Another group of variables accounted for differences in the amount of children's exposure to school and the kind of summer education they received. These measures included whether children attended a *full-day kindergarten* program (versus half-day), *attended pre-school*, and *repeated kindergarten*, all coded 1 for yes and 0 for no. Regarding possible differences between year-round and 9-month schools, it must be noted that the experimental treatment includes two forms of YRE: summer school and year-round school. We distinguished between these two arrangements by estimating differences in the *type of summer* instruction (1 = year-round, 0 = summer school) experimental students experienced.

In order to take advantage of the unique way both treatments inform questions of residential stratification, we used a NCES companion data file that linked ECLS-K children to characteristics of their zip-codes (Beveridge et al. 2004). While the imperfections of census data as proxies for residential areas have been noted (Jencks and Mayer 1990), they present an objective appraisal of areas that complemented the more subjective parent reports of neighborhood conditions that we used in this analysis. These variables included zip-codes' *median family income* and the *percentage minority*. The median income variable was created by first using a natural log transformation to achieve a more suitable distribution of incomes, then converting those values into z-scores. For the sake of interpretation, Table 3 reports the original values of this variable. We combined measures of the proportion of African American and Hispanic individuals to create the zip-code's *percentage minority* measure because those racial groups have the highest metropolitan segregation levels, and the largest proportion of their populations located in hyper-segregated areas (Logan, Stults and Farley 2004).

The ECLS-K provided a location type variable to identify children that resided in central cities and also asked parents their perceptions of their neighborhood, which we used to create a composite indicator of neighborhood social disorganization. The *city* variable (1 = yes, 0 = no) permitted us to account for the fact that year-round schools are present in and outside of central cities and to remain open to the ways in which city schools might differ from others. Regarding social disorganization, parents were asked: "*how much of a problem is burglary*", "*violent crime*" and "*selling/using drugs in the area*" (1 = big problem, 2 = somewhat a problem, 3 = no problem). Our *social disorganization* composite was coded 1 for yes and 0 for no if parents indicated that any of these factors were a big problem. Although it has been stated that areas with high minority compositions face more social problems (Jargowsky 1997), our diagnostics produced no concerns of multicollinearity among these neighborhood dimensions.

ESTIMATION

Using HLM version 6.08 (Raudenbush and Bryk 2002), we specified a 3-level model consisting of child test-scores at Level 1, between-child measures reflecting his or her social background and educational exposure at Level 2, and residential dimensions at Level 3. Given test-score change can happen in different periods of the year, test-score parameters are estimated separately for the 9-month school session, the summer and also for the entire calendar year, yielding the Level 1 equation:

$$Y_{tcn} = \pi_{0cn} + \pi_{1cn}(\textit{Spring kindergarten assessment}_{tcn}) + \pi_{2cn}(\textit{Fall 1}^{st}\textit{ grade assessment}_{tcn}) + \pi_{3cn}(\textit{Spring 1}^{st}\textit{ grade assessment}_{tcn}) + e$$

Where test-scores Y_{tcn} is a function of an intercept representing reading and math for child *c* in neighborhood *n*, and her or his exposure to periods that span spring kindergarten test-score to fall 1st grade score (summer), the beginning and end-score of 1st grade (9-month), and the end of kindergarten to the end of 1st grade (year-round) at the time of test *t*. Test-scores during these time-spans are estimated for children that are enrolled in year-round and 9-month schools together in an unconditional analysis.

Within each time-span, our counterfactual approach assumed that every individual has a potential outcome in both treatment conditions, even if each child can be observed in only one school treatment at any one time (Morgan and Winship 2007). We express this assumption as:

$$\delta_i = Y_i^e - Y_i^c$$

where, Y_i = reading or math outcomes and *e* and *c* indicate whether test-scores are of the experimental or control condition. The matching procedures that we described earlier addressed the fact that we can only observe child *i* in one treatment and not both, allowing us to continue with the specification of the causal effect on child *i* as an expected value of difference between Y^e and Y^c:

$$\overline{\delta} = \overline{Y}^e - \overline{Y}^c$$

The average treatment effect is then the difference between these two estimated means as indicated by an *all-year* variable (1 = yes, 0 = no).

