Are Schools the Great Equalizer?
School and Non-School Sources of Inequality in Cognitive Skills
(Running Head: Are Schools the Great Equalizer?)

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Abstract

How does schooling affect inequality in cognitive skills? Reproductionist theorists have argued that schooling plays an important role in reproducing and even exacerbating existing disparities. But seasonal comparison research reveals that socioeconomic and racial/ethnic gaps in reading and math skills grow primarily during the summer, suggesting that non-school factors (e.g., family and neighborhood) are the main source of inequality. We use the *Early Childhood Longitudinal Study–Kindergarten Cohort of 1998-99* to improve upon past estimates of school and non-school effects on cognitive skill gains. Like past researchers, we consider how socioeconomic and racial/ethnic gaps in skills change when school is in session versus when it is not. But we go beyond past research by examining the considerable inequality in learning that is not associated with socioeconomic status and race. This "unexplained" inequality is more than 90% of the total, and it is much smaller during school than during summer. Our results suggest, therefore, that schools serve as important equalizers: nearly every gap grows faster during summer than during school. The black/white pattern, however, represents a conspicuous exception.

Children begin formal schooling with different skill levels, in part because they are exposed to different home environments and neighborhoods. But how does schooling affect these initial disparities? One argument is that schools play an active role in reproducing or even exacerbating inequalities. Initially advantaged students often attend schools with higher resource levels (Condron and Roscigno 2003), are assigned to higher tracks and ability groups (Oakes 1985; Gamoran and Mare 1989), and enjoy more favorable interactions with teachers (Bourdieu and Patterson 1977; Downey and Pribesh forthcoming; Lareau 2000). A competing argument is that, while important inequalities in school resources persist, schools serve to reduce disparities in skills between advantaged and disadvantaged students. The claim from this side is that all children attend roughly the same "common school," (Cremin 1951) so that schools serve as the Great Equalizer.

At first glance these traditions appear incompatible, but it is possible that both are correct. Even if schools are unfair, they may serve as equalizers if the variation in school environments is smaller than the variation in non-school environments. Figure 1 shows this schematically. Some children may have relatively poor school experiences, but the disadvantages in their non-school environments may be even more severe. As a result, a disadvantaged child attending a low-quality school can enjoy a larger "school boost" than an advantaged child attending a high-quality school. In this way schools can favor advantaged students, yet still serve as equalizers.

Figure 1 near here

To improve our understanding of the relationship between schooling and inequality, we analyze data from a nationally representative sample of children as they progress from the beginning of kindergarten to the end of first grade *(Early Childhood Longitudinal Study–Kindergarten Cohort*, or *ECLS-K*). The data allow us to estimate learning rates both when school is in session (academic year) and when it is not (summer vacation), shedding light on whether schools increase inequality or decrease it.

To foreshadow the results, we find that students learn at much more equal rates when school is in session than when it is not. When we relate learning rates to classic dimensions of stratification, we get a more nuanced story: schools reduce socioecononomic inequality, have a mixed effect on racial inequality, and have little if any effect on gender inequality. These dimensions, however, explain less than 10% of the inequality in learning rates. Going beyond past researchers, we look at how schools affect the remaining, much greater inequality that is unrelated to socioeconomic status and race (and gender). This inequality is more than 90% of the total, and our tests show that it is much smaller during school than during summer. For most inequality, then, schools are indeed a great equalizer.

Schools and Inequality

When it comes to inequality, are schools part of the problem or part of the solution? Because public schooling is free for all, Americans typically view the U.S. school system as a leveling institution. But the reproductionist perspective emphasizes the opposite view. While there are many strains of reproduction theory, the central theme is that schools serve the interests of those at the top rather than the bottom of the stratification ladder (Bowles and Gintis 1976; Bourdieu and Patterson 1977). Reproduction can occur, in part, because advantaged children attend schools with more resources than do disadvantaged children (Condron and Roscigno 2003), or because schools serving low-socioeconomic students more often stress rote memory skills, obedience, punctuality, and other skills that prepare them for low-wage jobs rather than managerial positions (Bowles and Gintis 1976).

Of course, the claim that differences between schools account for differences among students' cognitive skills has been vigorously debated ever since the Coleman Report challenged the importance of between-school differences (Coleman et. al. 1966). But regardless of this dispute, reproductionists point out that schools also shape inequality by providing varying opportunities *within* schools. For example, Bourdieu (1977) argued that schools reproduce inequalities by rewarding students' noncognitive characteristics (e.g., speech, dress, style), thereby ensuring that those skilled at signaling affiliation with elite culture maintain their privileged position. Teachers do reward students for knowledge of highbrow arts or other cultural knowledge (DiMaggio 1982), and student/teacher interactions can be strained by racial mismatches (Downey and Pribesh forthcoming). In addition, institutional processes within schools, such as ability grouping and tracking, can reproduce or even exacerbate inequality (Gamoran and Mare 1989; Oakes 1985).

Another line of research consistent with reproductionists' claims compares test score gaps at the beginning of first grade with gaps at the end of high school. The reasoning here is that a growing black/white test score gap, for example, is consistent with the reproductionist hypothesis that schools contribute to inequality. Phillips, Crouse, and Ralph (1998) reviewed the research on black/white test score gaps, along with their own analyses of eight national surveys. They concluded that at the beginning of first grade, black children are roughly one-half a standard deviation behind white children on tests of math, reading and vocabulary. By the end of high school, however, black children are a full standard deviation behind white children on these same measures.

The Non-School Environment

While reproductionists point to these patterns as evidence that schools contribute to inequality, an overlooked weakness of this conclusion is the failure to allow for how school success is influenced by non-school factors. Children spend the vast majority of their time *outside of school*—Walberg (1984) estimated that the typical 18 year-old has been in school for only 13% of his or her waking hours. On its surface this estimate appears low, but note that children usually spend only 6-7 hours a day in school and do not attend school on weekends, holidays, or during the summer. Even with perfect attendance, most children spend fewer than one-half the days a year in school (180/365) and children's pre-kindergarten years are spent primarily in "non-school" environments. Even if we limit our focus to the typical 9-month academic year, the average child still spends two thirds of his/her waking hours in the non-school environment (Hofferth and Sandberg 2001).

Not only do children spend most of their time outside of school, but the quality of their non-school environments is extremely variable. Hart and Risley (1995) observed that among children six months to three years old, welfare children had 616 words per hour directed to them compared to 1,251 for working-class and 2,153 for professional-family children. The authors extrapolated these patterns to highlight the cumulative impact of this disparity up until age four. If we extrapolate another year and a half, the results from this study suggest that, by the beginning of kindergarten, the average child in a professional family has had 61 million words directed their way, compared to 36 million for working-class and only 18 million for welfare children.[1] And of

[1] These estimates assume that children are awake for 14 hours a day and that observations of parent-child interaction made during six months to three years remain constant until the child enters kindergarten at 5½ years of age.

course, once kindergarten begins, these non-school disparities continue to matter in the evenings, on the weekends, and during the summer.

Because children spend so much of their time outside of school, and because non-school environments are so variable, it is hard to know whether disadvantaged children have lower achievement because of school or non-school influences. It is unclear, for example, whether the black/white gap grows because of school or in spite of it (Phillips et. al., 1998).

Seasonal Comparisons as a Way To Assess Schooling's Impact on Inequality

There is considerable evidence that disadvantaged children have poorer learning opportunities both inside and outside of schools. But the question we are interested in is whether schooling increases or decreases the inequality that occurs when school is not in session. To address this issue we need to observe how inequality changes under school and non-school conditions.

A powerful study would involve randomly assigning students of various subgroups to "school" and "non-school" conditions and observing how much inequality develops in each group over time. Evidence that the "school" condition produces greater inequality than the "non-school" condition would be consistent with reproductionists' claims that something about schooling contributes to inequality. Of course this kind of study is not practical or ethical, but a similar comparison is possible when the same students are observed during school and summer periods.

Heyns (1978, 1987) was not the first to study seasonal differences in learning by socioeconomic status and race (see also Murnane 1975), but her study is considered seminal because she conceptualized "summer parameters." *By comparing students' cognitive gains during* *the summer versus gains during the school year, Heyns reasoned that school-year learning was the product of both school and non-school factors, while summer learning reflected only the influence of non-school factors—such as the home and neighborhood.* The idea is analogous to a crossover design in health research, where the same patients are exposed to different treatments in different periods.

Many studies provide calculations of how much inequality grows during a calendar year, but by highlighting what happens in the summer Heyns provided the best estimates for the influence of non-school factors. Analyzing a sample of approximately 3,000 fifth and sixth graders in 42 Atlanta schools, Heyns found that during the summer, when school was out and non-school influences were dominant, the gap between disadvantaged and advantaged children's test scores grew, representing disparate home and neighborhood environments. When school was in session, however, advantaged and disadvantaged children gained at roughly the same rate. Heyns concluded that while schools are not equalizing in the absolute sense, schooling attenuates achievement gaps between children of varying backgrounds.

