Exploring the Effects of School Context on Educational Outcomes: How Do Segregation and Sector Affect Educational Inequality in Elementary School?

DISSERTATION

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Abstract

For decades, sociologists of education have tried to determine the extent to which schools either promote social mobility or reinforce the stratification system. Wide-ranging research suggests that schools do both. Socioeconomically disadvantaged students and racial minorities have inferior educational outcomes in terms of test scores and graduation rates. It is possible that schools offer learning environments that produce unequal outcomes, but seasonal comparison research demonstrates that schools may actually serve to equalize educational opportunities, especially when compared to the resources and learning opportunities children have access to outside of school. This dissertation explores specific school contexts that could either promote or obstruct equal educational outcomes of students from disadvantaged social positions.

Using data from the Early Childhood Longitudinal Study – Kindergarten Class 1998-1999 (ECLSK), I explore how different educational contexts, in particular the racial composition of schools and school sector (Catholic versus public), affect math and reading learning rates in kindergarten, first grade, and the intervening summer. I use seasonal comparison analysis, multilevel modeling, and propensity score matching to estimate the effect of these school contexts and to overcome many of the methodological limitations of prior research.

This dissertation shows that the context of schooling plays a meaningful role in academic inequalities, but not necessarily in the ways that prior research would predict. Students in minority-segregated schools gain math skills at the same rate as students in schools with few racial minorities, but in the first grade, students in minority-segregated schools gain reading skills significantly slower than those students in schools with few racial minorities. However, when we take into account summer learning, black students experience the largest disadvantage compared to whites in schools with few racial minorities, but blacks experience no disadvantages compared to whites in minority-segregated or racially integrated schools. Regardless of school racial composition, black students tend to gain skills more slowly than whites during the school year but not during the summer. Latino student learning is largely unaffected by school racial composition.

When examining school sector, this research shows that students in Catholic schools experience a significantly smaller increase in their math learning rate than they would have experienced in public schools. Black students experience smaller increases in their learning rates in Catholic schools compared to public schools, and blacks also experience larger black-white gaps in Catholic schools than in public schools. Latino students, on the other hand, are better off in Catholic schools as they gain reading skills at a faster rate in Catholic schools than in public schools. Also, students from the bottom of the socioeconomic distribution experience significantly larger learning rate benefits from schooling than their high socioeconomic peers in public schools, but this is not true in Catholic schools. Overall, these results indicate that Catholic schools are neither more effective than public schools, nor are they more likely to reduce educational inequalities.

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Chapter 1: Introduction

Understanding how much schools shape social stratification is a critical task for sociologists of education. Sociological research has long shown that social origins affect academic achievement and that educational attainment plays an important role in occupational outcomes (Blau and Duncan 1967, Sewell, Haller, and Portes 1969). These relationships remain true today. Students from low socioeconomic backgrounds and racial minorities begin schooling with far fewer math and reading skills than students from high socioeconomic backgrounds and white or Asian students (Duncan and Magnuson 2011; Farkas 2011; Lee and Burkam 2002). Math and reading achievement gaps between rich and poor students have actually grown by as much as 50 percent in the past 4 decades (Reardon 2011). Partly as a result of their lower academic skills, racial minorities and children from low socioeconomic backgrounds are less likely to complete high school or graduate from college than white and Asian children and children from high socioeconomic backgrounds (Haskins 2007; NCES 2012). These educational inequalities have important consequences for patterns of social stratification because wages for college degree holders, and especially graduate/professional degree holders, have risen since the mid-1970s, but wages for workers with only a high school diploma have been stagnant (Haskins 2007; Hout 2012; Autor 2010). The linkages between social origins, educational attainment, and labor market outcomes compel scholars to

continue studying how schools shape patterns of educational outcomes and the extent of schools' role in intergenerational inequality.

A key concern for sociologists of education is whether schools are able to reduce the importance of social origins for life outcomes by giving every child an equal chance to gain the skills and credentials needed to succeed in the labor market. Are schools, as Horace Mann proposed, "the great equalizer of the conditions of men....the balance-wheel of the social machinery" that offer all children a reasonable opportunity for labor market success (Mann 1848: 669)? Or, do schools reinforce current social inequalities by promoting educational inequality? American schools have a long history of racial segregation and unequal treatment of students from disadvantaged backgrounds, such as class-based tracking and differential socialization (Bowles and Gintis 1976; Lucas 1999), so it seems unlikely that schools have been appropriately structured to provide the equal opportunities that Mann optimistically describes. Even after decades of reform efforts intended to desegregate schools, weaken rigid tracking systems, and leave "no child behind", inequalities in achievement by race and socioeconomic status persist. But are these inequalities the fault of schools or are they the fault of unequal opportunities outside of schools?

A significant body of research indicates that the structures of inequality in schools are associated with unequal schooling outcomes. Schools serving more low income students and racial minorities have fewer financial resources (Biddle and Berliner 2002; Condron and Roscigno 2003). And, teachers have lower educational expectations of racial minorities and socioeconomically disadvantaged students (Alexander, Entwisle and

Thompson 1987; Auwarter and Arguete 2008; and Roscigno 1998). Socioeconomically

disadvantaged students are less likely to be placed in high ability groups and academic

tracks (Haller and Davis 1980; Rist 1970; Lucas 1999). Such inequalities in resources

and teacher treatment affect patterns of academic outcomes to the detriment of minorities

and lower socioeconomic students (for examples see, Greenwald, Hedges, and Laine

1996; Jussim and Harber 2005; Roscigno 1998).

Despite inequalities within schools, non-school factors play a profound role in

educational inequality, and these factors explain a large portion of the educational gaps

among socioeconomic and racial groups. Importantly, students begin schooling with

sizable skills gaps in reading and math which develop in the non-school environment and

cannot be attributed to schools (Downey, von Hippel, and Broh 2004; Duncan and

Magnuson 2011; Lee and Burkam 2004). Coleman and colleagues (1966) and

subsequent follow-up studies (e.g. Jencks et al. 1972) found that observable school

characteristics, such as teacher-student ratios, teacher credentials, and school racial

composition, explained only a small portion of variation in academic achievement, while

non-school factors, such as parental income and education, explained a sizable portion of

that variation. Finally, studies using seasonal comparison techniques show that all

students, regardless of race or socioeconomic status, gain skills during the school year.

However, during the summer, socioeconomically disadvantaged students lose math and

reading skills, while socioeconomically advantaged students retain their skills and

possibly gain new skills (Alexander, Entwisle, and Olson 2007; Burkam et al. 2004;

Downey et al. 2004; Entwisle and Alexander 1992, 1994; Heyns 1978). While the gap in

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skills between socioeconomic groups may grow during the school year, it grows far more rapidly during the summer, suggesting that non-school factors play an important role in educational inequalities during the academic career (Downey et al. 2004).

Together, these two strands of research leave scholars with conflicting understandings of the role of schools in the stratification system. On one hand, schools exhibit numerous mechanisms that appear to constrain the educational success of students from disadvantaged groups. On the other hand, those school-based mechanisms may not be responsible for much of the inequality in educational outcomes. Given these seemingly incompatible patterns, it makes sense to explore more deeply the contexts in which schools are more or less compensatory for disadvantaged students. Perhaps these patterns in educational outcomes can coexist if schools produce reproductive educational outcomes in some contexts and compensatory outcomes in other contexts. For example, schools heavily populated by racial minorities could produce worse academic outcomes for black and Latino students than schools with few racial minorities. Or, academic outcomes of socioeconomically disadvantaged students might be more closely matched to their socioeconomically advantaged counterparts in Catholic schools than they are in public schools.

Given the possibility of varying effects across school contexts, scholars, educators, and policymakers need to understand the circumstances under which schools are more or less equalizing. Specifically, it is important to understand how certain school environments might produce favorable or unfavorable outcomes for students of different races or socioeconomic backgrounds. Prior research has explored the effects of school

context on differential learning outcomes, especially racial segregation and school sector, but the methodological limitations of this research and its mixed findings mean that we do not yet have a clear picture of whether there are circumstances under which schools are highly compensatory and circumstances under which schools are highly reproductive of current social positions.

To determine how the racial context of schools and school sector may affect educational inequality, I analyze a nationally representative sample of elementary school students with multilevel models, propensity score matching, and a seasonal comparison framework. This approach enables me to address the methodological limitations of prior research while adding nuance to our existing understanding of schools. The findings of these analyses will clarify the role of schools in society by determining if schools are the "great equalizer" in certain contexts but not in others.

I focus on school racial composition and school sector because these are two important educational contexts that have been widely studied for decades, yet our understanding of how these two educational contexts influence learning and academic inequality is incomplete. Many scholars believe that racially segregated schools are harmful to racial minorities, especially black students (c.f. Condron 2009; Cook 1984; Hanushek, Kain and Rivkin 2009; Roscigno 1998). But, most prior research has methodological limitations, especially failure to account for summer learning, and some evidence suggests that minorities attending minority-segregated schools actually experience academic and social-psychological benefits (Entwisle and Alexander 1992; Goldsmith 2004).

