01 (0.03)
Mathematics teacher taught subject in adjacent grade prior school year		–0.02 (0.03)		–0.05 (0.05)

Table F.11—Continued

Characteristic	Fall 2013 Estimate (SE)	Spring 2014 Estimate (SE)	Fall 2014 Estimate (SE)	Spring 2015 Estimate (SE)
LA Grades				
LA classroom instructional quality scale		0.02 (0.01)		0.00 (0.02)
Student discipline and order		–0.01 (0.02)		0.01 (0.02)
LA teacher taught subject in adjacent grade prior school year		0.04 (0.03)		0.02 (0.04)
Suspensions				
Positive school climate		–1.6% (0.01)		–0.7% (0.01)
Student discipline and order		0.7% (0.01)		–0.9% (0.01)
Attendance				
High instructional quality in LA or mathematics		–0.1% (0.00)		–0.1% (0.00)
Positive school climate		0.00% (0.00)		–0.2%(0.00)
Student discipline and order		0.1% (0.00)		–0.3% (0.00)

References

Alexander, Karl L., Doris R. Entwisle, and Linda Steffel Olson, "Lasting Consequences of the Summer Learning Gap," *American Sociological Review*, Vol. 72, No. 2, 2007, pp. 167–180.

Allington, Richard L., Anne McGill-Franzen, Gregory Camilli, Lunetta Williams, Jennifer Graff, Jacqueline Zeig, Courtney Zmach, and Rhonda Nowak, "Addressing Summer Reading Setback Among Economically Disadvantaged Elementary Students," *Reading Psychology*, Vol. 31, No. 5, October 2010, pp. 411–427.

Angrist, Joshua D., Guido W. Imbens, and Donald B. Rubin, "Identification of Causal Effects Using Instrumental Variables," *Journal of the American Statistical Association,* Vol. 91, No. 434, 1996, pp. 444–455.

Arbreton, Amy J. A., Jean Baldwin Grossman, Carla Herrera, and Leigh L. Linden, *Testing the Impact of Higher Achievement's Year-Round Out-of-School-Time Program on Academic Outcomes*, New York: Public/Private Ventures, 2011. As of March 21, 2016: http://ppv.issuelab.org/resource/testing_the_impact_of_higher_achievements_year_round_out_of_school_time_program_on_academic_outcomes

Atteberry, Allison, Andrew McEachin, and Aryn Bloodworth, "School's Out: Summer Learning Loss Across Grade Levels and School Contexts in the U.S. Today," in K. Alexander, M. Boulay, and S. Pitcock, eds., *Summer Learning and Summer Learning Loss: Theory, Research and Practice*, New York: Teachers College Press, forthcoming.

Augustine, Catherine H., Jennifer Sloan McCombs, Heather L. Schwartz, and Laura Zakaras, *Getting to Work on Summer Learning: Recommended Practices for Success*, Santa Monica, Calif.: RAND Corporation, RR-366-WF, 2013. As of March 18, 2016: http://www.rand.org/pubs/research_reports/RR366.html

Benjamini, Yoav, and Yosef Hochberg, "Controlling the False Discovery Rate: A Practical and Powerful Approach to Multiple Testing," *Journal of the Royal Statistical Society, Series B (Methodological)*, Vol. 57, No. 1, 1995, pp. 289–300.

Benson, James, and Geoffrey Borman, "Family, Neighborhood, and School Settings Across Seasons: When Do Socioeconomic Context and Racial Composition Matter for the Reading Achievement Growth of Young Children? *Teachers College Record*, Vol. 112, No. 5, 2010, pp. 1338–1390.

Bloom, Howard S., *The Core Analytics of Randomized Experiments for Social Research MDRC Working Papers on Research Methodology*, MDRC Working Papers on Research Methodology, New York: MDRC, 2006. As of October 22, 2014: http://www.mdrc.org/sites/default/files/full_533.pdf

Bloom, Howard, and Christina Weiland, *Quantifying Variation in Head Start Effects on Young Children's Cognitive and Socio-Emotional Skills Using Data from the National Head Start Impact Study*, New York: MRDC, 2014. As of April 13, 2016: http://www.mdrc.org/sites/default/files/quantifying_variation_in_head_start.pdf

Borenstein, Michael, Larry Hedges, and Hannah Rothstein, *Meta-Analysis: Fixed Effect vs. Random Effects*, Meta-Analysis.com, 2007.