The credibility of this position was strengthened considerably when a second group of researchers reported similar patterns in a different sample. Entwisle and Alexander (1992, 1994) analyzed 790 children who began first grade at one of 20 randomly sampled Baltimore schools in 1982. Those concerned that about the generalizability of Heyns's findings in Atlanta were encouraged to see Entwisle and Alexander report largely similar patterns in Baltimore. Importantly, while Heyns studied children in fifth grade, Entwisle and Alexander began following children at the beginning of *first* grade. Entwisle and Alexander argued that studying children early in their educational careers was crucial because young children are "maximally sensitive to home and school influences" and "cognitive growth rates are higher in the first few grades than they are later on..." (Entwisle and Alexander 1992:73). In their sample, Entwisle and Alexander noted that gaps in reading skills grew across both socioeconomic status and race, and that these gaps grew at different rates in different seasons. Like Heyns' study, Entwisle and Alexander's study suggested that socioeconomic gaps form mainly in the summer, when school was not in session.

Taken together, the seasonal comparison research by Heyns (1978, 1987) and Entwisle and Alexander (1992, 1994) makes a formidable case for the view that, when it comes to inequality by socioeconomic status, schools are part of the solution rather than part of the problem. In contrast, the evidence for how schools shape the black/white gap in cognitive skills is less clear. Heyns (1978) and Murnane (1975) contend that the black/white gap forms largely over the summer while Entwisle and Alexander (1994) were less convinced that schools also equalize skills by race. The authors speculated that learning to read is especially sensitive to the racial composition of the school because racial/ethnic differences between students and others (especially teachers) might lead to different dialects, a problem most likely to occur in racially integrated schools. Their data, however, did not allow them to pursue this explanation further.

Extending Seasonal Comparison Research

Some researchers may be skeptical that schools really serve as equalizers until relevant seasonal patterns are found in nationally representative data.[2] Past seasonal research has focused

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[2] Karweit and Ricciuti (1997) provide one of the few seasonal comparisons based on a nationally representative sample, the Prospects Data of first graders. While some patterns were similar to those reported elsewhere (Heyns 1978; Entwisle and Alexander 1992, 1994; and the present study), Karweit and Ricciuti were unique in finding no relationship between summer gains and SES. In light of this odd result, it is worth noting that their analysis was restricted to only 14% of

on disadvantaged urban children, and may not fairly represent school and non-school effects in the U.S. One possibility, for example, is that disadvantaged urban samples have less variation in non-school environments than what might be obtained with from a national sample. If that were the case, past seasonal comparison studies may have underestimated disparities in non-school learning and, as a result, also underestimated the degree to which schools equalize learning. On the other hand, variation in school experiences may also have been attenuated in urban schools, making comparisons of rates of cognitive growth during the school year misleading. These concerns suggest that seasonal comparison research would be strengthened considerably by using more generalizable data.

In addition, studying children at an early point in their school career allows for estimates of non-school effects that are arguably less contaminated by school processes than estimates based on samples of older students. In this respect, the Baltimore sample, which began with first graders is better than the Atlanta sample, which began with fifth graders. But formal schooling often begins even before first grade, since kindergarten is now mandatory in most states. If kindergartens vary in quality in meaningful ways then the initial skill gaps estimated by Entwisle and Alexander in first grade were already shaped by schooling processes—perhaps in important ways.

To reduce further the contamination between school and non-school processes, we pay careful attention to the date on which cognitive tests were given. Most previous researchers have estimated summer learning simply by subtracting a spring test score from a fall test score. Such estimates are usually contaminated, because the time between spring and fall tests may contain

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the sampled children, and that gains were regressed on initial status—a practice that can yield biased results (Thomson 1924; Blomqvist 1977).

weeks or months of school as well as summer. Using our estimates of school-year learning rates, we attempt to remove this contamination.[3] Our adjusted estimates of summer learning rates are considerably reduced, suggesting that much previous research may have overestimated summer learning, and consequently underestimated the difference between summer and school learning rates.

Finally, we extend the seasonal approach beyond socioeconomic and racial inequality. We look at gender inequality, and—most importantly—we look at the roughly 90% of inequality that is not associated with gender, race, or socioeconomic status. This “unexplained” inequality in cognitive skills is considerably smaller during school than during summer, suggesting that in general—notwithstanding the details of inequality by race, gender, and socioeconomic status—schools really are great equalizers.

Data

We use the Early *Childhood Longitudinal Study–Kindergarten Cohort* (*ECLS-K*). ECLS-K is a nationally representative survey of about twenty thousand children in roughly a thousand schools. *ECLS-K* began following these children in the fall of kindergarten 1998, and has followed them, so far, through the spring of first grade 2000. (Data for third grade have been collected, but are not yet available.) Comprehensive documentation is available from the website of the National Center for Educational Statistics (2002).

[3] Reardon (2003) and Burkham, Ready, Lee, and LoGerfo (2004) have also recognized this source of contamination in ECLS-K. Reardon’s correction is the same as our own; Burkham et al. make adjustments in the context of an ordinary regression model.

Dependent variables: Reading and math scores

Children's reading and math skills were tested on four occasions: the spring and fall of kindergarten (1998-99) and the fall and spring of first grade (1999-2000). The first, second, and fourth tests were given to all available students; the third test, in the fall of first grade, was given in a 30% random subsample of schools.

Tests followed a two-stage format designed to reduce ceiling and floor effects. In the first stage, children took a "routing test" containing items of a wide range of difficulty. In the second stage, children took a test containing questions of "appropriate difficulty" given the results of the routing test (NCES 2000). Item Response Theory (IRT) was used to map children's answers onto a common 64-point scale for mathematics and a 92-point scale for reading. Few scores were clustered near the top or bottom of the IRT scales, suggesting that ceiling and floor effects were successfully avoided. In addition, the IRT scales improved validity and reliability by downweighting questions with poor discrimination or high "guessability" (Rock and Pollack 2002).

For an average child, 1 point in reading or mathematics was roughly the amount learned in 2 weeks of school. On the reading test, the skills tested were (1) knowing upper- and lower-case letters of the alphabet by name, (2) knowing the sounds of letters at the *beginnings* of words, (3) knowing the sounds of letters at the *ends* of words, (4) recognizing common words by sight, and (5) reading words in context. The math test also gauged five levels of proficiency: (1) identifying one-digit numerals, (2) recognizing a sequence of patterns, (3) predicting the next number in a sequence, (4) solving simple addition and subtraction problems, and (5) solving simple multiplication and division problems and recognizing more complex number patterns.

Missing values

Like many surveys, *ECLS-K* has a fair number of missing values. In addition, we deleted a few implausible values, and we deleted scores for any tests taken after a child had transferred from his original school.[4]

We assumed that values were *missing at random* in the limited sense defined by Rubin (1976).[5] Randomly missing test scores present no problem, since our longitudinal models do not require that all children be tested on all occasions (Singer and Willett 2003; Raudenbush and Bryk 2002). Randomly missing predictors, however, can produce bias and inefficiency.

We compensated for missing predictors using a *multiple imputation* strategy (Schafer 1997; Rubin 1987; Allison 2002). Before analysis, each missing value was replaced with three plausible imputations.[6] After analysis, results were summarized using formulas that account for

[4] As described later, our multilevel model assumes a strict hierarchy where students are nested within schools. A cross-classified model would allow for transfer students, but current software (e.g., SAS, HLM, S-Plus) cannot fit a cross-classified model to a survey as large as ECLS-K.

[5] We cannot test whether data are missing at random, but the assumption is usually more plausible than the assumptions implicit in deleting incomplete cases. In Allison's (2002, p. 4) paraphrase, "data on *Y* are said to be missing at random if the probability of missing data on *Y* is unrelated to the value of *Y*, after controlling for other [observed] variables…." This assumption is more plausible when there are more variables in the analysis, particularly if some variables are related to missingness. Our analyses, for example, included the number of days a child was absent from school, which should predict whether the child and his parents were present for tests and questionnaires. Note also that a lot of test scores must be missing at random; in the fall of first grade, the schools selected for testing were subsampled in a random fashion.

[6] Inferences based on three imputations (or even two) should be valid since no estimate has more than 20% missing information (Rubin 1987, Table 5.2).

the uncertainty of the imputed values. We imputed both continuous and categorical variables[7] using a model that incorporated all the variables in our analyses, as well as several auxiliary variables.[8] The imputations fully accounted for the correlations among test scores from the same child,[9] but could only partially account for the correlation among children from the same school.[10]

Our analyses use imputed predictors, but do not use imputed test scores. Test scores are the dependent variables, and imputed dependent variables contribute no information to parameter estimates (Little 1992; R.J.A. Little, personal communication, April 23, 2004). In addition, we could not impute test scores in a way that reflected the multilevel variation assumed by our analytic models (below). To clarify: we did use observed test scores to impute other variables (as is appropriate; see Allison 2002), but following imputation all imputed test scores were deleted. If

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[7] We used IVEware software, which imputes normal and categorical variables using normal and logistic regression (Raghunathan, Solenberger, and Van Hoewyk 2001). In principle, IVEware can also impute count variables using Poisson regression; however, the software displayed odd behavior wherein the imputed count could be several orders of magnitude larger than the observed counts. To avoid this, we transformed count variables to approximate normality (Box and Cox 1964; von Hippel 2003), imputed them using a normal model, then reversed the transformation.

[8] Probably the most useful auxiliary variable came from an interview where parents were asked to recall the last date of kindergarten and the first date of first grade. Although individual memories were quite fallible, school medians for remembered dates proved quite helpful for imputing the true dates for the beginning and end of each school year.