Other questions in our analysis concern variation in treatment effects across racial/ethnic groups, social background and school dimensions. Level 2 of the multilevel model specified social background, educational exposure and residential characteristics in all three time-spans for both schooling types separately. Each Level 2 parameter represents the adjustment in the area's average performance slope, β_{10n}. Test-score growth π_{1cn} is a function of children's age, gender, and single parent family structure; whether they repeated kindergarten; attended full-day kindergarten and a preschool program; the type of summer instruction; and, their family social class quintile (with the middle quintile excluded), race and city residency. The only way in which Level 2 differed across the three periods is that the variable, summer type, was withheld from the estimation of 9-month performances since there is no school exposure for control group children in this period. The full Level 2 equation is as follows:

$$\pi_{1cn} = \beta_{10n} + \beta_{1n}(\mathit{Age}_{cn}) + \beta_{12n}(\mathit{Gender}_{cn}) + \beta_{13n}(\mathit{Single\ parent}_{cn}) + \beta_{14n}(\mathit{Repeated\ kindergarten}_{cn})$$
$$\beta_{15n}(\mathit{Full\ day\ kindergarten}_{cn}) + \beta_{16n}(\mathit{Preschool\ program}_{cn}) + \beta_{17n}(\mathit{Summer\ type}_{cn}) + \beta_{1,8\text{-}11n}(\mathit{SES\ quintiles}_{cn})$$
$$+ \beta_{1,12\text{-}15n}(\mathit{Race}_{cn}) + \beta_{116n}(\mathit{City}_{cn})\ a_{cn}$$

Recall that children in our analysis are nested within residential areas, 263 and 363 zip-codes for year-round and 9-month school children, respectively. We therefore model "neighborhood-to-neighborhood" variation in residential characteristics with random intercept models in all three periods and both school conditions. Hence, test-score change in each time-span, β_{10n} is a function of zip-codes' median family income and percentage African American and Hispanic, both segmented into equal thirds and a social disorganization composite. We express this Level 3 equation as:

$$\beta_{10n} = \gamma_{100} + \gamma_{101,2n}(\mathit{Median\ family\ income}_n) + \gamma_{103,4n}(\%\ \mathit{Minority}_n) +$$
$$\gamma_{105n}(\mathit{Social\ disorganization}_n) + r_{10n}$$

Where, the intercept γ_{100}, represents the average test performance of a specific residential area for all areas in the sample, γ_{101n} through γ_{104n} indicates the estimated deviation from the area mean test performance associated with a point increase among those characteristics, and γ_{105n} represents the average point change in children's mean test performance associated with a residential area's identification as having those problems.

--TABLE 3 NEAR HERE--

ANALYSIS

DESCRIPTIVE STATISTICS

Our sampling strategies achieved a sample more diverse than in previous research, giving us the freedom to provide a detailed analysis of racial/ethnic differences. Although von Hippel (2007) was one of the few studies that included separate racial/ethnic categories, the proportion of African Americans in his sample barely reached 5 percent whereas ours exceeded 12 percent. At the high end, our sample had more white (44%) than Hispanic (31%) children in contrast to other studies, such as von Hippel's, that had majority Hispanic samples.

While confirming that the treatment samples are balanced is difficult to do for the reasons outlined by Morgan and Winship (2007, p. 114), we argue that balance would be more apparent if the pre-treatment characteristic that seemed most highly correlated with the outcome, but was not a dimension on which children were stratified in our matching procedures, appeared sufficiently equal across groups after matching. Test-scores that were measured prior to treatment at the start of children's educational careers, for example, should appear similar across treatment groups under an assumption of balance. This is in fact the case; the mean-difference at kindergarten's start between the two treatment conditions was just .70 points (31.41 versus 32.11) in reading and .80 points in math (21.43 versus 22.23), both insignificantly higher for children in 9-month schools. We therefore have concluded that the variables on which we estimated propensity scores and our match optimization strategies yielded treatment groups without confounding pre-treatment test-score differences.

UNCONDITIONAL ANALYSIS

Our first analysis question asked whether significant test-score differences existed between year-round and 9-month school children and if so, did they vary according to the season? Our analysis shown in Table 4 suggests the answer to both of these questions is "yes." In Table 4 are estimates for children's reading and math test-scores in all three time-spans for the full sample analysis. The first column provides the mean test-score while the second column labeled "difference" includes the all-year variable, which represents the estimated difference of year-round test-score performances from the mean. For the calendar year, the first row of Table 4 shows that experimental group children accumulated 4.097 (p. = .001) fewer test-score points in reading, leading to a gap between them and control group children of just over a quarter of a standard deviation unit. YRE students also accumulated about 2.143 (p. = .002) points less than control group students in math, equaling a gap of about .178 standard deviation units.

In the second row, the same models are specified for summer test-score growth. The reading analysis reveals no significant test-score growth or loss for students in general (-0.256, p. = .52) or year-round students in particular (1.007, p. = .160). However, the math analysis shows that all children experienced summer-time test-score gains (1.686, p. = .001), and that this growth did not appear to vary significantly according to school type (.824, p. = .137).