Borman, Geoffrey D., James Benson, and Laura T. Overman, "Families, Schools, and Summer Learning," *Elementary School Journal*, Vol. 106, 2005, pp. 131–150.

Borman, Geoffrey D., and N. Maritza Dowling, "Longitudinal Achievement Effects of Multiyear Summer School: Evidence from the Teach Baltimore Randomized Field Trial," *Educational Evaluation and Policy Analysis*, Vol. 28, No. 1, 2006, pp. 25–48.

Borman, Geoffrey D., Michael Goetz, and N. Maritza Dowling, "Halting the Summer Achievement Slide: A Randomized Field Trial of the KindergARTen Summer Camp," *Journal of Education for Students Placed at Risk* (JESPAR), Vol. 14, No. 2, April 2009, pp. 133–147.

Burkham, David T., Douglas D. Ready, Valerie E. Lee, and Laura F. LoGerfo, "Social-Class Differences in Summer Learning Between Kindergarten and First Grade: Model Specification and Estimation," *Sociology of Education*, Vol. 77, No. 1, 2004, pp. 1–31.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, "Bootstrap-Based Improvements for Inference with Clustered Errors," *Review of Economics and Statistics*, Vol. 90, No. 3, 2008, pp. 414–427.

Chaplin, Duncan, and Jeffrey Capizzano, *Impacts of a Summer Learning Program: A Random Assignment Study of Building Educated Leaders for Life* (BELL), Washington, D.C.: Urban Institute, 2006.

Coleman, J. S., E. Q. Campbell, C. J. Hobson, J. McPartland, A. M. Mood, F. D. Weinfeld, et al., *Equality of Educational Opportunity*. Washington, D.C.: U.S. Government Printing Office, 1966.

Cooper, Harris, Kelly Charlton, Jeff C. Valentine, Laura Muhlenbruck, and Geoffrey D. Borman, *Making the Most of Summer School: A Meta-Analytic and Narrative Review*, Vol. 65, Monographs of the Society for Research in Child Development, Malden, Mass.: Blackwell Publishers, 2000.

Cooper, Harris, Barbara Nye, Kelly Charlton, James Lindsay, and Scott Greathouse, "The Effects of Summer Vacation on Achievement Test Scores: A Narrative and Meta-Analytic Review," *Review of Educational Research*, Vol. 66, No. 3, 1996, pp. 227–268.

Danielson Group, "Promoting Effective Teaching and Professional Learning," website, 2013. As of March 23, 2016:
http://www.danielsongroup.org/

Downey, Douglas B., Paul T. Von Hippel, and Beckett A. Broh, "Are Schools the Great Equalizer? Cognitive Inequality During the Summer Months and the School Year," *American Sociological Review*, Vol. 69, No. 5, 2004, pp. 613–635.

Duflo, Esther, Rachel Glennerster, and Michael Kremer, "Using Randomization in Development Economics: A Toolkit," in T. P. Schults and J. A. Strauss, eds., *Handbook of Development Economics*, Vol. 4, Amsterdam: North-Holland, Elsevier B.V., 2007, pp. 3862–3895.

Eicker, Friedhelm, "Limit Theorems for Regression with Unequal and Dependent Errors," *Proceedings of the Fifth Berkeley Symposium on Mathematical Statistics and Probability*, Vol. 1, Berkeley, Calif.: University of California Press, 1967, pp. 59–82.

Entwisle, Doris R., and Karl L. Alexander, "Beginning School Math Competence: Minority and Majority Comparisons," *Child Development*, Vol. 61, No. 2, 1990, pp. 454–471.

———, "Summer Setback: Race, Poverty, School Composition, and Mathematics Achievement in the First Two Years of School," *American Sociological Review*, Vol. 57, No. 1, 1992, pp. 72–84.

Entwisle, Doris R., Karl L. Alexander, and Linda Steffel Olson, "Keep the Faucet Flowing: Summer Learning and Home Environment," *American Educator*, Vol. 25, No. 3, Fall 2001, pp. 10–15, 47.