Similarly, the effect of Catholic schools on educational outcomes is unclear. Research on Catholic secondary schools often shows that these schools produce better academic outcomes than public schools including higher achievement, faster skill growth, and superior high school graduation rates (c.f. Bryk, Lee and Holland 1993; Coleman, Hoffer and Kilgore 1982a, 1982b; Hoffer, Greeley and Coleman 1985). Furthermore, Catholic secondary schools are particularly effective for racial minorities and socioeconomically disadvantaged students (Morgan 2001; Grogger and Neal 2000). Few studies have examined Catholic elementary schools, but extant research indicates that Catholic elementary schools are no more effective than public schools, and they do not produce better outcomes for racial minorities or socioeconomically disadvantaged students (Elder and Jepsen 2013; Jepson 2003; Lubienski and Lubienski 2006; Reardon, Cheadle and Robinson 2009). Catholic school research also has important methodological shortcomings, namely the challenges of dealing with selection bias into Catholic or public schools, unobserved variable bias, and a failure to account for summer learning, therefore scholars cannot confidently describe how Catholic schools shape educational inequalities.

This dissertation employs a stronger methodological approach than prior research to answer the following questions: Do schools with low proportions of minority students (i.e. white-segregated schools) promote faster learning than less segregated schools (i.e. racially integrated schools) or highly-segregated schools with high proportions of minority students (i.e. minority-segregated schools)? Do Catholic schools increase learning rates more than public schools? The findings of this dissertation illuminate the

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school contexts that are compensatory or reproductive of existing racial and socioeconomic inequalities in educational outcomes, and they provide education scholars a clearer understanding of the effects of these school contexts and how to appropriately measure their effects. Furthermore, the findings are important for policymakers and educators trying to improve educational outcomes for racial minorities and socioeconomically disadvantaged students. By knowing which school contexts are more helpful or harmful for disadvantaged students, we can target particular types of schools for interventions to improve learning or investigate processes that are helping disadvantaged students learn faster in certain school contexts.

This dissertation examines how school contexts affect student learning rates in math and reading, as measured by standardized achievement tests. Examining learning rates during the school year and the summer is critical for understanding how schools affect learning, and potentially inequality in life outcomes, for three reasons. First, math and reading learning rates are calculated from standardized achievement test scores. While some question the efficacy of achievement test scores and whether they truly capture student intelligence (McClelland 1973), a great deal of prior research demonstrates their importance for life outcomes. Status attainment theorists demonstrated that "mental ability" - measured by achievement test scores – is an important predictor of educational and occupational attainment (Sewell et al. 1969; Blau and Duncan 1967). Therefore, learning rates based on achievement test scores have important long-term consequences for life outcomes. Second, I examine learning rates instead of simple achievement test scores because seasonal comparison researchers argue

that cognitive skill inequality accelerates during the summer, not the school year (Heyns 1978; Entwisle and Alexander 1992; Downey et al. 2004). Therefore examining learning rates is necessary to really capture the effect that schools have on student learning. Finally, learning rates are an appropriate outcome of interest when examining school effects since a primary purpose of schools is to help children learn new skills (i.e. improve learning). Furthermore, learning rates are becoming increasingly important for understanding school effectiveness since President Obama is revising the No Child Left Behind law to focus not on overall achievement test scores but on value-added measures of schools (calculated by student learning rates) (U.S. Department of Education 2010).

The remainder of this dissertation is organized as follows: In Chapter 2, I use multilevel modeling to examine the how the racial and socioeconomic composition of schools shape racial inequality in math and reading learning in kindergarten, first grade, and the intervening summer. These analyses indicate that racial segregation has almost no effect on average math learning rates or racial gaps in math learning, but minority-segregated schools have significantly slower average reading learning rates in the first grade. However, it is difficult to be certain if racial segregation slows learning, or if unobserved factors correlated with racial segregation drive some or all of the observed racial segregation effect on first grade reading. I use the full set of seasonal results to explore this further. If school racial composition affects summer learning rates, then this suggests that attending a segregated school does not, in itself, lead to slower learning. Students in segregated schools may be similar in unobserved ways related to their selection into these schools that limit their learning. To understand this, I next examine

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how much schools increase student learning rates, net of their summer learning rates (i.e. the learning rate boost from schooling), in schools with different racial compositions. Evidence from these comparisons suggests that predominately white and Asian schools (with 75 percent or more white/Asian students) boost black students' reading skills less than they boost white students' reading skills, but this is not true in racially integrated schools or minority-segregated schools. These findings persist even after controlling for school sector and school socioeconomic composition. Importantly, regardless of the racial composition of schools, black students tend to gain skills slower than white students during the school year, but not during the summer. This suggests that something in the schooling process other than the racial composition of schools may be producing racial inequalities in learning rates. On the other hand, school racial composition appears to play almost no role in average Latino learning or Latino-white gaps. Overall, these analyses suggest that racial composition rarely plays an important role in average learning or racial inequality. When it does (i.e. first grade reading), additional analyses suggest that minority-segregated schools themselves do not promote slower average reading learning rates or black-white gaps, rather it is more likely that factors associated with attending a minority-segregated school drive these outcomes.

In Chapter 3, I compare the effectiveness of Catholic and public schools by estimating how much each school setting increases math and reading learning rates for first graders. To do this, I use propensity score matching and multilevel models to estimate the effect of school sector on *Student Impact*. *Student Impact* is the difference between the school year learning rate and the summer learning rate for each student, and

this method isolates the effect of schools on learning by removing the contribution of the non-school environment. These analyses indicate that Catholic schools are no more effective at increasing reading learning rates than public schools, but Catholic schools are significantly less effective at increasing math learning rates of first grade students. The difference in math effectiveness is particularly large in urban areas. The results also suggest that Catholic schools are generally not more compensatory than public schools. Black-white gaps (i.e. a black disadvantage) in math and reading *Student Impact* are significant in Catholic schools but not in public schools, and the Catholic school treatment effect is significant and negative for black students in both subjects. In addition, public school students from the bottom socioeconomic quintile experience significantly higher *Student Impact* in math and reading than students from the top socioeconomic quintile. This is not true in Catholic schools. Finally, Latino students appear to benefit from Catholic schooling in reading. Propensity score matching models show that Catholic schools have a significant positive treatment effect for Latinos in reading, while multilevel models suggest that Latinos in Catholic schools (but not in public schools) experience a marginally significant *Student Impact* advantage in reading compared to their white counterparts.

I conclude in Chapter 4 with a discussion of the key findings of this research and their implications for the effects of specific schooling contexts on educational inequality. I will make the case that school context plays a role in the compensatory effects that schools have on cognitive skills in the early grades. However, the schools that tend serve advantaged students (i.e. white-segregated schools and Catholic schools) are not

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necessarily the best learning environments for racial minorities or students from lower socioeconomic backgrounds in kindergarten and first grade. Overall, the schools that tend serve disadvantaged students (e.g. urban public schools, minority-segregated schools) appear offer a good learning environment for disadvantaged students to gain math and reading skills and where their learning is most similar to their more advantaged peers. Schools operating in such dire circumstances are able to overcome the challenges they face to provide the most compensatory learning environments for disadvantaged students.

Chapter 2: School Racial Segregation and Racial Inequalities in Learning Rates

Abstract: Educational inequality across racial groups remains sizable after decades of effort to eliminate them, and many scholars point to school segregation as an important driver of these inequalities. This research examines a nationally representative sample of 4,000 students from the ECLSK using seasonal comparison techniques and multilevel modeling to assess the effect of school racial composition on math and reading learning rates as well as racial learning rate gaps. I find (1) that school racial composition does not significantly affect math learning in kindergarten, first grade, or the intervening summer, (2) students in minority-segregated schools (where 75 percent or more of the study body is black or Latino) have significantly slower average reading learning rates in the first grade compared to predominately white/Asian schools, (3) school socioeconomic composition does not mediate the effects of racial composition on learning, (4) schools that are predominately white/Asian produce the largest black-white gap in reading, while minority-segregated school produce no black-white gap, and (5) Latino learning and Latino-white gaps are not meaningfully affected by school racial composition. These results hold true even after accounting for school sector and school socioeconomic composition. Overall, these results indicate that the racial segregation of schools does not play a major role in maintaining black-white or Latino-white cognitive skills gaps in kindergarten and first grade.