--TABLE 4 NEAR HERE--

Estimates of 9-month academic year learning are detailed in the final row, and there we find the most notable analysis results. YRE children accumulated 4.747 (p. = .001) points less than their 9-month counterparts in reading and 3.609 (p. =. 001) points fewer in math. While the finding that year-round children accumulated fewer points than their 9-month counterparts should have been expected given that they did so for the calendar year, we must explain why the relative loss is of a greater magnitude (29.48 and 29.05 standard deviation unit difference in reading and math, respectively). For children receiving YRE, this lower 9-month accumulation of test-score points is expected due to there being nearly 13 fewer instructional days than what year-round children would have received within a calendar year and that children in 9-month schools receive. If this slower YRE 9-month growth is among summer school children, we reason that children might have been placed in summer school because they had performed less well during the 9-month school term. Moreover, these results cast doubt on the spacing effect hypothesis (i.e. less slippage due to shorter summer breaks or a quicker resumption of learning than children in traditional 9-month schools) because 9-month gains in year-round schools are .16 and .20 points per month lower in reading and math, respectively, than they are for the calendar year. While the seasonal dimension of our counterfactual framework has revealed that more instructional days led to test-score gains among year-round children, a .16 to .20 points per month gain suggest there are not enough months of the year in which to add instruction that would result in the elimination of the gap between them and 9-month children. For example, the summer would need to be 10.715 months long in order to totally offset the 9-month math shortfall of -3.61 points.

MIXED MULTIVARIATE LINEAR MODELS

We also asked was there variation in how race, social class and residency related to test-scores across seasons and school schedules. With this question we addressed longstanding speculation within research about schools as sites where academic differences grow (Downey, von Hippel and Broh 2004; Condron 2009), and the school's possible mitigation of residential effects (Johnson 2012a; Rendon 2014). Pursuant to these interests, we specified conditional models for each treatment and time-span and reported the results in Tables 5 and 6. Addressing the racial concerns first, the reading results in Table 5 show that only the test-scores of Asian-Pacific Islander children differ from the average test-score growth of year-round school children. This stronger than average gain of 6.675 points (*p*. = .048) occurred mostly during the 9-month period (5.251, *p*. = .021). Moreover, stratification along the dimensions of race/ethnicity and social class appears surprisingly flat within year-round schools. So while year-round schools may not yield higher test-scores than 9-month schools, they appear to be institutions with more uniform benefits.

The story is quite different in 9-month schools where test-score gaps resemble those found in previous research (Downey, von Hippel and Broh 2004; Johnson 2014). Even as social class is considered in this model, African Americans still experienced a sizable setback (-8.231, *p*. = .001) of approximately .458 standard deviation units. However, their negligible gains were offset by significant growth during the summer (2.064, *p*. = .029) relative to white children, leading to a calendar year shortfall of 4.397 points (*p*. = .017). It appears as though 9-month schools are not contexts where all status groups have kept pace with or exceeded mean test-score growth as they apparently have in year-round schools. Most important however, is that despite the higher mean achievement of children in 9-month schools (34.468 points), African American test-score growth in them totaled just 26.23 points, which is lower than the 9-month (30.119) and yearly (30.888) point-estimates of African Americans in year-round schools. That the 9-month African American estimate is lower than the 9-month YRE estimate is indeed surprising because the latter has fewer days of instruction in this period than the former. It is worth noting that 9-month schools were also especially effective in social class sorting since children in the lowest social class accumulated fewer points (-9.813 *p*. = .001) with the majority of this slippage having occurred while they were in school (-8.596 *p*. = .001). This large shortfall, equaling .546 standard deviation units, left their total accumulated points at 25.87, far lower than the mean test-score of children in the lowest social class in year-round schools.

--TABLE 5 NEAR HERE--

The estimates of the math analysis shown in Table 6 were more varied, but mirrored the reading results in important ways. In regards to race/ethnicity, Asian-Pacific Islander children were once again the only racial/ethnic group that exhibited significantly different math gains, but this time they had a lower rate of growth over the calendar year (-3.990, *p*. = .015). In 9-month schools, Table 6 shows that racial/ethnic inequality is greater than it appeared in year-round schools. African Americans (-5.839 *p*. = .001) and Hispanics (-3.086 *p*. = .026) accumulated fewer points only while school was in session and experienced large calendar year setbacks equaling .463 standard deviation units for the former and .244 for the latter group. In fact, the 20.759 points that African Americans gained during the 9-month traditional academic year is less than African Americans gained with 9 months (21.356 points) and a full year (24.017 points) of YRE. Regarding social class differences, lower than average gains occurred for children in the lowest social class during the summer (-3.842, *p*. = .001), but they became insignificantly different than mean calendar-year growth once combined with their growth during the 9-month period. Year-round and 9-month children in the higher social classes experienced stronger than average gains over the calendar year. Consistent with the reading analysis, the math analysis has shown a greater degree of stratification in 9-month schools than year-round schools.