[9] Following a suggestion of Allison (2002, p. 74), we formatted the data so that each child's test scores appeared on a single line that also contained all the child's other variables (as well as variables characterizing the child's school). In this way, correlations among tests of the same child were modeled just like correlations among the child's other variables.

[10] Imputation models that account for clustering are relatively undeveloped. For this reason, our imputed values assume that all variation is at the child level rather than the school or cluster level. The resulting bias is likely trivial, since there were few missing values among the variables with substantial school-level variation. The only school-level variable with substantial missingness were the dates for the beginning and end of the school year. To remove child-level variation from imputed dates, we replaced the imputed dates with school means.

a child took three tests, we used them, but we did not use an imputed score for the fourth test. Since we did not use imputed test scores, our analyses focus on the 17,212 children in 992 schools who took at least one reading or math test.

Analyses

To review, each child was tested on up to four occasions: the spring and fall of kindergarten and the spring and fall of first grade. These four test occasions provide enough information to estimate three learning rates: the kindergarten learning rate, the summer learning rate, and the first grade learning rate. If children were tested at the beginning and end of each school year, estimating these learning rates would be a simple matter of connecting the dots (test scores) in Figure 2; for example, the summer learning rate would be the estimated using the difference between the second and third test scores, divided by the months elapsed between them.

Figure 2 near here

On average, however, tests were scheduled a little more than a month from the beginning and end of each school year. As a result, the time between the second and third test contained not just 2½ months of summer, but also 2½ months of school—1.13 months of kindergarten and 1.35 months of first grade. If we used the simple difference between the second and third tests, our estimate of the summer learning rate would be contaminated by intervening school days. Past seasonal comparison work lacked the dates needed to remove this contamination; as a result, summer learning may have been overestimated, and the difference between summer and school learning rates may have been underestimated.

Instead of connecting the dots, our model extrapolates, in effect, to the scores that *would* have been obtained on the last day of kindergarten and the first day of first grade (Figure 2). The difference between these extrapolated scores is a sounder basis for estimating summer learning. The extrapolations may be slightly biased if learning speeds up or slows down at the beginning and end of the school year[11], but these biases are probably much smaller than if the intervening school days were ignored. In a similar fashion, we can extrapolate to the score that would have been obtained on the first day of kindergarten or the last day of first grade. Our model makes these extrapolations by using information about the date of each test relative to the first and last days of school.

Models and estimates

We estimate knowledge and learning rates using a multilevel growth model (Raudenbush and Bryk 2002; Singer and Willett 2003). In this model, we view tests (level 1) as nested within children, and children (level 2) as nested within schools (level 3). At level 1, we use test scores to estimate each child's knowledge and learning rates. At levels 2 and 3, we model the way that knowledge and learning rates vary across students and schools. Reardon (2003) has independently fit very similar models to these data.

Model 1. Total inequality

In model 1, we estimate total inequality or variance in learning rates, which we find is much smaller when school is in session than when it is not.

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[11] Supplementary analyses suggested that learning rates are approximately constant for much of the school year. These analyses take advantage of the fact that different schools were tested at slightly different times. Since no schools were tested in the first or last weeks of the year, however, the possibility of different learning rates during these periods cannot be evaluated.

Specification

To model learning rates, we view each test score Y_{tcs} as a linear function of the months that child c in school s has been exposed to KINDERGARTEN, SUMMER, and FIRST GRADE at the time of test t:[12]

$$Y_{tcs} = \alpha_{0cs} + \alpha_{1cs}\,\mathrm{KINDERGARTEN}_{tcs} + \alpha_{2cs}\,\mathrm{SUMMER}_{tcs} + \alpha_{3cs}\,\mathrm{FIRST\ GRADE}_{tcs} + e_{tcs}$$

Here the intercept α_{0cs} is initial knowledge—an extrapolation to the score that the child would have received on the first day of kindergarten 1998. The slopes α_{1cs}, α_{2cs}, and α_{3cs} are the learning rates during kindergarten 1998-99, summer 1999, and first grade 1999-2000. Since the exposure variables KINDERGARTEN, SUMMER, and FIRST GRADE are measured in months, the learning rates α_{1cs}, α_{2cs}, and α_{3cs} are the number of points gained per month.

The residual term e_{tcs} is measurement error—the departure of the test score Y_{tcs} from the true achievement level of child c. The variance of the measurement error can be derived using test-reliability estimates from Rock and Pollack (2002); Table 1 gives the requisite calculations. Each of the four reading and math tests has a slightly different estimated error variance. We assume that errors on different tests are normal and independent random variables. Errors for tests of the same child are not independent, of course, but we account for this below by including random effects for each child and school.

Table 1 near here

Each child's knowledge and learning parameters α_{cs} are broken into three components γ_0, b_s, and a_c:

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

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

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

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

In the language of multilevel models[13], the first component γ_0 is a "fixed effect" representing the grand average for each parameter α. The second component b_s is a "random effect" representing the departure of school s from the grand average, and the third component a_c is a random effect representing the departure of child c from the average for school s. b_s and a_c are assumed to be independent normal variables with means of zero.

The model allows for correlations among the child-level random effects (a_{0c}, a_{1c}, a_{2c}, a_{3c}) and among the school-level random effects (b_{0s}, b_{1s}, b_{2s}, b_{3s}). For example, if, within the same school, children with greater initial knowledge also tend to have faster kindergarten learning rates, then there would be a positive correlation between the child-level random effects a_{0c} and a_{1c}. Note that such correlations are difficult to estimate using ordinary regression models. In ordinary regression, the estimated correlation between initial status and subsequent change is attenuated and negatively biased, because measurement error is confounded with true variation in initial status and change (Thomson 1924; Blomqvist 1977). Our multilevel growth model avoids this bias by separating school- and child-level variation (b_s and a_c) from variation due to test-level measurement error (e_{tcs}).

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[12] These exposure variables are obtained by comparing a child's test date to the first and last school days in the child's school.

[13] Some readers may be used to seeing multilevel models presented one level at a time. Our specification is equivalent, but for brevity we present the child and school levels simultaneously.

Estimates and interpretation

Table 2 estimates average knowledge and learning rates (γ_0), the variation of schools and children around these averages (i.e., the variances of b_s and a_c), and the correlations among knowledge and learning rates at different times (e.g., the correlation between a_{0c} and a_{1c}). We focus our interpretation on the results for reading; results for mathematics are similar.

The reading estimates suggest several benefits of schooling. First, average learning rates are faster when school is in session than when it is not. On a 92-point reading scale, children gain an average of 1.65 points per month of kindergarten and 2.49 points per month of first grade, but during summer vacation they lose a nonsignificant 0.01 points per month.[14] (This trajectory was plotted in Figure 2.) The average summer learning rate is 2.09 points per month slower than the average of the kindergarten and first grade learning rates, suggesting that schools accelerate learning—a finding that parents and taxpayers should find reassuring.

Table 2 near here

Second, in addition to increasing *average* learning rates, it seems that schools reduce *inequality* in learning rates. The evidence for this is that learning rates are more equal (less variable) during the school year than during summer vacation. At the child level—that is, for children in the same school—the standard deviation in monthly reading gains is 1.52 points during summer, but only 0.78 points during kindergarten and 0.94 points during first grade. At the school

[14] The summer estimate suggests that the average child gains no reading skills over the summer. This is surprising, and could be an artifact of adjustments made by the survey administrators. Between kindergarten and first grade, ECLS-K switched from a 72- to a 92-point test; to compensate for this, the kindergarten scores were adjusted upward. If we use the adjusted scores, we get the results in Table 2. If we ignore the adjustment, we get similar results, except that the summer reading gains, like the summer mathematics gains, become positive. Whether we use the

level—that is, between one school's average and another—the summer standard deviation is .57 points per month, but the kindergarten and first grade standard deviations are just .38 and .43. At both the school level and the child level, the summer variance is significantly larger than the average of the kindergarten and first grade variances.

Third, schools rein in the initial advantages of some students and student bodies. Initial advantage grows more slowly during the school year than during summer vacation. To see this, consider that advantage grows whenever initial status is positively correlated with subsequent growth. Under positive correlation, initial gaps fan out: children who start out ahead pull further ahead, and children who start out behind fall further behind (Rogosa, Brandt, and Zimowski 1982). At the child level, we see such a positive correlation during summer only; initial reading knowledge is positively correlated with summer learning (.18), but negatively correlated with learning during kindergarten (–.08) and first grade (–.12). This suggests that, for students in a typical school, the non-school environment encourages advantaged children to pull ahead, but the school environment helps disadvantaged children to catch up. The pattern is subtler at the school level, but has the same general thrust. Specifically, although a school's initial reading level is positively correlated with learning rates during all three periods, the correlation during summer (.38) is significantly larger than the average of the correlations during kindergarten (.16) and first grade (.12). This suggests that, although advantaged student bodies pull away all year round, they would pull away faster if not for schools.

The results also suggest that children who fall behind during summer tend to catch up when school is in session. This catch-up is reflected in the negative correlation between summer

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adjusted or unadjusted scores, however, we still find that summer learning is significantly and substantially slower than school-year learning.

learning rates and first grade (as well as kindergarten) learning rates. These negative correlations are evident at both the child level and the school level. If we view “summer setback” as a sign of disadvantage (Entwisle and Alexander 1992), we should view “school-year catch-up” as evidence that disadvantage is reduced by schooling.