Introduction

Racial inequalities in education are persistent and well documented. Black and Latino children start school with fewer academic skills than whites and Asians (Lee and Burkam 2002). Black-white skill gaps expand throughout formal schooling, while Latino-white gaps shrink, but do not completely disappear (Duncan and Magnuson 2011; Farkas 2011; Fryer and Levitt 2004, 2006; Phillips, Crouse, and Ralph 1998). These skill gaps portend profound racial inequalities in educational attainment. For example, blacks and Latinos are less likely than whites and Asians to obtain a high school diploma or college degree (NCES 2012).

There are many explanations for racial differences in educational outcomes. Theories that emphasized racial differences in innate intelligence have been discredited (see Nisbett 1998 for a review), and education scholars now focus on differences in access to educational resources, such as books or computers, and learning opportunities, such as talking or reading with parents, as primary sources of educational inequality. Unequal access to important resources and opportunities in the home produce sizable gaps in academic skills in early childhood even before the start of formal schooling (Downey, von Hippel and Broh 2004; Entwisle and Alexander 1992; Hart and Risley1995; Lee and Burkam 2002).

While inequality in homes and communities is an important component of racial gaps in education, the broader contexts of schools, specifically the racial segregation of schools, may contribute to racial gaps in educational outcomes. Schools remain highly segregated by race. For example, 75 percent of blacks and Latinos attend schools where

the majority of students are minorities (Orfield, Kucsera and Siegel-Hawley 2012). Highly minority-segregated schools (i.e. schools with very few white or Asian students), especially those comprised of socioeconomically disadvantaged minorities, tend to have fewer financial resources, less qualified teachers, and inferior school facilities, which may hinder academic achievement (Biddle and Berliner 2002; Clotfelter, Ladd, and Vigdor 2004, 2006; Condron and Roscigno 2003; Greenwald, Hedges, and Laine 1996). On average, minorities in minority-segregated schools learn less than minorities enrolled in racially integrated or predominately white schools (Condron 2009; Cook 1984; Hanushek, Kain, and Rivkin 2009; Murnane, Willett, Bub, and McCartney 2006), and this has long-term consequences. Students who attend minority-segregated elementary and high schools are less likely to enroll in or complete college (Crain and Mahard 1978).

One major challenge of determining whether racial segregation is harmful to academic achievement is that school racial composition is strongly correlated with many other factors that could explain lower academic achievement in minority-segregated schools, namely socioeconomic status. Since race and socioeconomic status are intertwined, it is possible that the socioeconomic composition of schools, not the racial composition, may be the cause of educational inequality across schools (Jencks 1972; Roscigno 2000). In order to fully understand the role of school racial composition, research must also account for the influence of school socioeconomic composition.

Another important challenge to this research is the fact that students spend a lot of time outside of school, both during the school year and during the summer, and students learn at very different rates while they are out of school. Scholars must disentangle the

contributions of school and non-school environments to understand the importance of school racial composition, but prior research examining school racial composition rarely makes that effort. Therefore, our understanding of how school racial segregation affects learning is limited and potentially incorrect.

Separating the effects of school and non-school factors is critical because school assignment is largely determined by where students live. Where a person lives is the result of myriad observed and unobserved factors, including race, socioeconomic status, housing discrimination, schooling preferences, job location, social networks, and so forth (for discussions, see Leventhal and Brooks-Gunn 2000; Sampson, Morenoff and Gannon-Rowley 2002). Students are clustered in neighborhoods based, in part, on many educationally salient characteristics, and they are therefore enrolled in schools in non-random ways. If scholars fail to account for the observed and unobserved characteristics that affect both the placement of students within schools and learning opportunities outside of schools, then their conclusions about school context effects may be driven by spurious relationships. It is possible that attending a segregated school does not, in itself, lead to lower academic performance. Rather, students in segregated schools may be similar in unobserved ways that limit their academic success. If this is true, then examining both the school and non-school environments is critical in order to make convincing claims about the effects of racial segregation on student outcomes.

Seasonal comparison analysis can improve our understanding of how racial segregation influences academic outcomes because it examines school year learning and summer learning separately. By separating school year learning from summer learning,

one can explore how students learn while they are in school (what prior research typically observes) and out of school (what prior research fails to observe). Examining seasonal learning patterns provides useful information about the effects of school and non-school environments, and allows us to account for observed and unobserved predictors of student learning.

Using data from the Early Childhood Longitudinal Study - Kindergarten Class 1998-1999 (ECLSK), a nationally-representative sample of kindergarteners, this paper applies seasonal comparison techniques to study how school racial segregation shapes learning. This research examines the effects of school racial composition on average math and reading learning rates as well as racial differences in math and reading learning rates over the course of kindergarten, first grade, and the intervening summer. The results lead to a better understanding of how school racial composition affects learning and inequality by accounting for the effects of school and non-school environments. Furthermore, this paper examines whether school racial composition has an independent effect on learning, net of school socioeconomic composition. The study will answer the following questions: First, how does the racial composition of schools affect learning rates in math and reading for all students? More specifically, do white-segregated schools -with low proportions of minority students - increase learning rates more than racially integrated schools or minority-segregated schools - with high proportions of minority students? Second, how does attending a minority-segregated or racially integrated school affect racial gaps in math and reading learning rates during the school year and the summer compared to attending a white-segregated school? Third, does

school racial composition affect educational outcomes net of school socioeconomic context? Finally, do racial learning gaps differ across schools with different levels of racial segregation?

Literature Review

Racial gaps in educational achievement remain large, and progress toward reducing these gaps has been uneven over the past four decades (Jencks and Phillips 1998; Magnuson and Waldfogel 2008). Racial minorities, particularly blacks and Latinos, have lower achievement test scores than whites and Asians from kindergarten through high school (Magnuson and Waldfogel 2008; Duncan and Magnuson 2011; Farkas 2011). Standardized test scores, and the skills they measure, are critical for reaching important educational milestones. Partially as a result of their lower academic skills, black and Latino students are less likely to complete college. Nearly 40 percent of whites and 57 percent of Asians obtain a bachelor's degree by age thirty, but only 20 percent of blacks and 13 percent of Latinos complete a bachelor's (NCES 2012). A college degree has large economic returns, including higher wages, greater occupational mobility, and less unemployment as well as large quality of life returns, like better health and greater family stability (Haskins 2007; Hout 2012). Therefore, racial inequalities in education have large implications for ongoing racial stratification more broadly.

Numerous explanations of racial inequalities in education have been offered to describe the relatively poor academic performance of minorities. A great deal of evidence suggests that the non-school environment heavily contributes to educational inequalities. Blacks and Latinos begin kindergarten approximately one-half of a standard

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deviation behind whites in math and reading skills (as measured by standardized tests), and most of this gap is explained by racial differences in socioeconomic status and access to learning resources and activities prior to kindergarten (Lee and Burkam 2002). Seasonal comparison research shows that class-based skill gaps expand at greater rates during the summer than the school year and overall learning rate inequality is smaller during the school year (Downey, von Hippel, and Broh 2004; Entwisle and Alexander 1992, 1994; Heyns 1978). Finally, Coleman and colleagues (1966) concluded that non-school factors played a much more prominent role than school factors in explaining racial educational inequality.

Black-white skill gaps increase throughout the academic career, while Latino-white gaps shrink slightly in elementary school, although they never completely disappear (Duncan and Magnuson 2011; Farkas 2011). Black students, and to a lesser extent Latinos, gain skills at slower rates than whites during the school year, but blacks, whites, and Latinos learn at similar rates during the summer (Downey, von Hippel, and Broh 2004). This suggests that blacks and Latinos are disadvantaged during the school year when both school and non-school factors shape learning, but they are not disadvantaged during the summer when non-school experiences dominate learning. In other words, if schools promoted learning equally for all racial groups, then we would expect black and Latino students to gain skills at faster rates during the school year considering their summer learning rates. The fact that black and Latino students learn slower than whites during school suggests that schools unequally promote learning. There is also evidence that inequality between schools plays an important role in racial

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gaps in education, especially the expansion of black-white gaps (Coleman et al. 1966; Condron 2009; Fryer and Levitt 2004, 2006; Phillips, Crouse, and Ralph 1998). Along these lines, Condron (2009) argues that schools might reduce learning inequalities across socioeconomic groups, but they might simultaneously exacerbate educational inequalities across racial groups. Simply, schools do a better job at teaching white students than minorities, and this might contribute to racial educational inequality.

It is easy to point to racial segregation as a cause of racial inequality, yet evidence of the effects of racial segregation on student learning is mixed and the quality of this research is wide-ranging and often methodologically flawed. Much of the prior research lacked longitudinal data or did not use appropriate control variables (see St. John 1970 for a review). More recent studies with access to better, longitudinal data failed to account for unobserved non-school factors that contribute to inequalities in learning (see for example, Caldas and Bankston 1998; Roscigno 1998; Condron 2009), which the current paper addresses by using seasonal comparison analysis. A more detailed discussion of the flaws of past research and the benefits of using seasonal comparison appears in the section "*Modeling Strategies and Shortcomings in Prior Research on Racial Segregation*" below, but the methodological flaws of prior research and uncertainty about causal mechanisms make our understanding of role school racial segregation murky at best.