Fisher, Charles W., David C. Berliner, Nikola N. Filby, Richard Marliave, Leonard S. Cahen, and Marilyn M. Dishaw, "Teaching Behaviors, Academic Learning Time, and Student Achievement: An Overview," in Carolyn Denham and Ann Lieberman, eds., *Time to Learn: A Review of the Beginning Teacher Evaluation Study*, Sacramento, Calif.: California State Commission for Teacher Preparation and Licensing, 1980, pp. 7–32.

Gershenson, Seth, "Do Summer Time-Use Gaps Vary by Socioeconomic Status?" *American Educational Research Journal*, Vol. 50, No. 6, 2013, pp. 1219–1248.

Gershenson, Seth, Alison Jacknowitz, and Andrew Brannegan, "Are Student Absences Worth the Worry in U.S. Primary Schools?" Bonn, Germany: Institute for the Study of Labor (IZA), IZA DP No. 9558, 2015. As of March 21, 2016:
http://ftp.iza.org/dp9558.pdf

Glass, Gene V., "Primary, Secondary, and Meta-Analysis of Research," *Educational Researcher*, Vol. 5, No. 10, 1976, pp. 3–8.

Gottfried, Michael, "Excused Versus Unexcused: How Student Absences in Elementary School in Elementary School Affect Academic Achievement," *Educational Evaluation and Policy Analysis*, Vol. 31, No. 4, 2009, pp. 392–419.

———, "The Detrimental Effects of Missing School: Evidence from Urban Siblings," *American Journal of Education*, Vol. 117, No. 2, 2011, pp. 147–182.

Guryan, Johnathan, James S. Kim, and Kyung Park, *Motivation and Incentives in Education: Evidence from a Summer Reading Experiment*, Cambridge, Mass.: National Bureau of Economic Research, NBER Working Paper 20918, 2015. As of March 21, 2016: http://www.nber.org/papers/w20918

Harnischfeger, Annegret, and David E. Wiley, "The Teaching-Learning Process in Elementary Schools: A Synoptic View," *Curriculum Inquiry*, Vol. 6, No. 1, 1976, pp. 5–43.

Hawley, Willis D., Susan Rosenholtz, Henry J. Goodstein, and Ted Hasselbring, "Good Schools: What Research Says About Improving Student Achievement," *Peabody Journal of Education*, Vol. 61, No. 4, 1984, pp. iii–178.

Hayes, Michael S., and Seth Gershenson, "The Estimation of Summer Learning Rates," paper presented at the 2014 AEFP Annual Conference, San Antonio, Tex., 2014. As of April 18, 2016: http://nebula.wsimg.com/328860133db06439c33f2f6179c92ae3?AccessKeyId=33C759F2990E6F78DB85&disposition=0&alloworigin=12014

Hedges, Larry V., "Estimation of Effect Size from a Series of Independent Experiments," *Psychological Bulletin*, Vol. 92, No. 2, 1982, pp. 490–499.

Heyns, Barbara, *Summer Learning and the Effects of Schooling*, New York: Academic Press, 1979.

Huber, Peter J., "The Behavior of Maximum Likelihood Estimates Under Nonstandard Conditions," *Proceedings of the Fifth Berkeley Symposium on Mathematical Statistics and Probability*, Vol. 1, Berkeley, Calif.: University of California Press, 1967, pp. 221–233.

Imbens, Guido W., *Experimental Design for Unit and Cluster Randomized Trials*, Cambridge, Mass.: Harvard University, Department of Economics, 2011.

Jacob, Brian A., and Lars Lefgren, "Remedial Education and Student Achievement: A Regression-Discontinuity Design," *Review of Economics and Statistics*, Vol. 86, No. 1, 2004, pp. 226–244.

Jo, Booil, Tihomir Asparouhov, and Bengt O. Muthén, "Intention-to-Treat Analysis in Cluster Randomized Trials with Noncompliance," *Statistics in Medicine*, Vol. 27, No. 27, 2008, pp. 5565–5577.

Karweit, Nancy, "Should We Lengthen the School Year?" *Educational Researcher*, Vol. 14, No. 6, 1985, pp. 9–15.