To sum up, the estimates for model 1 suggest that schools

(1) increase average learning rates,

(2) reduce inequality (variation) in learning rates, and

(3) reduce the tendency for initial advantages to compound over time.

More simply, schools accelerate and equalize learning.

Model 2. Race, gender, and socioeconomic status

Model 1 captures total inequality, providing estimates of how much knowledge and learning rates vary across children and schools. In model 2, we relate a small amount of this variation to children's RACE, GENDER, and socioeconomic status (SES).

Specification

We use RACE, GENDER, and SES to predict each child's knowledge and learning rates α_{cs}:

$$\alpha_{0cs} = \gamma_{00} + \beta_{01s}\,\mathrm{GENDER}_{cs} + \beta_{02s}\,\mathrm{RACE}_{cs} + \beta_{03s}\,\mathrm{SES}_{cs} + b_s + a_c$$

$$\alpha_{1cs} = \gamma_{10} + \beta_{11s}\,\mathrm{GENDER}_{cs} + \beta_{12s}\,\mathrm{RACE}_{cs} + \beta_{13s}\,\mathrm{SES}_{cs} + b_s + a_c$$

$$\alpha_{2cs} = \gamma_{20} + \beta_{21s}\,\mathrm{GENDER}_{cs} + \beta_{22s}\,\mathrm{RACE}_{cs} + \beta_{23s}\,\mathrm{SES}_{cs} + b_s + a_c$$

$$\alpha_{3cs} = \gamma_{30} + \beta_{31s}\,\mathrm{GENDER}_{cs} + \beta_{32s}\,\mathrm{RACE}_{cs} + \beta_{33s}\,\mathrm{SES}_{cs} + b_s + a_c$$

Here the βs are fixed (not random) parameters for the child-level variables GENDER, RACE, and SES. GENDER is a binary variable indicating whether the child is a girl (1) or boy (0). RACE is a column of binary variables indicating whether the child is black, mixed race, Hispanics (any race), Asian, or Native American (including Alaskan or Hawaiian); the reference category is white non-Hispanics. SES is defined by ECLS-K as a composite of household income, parents' education, and parents' occupational status. Since SES was measured in both kindergarten and first grade, we averaged the two measurements to reduce the effect of measurement error; we then standardized SES to have a mean of 0 and a standard deviation of 1.[15]

Estimates and interpretation

Table 3 gives estimates for model 2's fixed effects γ and β, omitting the random effects b_s and a_c. Again we focus our discussion on reading; the results for mathematics are fairly similar.

Table 3 near here

The estimates suggest that schools are not entirely to blame for achievement gaps associated with RACE, GENDER, and SES. The most obvious evidence is that nascent gaps are already present before school begins. On the first day of kindergarten, Hispanic and Native American children are more than 2 points behind white children in reading skills, black children are .86 points behind, and Asian children are .70 points ahead. Girls start kindergarten .99 points ahead of otherwise similar boys. A standard deviation's advantage in SES predicts a 2.92 point advantage in initial reading skill.

The SES gap continues to grow after schooling starts, but it grows much more slowly when school is in session than when it is not. In reading, a standard deviation's advantage in SES predicts a relative gain of .16 points per month during summer, but only .07 points per month

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[15] The correlation between the two SES measures was .89, suggesting that at most $1-.89^2$=19% of the of the variation in measured SES was due to random measurement error. Unknown components were due to systematic measurement error and to true change between kindergarten and first grade.

during kindergarten and .05 points per month during first grade. The summer SES gap is .10 points per month larger than the average of the kindergarten and first grade gaps, suggesting that schools temper socioeconomic inequality.

Although schools tend to reduce socioeconomic inequality, the story for race inequality is not so heartening. Our results suggest that schools increase the reading gap between black and white children. The black disadvantage is .15 points per month during kindergarten, and .19 points per month during first grade, but during summer blacks have a (nonsignificant) *advantage* of .13 points per month. The summer gap is .29 points per month more favorable to blacks, suggesting that schools exacerbate black-white inequality.

We might expect a similar pattern for other disadvantaged ethnic groups, namely Hispanics and Native Americans, but the results for these groups are not convincing. The seasonal contrast for Hispanics looks vaguely like that for blacks, but it is smaller, less consistent, and not statistically significant. There is no clear seasonal contrast for Native Americans or for children of mixed race.

The pattern for Asian-Americans, however, is striking. Outside of school, Asian-Americans seem to have a substantial advantage over whites of similar SES. In reading, Asian-Americans begin school .70 points ahead, and during summer Asians learn .41 points per month faster than whites. But Asians and whites are roughly equal when school is in session. During kindergarten, Asian Americans learn just .12 points per month faster than whites of similar socioeconomic status, and during first grade Asian Americans learn .17 points per month *slower*. Compared to the summer learning gap, the school-year gap is .29 points per month less favorable to Asians, suggesting that schools temper the Asian advantage.

In short, our models suggest that schooling advantages white children relative to African- and Asian-American children of the same socioeconomic status. Note that the Asian result is not inconsistent with an equalization hypothesis. White students have an initial disadvantage relative to Asian students, and that disadvantage is reduced by schooling.

Figure 3 gives average trajectories for each race, assuming male gender and middling socioeconomic status (GENDER=0, SES=0). At the beginning of kindergarten, blacks are nearly a point behind whites, but more than a point ahead of Hispanics and Native Americans. During kindergarten, however, blacks fall further behind whites and become comparable to Hispanics and Native Americans. In first grade, blacks fall even further behind, while whites close about half the gap on Asian children. The patterns are quite different during summer, when whites lose ground to Asians, and perhaps to blacks as well.

Figure 3 near here

In addition to seasonal variation in RACE and SES gaps, we extend the seasonal approach to the GENDER gap as well. Like previous researchers, we find that girls lead boys in learning to read, though not in learning mathematics. To some observers, the gender gap suggests that girls are more compatible with the school environment. But our results do not support this conclusion. The gender gap in reading is already present on the first day of kindergarten, when girls start .99 points ahead of boys. Compared to boys, girls learn to read .11 points per month faster during kindergarten, .06 points per month faster during summer, and .03 points per month faster during first grade. On average, the gender gap in reading does not grow faster during school than during summer; instead, it seems to grow more slowly as children mature.

Model 3. Adjusting for differences in exposure

In a controlled experiment, children would be equal with respect to school and non-school exposures. The observed data depart slightly from this ideal. Children began kindergarten at different ages, and a few began before the fall of 1998. A few children attended summer school in 1999, and a few repeated kindergarten or skipped to second grade, instead of moving on to first grade. A few more were in ungraded classrooms in which the progress from kindergarten to first grade is a moot point. Students also differ in the intensity of their school exposure. About 40% of students attend half- rather than full-day kindergarten, and about 40% of children are absent one or more days per month. In addition, about 3% of children attend year-round schools where, roughly speaking, the usual 180 school days are parceled into eleven 16-day months rather than nine-and-a-half 19-day months.

We account for variation in school exposure by adding covariates to our knowledge and learning-rate equations.

Specification

For initial knowledge α_{0cs}, the equation is

$$\alpha_{0cs} = \gamma_{00} + \beta_{01s}\,\mathrm{AGE}_{cs} + \beta_{02s}\,\mathrm{PRIOR\ KINDERGARTEN}_{cs} + \beta_{03s}\,\mathrm{GENDER}_{cs} + \beta_{04s}\,\mathrm{RACE}_{cs} + \beta_{05s}\,\mathrm{SES}_{cs} + b_{0s} + a_{0c}$$

Here PRIOR KINDERGARTEN is a binary variable indicating whether the child was (1) or was not (0) in kindergarten before fall 1998. AGE on the first day of kindergarten is measured in months and is centered around its mean of 66 months (5½ years).[16] By mean-centering AGE, we can continue to interpret the intercept γ_{00} as average points on the first day of kindergarten.

For the kindergarten learning rate α_{1cs}, the equation is

$$\alpha_{1cs} = \gamma_{10} + \gamma_{11}\ \text{HALF-DAY}_s + \gamma_{12s}\ \text{DAYS SCHEDULED}_{cs} + \beta_{12s}\ \text{DAYS ABSENT}_{cs}$$
$$+ \beta_{13s}\ \text{GENDER}_{cs} + \beta_{14s}\ \text{RACE}_{cs} + \beta_{15s}\ \text{SES}_{cs} + b_{1s} + a_{1c}$$

(We use the symbol γ for school-level variables, and β for child level variables.) To begin with the school-level variables, HALF-DAY is a binary variable indicating whether the child was in half-day (1) or full-day (0) kindergarten.[17] A very few children were in half-day kindergarten only part of the year; for these children, HALF-DAY=½. DAYS SCHEDULED is the number of days scheduled for school per month of kindergarten, centered around an approximate mean of 19 days; this variable tends to have lower values for schools on year-round calendars, since most year-round schools spread the usual number of school days over a larger number of months. The new child-level variable, DAYS ABSENT is the number of days that the child missed school per month of kindergarten; the reference child is one with perfect attendance (DAYS ABSENT=0).

For the summer learning rate α_{2cs}, the equation is

$$\alpha_{2cs} = \gamma_{20} + \beta_{21s}\ \text{SUMMER SCHOOL}_{cs}$$
$$+ \beta_{22s}\ \text{GENDER}_{cs} + \beta_{23s}\ \text{RACE}_{cs} + \beta_{24s}\ \text{SES}_{cs} + b_{2s} + a_{2c}$$

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[16] AGE on the first day of kindergarten was estimated by comparing the first date of kindergarten to the ages measured on the first, second, third, and fourth test occasions.