Racial Segregation in Schools and Educational Inequality

The 1954 *Brown v. Board of Education* decision outlawed government sponsored "separate but equal" schooling, calling such racially segregated schools "inherently

unequal." Several subsequent rulings by the US Supreme Court between 1955 and the early 1970s clarified and strengthened *Brown v. Board*, and school integration made steady progress throughout the 1960s and 1970s, especially in the South (Orfield and Eaton 1996). From the mid-1970s to the mid-1990s, Supreme Court decisions slowly limited the effectiveness of *Brown v. Board* and made it more difficult for some districts to desegregate. For example, districts now have the ability to obtain exemptions from court-supervised desegregation efforts, opening the door to unimpeded re-segregation of schools in exempted districts (Orfield and Eaton 1996). Partially as a result of the restrictions placed on *Brown v. Board,* public schools remain racially segregated. Recent research suggests that schools are actually becoming *more* segregated, not less (Orfield and Eaton 1996; Vigdor and Ludwig 2008). Approximately three-fourths of black and Latino students attend schools in which at least half of the students are racial minorities, while approximately 40 percent of blacks and Latinos attend schools in which 90 percent or more of the students are racial minorities (Orfield, Kucsera and Siegel-Hawley 2012). If school racial segregation limits educational achievement for minorities, then a large proportion of America's black and Latino children are at risk.

School racial segregation is linked to unequal experiences and opportunities for racial minorities. White and minority students often attend different schools, which could explain why it appears that schools do not teach minorities as well as whites. Racial minorities, especially poor minorities, attend schools that generally have fewer financial resources and inferior facilities, and having fewer school resources significantly decreases academic achievement (Biddle and Berliner 2002; Condron and Roscigno

2003; Greenwald, Hedges, and Laine 1996; Kozol 1991). Minority-segregated schools are more likely to have inexperienced teachers. In studies of North Carolina schools, Clotfelter and colleagues found that school districts were more likely to assign novice teachers to schools with higher proportions of black students, while teachers with better qualifications, such as more experience and better test scores, were more likely to work in schools with more advantaged student bodies (Clotfelter, Ladd, and Vigdor 2005, 2006). Teacher experience and teacher tests scores are some of the few known characteristics associated with teacher effectiveness (Clotfelter et al. 2006, 2007; Rivkin, Hanushek, and Kain 2005).

Teachers tend to have lower educational expectations of minorities, and teacher expectations have a small but significant effect on educational outcomes (Lee and Eccles 1992; Lee and Harber 2005; Roscigno 1998). Schools with more minority and low-income students have lower math skills and higher prevalence of behavioral problems, on average, which can affect individual student learning (Duncan and Magnuson 2011). Disruptive student behaviors may lead to less teaching time and fewer learning opportunities in the classroom and ultimately lower classroom achievement (Carrell and Hoekstra 2010; Duncan and Magnuson 2011; Figlio 2005). In a study of Texas elementary schools, Hoxby (2000) found that classroom peers affect how much students learn. In classrooms where students had lower achievement scores, on average, all students subsequently learned less, and this effect was stronger for blacks and Latinos than whites. The fact that blacks and Latinos are more likely to be in classes with more minorities with lower average achievement scores, especially in minority-segregated

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schools, means that minorities will likely continue to learn less than their white peers. This accumulation of disadvantages that blacks and Latinos are more likely to experience in minority-segregated schools could limit their learning and promote skill deficits relative to whites.

While the majority of prior research suggests that attending a segregated school results in multiple disadvantages, there is some evidence that school racial segregation enhances the educational experiences of racial minorities. Students in segregated schools are more likely to have same-race teachers, which can lead to better academic performance and more favorable teacher assessments of student behavior (Dee 2004, 2005; Downey and Pribesh 2004). Relative deprivation theory would suggest that students in racially homogeneous schools may experience less relative deprivation if they are treated similarly to their same-race peers. Minorities in racially heterogeneous schools, or predominantly white schools, may experience greater relative deprivation if they compare themselves to whites within their schools. Minorities in minority-segregated schools could avoid some of the potentially harmful social-psychological effects that they might experience in schools with very few other racial minorities (Davis 1966; see also Mayer 2002 discussing neighborhood effects). Goldsmith (2004) offers some support for this explanation. He found that black and Latino students in minority-segregated schools had greater pro-school attitudes than racial minorities in racially integrated or predominantly white schools (Goldsmith 2004).

As prior work demonstrates, numerous processes simultaneously operate within schools that produce both harmful and potentially beneficial outcomes for racial

minorities in minority-segregated and racially integrated schools compared to schools where the vast majority of students are white (white-segregated schools). This study focuses on how school racial composition affects learning rates. Therefore, I look broadly at the net effect of school racial composition on learning rates for various racial groups. Many mechanisms are likely occurring within schools (and the non-school environment) to produce these net effects. While this study is unable to pinpoint the mechanisms at work, the goal is to answer the broad question: how does attending a minority-segregated school (compared to white-segregated or racially integrated school) affect average learning rates and racial gaps in math and reading learning? To answer this, I consider both school and non-school environments as well as the most likely competing explanations of why racial differences in learning might exist - socioeconomic composition.

Is Racial Segregation Really Socioeconomic Segregation in Disguise?

Sociologists recognize that race and class are intertwined in important and complex ways, and this is evident within schools. Nearly 90 percent of minority-segregated schools are high-poverty schools with more than half of their students eligible for free or reduced-price lunches. In contrast, less than 15 percent of low-minority schools are high-poverty schools (Orfield and Lee 2005). Minority-segregated schools are far more likely to serve low-income students than schools with few minorities. Because of the strong correlation between race and socioeconomic status, school segregation research must disentangle the effects of school racial composition from those of school socioeconomic composition. Failure to parse these effects can lead to

inaccurate conclusions about the impact of racial segregation, yet prior research has not always carefully considered school socioeconomic composition.

A classic example of the failure to disentangle racial from socioeconomic segregation is the *Coleman Report*. Coleman and colleagues (1966) found that black students attending minority-segregated schools had worse academic skills than black students attending schools with fewer minorities, suggesting racial integration improves academic skills for minorities. Yet, Coleman did not account for the socioeconomic makeup of the schools. Jencks and colleagues (1972) re-analyzed the data from the *Coleman Report* and found that black students benefitted from attending racially integrated schools only when those schools were comprised of middle-class students. Blacks attending racially integrated schools with lower-class peers did not benefit (Jencks et al. 1972).

Recent research that attempted to disentangle the effects of school racial and socioeconomic segregation produced mixed results. Caldas and Bankston (1998) and Roscigno (1998) found that school racial and socioeconomic composition significantly affected black-white test score gaps. Rumberger and Palardy (2005) found that school socioeconomic composition affected achievement growth during high school, but the racial composition of schools had no significant influence. Condron (2009) found that black-white differences in first grade math and reading learning rates were explained by school racial composition but not school socioeconomic composition. While these studies all have methodological limitations (to be discussed), the mixed results beg the question: does racial segregation have an independent or spurious effect on educational

outcomes. In other words, does racial segregation affect student learning rates, net of

school socioeconomic composition?

The Importance of the Non-School Environment for Understanding What Happens in

Schools

Most research examining school racial segregation cannot rule out the possibility

that non-school factors, such as experiences in homes or neighborhood segregation, that

are correlated with school segregation are actually causing educational inequality - rather

than segregation itself. Children tend to go to schools in their neighborhoods, and

residential location is shaped by many factors. This makes it difficult to determine

whether school segregation or other factors drive educational inequalities. It is possible

that the non-school environment affects learning inequality as much as the school

environment. For example, Card and Rothstein (2007) found that neighborhood racial

segregation, but not school racial segregation, negatively affected black-white SAT score

gaps. While Entwisle and colleagues (1994) found that neighborhood resources affected

boys' math learning in early grades net of school racial composition.

While integrated schools may appear more effective for minorities than

segregated schools, learning that happens outside of schools may account for the apparent

effectiveness of integrated schools. Entwisle and Alexander (1992, 1994) show that

minorities attending minority-segregated elementary schools fall behind minorities

attending racially integrated elementary schools because students in minority-segregated

schools tend to lose more math and reading skills during the summer - when learning is

primarily driven by non-school learning resources. During the school year, minorities in

minority-segregated schools learn about as much minorities in integrated schools. These findings suggest that factors outside of school, such as home or community characteristics, may be particularly important in shaping educational disadvantage. Entwisle and Alexander's research also highlights the importance of modeling strategies.