Karweit, Nancy, and Robert E. Slavin, "Time-on-Task: Issues of Timing, Sampling, and Definition," *Journal of Education Psychology*, Vol. 74, No. 6, 1982, pp. 844–851.

Kim, James S., "Summer Reading and the Ethnic Achievement Gap, *Journal of Education for Students Placed at Risk*, Vol. 9, No. 2, 2004, pp. 169–188.

———, "Effects of a Voluntary Summer Reading Intervention on Reading Achievement: Results From a Randomized Field Trial," *Educational Evaluation and Policy Analysis*, Vol. 28, No. 4, 2006, pp. 335–355.

Kim, James S., and Jonathan Guryan, "The Efficacy of a Voluntary Summer Book Reading Intervention for Low-Income Latino Children from Language Minority Families," *Journal of Educational Psychology*, Vol. 102, No. 1, 2010, pp. 20–31.

Kim, James S., and David M. Quinn, "The Effects of Summer Reading on Low-Income Children's Literacy Achievement from Kindergarten to Grade 8: A Meta-Analysis of Classroom and Home Interventions," *Review of Educational Research,* Vol. 83, No. 3, 2013, pp. 386–431.

Kim, James S., and Thomas G. White, "Scaffolding Voluntary Summer Reading for Children in Grades 3 to 5: An Experimental Study," *Scientific Studies of Reading*, Vol. 12, No. 1, 2008, pp. 1–23.

LeBuffe, Paul, Valerie Shapiro, and Jack Naglieri, *Devereux Student Strengths Assessment (DESSA),* Villanova, Pa.: Devereux Center for Resilient Children, 2009.

Lockwood, J. R., H. Doran, and D. F. McCaffrey, "Using R for Estimating Longitudinal Student Achievement Models," *R News*, Vol. 3, No. 3, 2003, pp. 17–23.

Lohr, Sharon, Peter Z. Schochet, and Elizabeth Sanders, *Partially Nested Randomized Controlled Trials in Education Research: A Guide to Design and Analysis*, Washington, D.C.: National Center for Education Research, NCER 2014-2000, 2014.

Lomax, Richard G., and William W. Cooley, "The Student Achievement-Instructional Time Relationship," paper presented at the Annual Meeting of the American Educational Research Association, San Francisco, Calif., April 1979.

Lumley, T., “rmeta: Meta-analysis,” web page, R package version 2.16, 2012. As of April 13, 2016:
https://CRAN.R-project.org/package=rmeta

Mac Iver, Martha Abele, and Douglas J. Mac Iver, “‘STEMming’ the Swell of Absenteeism in Urban Middle Grades Schools: Impacts of a Summer Robotics Program,” paper presented at the Society for Research on Educational Effectiveness, Washington, D.C., 2014.

Marcotte, Dave E., and Steven W. Hemelt, “Unscheduled School Closings and Student Performance,” *Education Finance and Policy,* Vol. 3 No. 3, 2008, pp. 316–338.

Mariano, Louis T., and Paco Martorell, “The Academic Effects of Summer Instruction and Retention in New York City,” *Educational Evaluation and Policy Analysis*, Vol. 35, No. 1, 2013, pp. 96–117.

Matsudaira, Jordan D., “Mandatory Summer School and Student Achievement,” *Journal of Econometrics*, Vol. 142, No. 2, 2008, pp. 829–850.

McCaffrey, Daniel F., Bing Han, and J. R. Lockwood, “Using Auxiliary Teacher Data to Improve Value-Added: An Application of Small Area Estimation to Middle School Mathematics Teachers,” in Robert W. Lissitz and Hong Jiao, eds., *Value Added Modeling and Growth Modeling with Particular Application to Teacher and School Effectiveness*, Charlotte, N.C.: Information Age Publishing, 2013.

McCoach, D. Betsy, Ann A. O’Connell, Sally M. Reis, and Heather A. Levitt, “Growing Readers: A Hierarchical Linear Model of Children’s Reading Growth During the First 2 Years of School,” *Journal of Educational Psychology*, Vol. 98, No. 1, 2006, pp. 14–28.