Turning our attention to the subject of residential effects, the reading analysis displayed in Table 5 reveals YRE children's growth in low income residential areas was about -6.165 points (*p*. = .002) or .453 standard deviation units less than mean growth during the 9-month period. The fact that the slippage reduced to just .264 standard deviation units for the calendar year (-4.179, *p*. = .032) suggest that while summer instruction is not significant, it effectively offset 9-month losses. Equally large residential shortfalls occurred in reading in 9-month schools, but this time it was related to the neighborhood's social disorganization (-4.831 *p*. = .024). Again, we see a decreased magnitude of the neighborhood effect once we considered 9-month school students' calendar year gains (-4.768 *p*. = .050, 26.98 sd). Thus, in reading we have concluded that neighborhood effects on test-scores 1) were strongest during the 9-months of both school types where children had the maximum percentage (55.19 to 62.99) of school days, 2) modest in the calendar year where the percentage of instructional days ranged from 49.32 to 52.33, and 3) small to non-existent during the summer when children had the smallest percentage (0 to 27.44) of school days. In other words, neighborhood effects appeared strongest in periods with more school days, not fewer.

In math, the summer time appeared much more consequential to an understanding of neighborhood-based learning than it was in the reading analysis. For instance, children that resided in high income areas and attended year-round schools had stronger than average gains during the summer (2.767 *p*. = .022). This positive association may have resulted from their receipt of summer instruction because the estimated effect of summer type showed a nearly significant advantage for YRE over summer school (1.621 *p*. = .077). YRE includes approximately 7 more days of instruction during the summer than does summer school.

DISCUSSION

This study of race and residential effects in year-round and 9-month schools addressed a major void within research on an important question. The question of whether schools exacerbate or equalize disparities in children's test performance is an essential one to address in a society where education is the key to social mobility. There are voids in the literature regarding the usefulness of YRE as a viable policy option because we know little about what form it should take to secure the desired outcomes for children, and how it functions with regard to race/ethnicity and residency—two dimensions of social stratification that many give as much credit for the social reproduction of status hierarchies as is given to the family. About these inequalities, our analysis has led us to conclude that schools can be greater equalizers according to race than they are currently, but may simultaneously serve to enable neighborhoods' impressive stratifying effects.

Regarding the overall effects of YRE, this analysis has shown that children in year-round schools do less well and gain the least during the traditional 9 month period, casting doubt on the spacing effect hypothesis. Yet smaller relative losses as children's learning extended into the academic year suggested that more time in them does improve test scores, but not significantly enough to make up the gap with traditional school children in just three months' time. Consequently, we conclude that test-score equality would not be achieved by simply lengthening the school year.

To the extent that year-round schools were a benefit during the summer, this analysis gave the edge to those with modified calendars over those that included summer school. At no time in this analysis of summer-time learning did summer school appear more beneficial than year-round school. Summer school effects have appeared positive with some consistency in research, but finding stronger effects for year-round schools is reasonable since year-round schools have more summer instructional days than do summer schools. We make this distinction cautiously because it would be erroneous to presume that the number of days is the only way in which these schools differ. Future research will need to illuminate to what extent their unique social organization contributes to differing impacts while accounting for variation in school dosage. Nonetheless, we assume that social organization differences are not spurious, and are in fact related to, if not caused by, the two treatments.

But this analysis did not estimate these general effects and then assume that they would apply equally to all racial/ethnic groups. Instead, we explored whether there was variation in how race/ethnicity related to test-scores across these forms of schooling and seasons. Regarding within-race/ethnicity differences between the two school types, African Americans were the only race/ethnic group that gained more in year-round schools than in 9-month schools in both subjects. Not only did African Americans in year-round school gain on co-ethnics in 9-month schools, their test-scores differed insignificantly from those of whites and Hispanics, while in comparison to Asian-Pacific Islanders they did slightly better in math and worse in reading. In contrast, the test-score growth of African Americans in 9-month schools lagged behind that of all racial groups in 9-month and year-round schools. This analysis therefore found that their greatest potential to serve as equalizers was for African Americans, the group that previous research suggested had disproportionately shouldered its stratifying effects. While this analysis had not found that African Americans in year-round schools had gains strong enough to equal the mean performance levels of white children in traditional schools, this was nearly accomplished in math while the reading gap was cut by over half (from 8.23 to 3.58 points). Apparently, year-round schools' compensatory capacity has been, until now, hidden away in existing research within the mean effects of unbalanced natural experiments and aggregated estimates of racial/ethnic groups.