As previously noted, substantial learning occurs prior to the start of schooling and during the summer when school is out, and this learning is not equal for all groups. When education researchers do not separate school-supported learning from learning that is driven by the non-school environment, they fail to account for the separate contributions of each environment to learning. To illustrate this point, consider that students in a disadvantaged neighborhood might have access to a community center with high quality summer programs that bolster student learning in several ways. For example, they might increase students' exposure to school-like environments or build neighborhood collective efficacy through interactions of parents with each other and community leaders, which in turn, builds students' social capital. We might measure higher levels of student learning in schools in this neighborhood, but the higher learning is actually attributable to community center programs and not schools. Of course, researchers often do not have access to detailed data on neighborhoods, let alone information on specific programs, so we often cannot distinguish the effects of school and non-school factors. However, separating school from summer learning provides us with a clearer picture of how schools shape learning.

Modeling Strategies and Shortcomings in Prior Research on Racial Segregation

Prior research on racial segregation often ignores the potential influence of school socioeconomic composition as well as the importance of non-school factors on educational outcomes. Furthermore, existing findings are inconsistent or contradictory. As a result, there are still questions about the effects of school racial segregation on student outcomes. Early school segregation research, which focused on desegregation efforts in the late 1950s and 1960s, was plagued with methodological problems, such as the lack of comparison groups, the lack of statistical controls, self-selection bias, and cross-sectional designs (see St. John 1970 for a review). As a result, conclusions from early studies on segregation effects are dubious. A handful of quasi-experimental studies found that black students who were bused to integrated schools had slightly higher academic skills than their counterparts who remained in segregated schools (St. John 1970). Meta-analyses of higher quality studies found that attending integrated schools had a small positive effect on minority reading achievement (Cook 1984). The benefit was equivalent to between .5 and 1.5 months of additional skills growth, but there was almost no effect on math achievement (Cook 1984). More recent research (discussed earlier) has taken advantage of better data and research designs to measure the effects of racial segregation in schools, but this research also has notable limitations.

Modeling approaches used to study the effects of racial segregation on student outcomes have improved over time, but this research should also incorporate the potential influence of unobserved non-school factors. Some studies have used cross-sectional data with standardized test scores, but these studies did not to take into account accumulated

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educational disadvantages that occur earlier in the life course both in school and out of

school (see Caldas and Bankston 1998; Roscigno 1998). Rumberger and Palardy (2005)

used a longitudinal design to examine the effects of segregation on skill growth in high

school, controlling for 8th grade achievement, but this approach still fails to control for

learning differences that occur during intervening summers (Downey et al. 2004).

Condron's (2009) study examined learning over the course of a school year (from fall to

spring of first grade), and found that school-level factors, especially racial segregation,

explained more of the black-white gap in math and reading learning than family

background. Based on these findings, he concluded that school-level factors were the

most prominent cause of black-white achievement gaps during the school year.

However, because this study does not also examine how much students learn during the

summer (i.e. in the non-school environment), it cannot rule out the possibility that

unobserved variables in the non-school environment that are correlated with school

segregation and educational achievement, such as neighborhood segregation or shared

characteristics or practices of individuals within neighborhoods, are actually driving

racial learning gaps.

Economists have used "peer effect" designs to study racial segregation. Peer

effect models capture the amount of learning spillover that occurs from "good" peers -

those who have higher achievement scores and produce positive learning spillovers to

their classmates - and "bad" peers who have the reverse effect. This research generally

shows that higher percentages of racial minorities in schools and classrooms have a

negative effect on minority academic achievement; however the effects on white student

achievement are smaller or trivial (Angrist and Lang 2004; Hanushek, Kain, and Rivkin 2009; Hoxby 2000). Minority students are more likely to have "bad" peers because they are in primarily minority-segregated and socioeconomically disadvantaged schools. However, peer effect models have the same difficulty measuring non-school environments as other prior research, despite the use of advanced modeling techniques, because they fail to distinguish summer from school-year learning.

Seasonal Comparison Analysis

Seasonal comparison research can overcome shortcomings of prior modeling strategies by examining the effects of school segregation on learning rates during the school year and the summer separately. By separating learning into seasons, this approach examines learning when both school and non-school factors affect learning (i.e. during the school year) and when primarily non-school factors shape learning (i.e. during the summer). Seasonal comparison therefore offers a better way to understand the effects of school segregation on academic outcomes.

To demonstrate the insight that seasonal comparison analysis offers, consider two scenarios. In Scenario 1, black students gain reading skills slower than white students during the summer, when non-school factors drive learning, and it is reasonable to conclude that black students have non-school learning opportunities and resources that are less enriching than those of white students. During the school year, black students gain reading skills slower than white students, when both school and non-school factors drive learning. Therefore, some might conclude that black students have access to a less enriching combination of school and non-school learning experiences compared to white

students. In Scenario 1, it is difficult to conclude that schooling is the primary cause of relatively slow black learning because black students' non-school environment continues to be less supportive of learning during the school year. Without information about summer learning, however, schools would appear to be less effective for blacks.

In Scenario 2, black students gain reading skills faster than white students during the summer, and it is reasonable to believe that black students have non-school resources and experiences that are more enriching than those of white students. During the school year, black students gain reading skills slower than white students. Considering the non-school learning resources and experiences of black and white students, one could argue that black students in Scenario 2 under-perform during school while white students over-perform during school. If one assumes that the non-school learning experiences of black students remain more enriching than those of white students during both the summer and school year, then it appears that black students' school experiences are less enriching than those of white students. Schools in Scenario 2 appear to be serving white students better than black students, but this is only clear when we have seasonal information.

This study uses seasonal comparison techniques to examine how school racial segregation affects learning rates during kindergarten and first grade as well as the intervening summer. This research improves upon prior school racial segregation research by examining its relationship with school year and summer learning, and it improves upon Entwisle and Alexander's work (1992, 1994) by using a nationally representative dataset and more sophisticated statistical techniques that produce more precise estimates of summer learning rates. Like Entwisle and Alexander, I examine the

relationship between school racial composition and summer learning. If being from a minority-segregated school in kindergarten is significantly associated with summer learning rates, then it is likely that school racial segregation captures some of the unobserved factors in the non-school environment that affect learning rates.

In addition to using better modeling techniques and more representative data than prior research, this study makes three additional contributions to the literature on racial segregation and racial differences in math and reading learning. First, the vast majority of prior research examines the impact of racial segregation in black and white students' educational outcomes. This study also examines Latino students, providing a more comprehensive picture of how racial segregation affects racial gaps in education.[1] Second, prior research that examines both the racial and socioeconomic composition of schools has produced inconsistent patterns. To improve upon these findings, I use seasonal comparison analysis to reassess how each school context shapes math and reading learning. Finally, prior research finds that the effects of racial composition are often moderated by student race (i.e. racial composition matters for blacks but not for whites), therefore, this work will also examine whether the effects of school racial composition vary by student race.

I expect that students in minority-segregated schools will have slower learning rates, on average, than students attending schools with few minorities. However, students in minority-segregated schools will have slower learning rates during both the school year and the summer, suggesting that unobserved factors associated with minority-

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[1] I present but do not discuss results for Asian students (who are not considered racial minorities in this study) and students who fall into a broad "Other Race" category – which includes Native American, Native Alaskan, Native Hawaiian, Pacific Islander, and more than one race but non-Hispanic.

segregated school attendance are responsible for some of the observed racial segregation effect. Second, I expect that attending minority-segregated schools will increase the black-white and Latino-white learning gaps. Third, I expect that the estimated effects of school racial segregation will reduce to non-significance after school socioeconomic context is taken into account, indicating that racial segregation is only a proxy for the true cause of learning disadvantages in minority-segregated schools - socioeconomic composition. Finally, I expect that black-white and Latino-white learning differences will be greatest in minority-segregated schools while they will be smallest in schools comprised primarily of white and Asian students.

Data, Measurement, and Analytic Strategy

I analyze data from the Early Childhood Longitudinal Study - Kindergarten Class of 1998-1999 (ECLSK). The ECLSK is a nationally representative sample of more than 21,000 students in nearly 1,300 schools who started kindergarten during the 1998-1999 school year. These data contain measures of student achievement on standardized tests, student characteristics, and school characteristics. Data from the ECLSK are ideal for this research because they are nationally representative, have a large sample size, and have math and reading assessment scores at four time points, the fall and spring of kindergarten and first grade. These scores enable me to estimate learning rates for the two school years and the intervening summer. Finally, the restricted use ECLSK provides school calendar dates and assessment dates in kindergarten and first grade. These are necessary to estimate school year learning rates and "uncontaminated" summer learning rates.