McCombs, Jennifer Sloan, Catherine H. Augustine, Heather L. Schwartz, Susan J. Bodilly, Brian McInnis, Dahlia S. Lichter, and Amanda Brown Cross, *Making Summer Count: How Summer Programs Can Boost Children's Learning*, Santa Monica, Calif.: RAND Corporation, MG-1120-WF, 2011. As of March 18, 2016:
http://www.rand.org/pubs/monographs/MG1120.html

McCombs, Jennifer Sloan, Sheila Nataraj Kirby, Louis T. Mariano, eds., *Ending Social Promotion Without Leaving Children Behind: The Case of New York City*, Santa Monica, Calif.: RAND Corporation, MG-894-NYCDOE, 2009. As of March 21, 2016:
http://www.rand.org/pubs/monographs/MG894.html

McCombs, Jennifer Sloan, John F. Pane, Catherine H. Augustine, Heather L. Schwartz, Paco Martorell, and Laura Zakaras, *Ready for Fall? Near-Term Effects of Voluntary Summer Learning Programs on Low-Income Students’ Learning Opportunities and Outcomes*, Santa Monica, Calif.: RAND Corporation, RR-815-WF, 2014. As of March 18, 2016:
http://www.rand.org/pubs/research_reports/RR815.html

Mood, Carina, "Logistic Regression: Why We Cannot Do What We Think We Can Do, and What We Can Do About It," *European Sociological Review*, Vol. 26, No. 1, 2010, pp. 67–82.

Nordmann, Alain J., Benjamin Kasenda, and Matthias Briel, "Meta-Analyses: What They Can and Cannot Do," *Swiss Medical Weekly*, Vol. 142, 2012, p. w13518.

Pischke, Jörn-Steffen, "Probit Better than LPM?" *Mostly Harmless Econometrics*, 2012. As of April 13, 2016:
http://www.mostlyharmlesseconometrics.com/2012/07/probit-better-than-lpm/

Quinn, David M., "Black-White Summer Learning Gaps: Interpreting the Variability of Estimates Across Representations," *Educational Evaluation and Policy Analysis*, 2014.

Rambo-Hernandez, Karen E., and D. Betsy McCoach, "High-Achieving and Average Students' Reading Growth: Contrasting School and Summer Trajectories," *The Journal of Educational Research*, Vol. 108, No. 2, 2015, pp. 112–129.

Ready, Douglas D., "Socioeconomic Disadvantage, School Attendance, and Early Cognitive Development: The Differential Effects of School Exposure," *Sociology of Education*, Vol. 83, No. 4, 2010, pp. 271–286.

Reardon, Sean F., "The Widening Academic Achievement Gap Between the Rich and the Poor: New Evidence and Possible Explanations," in Greg J. Duncan and Richard J. Murnane, eds., *Whither Opportunity? Rising Inequality and the Uncertain Life Chances of Low-Income Children*, New York: Russell Sage Foundation Press, 2011.

Reardon, Sean F., Joseph P. Robinson-Cimpian, and Ericka S. Weathers, "Patterns and Trends in Racial/Ethnic and Socioeconomic Achievement Gaps," in Helen F. Ladd and Margaret E. Goertz, eds., *Handbook of Research in Education Finance and Policy*, 2nd edition, New York: Routledge, 2015, pp. 491–509.

Robinson, Laurence D., and Nicholas P Jewell, "Some Surprising Results About Covariate Adjustment in Logistic Regression Models," *International Statistical Review/Revue Internationale de Statistique*, Vol. 59, No. 2, 1991, pp. 227–240.

Roderick, Melissa, Mimi Engel, and Jenny Nagaoka, *Ending Social Promotion: Results from Summer Bridge*, Chicago, Ill.: Consortium on Chicago School Research, 2003. As of March 21, 2016:
https://consortium.uchicago.edu/publications/ending-social-promotion-results-summer-bridge

Rohrer, Doug, and Harold Pashler, "Recent Research on Human Learning Challenges Conventional Instructional Strategies," *Educational Researcher*, Vol. 39, 2010, pp. 406–412.

Rohrer, Doug, and Kelli Taylor, “The Effects of Overlearning and Distributed Practice on the Retention of Mathematics Knowledge,” *Applied Cognitive Psychology*, Vol. 20, No. 2, 2006, pp. 1209–1224.