[17] Strictly speaking, HALF-DAY is a classroom-level variable, which is problematic since our model includes no classroom-level error term. Unfortunately, including a classroom error is not practicable; our model already has three levels, and with a large data set a four-level model is impossible to estimate in SAS or HLM. Fortunately, 94% of the variation in HALF-DAY is between

where SUMMER SCHOOL is a binary variable indicating whether the child was (1) or was not (0) in school during summer 1999. Note that children in year-round school are not considered to be in SUMMER SCHOOL. Instead, the effect of year-round schooling shows up as a shorter SUMMER exposure and a lower number of school DAYS SCHEDULED per month.

For the first-grade learning rate α_{3cs}, the equation is

$$\alpha_{3cs} = \gamma_{30} + \gamma_{31s}\text{ DAYS SCHEDULED}_{cs} + \beta_{32s}\text{ DAYS ABSENT}_{cs} + \beta_{34s}\text{ NOT IN FIRST GRADE}_{cs} + \beta_{35s}\text{ GENDER}_{cs} + \beta_{36s}\text{ RACE}_{cs} + \beta_{37s}\text{ SES}_{cs} + b_{1s} + a_{1c}$$

where DAYS SCHEDULED and DAYS ABSENT are defined as they were for kindergarten. The new variable NOT IN FIRST GRADE is a binary variable indicating whether a child was in first grade (1) or not (0) during 1999-2000. Children who were not in first grade during 1999-2000 were either repeating kindergarten, skipping to second grade, or in an ungraded classroom. Very few children were NOT IN FIRST GRADE, and breaking these children into finer categories did not affect our key results.

Estimates and interpretation

Estimates are given in Table 3, Model 3. Our key estimates are substantially unchanged. The results continue to suggest that schooling increases the black-white gap, curtails the Asian-white and socioeconomic gaps, and has no effect on other race gaps or the gender gap. In addition, the control variables suggest that extra school days increase learning, and that missing school decreases learning—but that children who attend summer school or repeat kindergarten learn more slowly than children who do not. The summer-school result is discomfiting but not unprecedented (Heyns 1987), suggesting unobserved disadvantages that cause students to attend summer school.

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rather than within schools, so associating HALF-DAY with the school-level error is not unreasonable.

Similarly, repeating kindergarten may be a result of unobserved disadvantages; in addition, children who repeat kindergarten may have less to gain because much of the curriculum is familiar the second time through.

Model 4. Learning before kindergarten

In model 3, we used AGE to predict knowledge α_{0cs} at the beginning of kindergarten. This relationship between age and initial knowledge may be interpreted as the learning rate in the months immediately before kindergarten. For example, if a three-month difference in initial AGE predicts a one-point difference in initial knowledge α_{0cs}, then the pre-kindergarten learning rate would be estimated at one-third of a point per month.[18]

The pre-kindergarten learning rate is of interest because, like the summer rate, it suggests how quickly children learn outside of school. In model 4, we incorporate pre-kindergarten learning into our comparison of school- and non-school learning rates.

Specification

To estimate how pre-kindergarten learning varies with RACE, GENDER, and SES, we let these variables interact with AGE in the equation for initial knowledge α_{0cs}:

$$\begin{aligned}\alpha_{0cs} = \gamma_{00} &+ \beta_{01s}\,\text{AGE}_{cs} \\ &+ \beta_{02s}\,\text{PRIOR KINDERGARTEN}_{cs} \\ &+ \beta_{03s}\,\text{GENDER}_{cs} + \beta_{04s}\,\text{RACE}_{cs} + \beta_{05s}\,\text{SES}_{cs}\end{aligned}$$

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[18] Estimates of pre-kindergarten learning may be biased if age at entry depends on achievement. Specifically, pre-kindergarten learning rates would be underestimated if fast-learning children start kindergarten early, or if slow-learning children start kindergarten late. This bias seems to be modest, however, since the estimates change little when we exclude the youngest and oldest children.

$$+ \beta_{06s}\ \mathrm{AGE}_{cs}\ \mathrm{GENDER}_{cs} + \beta_{07s}\ \mathrm{AGE}_{cs}\ \mathrm{RACE}_{cs} + \beta_{08s}\ \mathrm{AGE}_{cs}\ \mathrm{SES}_{cs}$$

$$+ b_{0s} + a_{0c}$$

The other equations are the same as in model 3.

Estimates and interpretation

Estimates are given in Table 4. Average pre-kindergarten learning rates are about what we would expect. Before kindergarten, the reference group gains .28 points per month in reading and .38 points per month in mathematics. These rates are much slower than those observed during kindergarten and first grade, and comparable (at least in mathematics) to those observed during summer vacation.[19]

Table 4 near here

The pre-kindergarten race, SES, and gender gaps are also as expected. In the months before kindergarten, a standard deviation's advantage in SES predicts a .08 point per month advantage in reading and a .05 point per month advantage in mathematics. Girls learn to read .11 points per month faster than boys, on average, but do not have a significant advantage in learning mathematics. Race differences before kindergarten are nonsignificant but in the expected directions, with whites learning slower than Asians but faster (for the most part) than other races.

In previous models, we evaluated the effect of schooling by contrasting summer learning rates with the average of kindergarten and first grade learning rates. We can now extend this contrast to include the pre-kindergarten learning rates. To illustrate, consider the reading gap

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[19] For reading, the summer and pre-kindergarten rates are rather different, but this discrepancy may be an artifact. As remarked earlier, ECLS-K adjusted kindergarten scores upward in order to accommodate the shift from a 72-point test in kindergarten to a 92-point test in first grade. If this

between black and white learning rates. On average, black children fall behind by .16 points per month during kindergarten and .20 points per month during first grade. In the months before kindergarten, however, black children fall behind by just .05 points per month, and during summer vacation they pull ahead by .16 points per month. The two school-year gaps favor white children by an average of .18 points per month, whereas the two non-school gaps favor black children by an average of .06 points per month. The difference between the school and non-school gaps, .24 points per month, estimates the extent to which schools increase the black-white reading gap.

In general, the contrasts between school and nonschool learning rates (kindergarten and first grade vs. pre-kindergarten and summer) are much as they were before the pre-kindergarten learning rates were included. The contrasts suggest that schools increase average learning rates, increase black disadvantage, decrease Asian advantage, and decrease advantages associated with higher SES. The other sociological gaps—between girls and boys, between Hispanics and whites, and between Native Americans and whites—seem to be little affected by schooling.

“Unexplained” inequality

Models 2, 3, and 4 focus on the classic sociological dimensions of inequality: socioeconomic status, race/ethnicity, and gender. We emphasize, however, that little of the inequality in learning rates is related to these variables. To drive this point home, Table 5 summarizes the residual variation from Model 4. This is the school and child-level variation (in $\boldsymbol{b}_s$ and $\boldsymbol{a}_c$) that remains “unexplained” after we account for RACE, GENDER, SES, and assorted controls and interactions.

Table 5 near here

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adjustment was too large, then the summer learning rate is underestimated for reading. The true

For learning rates, the residual variances in Table 5 are just 1-8% smaller than those in Table 1—even though the model in Table 1 did not include RACE, GENDER, or SES. What this means is that RACE, GENDER, and SES explain just 1-8% of the variation in learning rates; that is, 92-99% of the inequality in learning occurs among children of similar RACE, GENDER, and SES. Although some of this unexplained variation may be attributed to omitted or mismeasured covariates, it seems unlikely that better-specified models could explain even the majority of variation. Children vary: even siblings raised in the same family can have quite different educational outcomes (Kuo and Hauser1997).

Since the bulk of inequality is unexplained by RACE, GENDER, and SES, it is important to ask whether this unexplained inequality is also reduced by schooling. The answer is yes. When we estimate contrasts for the residual inequality in Table 5, we get the same basic results as we did for the total inequality in Table 1:

(1) Learning rates are less variable during the school year than during summer vacation.

(2) During the school year, learning rates tend to be negatively correlated with initial status—a pattern that decreases inequality. By contrast, during summer vacation, learning rates tend to be *positively* correlated with initial status—a pattern that *increases* inequality.

In sum, the results suggest that schools go beyond reducing the inequality between students of different socioeconomic status and (less consistently) race. Schools also temper the much *greater* inequality between students of *similar* socioeconomic status and race.

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summer learning rate would be larger and probably closer to the pre-kindergarten learning rate.

Discussion

Our study helps clarify how schooling affects inequality. Past seasonal researchers have argued that the vast majority of inequality in cognitive skills emerges when school is not in session, and is likely a function of different family and neighborhood experiences (Heyns 1978; Entwisle and Alexander 1992, 1994). With substantially better data than previous researchers, we provide the strongest support to date for this position. We find not only that schools reduce inequality across socioeconomic status, but that schools reduce the much greater inequality that cannot be explained in terms of obvious ascribed characteristics. Schools generally do serve as great equalizers, therefore, but there is one important exception. The gaps in cognitive skills between blacks and whites grow faster than expected when school is in session, directing our attention to schooling as a source of black/white inequality.