While all students took the math and reading tests during the fall and spring of kindergarten and the spring of first grade, the ECLSK randomly selected 30 percent of the original sample's schools to administer tests during the fall of first grade. The analytic sample is restricted to students who took all four tests so I can calculate school-year and summer learning rates, and it is therefore restricted to the students in 30 percent subsample. I exclude all students in year-round schools because those schools and their students are difficult to incorporate into seasonal comparison analyses. In addition, I exclude students who moved during the school year since they cannot be associated with a single school during an academic year. I do not exclude students who transferred schools between kindergarten and first grade if I have appropriate school identification numbers for each of their schools. I also omit students who have missing school IDs because I cannot match them to a school. Finally, a small number of student with missing race or gender information are excluded.

To address missing values on the independent variables, I use multiple imputation (Rubin 1987). I use Stata's *ice* command and create 5 versions of the data. I include all cases while imputing the data, including cases with missing values on the dependent variable (Allison 2002) but drop cases with missing values on the dependent variable from the analyses. This approach, known as multiple imputation, then deletion (MID), allows cases with missing Ys to provide useful information for the imputation of Xs in other cases, and it yields more efficient estimates than would be obtained without including Y in the imputation. MID also eliminates the risk of having poorly imputed Y values in the final analysis (von Hippel 2007). To account for clustering of students

within schools as well as assessment scores within students, I include school-

identification number as a variable in my imputation models and use wide-format data

that includes student-level and school-level variables on a single row that represents a

participating student. This enables student-level and school-level characteristics to serve

as predictors of missing values within each student (see Allison 2002; Downey et al.

2004). Student-level and school-level variables were imputed separately and then

combined so that school-level variables had the same value for all students within a given

school (Downey et al. 2008). After imputation (and deletion of cases with missing Ys),

the final analytic sample is approximately 4,000 students within 380 schools. Since I am

analyzing data with a restricted data license from the National Center for Education

Statistics, all sample sizes in this study are rounded to the nearest 10 to ensure student

anonymity.[2]

Dependent Variables

The dependent variables in this research are monthly learning rates of math and

reading skills for kindergarten, the summer between kindergarten and first grade, and first

grade.[3] They are constructed from math and reading Theta scores from the fall and

spring of kindergarten and the fall and spring of first grade. Monthly learning rates are

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[2] The author analyzed restricted data from the ECLSK under terms of a license (#12100030) between Dr. Anne McDaniel, University of California-Irvine, and the National Center for Education Statistics.
[3] Math and reading standardized test scores in general, and changes in math and reading skills represent two facets of learning that commonly assessed and analyzed in education research (especially in research on the black-white test score gap), they certainly do not represent all types of learning. For example, there may be important differences in learning in other subjects and learning of social and behavioral skills that lead to differing racial outcomes in education. Furthermore, one could argue that skills captured by standardized tests may be less meaningful than grades in school or educational attainment for life outcomes. However, in this study I focus exclusively on math and reading learning in order to reassess the claims made by prior research on black-white and Latino-white test score gaps. An additional discussion of the strengths and weaknesses of analyzing math and reading test scores can be found in Chapter 4.

obtained by calculating the change in Theta scores between two assessment periods

divided by the time in months between those two periods.

Math and reading Theta scores were obtained using Item Response Theory (IRT)

techniques. IRT uses patterns of correct and incorrect answers to assign a score, Theta,

that can be used order test-takers by ability as well as compare students' results from

different tests (Sijtsma and Molenaar 2002). In addition, the NCES reduced the

likelihood of floor and ceiling effects by using a two-stage approach. First, each test

taker received an initial routing assessment in stage one. Then each test taker was

assigned an ability-appropriate assessment in stage two.

Recent seasonal comparison research (c.f. Downey et al. 2004; Downey et al.

2008) used IRT scale scores. However scale scores have limitations for this type of

research. Scale scores are likely to be sensitive to the particular subset of test questions

that each student answers, and scale scores are often positively skewed during early

grades (LoGerfo, Nichols, and Reardon 2005). Theta scores, however, are less

vulnerable to the specific test questions students take and have a more normal

distribution.[4] Both scale score and Theta scores have arbitrary value ranges, and Theta

scores range from -5 to 5 in these data (LoGerfo et al. 2005).

In reading, students were assessed in the following areas: (1) letter knowledge -

through identifying and naming upper and lower case letters (2) beginning sounds -

associating letters with sounds at the beginning of words (3) ending sounds - associating

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[4] For examples of research using Theta scores, see: Ready (2010) and LoGerfo et al. (2005).

letters with sounds at the end of words (4) sight words - recognizing common "sight" words and (5) words in context - reading words in context.

In math, students were assessed in the following areas: (1) number and shape - identifying one-digit numerals, recognizing geometric shapes, and counting up to 10 objects (2) relative size - reading all one-digit numerals, counting beyond 10, recognizing a sequence of patterns, and using nonstandard units of length to compare the size of objects (3) ordinality and sequence - reading two-digit numerals, recognizing the next number in a sequence, identifying the ordinal position of an object, and solving a simple word problem (4) addition and subtraction - solving simple addition and subtraction problem, and (5) multiplication and division - solving simple multiplication and division problems and recognizing complex number patterns.

Calculating School Year and Summer Learning Rates

I use the four time points with skill assessments in kindergarten and first grade to estimate three separate seasonal learning rates for reading and mathematics (i.e. the dependent variables): (1) the learning rate during the kindergarten school year, (2) the learning rate during the summer between kindergarten and first grade, and (3) the learning rate during the first grade school year. Monthly learning rates during the school year are a function of the change in Theta scores during the school year (e.g. spring kindergarten assessment score minus fall kindergarten assessment score) divided by the time in months between assessments.[5]

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[5] Allison (1990) and Morgan (2001) argue that change measures are superior to techniques that use prior achievement as a control variable. The control variables tend to under-adjust for prior achievement, and including them in regression models can result in bias of the other coefficients in the model.

$$Monthly\ Learning\ Rate\ ij\ Kindergarten = \frac{(\theta ijt2 - \theta ijt1)}{(tj2 - tj1)/30.42}$$

$$Monthly\ Learning\ Rate\ ij\ First\ Grade = \frac{(\theta ijt4 - \theta ijt3)}{(tj4 - tj3)/30.42}$$

Where $(\theta_{ijt2} - \theta_{ijt1})$ and $(\theta_{ijt4} - \theta_{ijt3})$ represent the change in assessment scores for child i in school j between the start and end of kindergarten and first grade respectively, and $(t_{j2}-t_{j1})/30.42$ and $(t_{j4}-t_{j3})/30.42$ represent time in months between the fall and spring assessments in school j in kindergarten and first grade respectively.

Summer learning rates are more challenging to calculate because skill assessments were rarely taken on last day of kindergarten or the first day of first grade. Because of that, measured skills at the end of kindergarten and the beginning of first grade are "contaminated" by varying periods of school exposure. For example, a skills assessment taken on the 11th day of class in the first grade would include 10 days of learning at the first-grade learning rate, which is much faster than the summer learning rate for almost all students. If this school exposure were not taken into account when calculating summer learning, then measured summer learning would likely be greater than actual summer learning.

To obtain estimates of summer learning rates, one must use measured learning rates during the school year and the amount of school exposure during the period of contamination to extrapolate skills on the last day of kindergarten and the first day of first grade. For example, if a given student gains an average of one point of reading skill per month during kindergarten, and they take their end-of-the-year assessment one month before the end of kindergarten, then the student's extrapolated reading skill level on the

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first day of summer would be their measured score plus one point. This point is added to their measured reading score to create an adjusted reading score on the first day of summer. These adjustments yield "uncontaminated" skill estimates on the last day of kindergarten and the first day of first grade. Failure to make this adjustment results in biased summer learning rate calculations. This adjustment improves estimates of the effects of explanatory variables on summer learning. [6]

$$Monthly\ Learning\ Rate\ ij\ Summer = \frac{(\theta'ijt3 - \theta'ijt2)}{(t'j3 - t'j2)/30.42}$$

The adjusted first grade achievement score for student i in school j ($\theta'ijt3$) is a function of:

$$= \theta ijt3 - [Monthly\ Learning\ Rate\ ij\ First\ Grade * (tj3 - t'j3)/30.42]$$

Where $\theta ijt3$ is the assessed ability of child i in school j in fall of first grade, and $tj3$ represents the assessment date in the fall of first grade in school j, and $t'j3$ is the first day of first grade in school j. Therefore, the expression $(tj3 - t'j3)/30.42$ represents the amount of time in months students in school j are exposed to schooling between the first day of first grade and the fall of first grade assessment date.

The adjusted kindergarten achievement score ($\theta'ijt2$) is calculated by:

$$= \theta ijt2 + [Monthly\ Learning\ Rate\ ij\ Kindergarten * (t'j2 - tj2)/30.42]$$

Where $\theta ijt2$ is the assessed ability if child i in school j in the spring of kindergarten, $tj2$ is the spring of kindergarten assessment date, and $t'j2$ is the last day of kindergarten.