Rosenthal, Robert, and Donald B. Rubin, “Comparing Effect Sizes of Independent Studies,” *Psychological Bulletin*, Vol. 92, No. 2, 1982, pp. 500–504.

Schacter, John, and Booil Jo, “Learning When School Is Not in Session: A Reading Summer Day-Camp Intervention to Improve the Achievement of Exiting First-Grade Students Who Are Economically Disadvantaged,” *Journal of Research in Reading*, Vol. 28, No. 2, 2005, pp. 158–169.

Schochet, Peter Z., *Technical Methods Report: Guidelines for Multiple Testing in Impact Evaluations*, Washington, D.C.: National Center for Education Evaluation and Regional Assistance, Institute of Education Sciences, U.S. Department of Education, NCEE 2008-4018, 2008.

Skinner, Ellen A., Thomas A. Kindermann, and Carrie J. Furrer, “A Motivational Perspective on Engagement and Disaffection: Conceptualization and Assessment of Children’s Behavioral and Emotional Participation in Academic Activities in the Classroom,” *Educational and Psychological Measurement*, Vol. 69, No. 3, 2009, pp. 493–525.

Snipes, Jason, Chung-Wei Huang, Karina Jaquet, and Neal Finkelstein, *The Effects of the Elevate Math Summer Program on Math Achievement and Algebra Readiness*, Washington, D.C.: U.S. Department of Education, Institute of Education Sciences, REL 2015–096, 2015. As of March 21, 2016:
http://ies.ed.gov/ncee/edlabs/regions/west/pdf/REL_2015096.pdf

Somers, Marie-Andrée, Rashida Welbeck, Jean B. Grossman, and Susan Gooden, *An Analysis of the Effects of an Academic Summer Program for Middle School Students*, New York: MDRC, 2015.

Spanos, Aris, “Where Do Statistical Models Come From? Revisiting the Problem of Specification,” *IMS Lecture Notes–Monograph Series 2nd Lehmann Symposium–Optimality*, Vol. 49, 2006, pp. 98–119.

Teachstone Training, “Classroom Assessment Scoring System (CLASS),” web page, 2016. As of March 23, 2016:
http://teachstone.com/classroom-assessment-scoring-system/

U.S. Department of Education, *What Works Clearinghouse: Procedures and Standards Handbook*, Version 3.0, Washington, D.C.: Institute of Education Sciences, 2014.

Von Hippel, Peter T., Caitlin Hamrock, and Mina Kumar, *Do Test Score Gaps Grow Before, During or Between the School Years? Measurement Artifacts and What We Can Know in Spite of Them*, Working Paper, Social Science Research Network, 2016.

Walberg, Herbert J., “Synthesis of Research on Time and Learning,” *Educational Leadership*, Vol. 45, No. 6, 1988, pp. 76–85.

White, Halbert, “A Heteroskedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroskedasticity,” *Econometrica*, Vol. 48, No. 4, 1980, pp. 817–838.

White, Thomas G., James S. Kim, Helen Chen Kingston, and Lisa Foster, “Replicating the Effects of a Teacher-Scaffolded Voluntary Summer Reading Program: The Role of Poverty,” *Reading Research Quarterly*, Vol. 49, No. 1, 2014, pp. 5–30.

Wright, Benjamin D., and Mark H. Stone, *Making Measures*, Chicago, Ill.: The Phaneron Press, 2004.

Yin, Robert K., R. James Schmidt, and Frank Besag, “Aggregating Student Achievement Trends Across States with Different Tests: Using Standardized Slopes as Effect Sizes,” *Peabody Journal of Education*, Vol. 81, No. 2, 2006, pp. 47–61.

Zvoch, Keith, and Joseph J. Stevens, “Summer School and Summer Learning: An Examination of the Short- and Longer Term Changes in Student Literacy,” *Early Education and Development,* Vol. 22, No. 4, 2011, pp. 649–675.

———, “Summer School Effects in a Randomized Field Trial,” *Early Childhood Research Quarterly,* Vol. 28, No. 1, 2013, pp. 24–32.

———, “Identification of Summer School Effects by Comparing the In- and Out-of-School Growth Rates of Struggling Early Readers,” *Elementary School Journal*, Vol. 115, No. 3, 2015, pp. 433–456.