Seasonal comparisons provide important leverage for assessing the relationship between schooling and inequality. Had we simply observed students' yearly gains we would have learned little about whether school or non-school factors were responsible for the socioeconomic and racial gaps that develop during the first two years of school. Instead, by splitting the gains into seasons, we were able to show that most gaps in cognitive skills grow more slowly when school is in session than when it is not.

With respect to socioeconomic status, the primary source of inequality lies in children's disparate non-school environments. The evidence for this claim is abundant in our data. First, the gap between high and low-socioeconomic children is substantial at the very beginning of schooling. Second, although schooling does not equalize high- and low-socioeconomic status children in the absolute sense, and although schooling does not necessarily ensure that they learn at the same rate when school is in session, schooling does reduce the rate at which inequality grows relative to a world without schools.[20]

To make this result more concrete, consider two hypothetical children with standardized SES values of -.66 and 1.67. The low-SES child has a household income of $40,000; his parents are high school graduates who work as a bartender and a garbage collector. The high-SES child has a household income of $100,000; his parents are a nurse with a B.S. degree and a lawyer with a J.D. Using estimates from our model 3, the SES difference between these children predicts a reading gap of 6.90 points on the first day of kindergarten, which widens to 8.44 points by the start of first grade. This widening reflects 9.5 months of divergence at the kindergarten rate and 2.5 months of divergence at the summer rate. In the absence of schooling, however, these children might diverge at the summer rate for all 12 months, in which case the gap at the start of first grade would be even greater—not just 8.44 points but 11.37 points. Although the gap does not *close* in school, it does not widen as fast as it otherwise might.

How do we reconcile our main conclusion—that schools reduces inequality—with the broad range of previous education research showing that advantaged students have better

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[20] Of course, separating school and non-school effects may be more complex than seasonal comparisons suggest. It is likely, for example, that events of one season "spill over" to affect cognitive gains in the next season. For example, children's summer learning may not be completely a result of "non-school" factors if advantaged schools more often send their children home for the summer after kindergarten with specific guidelines for summer learning and preparation for first grade. Although we cannot explore all of possible spillovers, ECLS-K provides indicators of whether a child's parents received any information from the school about how to prepare their child for first grade and/or summer booklists and reading assignments. Socioeconomically advantaged children are more likely to receive a summer booklist from their kindergarten school, but less likely to receive a preparation "package" for first grade than their disadvantaged counterparts. Importantly, neither of these school practices is related to cognitive gains during the summer. And other scholars have found no relationship between kindergarten teaching techniques and summer learning (Georgies 2003).

schooling experiences (e.g., attend better quality schools, are assigned to more favorable tracks, and receive more favorable treatment from teachers) than disadvantaged students? Our answer is that the disadvantages low-socioeconomic students face in the neighborhood and home are greater than the ones these same students face at school. In other words, as Figure 1 suggested, because the variation in "non-school" environments is larger than it is for school environments, a disadvantaged child can attend a disadvantaged school and yet still receive a greater boost from this experience than an advantaged child attending an advantaged school. To reduce inequality, therefore, our best bet would be to improve disadvantaged children's non-school environments, or increase their exposure to schooling through summer school or increased school days per year. Efforts to equalize school conditions and experiences, while not fruitless, would probably do less to reduce inequality.

Learning that schools serve as equalizers may not be comforting to all, however. The equalizing effects we observed could be a result of elementary school teachers devoting more of their energy toward increasing the proportion of children in their class who meet minimum skill requirements versus challenging those children with advanced skills. If schools equalize cognitive skills because they fail to challenge children with advanced skills, then equalization comes at a cost. We recognize, therefore, that one interpretation of our results is that schools are a culprit, but in a way that most sociologists would not have guessed.

Our study is limited to kindergarten and first grade, and so one might reasonably ask whether schooling's effect on inequality changes as children progress through the schooling system. Heyns's analysis of 5th-8th graders suggests that schools continue to operate as equalizers for several more years, but we know little about how school and non-school environments shape inequality in high school. If the equalizing patterns we observed among the kindergartners and first graders in our data are a function of teachers' greater emphasis on teaching basic rather than advanced skills, then this pattern could change in high school, where tracking separates students more distinctly by skill, and teachers' goals may be less egalitarian. Of course, it would be remarkable if the strong equalizing effect we observe among young children here is completely reversed in high school.

While schooling generally reduces gaps in cognitive skills between groups, the black/white comparison provides the most obvious exception. Although we found large black-white gaps in reading gains during school, there were no significant differences in cognitive gains during the summer months or the few months before kindergarten. These results are consistent with the view that disparate non-school environments are not the main source of the growth in the black/white gap in skills during the first couple years of schooling. Instead, something about schooling in the first few years leads black and white students down different paths. When considering how schooling influences skill gaps across socioeconomic status, we suggested that schools might equalize skills because teachers spend disproportionate energies helping students with modest skills. But if teachers acted in this way irrespective of race/ethnicity, black students would gain more (not less) from schooling than white students, and so this account does not help explain our race patterns.[21] An alternative possibility is that linguistic differences between students and teachers result in less than expected reading gains for black students (Entwisle and Alexander 1994). Again, this cannot be the whole story, however, because non-white Hispanic children, who

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[21] The fact that black and white children are in classrooms with different racial/ethnic compositions complicates this assessment. Still, if the overall process leading schooling to equalize skills across socioeconomic status applied irrespective of race, we would not expect to see evidence that schooling exacerbates the black/white gap.

are at least as likely as blacks to have linguistic differences with their teachers, learn at the same rate as whites during the school years.

These socioeconomic and racial/ethnic patterns, however, should be put in perspective. Although past research has focused on race, gender, and socioeconomic status, those dimensions account for just 1-8 percent of the inequality in learning rates. In this paper, we also looked at the remaining 92-99 percent—the “unexplained” inequality among students of the same race, gender, and status. Examining this unexplained inequality, we found that the vast majority of students learn at much more unequal rates during the summer than during the school year. This finding constitutes new and very strong evidence that schools are, indeed, great equalizers.

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Tables

Table 1. Measurement error variance on four reading tests and four math tests.

Occasion	Reading			Math		
	Total variance	Reliability	Measurement error variance	Total variance	Reliability	Measurement error variance
Fall 1998	73.62	0.93	5.15	50.55	0.92	4.04
Spring 1999	117.72	0.95	5.89	76.39	0.94	4.58
Fall 1999	160.53	0.96	6.42	92.35	0.94	5.54
Spring 2000	200.79	0.97	6.02	90.25	0.94	5.42

Note. Reliabilities were calculated by Rock and Pollack (2002) using item response theory. If the reliability is r and the total variance of a test is $Var\ (Y_{sct})$, then the measurement error variance is $(1-r)\ Var\ (Y_{sct})$.

Table 2. Model 1: knowledge and learning rates across schools and children

a. Scores on a 92-point reading test

	Grand	School level				Child level				Total
	mean	variance	correlations			variance	correlations			variance
			Initial points	Kinder-garten gains	Summer gains		Initial points	Kinder-garten gains	Summer gains	
Initial points, first day of kindergarten 1998	19.34***	3.83^{2}***				7.47^{2}***				8.39^{2}
Point gained per month, kindergarten 1998-1999	1.65***	0.38^{2}***	0.16***			0.78^{2}***	-0.08***			0.87^{2}
Points gained per month, summer 1999	-0.01	0.57^{2}***	0.38***	-0.32***		1.52^{2}***	0.18***	-0.06*		1.62^{2}
Points gained per month, first grade 1999-2000	2.52***	0.43^{2}***	0.12*	-0.04	-0.23**	0.94^{2}***	-0.12***	-0.08***	-0.30***	1.03^{2}
Contrast: Summer minus average of kindergarten and first grade	-2.09***	0.16**	0.24*			1.56***	0.29***			

b. Scores on a 64-point math test

	Grand	School level				Child level				Total
	mean	variance	correlations			variance	correlations			variance
			Initial points	Kinder-garten gains	Summer gains		Initial points	Kinder-garten gains	Summer gains	
Initial points, first day of kindergarten 1998	16.87***	3.42^{2}***				6.00^{2}***				6.91^{2}
Point gained per month, kindergarten 1998-1999	1.31***	0.25^{2}***	0.22***			0.60^{2}***	-0.11***			0.65^{2}
Points gained per month, summer 1999	0.51***	0.58^{2}***	0.13^	-0.46***		1.47^{2}***	0.15***	-0.38***		1.58^{2}
Points gained per month, first grade 1999-2000	1.54***	0.25^{2}***	-0.23***	-0.07	-0.35***	0.59^{2}***	-0.22***	-0.04^	-0.36***	0.64^{2}
Contrast: Summer minus average of kindergarten and first grade	-0.91***	0.27***	0.13			1.81***	0.31***			

$^{\wedge}p<.10$, $*p<.05$, $**p<.01$, $***p<.001$.

Note 1. "Total variance" is the sum of the school-level variance and child-level variance. The measurement measurement-error variance from Table 1 is not included.