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[6] This approach carries with it the risks inherent in extrapolating beyond the available data. In particular, students may gain skills as different rates during the beginning and ending months of the school year. This would introduce bias into my estimates of summer learning rates if such learning rate deviations manifest in non-random ways (e.g. schools predominantly populated by high income students).

Therefore, $(t'j2 - tj2)/30.42$ represents the amount of time in months that students in school j are exposed to schooling between the spring of kindergarten assessment date and the last day of kindergarten.

Measured kindergarten achievement scores are adjusted upward based on the average monthly learning rate in kindergarten multiplied by the amount of time between the spring of kindergarten assessment and the last day of school. The first grade achievement score is similarly adjusted downward. Finally, the summer learning rate is calculated by simply subtracting the adjusted kindergarten achievement score ($\theta'ijt2$) from the adjusted first grade score ($\theta'ijt3$), and dividing that value by the time in months between the last day of kindergarten and the first day of first grade .

Individual-Level Independent Variables

This research uses multilevel models and incorporates variables at the individual- and school-level. On the individual-level (Level 1), I examine student race by including four dummy variables representing black, Latino, Asian, other race students with non-Hispanic white as the reference category. The "other race" category includes Native American, Native Alaskan, Native Hawaiian, Pacific Islander, and non-Hispanic multi-racial students. I account for several student background factors that reflect non-school resources and are known to shape learning. I include a measure of family socioeconomic status, which was calculated by NCES using parent/guardian education, parent/guardian occupation, and household income. I use the socioeconomic quintile measure that is available in the ECLSK (internally defined based on the ECLSK sample). I include four dummy variables representing socioeconomic quintile, with the highest (5th) quintile used

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as the reference category. Other measures of family background include parents' marital

status (0= married biological parents, 1=unmarried biological parents) and the student's

number of siblings at the start of kindergarten. I include a measure of age (in years) at

the beginning of each school year, and I center this variable about the grand mean for

each school year.[7] I include a dummy variable for gender (1=female). Given my interest

in Latino students, I also include a measure to account for the student coming from a non-

English speaking home (0=English is primary home language, 1=English is not the

primary home language).

To account for the amount of variation in students' exposure to school, I control

for the number of absences the child had during the school year and whether a student

attends full-day or half-day kindergarten (0=half-day, 1=full day).[8] Finally, I control for

whether a student repeated kindergarten. In year 1, a kindergarten repeater is a child who

attended kindergarten prior to the start of the study and remains in kindergarten in year 1

of the study. In year 2, a kindergarten repeater is a child who is was in kindergarten in

year 1 and remains in kindergarten in year two. For both variables, children who did not

repeat a grade are the reference group.

School-level Independent Variables

My key independent variables at the school level (Level 2) are the racial and

socioeconomic composition of schools. To measure school racial composition, I create

three racial composition categories: (1) schools with 75 percent or more racial minorities

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[7] I center student age about the grand mean at the start of kindergarten and first grade because it has no appropriate zero value in these data (i.e. no student begins kindergarten at age zero). I do not center other variables because I am primarily interested in the effects of categorical variables (student race, school racial composition group, school socioeconomic composition group) on learning outcomes.
[8] This measure is applied to kindergarten and summer models, but not first grade models.

(i.e. minority-segregated schools), (2) schools with 25 percent to 74.99 percent racial

minorities (i.e. integrated schools), and (3) schools with less than 25 percent racial

minorities (i.e. white-segregated schools). White-segregated schools are the reference

category.[9] Racial minorities include non-white and non-Asian students. Asian students

are not defined as racial minorities in this research because they are, on average, the most

academically successful racial group. Schools with less than 25 percent racial minorities

are referred to as white-segregated schools because estimates of the racial distribution of

children in the US for the year 2000 (two years after ECLSK students entered

kindergarten) indicate that almost 54 percent of children were white while only 5 percent

were Asian or Pacific Islanders (Child Trends 2012). Black and Latino children

comprised 14 percent and 23 percent of the population, respectively (the remaining 5

percent of children were multi-racial or American Indian). Similarly, my sample closely

matches these proportions, so most students in white-segregated schools are white.

While cut-points of the racial composition of schools (i.e. 75 percent minority v.

80 percent minority) are somewhat arbitrary, I choose the cut-points of less than 25

percent minority and more than 75 percent minority for two main reasons. First, Orfield

and Lee's (2005) national estimates from the 2002-2003 school year show that the

average white student in the US attended a school where 81 percent of students were

white or Asian and the average Asian student attended a school that was 67 percent white

or Asian (or 19 percent minority and 33 percent minority, respectively). Therefore, my

"white-segregated" schools are common and reflective of the average schooling

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[9] For each model, I ran analyses with alternative reference categories for school racial composition and note significant findings in the results section where appropriate.

experiences of white and Asian students. The average Latino and black student each attended schools there were 33 percent white or Asian on average (or 67 percent minority). These numbers suggest that my cut-points approximately reflect the typical level of segregation that each racial group experiences in the United States. Furthermore, only 2 percent of black and Latino students attend schools with very high proportions of white students (90 percent white or more), while no black or Latino students attend schools that are 99-100 percent white, according to Orfield and Lee (2005). Very few minority students attend extremely white-segregated schools, suggesting that my operationalization is realistic of student experiences. On the other hand, 38 percent of black and Latino students attend schools with 90-100 percent minority students, and 18 percent of blacks and 11 percent of Latinos attend schools that are 99-100 percent minority. In contrast, only 1 percent of whites attend schools with 90 percent minority students. Second, these cut-points have been used in prior research (see Roscigno 1998; Condron 2009), and a main goal of this study is to reassess the findings of prior research using seasonal comparison techniques. By using the same operationalization of racial segregation, my results are more directly comparable. Alternative specifications of racial composition of schools were tested, including a continuous measure of the percentage of racial minorities in the school and other cut-points of the percentage of racial minorities in the school (e.g. less than 15 percent and more than 85 percent). These alternative specifications did not alter the substantive findings of the research (details are discussed further in the section *Alternative Specifications*).

To account for school socioeconomic composition, I follow prior research and examine the percentage of students who receive free or reduced-price lunches in the school (Condron 2009; Downey et al. 2008). Schools where at least 75 percent of students receive free or reduced-price lunches are coded as high-poverty schools. Moderate or medium-poverty schools include schools where between 25 and 75 percent of students receive free or reduced-price lunches. Low-poverty schools, or schools in which less than 25 percent of students receive subsidized lunches, are the reference group. The cut-points for free/reduced-price lunches were chosen in order to make my findings comparable to prior research (Condron 2009) and because evidence shows that schools where have 75 percent of the student population receive free or reduced-price lunches have weaker educational environments. In these schools, teachers are three times less likely to be certified, they are less likely to be teaching in their subject area, and they have high levels of teacher turnover (Orfield and Lee 2005; Rothstein 2004). Additionally, more than 60 percent of black and Latino students attend high poverty schools compared to 30 percent of Asian students and 18 percent of whites (Orfield and Lee 2005). As with racial composition of schools, I tested alternative specifications of school socioeconomic status (including a continuous measure and varying cut-points of free/reduced lunch status), and results did not vary in meaningful ways. Finally, I include a control school sector (0=public school, 1=private school). School sector is not a focal point of this research, but I include this control to rule out a spurious relationship with racial and socioeconomic composition.

Analytic Strategy

        The ECLSK uses a clustered sampling design that has students nested within schools. I use multilevel model that account for the clustered nature of data when calculating the standard errors used to determine statistical significance. Multilevel modeling also allows me to generate separate estimates of the effects of school-level (Level 2) and individual-level factors (Level 1) on monthly learning rates in each season.[10] In the tables that follow, Model 1 includes the monthly learning rate of the outcome (reading or mathematics) with all individual-level controls as well as a control for school sector (public versus private). [11] The Level 1 predictors remain the same in all models, with one exception; the indicator of full-day versus half-day schooling is excluded from first grade models because all first-graders attend full-day schooling. Model 1 is specified as:

Model 1, Level 1

Learning Rate$_{ij}$ = β$_{0j}$ + β$_{1j}$SES Quintile + β$_{2j}$Gender + β$_{3j}$Race + β$_{4j}$English Language Home + β$_{5j}$ Marital Status + β$_{6j}$ Number of Siblings + β$_{7j}$Absences + β$_{8j}$School Year Repeater + β$_{9j}$Full Day Kindergarten + β$_{10j}$Age at School Start + r$_{ij}$

Model 1,Level 2

β$_{0j}$ = Υ$_{00}$ + Υ$_{01}$Private + μ$_{0j}$

        Model 1 examines the effects of individual characteristics on seasonal patterns of learning. The learning rate of student *i* in school *j* is a function of the average learning rate β$_0$ in school *j* plus additive effects of student socioeconomic quintile (β$_{1j}$), gender

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[10] All analyses are conducted with unweighted data to produce more accurate standard errors (see Winship and Radbill 1994).