The outcomes of this analysis identified for African Americans an educational policy option with some degree of transformative potential. However, we should be concerned about the gains of white children in YRE, and acknowledge that the lesser degree of racial stratification apparent in year-round schools comes at a cost to white children relative to those in traditional schools. So our task is to secure the gains of year-round schools for African Americans while maintaining the enrollment of other racial/ethnic groups in the traditional schools that seem to support their achievement best. The fact that schools in the U.S. are so racially segregated presents the ironic benefit of easily targeting African Americans for YRE reforms. But there are some obstacles in the way. As it stands, African Americans are relatively underserved by YRE in contrast to Asian American and Hispanic children, because they are underrepresented in the region where year-rounds schools are most popular (the West) and live in some states where YRE is generally unpopular. Of these states, Michigan, Mississippi, Virginia and Florida have instituted laws that restrict the start of the academic year to a period close to Labor Day while Alabama's law goes as far as to limit the number of school hours (i.e 1080) to that typical of a 180-day school-year. Arguments that reforms are good for African Americans rarely result in their adoption in a society that many people mistakenly claim to be post-racial and that prefers the appearance of race-neutral public policies. Federal incentives provided directly to schools would need to be significant enough to ease states' resistance to YRE.

This study also produced insight about how residential effects were related to test-outcomes given children's dose of school exposure. The pattern of relationships found in the reading analysis suggested that schools did not offset neighborhood effects as much as they functioned to relay them. The largest residential effects of low SES and social disorganization occurred during the nine month periods of both school forms, the schools that offered the greatest number of instructional days. Within the summer when the percentage of instruction was lowest, neighborhood disadvantages were insignificantly related to test-scores. There are a few conclusions that we have drawn about these residential effects. First, the covariance of residential effects and school dosage is not entirely counterintuitive since the former arises in large part through human interaction and schools are possibly the primary medium that facilitates interaction for children. Second, the aforementioned results suggest that the artificial isolation of neighborhoods from schools as posited in the autonomous institutional framework is invalid. Third, there is little evidence presented in this study that supports the presence of temporal fluctuations in residential effects in the way we imagined they might occur; they were not stronger during the summer as they appeared in the results of Benson and Borman (2010) and Johnson (2014). This study implies the relevance of a faucet theory of neighborhood effects can instead be applied to the academic season where stronger effects were present. After all, Johnson (2012b) held out the possibility that “institutional effects may mediate neighborhood influences as much as they might inspire them” (p. 37). We therefore recommend that the field moves away from an ecological correspondence theory which presumes a simple resemblance of neighborhoods and schools in their function. Instead, we conclude from this study that without schools, neighborhoods could not function to impact children’s test-scores. In other words, the school is “an essential organ” of the neighborhood, without which residential disadvantages cease functioning in relation to young children’s test-performances.

But there are some cautions related to research of this kind that we must mention. One caution is that counterfactual models address the bias of only observed characteristics that would be found in inferential studies and not unobserved ones. While several social background dimensions were used to construct propensity scores, it remains possible that relevant pre-treatment characteristics have been excluded. We sought to test this proposition, and hence the vulnerability of this study’s analysis, by applying our pre-treatment match criteria to a smaller matched sample of 1094 (from the ECLS-K subsample) with a .01 caliper threshold, and a larger one-to-many matched sample of 3818 children using a more stringent .005 caliper threshold to simulate the influence of less and more efficient matching estimators.[6] With both matched files, we were able to generate racial/ethnic and residential estimates that mirrored the patterns we shared in our main analysis. We are therefore optimistic that our findings are likely to withstand decreased propensity score differences among matches that could arise from the use of other matching characteristics. Another limitation is that our causal inferences pertain to year-round children that have 9-month matches. We highlighted the sample of the three that contained the greatest number of experimental children to enhance generalizability, but we make no claims that it is representative of ECLS-K children or children nationally in year-round schools. Of course, we encourage future studies to determine whether the findings of analyses like this one apply to children of other age groups.

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[6] The one-to-many matching strategy required the use of a weight to achieve balance in the probabilities of selection across the two conditions.

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TABLE 1. Counterfactual Design

	IN SCHOOL YEAR-ROUND (Experimental)	IN SCHOOL 9-MONTH (Control)
YEARLY GROWTH	Yearly growth with year-round schooling	Yearly growth with 9–month schooling
Number of School days (% School days)	190.99 (52.33%)	180 days (49.32%)
SUMMER GROWTH	Summer growth with year-round schooling	Summer growth with 9-month schooling
Number of School days (% School days)	22.28 days (27.44%)	0 days (0%)
9-MONTH GROWTH	9-month growth in year-round schools	9-month growth in 9-month schools
Number of School days (% School days)	157.72 days (55.19%)	180.00 days (62.99%)