Note 2. Contrasts are estimated in the usual way. Let $\boldsymbol{v}=(k\ s\ f)$ be a vector containing maximum likelihood estimates for kindergarten, summer, and first grade. The estimates can be mean learning rates, variances of learning rates, or correlations between learning rates and initial status. Let $\boldsymbol{c}=(-½\ 1\ -½)$ be the contrast vector. Then $\boldsymbol{cv}^{T}$ is a point estimate for the contrast between the summer parameter and the average of the kindergarten and first grade parameters. If $\boldsymbol{S}$ is the covariance matrix for the estimates in $\boldsymbol{v}$, then the point estimate $\boldsymbol{cv}^{T}$ has an asymptotically normal sampling distribution with a standard error of $(\boldsymbol{cSc}^{T})^{1/2}$.

Table 3. Models 2 and 3: knowledge and learning rates by race, gender, and socioeconomic status (SES)

	Reading		Mathematics	
	Model 2	Model 3	Model 2	Model 3
Points, first day of kindergarten 1998				
Reference group	19.29***	18.86***	17.70***	17.33***
standardized SES	2.92***	2.99***	2.37***	2.43***
Asian vs. white	0.70*	0.94**	0.10	0.39
Black vs. white	-0.86***	-0.61**	-1.90***	-1.65***
Hispanic vs. white	-2.09***	-1.92***	-2.39***	-2.10***
Mixed vs. white	-0.59	-0.41	-1.20***	-1.01**
Native vs. white	-2.30***	-2.04***	-2.44***	-2.17***
girl vs. boy	0.99***	1.24***	-0.04	0.19^
Age (months)		0.32***		0.39***
Child was previously in kindergarten		1.45***		-0.53^
Points per month, kindergarten 1998-9				
Reference group	1.61***	1.80***	1.37***	1.48***
standardized SES	0.07***	0.06***	0.05***	0.05***
Asian vs. white	0.13**	0.14***	-0.04	-0.04
Black vs. white	-0.15***	-0.17***	-0.18***	-0.19***
Hispanic vs. white	0.02	0.05	-0.06**	-0.05*
Mixed vs. white	0.05	0.06	-0.06	-0.05
Native vs. white	-0.02	0.00	-0.02	-0.01
girl vs. boy	0.11***	0.10***	-0.02^	-0.03*
Child was previously in kindergarten		-0.39***		-0.21***
Half-day kindergarten		-0.18***		-0.09***
Days scheduled per month		0.07***		0.05***
Days absent per month		-0.08*		-0.05^
Points per month, summer 1999				
Reference group	-0.08	-0.08	0.53***	0.53***
standardized SES	0.16***	0.16***	0.05	0.06^
Asian vs. white	0.41**	0.40*	0.36**	0.35**
Black vs. white	0.13	0.16	0.01	0.04
Hispanic vs. white	0.08	0.07	-0.02	-0.03
Mixed vs. white	0.05	0.06	-0.01	-0.01
Native vs. white	-0.09	-0.04	-0.16	-0.11
girl vs. boy	0.06	0.05	-0.03	-0.04
summer school		-0.32^		-0.40***
Points per month, first grade 1999-2000				
Reference group	2.57***	2.71***	1.56***	1.64***
standardized SES	0.05**	0.03^	-0.03*	-0.05***
Asian vs. white	-0.17**	-0.17**	-0.16***	-0.16***
Black vs. white	-0.19***	-0.20***	-0.07*	-0.08*

Hispanic vs. white	-0.13**	-0.11**	0.03	0.05
Mixed vs. white	-0.06	-0.05	0.00	0.01
Native vs. white	-0.10	-0.08	-0.08	-0.06
girl vs. boy	0.03	0.02	-0.01	-0.02
Days scheduled per month		0.14***		0.06***
Days absent per month		-0.07*		-0.04**
Not in first grade		-1.31***		-0.73***
Contrasts: Summer minus average of kindergarten and first grade				
Reference-group learning rate	-2.02***	-2.18***	-0.85***	-0.94***
Standardized SES	0.10*	0.12**	0.04	0.06
Girl vs. boy	-0.01	-0.01	-0.01	-0.01
Asian vs. white	0.43*	0.42*	0.46**	0.45**
Black vs. white	0.29*	0.34**	0.14	0.18
Hispanic vs. white	0.14	0.11	0.00	-0.03
Native vs. white	-0.03	0.00	-0.11	-0.08

^*p*<.10, **p*<.05, ***p*<.01, ****p*<.001.

Note. In model 2, the reference group is white non-Hispanic boys who are 5½ years old on the first day of kindergarten 1998. In model 3, the reference group also has perfect attendance, schedules an average number of school days per month, and progresses through school in the usual stages: 9½ months of full-day kindergarten, then 2½ months of summer vacation, then 9½ months of first grade.

Table 4. Model 4: learning rates before, during, and between the first two years of school.

	Reading	Math
Points per month, before kindergarten 1998		
Reference group	0.28***	0.38***
standardized SES	0.08***	0.05***
Asian vs. white	0.03	0.05
Black vs. white	-0.05	-0.04
Hispanic vs. white	-0.02	0.01
Mixed vs. white	-0.12	-0.20**
Native vs. white	-0.10	-0.03
girl vs. boy	0.11***	0.02
Points, first day of kindergarten 1998		
Reference group	18.87***	17.32***
standardized SES	2.95***	2.40***
Asian vs. white	1.01**	0.44^
Black vs. white	-0.60**	-1.64***
Hispanic vs. white	-1.94***	-2.12***
Mixed vs. white	-0.38	-0.98**
Native vs. white	-2.04***	-2.15***
girl vs. boy	1.21***	0.19^
Child was previously in kindergarten	1.71***	-0.40
Points per month, kindergarten 1998-9		
Reference group	1.80***	1.48***
standardized SES	0.06***	0.05***
Asian vs. white	0.14***	-0.04
Black vs. white	-0.16***	-0.19***
Hispanic vs. white	0.05	-0.05*
Mixed vs. white	0.06	-0.05
Native vs. white	0.00	-0.01
girl vs. boy	0.10***	-0.03*
Child was previously in kindergarten	-0.39***	-0.21***
Half-day kindergarten	-0.18***	-0.09***
Days scheduled per month	0.07***	0.05***
Days absent per month	-0.08*	-0.05^

Points per month, summer 1999		
Reference group	-0.08	0.53***
standardized SES	0.17***	0.06^
Asian vs. white	0.40*	0.35**
Black vs. white	0.16	0.04
Hispanic vs. white	0.07	-0.03
Mixed vs. white	0.06	-0.01
Native vs. white	-0.04	-0.11
girl vs. boy	0.05	-0.04
summer school	-0.32^	-0.40***
Points per month, first grade 1999-2000		
Reference group	2.71***	1.64***
standardized SES	0.03^	-0.05***
Asian vs. white	-0.17**	-0.16***
Black vs. white	-0.20***	-0.08*
Hispanic vs. white	-0.11**	0.05
Mixed vs. white	-0.05	0.01
Native vs. white	-0.08	-0.06
girl vs. boy	0.02	-0.02
Days scheduled per month	0.14***	0.06***
Days absent per month	-0.07*	-0.04**
Not in first grade	-1.31***	-0.73***
Contrasts: Average of summer and before kindergarten minus average of kindergarten and first grade		
Reference-group learning rate	-2.07***	-1.05***
Standardized SES	0.08**	0.06*
Girl vs. boy	0.00	0.03
Asian vs. white	0.23*	0.29**
Black vs. white	0.24**	0.14*
Hispanic vs. white	0.06	-0.01
Native vs. white	-0.03	-0.04

Table 5. Residual variances and correlations from Model 4

a. Scores on a 92-point reading test

	Reference	School level				Child level				Total	"R^2"
	mean	variance	correlations			variance	correlations			variance	
			Initial points	Kinder-garten gains	Summer gains		Initial points	Kinder-garten gains	Summer gains		
Initial points, first day of kindergarten 1998	18.87***	2.17^2***				7.06^2***				7.39^2	0.23
Point gained per month, kindergarten 1998-1999	1.80***	0.35^2***	-0.02			0.77^2***	-0.12***			0.85^2	0.05
Points gained per month, summer 1999	-0.08	0.55^2***	0.30**	-0.36***		1.52^2***	0.17***	-0.07**		1.62^2	0.01
Points gained per month, first grade 1999-2000	2.71***	0.37^2***	-0.23***	-0.16**	-0.22*	0.92^2***	-0.14***	-0.13***	-0.32***	0.99^2	0.08
Contrast: Summer minus average of kindergarten and first grade	-2.18***	0.17**	0.43**			1.60***	0.30***				

b. Scores on a 64-point math test

	Reference	School level				Child level				Total	"R^2"
	mean	variance	correlations			variance	correlations			variance	
			Initial points	Kinder-garten gains	Summer gains		Initial points	Kinder-garten gains	Summer gains		
Initial points, first day of kindergarten 1998	17.32***	1.67^2***				5.55^2***				5.80^2	0.30
Point gained per month, kindergarten 1998-1999	1.48***	0.23^2***	-0.19***			0.59^2***	-0.15***			0.63^2	0.05
Points gained per month, summer 1999	0.53***	0.56^2***	0.04	-0.53***		1.47^2***	0.15***	-0.40***		1.57^2	0.01
Points gained per month, first grade 1999-2000	1.64***	0.22^2***	-0.25**	-0.08	-0.29***	0.59^2***	-0.23***	-0.06**	-0.38***	0.63^2	0.03
Contrast: Summer minus average of kindergarten and first grade	-0.94***	0.27***	0.26^			1.82***	0.35***				

^$p<.10$, *$p<.05$, **$p<.01$, ***$p<.001$.