FIGURE 1. Nested Seasonal-Counterfactual Design

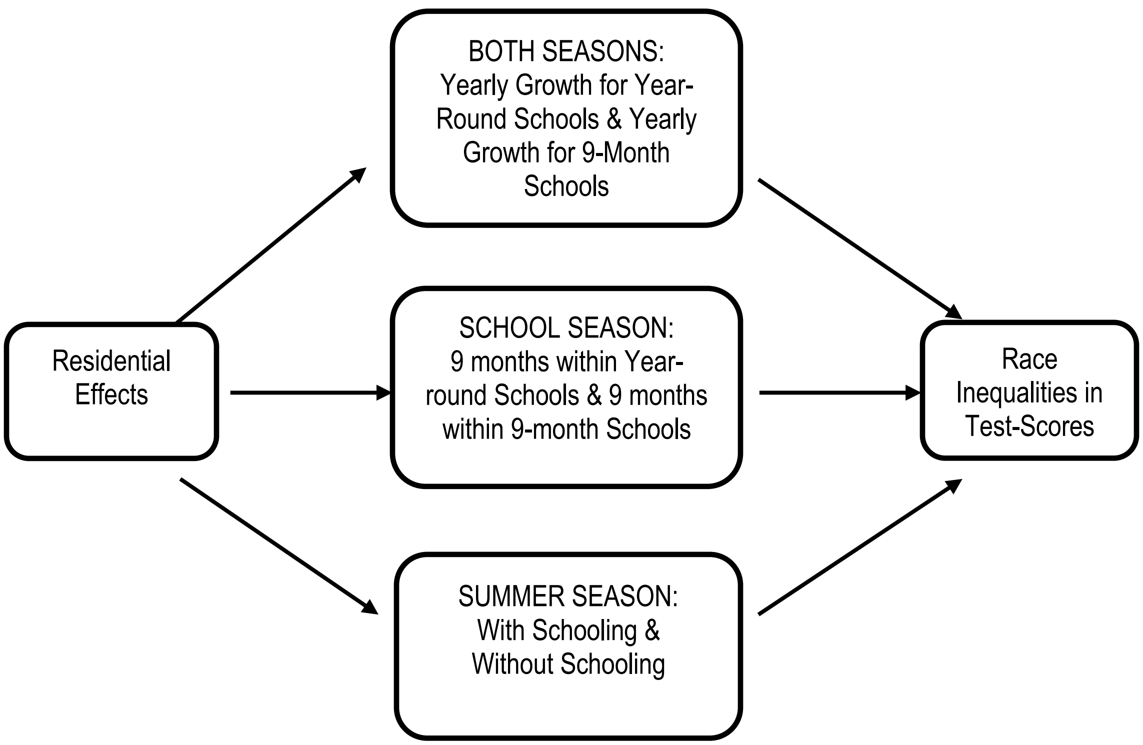

TABLE 2. Unmatched Mean Differences of Racial Groups in 9-Month Versus Year-Round Schools

Variables	Asian-PI Difference	Asian-PI SE	Black Difference	Black SE	Hispanic Difference	Hispanic SE	White Difference	White SE
Social class quintile (1 = low, 5 = high)	.13	.15	-.13	.14	.59***	.09	.08	.07
Gender (1 = female, 0 = male)	-.07	.05	-.01	.05	-.07+	.04	-.05+	.03
Single parent (1 = yes, 0 = no)	-.04	.04	.02	.05	.05+	.03	-.03	.02
Age in months (Months at kindergarten start)	.53	.43	.55	.45	1.28***	.30	.29	.25
Repeated kindergarten (1 = yes, 0 = no)	-.03*	.01	-.00	.02	.04*	.02	-.02*	.01
Full-day kindergarten (1 = yes, 0 = no)	.12*	.05	.12**	.04	.34***	.03	.06*	.03
Preschool (1=head start/center/day care, 0=no)	-.00	.03	.06	.04	-.02	.02	-.03**	.01
School sector (1 = public, 0 = private)	-.02	.04	-.02	.04	.12***	.02	.08***	.02
City location (1=yes, 0= no)	-.16***	.05	-.03	.05	.01	.03	.00	.02
Neighborhood disorganization (1=yes, 0=no)	.17***	.04	.01	.05	-.09*	.03	.01	.01
Zip code average percent male joblessness	-4.06***	1.11	-.50	1.33	-2.05*	.81	1.51**	.51
Zip code average median family income	5106.13*	2093.65	567.09	1418.65	4707.86***	1102.95	-1524.69	1095.40
Zip code mean percent minority	-8.13***	2.13	2.03	3.10	-14.61***	2.15	-4.42***	.83
Reading score kindergarten end to 1st grade start	-1.53*	.74	-.48	.68	-1.30*	.57	-1.43***	.35
Reading score kindergarten end to 1st grade end	.79	1.05	-1.23	1.04	2.15**	.81	2.07***	.56
Reading score grade 1 start to grade 1 end	2.20*	1.01	-.69	1.15	3.40***	.98	3.52***	.60
Math score kindergarten end to 1st grade start	.48	.63	-.95	.63	-.75+	.44	-.26	.33
Math score kindergarten end to 1st grade end	-.05	.73	-1.28+	.70	.95+	.55	.69+	.37
Math score grade 1 start to grade 1 end	-.51	.65	-.25	.79	1.66**	.53	1.10**	.41

TABLE 3. Descriptive Statistics, Matched Full (N = ~1830)

Variables	Mean	STDV
Asian/Pacific Islander (1 = yes, 0 = no)	.13	.34
Black (1 = yes, 0 = no)	.12	.32
Hispanic (1 = yes, 0 = no)	.31	.46
White (1 = yes, 0 = no)	.44	.50
Social class quintile (1 = low, 5 = high)	2.84	1.45
Low social class (1 = yes, 0 = no)	.26	.44
Middle low social class (1 = yes, 0 = no)	.19	.39
Middle social class (1 = yes, 0 = no)	.18	.38
Middle high social class (1 = yes, 0 = no)	.19	.39
High social class (1 = yes, 0 = no)	.18	.38
Gender (1 = female, 0 = male)	.54	.50
Single parent (1 = yes, 0 = no)	.19	.40
Age in months (months at kindergarten start)	65.11	4.38
Sector (1 = public, 0 = private)	.19	.39
Repeated kindergarten (1 = yes, 0 = no)	.04	.20
Full-day kindergarten (1 = yes, 0 = no)	.49	.50
Preschool (1=head start/center/day care, 0=no)	.09	.29
Year-round school (1=yes, 0=no)	.23	.42
All-year (1= summer school/year-round, 0= 9-month)	.50	.50
City location (1=yes, 0= no)	.45	.50
Zip code average median family income	51052.06	23217.06
Zip code median family income – Lower third	.39	.49
Zip code median family income – Middle third	.32	.47
Zip code median family income – Upper third	.29	.46
Zip code mean percent minority	34.30	29.75
Zip code percent minority – Lower third	.22	.41
Zip code percent minority – Middle third	.34	.48
Zip code percent minority – Upper third	.44	.50
Zip code percent male jobless in civilian labor force	36.17	11.89
Neighborhood disorganization (1=yes, 0=no)	.15	.35
Months from kindergarten end to grade 1 start	2.62	.28
Months from test 2 to kindergarten end	1.08	.49
Months from grade 1 start to test 3	1.43	.52
Months from grade 1 start to test 4	8.30	.57
Months from grade 1 start to grade 1 end	9.45	.36
Months from test 4 to grade 1 end	1.18	.51
Reading score kindergarten start	31.77	11.92
Reading score kindergarten end to 1st grade start	-.11	9.28
Reading score kindergarten end to 1st grade end	32.64	16.88
Reading score grade 1 start to grade 1 end	32.58	16.10
Math score kindergarten start	21.84	9.52
Math score kindergarten end to 1st grade start	1.64	8.64
Math score kindergarten end to 1st grade end	25.59	12.03
Math score grade 1 start to grade 1 end	23.93	12.42

Table 4. Unconditional Models of Reading and Math Test-Score Mean Growth and Year-Round/9-Month Differences, Full Sample (N= ~1830)

	Reading				Math			
	Mean		Difference		Mean		Difference	
	Growth	SE	Growth	SE	Growth	SE	Growth	SE
Yearly Growth								
Intercept	32.479***	0.594	32.299***	0.591	25.762***	0.373	25.730***	.374
All-Year	--	--	-4.097***	1.166	--	--	-2.143**	.693
Level 1 & 2 τ/STDV	216.10***	14.70	213.09***	14.59	106.17***	10.30	104.96***	10.25
Level 3 τ/STDV	37.864***	6.153	37.891***	6.155	11.242***	3.353	11.731***	3.425
Summer Growth								
Intercept	-0.256	0.399	-0.217	0.396	1.686***	0.320	1.701***	0.314
All-Year	--	--	1.007	0.599	--	--	.824	0.554
Level 1 & 2 τ/STDV	59.656***	7.724	59.717***	7.727	51.972***	7.209	52.163***	7.222
Level 3 τ/STDV	10.101***	3.178	9.750***	3.122	8.018***	2.831	7.516***	2.742
9-month Growth								
Intercept	32.466***	0.569	32.343***	0.551	23.846***	0.472	23.764***	.451
All-Year	--	--	-4.747***	1.060	--	--	-3.609***	.799
Level 1 & 2 τ/STDV	185.28***	13.61	181.85***	13.49	101.50***	10.07	100.51***	10.03
Level 3 τ/STDV	35.045***	5.919	33.991***	5.830	25.916***	5.091	23.459***	4.844