Note. "R^2" is the fraction by which total variance has decreased from model 1 to model 4. This is the proportion of variance that can be explained by race, gender, socioeconomic status, and the other variables in model 4.

Figure captions (titles in bold, explanatory footnotes in plain text).

Figure 1. How Unequal Schools Can Serve As Equalizers. Because non-school environments vary more than school environments, a child from a disadvantaged non-school environment can attend a disadvantaged school, yet still enjoy a greater school benefit than a child from an advantaged non-school environment, attending an advantaged school.

Figure 2. Estimating 3 seasonal learning rates (lines) from 4 test scores (dots). Reading tests (dots) were given in the fall and spring of both kindergarten and first grade. A naive “connect-the-dots” estimate of summer learning would compare the spring kindergarten score to the fall first-grade score (solid line). The naïve estimate is contaminated, however, since half the time between these tests is spent in school, not on summer vacation. To reduce contamination we extrapolate, in effect, to the reading scores that *would* be obtained at the beginning and end of summer (dashed line). The difference between these extrapolated scores is a more realistic estimate of summer learning.

Figure 3. Average learning rates for middle-class boys of 5 different races. When gender and socioeconomic status are held constant, the black-white gap opens up during the school year, not during summer vacation. But most other gaps (including many not shown here) grow fastest when school is not in session.

Figure 1. How Unequal Schools Can Serve As Equalizers

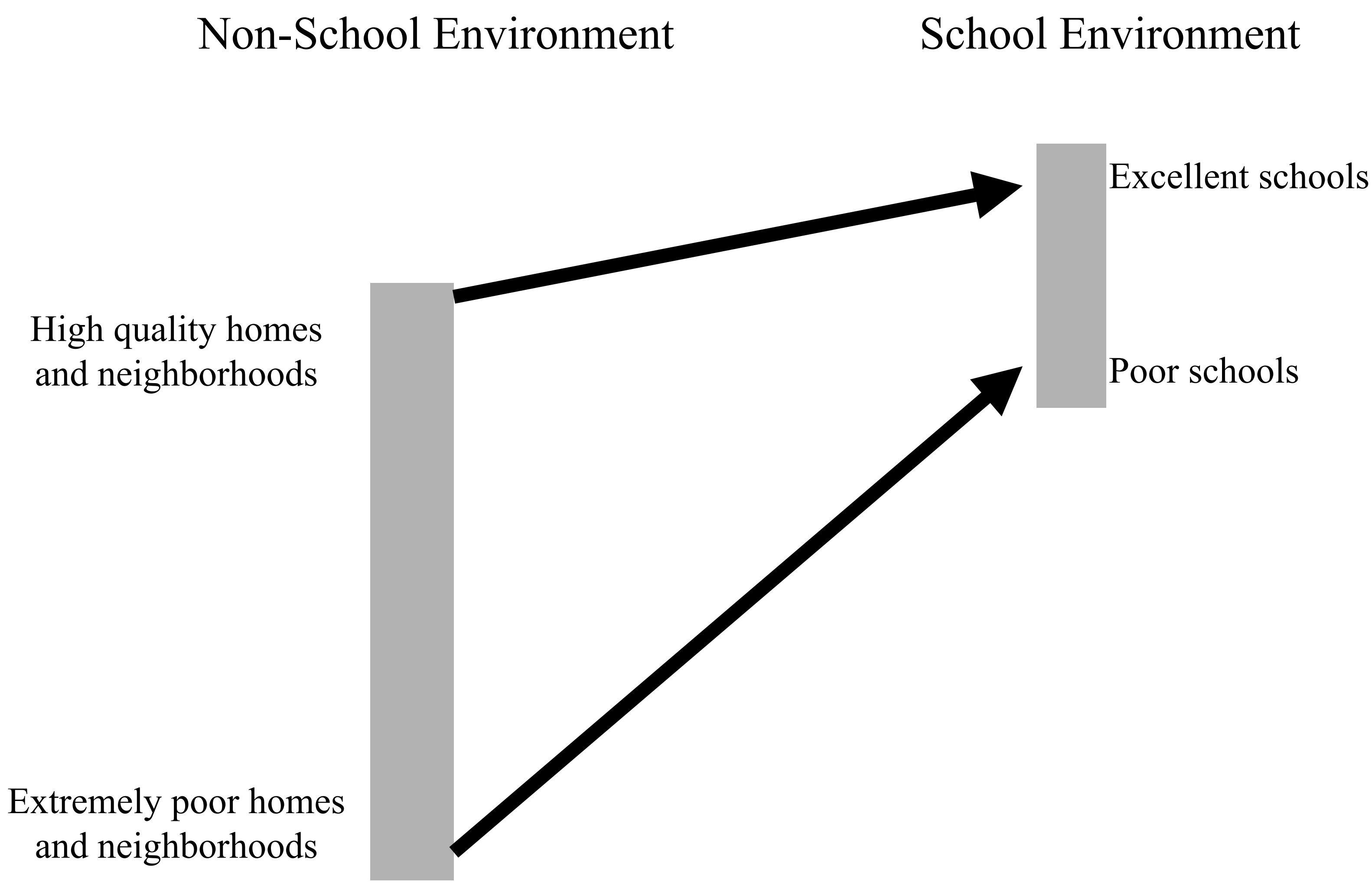

Figure 2. Estimating 3 seasonal learning rates (lines) from 4 test scores (dots)

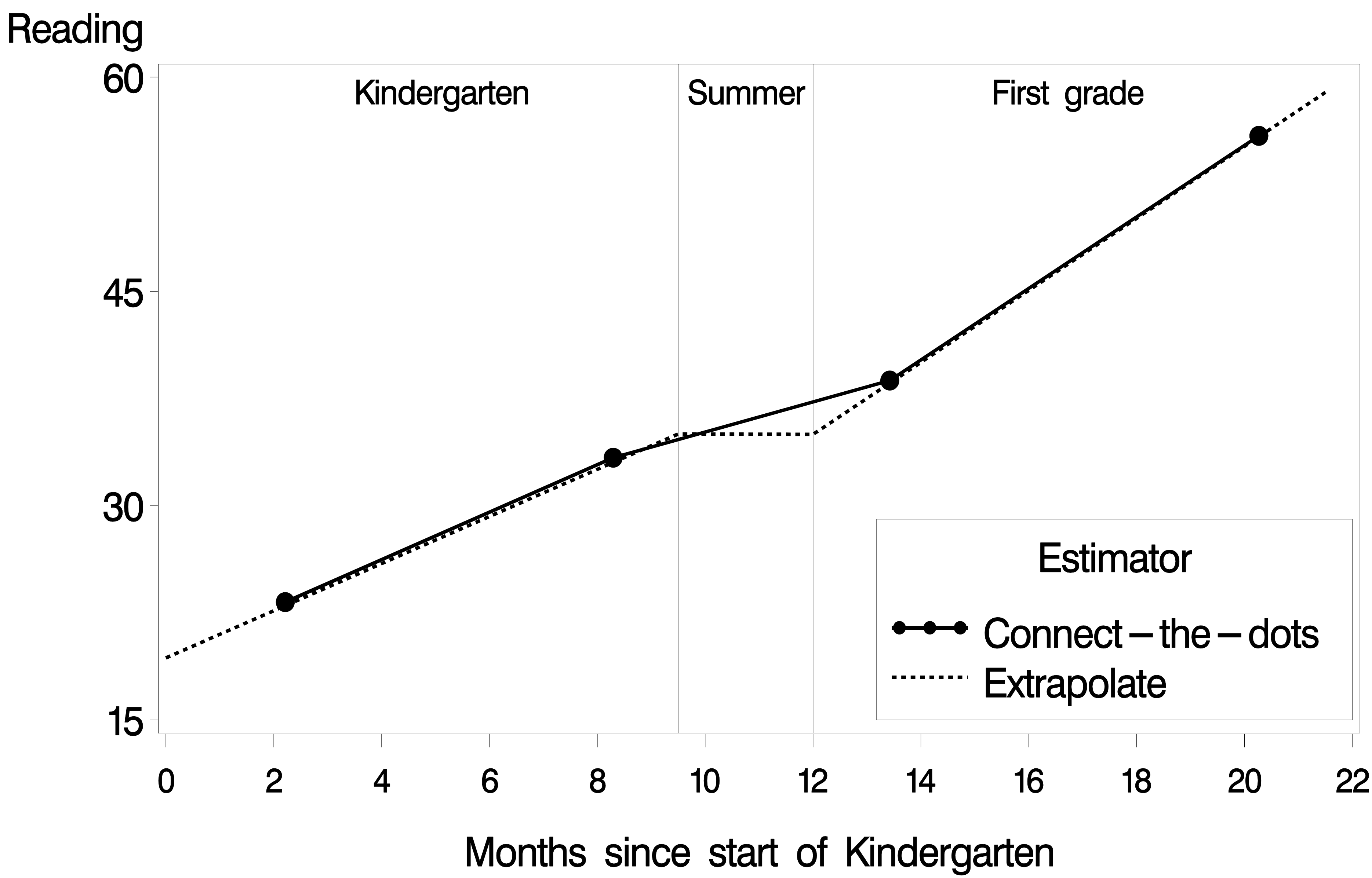

Figure 3. Average learning rates for middle−class boys of 5 different races