*** = p < .000, ** = p < .01, * = p < .05, + = p < .10

TABLE 5. Multivariate Models of Reading Growth, Year-Round & 9-Month

	Year-Round Enrollment			9-Month Enrollment		
Reading	Yearly	Summer	9-month	Yearly	Summer	9-month
Intercept	30.888***	0.134	30.119***	34.153***	- 0.395	34.468***
Age	0.324	0.105	0.157	-0.096	0.117	-0.207
Gender	-2.803	-0.999	-1.879+	-1.801	-1.212*	-0.370
Single parent	0.647	-0.395	2.432*	-1.697	-0.356	-1.184+
Repeated kindergarten	-8.443*	-3.412	-5.136+	-9.529*	-2.563	6.900+
Full day kindergarten	-1.171	-0.549	0.198	-0.988	0.248	-0.587
Pre-school program	-0.474	2.122	-0.739	-0.245	0.798	-0.622
Summer type	1.209	0.909	--	--	--	--
Low social class	-3.429	-2.704	1.367	-9.813***	-1.059	-8.596***
Mid low social class	2.713	0.702	1.142	-2.401	-0.067	-1.931
Mid high social class	0.585	0.295	-0.200	4.271+	1.040	3.679
High social class	2.450	1.777	2.619	3.535	2.457+	-0.203
Asian/Pacific Islander	6.676*	-1.109	5.251*	-2.052	1.972	-2.372
Black	2.233	0.049	1.413	-4.397*	2.064*	-8.231***
Hispanic	1.559	-1.638	2.127	0.271	1.139	-2.902
City	1.441	0.809	1.368	1.802	0.684	1.172
Low area income	-4.179*	-1.827	-6.165**	1.663	-1.367	3.371+
High area income	-0.017	-1.081	-1.795	-0.760	1.066	-0.843
Low minority percent	4.590+	1.290	0.974	0.024	0.370	-0.705
High minority percent	0.048	2.638*	- 2.806	0.317	0.887	0.031
Social disorganization	-3.012	2.302	-1.123	-4.768*	0.377	-4.831*
Level 1 & 2 variance	177.41***	72.189***	73.374***	215.529***	36.688***	231.64***
Standard deviation	13.320	8.496	8.566	14.681	6.057	15.219
Level 3 variance	29.370***	10.956***	120.76***	37.070***	7.510***	19.749*
Standard deviation	5.419	3.309	10.989	6.088	2.740	4.444

*** = p < .000, ** = p < .01, * = p < .05, + = p < .10

TABLE 6. Multivariate Models of Math Growth, Year-Round & 9-Month

	Year-Round Enrollment			9-Month Enrollment		
Math	Yearly	Summer	9-month	Yearly	Summer	9-month
Intercept	24.017***	2.342***	21.356***	26.846***	1.226***	25.668***
Age	0.107	0.262**	- 0.062	0.171+	0.075	0.030
Gender	2.531**	0.929	0.286	1.599+	0.068	1.704+
Single parent	-1.385	-1.591+	0.354	0.358	-0.996	1.164
Repeated kindergarten	-3.846+	-0.046	-2.619	-9.857***	-1.684	-8.328***
Full day kindergarten	-2.398*	-0.829	-1.874	-1.030	0.484	-2.442*
Pre-school program	-0.128	-0.032	-0.473	3.060	- 0.514	3.738
Summer type	0.506	1.621+	--	--	--	--
Low social class	-1.608	-3.842***	2.122	-2.081	-1.039	-3.507*
Mid low social class	1.106	-0.924	2.104	3.007+	-0.954	2.592
Mid high social class	3.008*	-0.786	3.943*	4.061**	-0.649	1.210
High social class	2.956+	-1.179	4.177*	3.589*	-0.024	0.949
Asian/Pacific Islander	-3.990*	3.019	-3.663	-2.403	1.227	- 4.471*
Black	-0.705	1.372	-0.103	-5.839***	0.778	- 4.909*
Hispanic	-1.819	0.362	-0.553+	-3.086*	0.254	-2.246
City	1.870+	-0.418	1.193	0.709	-0.388	1.261
Low area income	0.851	0.933	-0.493	1.883	-0.471	2.672
Upper area income	0.015	2.767*	-3.002	2.653*	1.530+	2.286
Low minority percent	0.361	-1.133	2.251	-1.338	-0.063	-1.426
High minority percent	0.990	1.677	-0.159	1.308	1.153	0.177
Social disorganization	-0.625	0.648	-0.002	-0.477	2.226*	-2.937+
Level 1 & 2 variance	86.623***	54.043***	56.566***	97.746***	35.967***	135.067***
Standard deviation	9.307	7.351	7.521	9.887	5.997	11.622
Level 3 variance	5.974**	3.380*	42.759***	6.918*	12.280***	16.747**
Standard deviation	2.444	1.839	6.539	2.630	3.504	4.092

*** = p < .000, ** = p < .01, * = p < .05, + = p < .10