[11] Models that exclude school sector (not shown) have very similar results to those presented. The conclusions drawn below would not change if this variable were excluded.

(β_{2j}), race (β_{3j}), family characteristics (β_{4j}, β_{5j}, β_{6j}), school exposure (β_{7j}, β_{8j}, β_{9j}), student age (β_{10j}), and student level random variation from the school mean (r_{ij}). While average learning rate (β_0) in school j is a function of the grand mean learning rate (Υ_{00}) plus the effect of school sector (Υ_{01}) and school level random variation (μ_{0j}) from the grand mean.

Model 2 adds two dichotomous indicators of school-level racial composition representing minority-segregated (75 percent or more minority) and racially integrated schools (25-75 percent minority) at Level 2. White-segregated schools (25 percent of fewer minorities) are the reference group. In Model 2, Level 1 remains the same as it is presented in Model 1. Model 2, Level 2 is specified as:

Model 2, Level 2

$\beta_{0j} = \Upsilon_{00} + \Upsilon_{01}Private + \Upsilon_{02}Minority\text{-}segregated\ School + \Upsilon_{03}Integrated\ School + \mu_{0j}$

Where average learning rate (β_0) in school j is a function of the grand mean learning rate (Υ_{00}) plus the effects of school sector (Υ_{01}), the effect of attending and minority-segregated school (Υ_{02}), the effect of attending a racially integrated school (Υ_{03}), and school-level random variation (μ_{0j}) from the grand mean.

Model 3 adds two dichotomous indicators of school socioeconomic composition to the previous model: high-poverty schools (75 percent or more receive free/reduced lunch) and moderate-poverty schools (25-75 percent receive free/reduced lunch) with low-poverty schools (25 percent or less receive free/reduced lunch) acting as the reference group. Again, Level 1 remains the same as Model 1:

Model 3, Level 2

$$\beta_{0j} = \Upsilon_{00} + \Upsilon_{01}Private + \Upsilon_{02}Minority\text{-}segregated + \Upsilon_{03}Integrated$$
$$+ \Upsilon_{04}High\ Poverty + \Upsilon_{05}Moderate\ Poverty + \mu_{0j}$$

In Model 3, Level 2, average learning rate (β_0) in school j is a function of the grand mean learning rate (Υ_{00}) plus the effects of school sector (Υ_{01}), the effect of attending a minority-segregated school (Υ_{02}), the effect of attending a racially integrated school (Υ_{03}), the effect of attending a high-poverty school (Υ_{04}), the effect of attending a moderate-poverty school (Υ_{05}), and school level random variation (μ_{0j}) from the grand mean.

These models are ideal for analyzing the effects of school context on average learning rates. The school-level coefficients represent the effects of school factors on learning rates for all students. Changes in black and Latino coefficients provide a sense of how school racial and socioeconomic context affect racial learning gaps, but we are still unable to clearly understand whether a given racial context is more beneficial to black and Latino students with these models alone. To understand this, I stratify the analytic sample by school racial composition to create three samples: students attending minority-segregated, racially integrated, and white-segregated schools. I re-estimate the Model 1 for each sample. If race coefficients vary widely across school groups, then it suggests that school racial contexts differently affect racial learning rate gaps.

Results

Table A.1 presents descriptive statistics for the analytic sample.[12] Consistent with prior research, learning rate patterns show that students learn much faster during the school year than the summer. In math, students gain an average of 0.075 Theta points per

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[12] Higher socioeconomic quintiles are slightly overrepresented while lower quintiles are slightly under-represented. However, between kindergarten and first grade, the overall distribution of participants in socioeconomic quintiles remains relatively stable, suggesting that there is no problem with differential attrition by socioeconomic status over time.

month during the school year in kindergarten and first grade, but gain only 0.019 Theta points per month during intervening summer. In reading, the difference is more dramatic. Students gain an average of 0.093 Theta points per month during the school year in kindergarten and first grade, while they gain 0.004 Theta points per month in reading skills during the intervening summer. Standard deviations for summer learning are much larger than school year learning, which indicates that learning rate inequality is greater during the summer than the school year (see Downey et al. 2004 for a similar pattern). It is also notable that approximately 45 percent of students in kindergarten attend either a minority-segregated or racially integrated school, and nearly 60 percent of kindergarteners attend a school that is highly or moderately socioeconomically disadvantaged. The share of students attending these types of schools is slightly lower in first grade.

Does Racial Segregation Affect Average Learning Rate? Mathematics

The results presented in Tables A.2 through A.5 include several notations for easier identification of models. I present three columns for learning rates in kindergarten, summer, and first grade. Models are labeled with the following prefixes: K=kindergarten learning, S=summer learning, F=first grade learning and the following suffixes: M=math and R=reading to clearly denote the season and dependent variable being discussed. For example, if I refer to K-2M in Table A.2, I am referring to the second model (2) of kindergarten learning rates (K) in math (M).

Table A.2 presents monthly math learning rates regressed on individual- and school-level variables for each time period. The first set of models (K-1M, S-1M, F-1M)

includes all individual-level variables described previously and a school-level control for school sector (public vs. private). The second set of models (K-2M, S-2M, F-2M) adds school-level measures of school racial composition. On the student level in the first set of models (K-1M, S-1M, F-1M), students in the first socioeconomic quintile (the most disadvantaged students) have significantly faster math learning rates in kindergarten compared to students in the highest quintile ($p<.10$), but socioeconomic status does not significantly affect summer learning rates. In first grade, students in the first, second, third and fourth quintiles learn at significantly faster rates than students in the highest socioeconomic quintile. These findings suggest that schooling equalizes socioeconomic gaps in math learning during the first grade. Male and female math learning rates are similar rates during kindergarten and the summer, but females learn math at a significantly slower rate in first grade. Living with married biological parents does not affect math learning rates during kindergarten, summer, or first grade. Living in a home where English is not the primary language does not affect math learning rates during kindergarten or the summer, but it significantly increases math learning during first grade. An increase in students' number of siblings increases learning rates during kindergarten and first grade but has no effect during summer. Age at the beginning of the school year has a significant negative effect on learning rates during kindergarten and first grade (i.e. older students have slower learning rates), but has no effect on summer learning. Repeating kindergarten significantly decreases learning rates during kindergarten but not in the summer or first grade. Attending full-day kindergarten significantly increases learning rates during kindergarten. School absences do not affect

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learning during either kindergarten or first grade, but having more absences in kindergarten is associated with a significant decrease in summer math learning.

Examining the second set of models (K-2M, S-2M, F-2M) in Table A.2, school racial composition does not significantly affect learning rates during kindergarten, summer, or first grade at the 95 percent confidence level (p<.05). The lone exception is in the summer between kindergarten and first grade. The coefficient for racially integrated schools is marginally significant (p<.10) for summer learning rates and unexpectedly positive. This suggests that students who attended racially integrated kindergartens, where 25 percent to 75 percent of students are racial minorities, gain math skills faster in the summer than students who attend white-segregated kindergartens. In models not shown, I change the reference category from white-segregated schools (with fewer than 25 percent racial minorities) to racially integrated schools and find that students in white-segregated *and* minority-segregated schools gain math skills at significantly slower rates during the summer compared to those in integrated schools. These findings suggest that students who attend racially-integrated schools have non-school environments that promote faster math learning in the summer than those of students who attend white-segregated or minority-segregated schools. This means that there are some common characteristics of students who attend racially-integrated schools (such as better home environments, neighborhoods or access to community resources that I cannot control for in my models) that increase summer math learning. While I cannot confidently state the mechanisms behind this finding, I can conclude that students who

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attend racially integrated schools have better math learning outcomes during the summer between kindergarten and first grade.

Does Attending a Racially Segregated School Increase Racial Learning Rate Gaps?

Mathematics

To assess whether attending a minority-segregated school affects racial learning gaps, I compare changes in the race coefficients in models 1M to 2M for each season. In kindergarten, blacks learn at significantly slower rates than whites. Being black decreases the monthly learning rate by 0.0094 Theta points (model K-1M). After accounting for school racial composition (model K-2M), the black-white math learning rate gap is 0.0108 Theta points, or approximately 0.25 standard deviations. After controlling for school racial composition, the black math learning rate deficit remains sizable in kindergarten, with blacks gaining 1.3 months' worth of math skills less than whites.[13] If blacks learned less than whites because of the harmful effects of racially segregated schools (as I hypothesized), then controlling for school racial composition should weaken the black coefficient (i.e. it should shift toward zero). However, the black coefficient becomes more negative (moving away from zero) between K-1M and K-2M. This suggests that attending segregated schools is not a cause of the black-white learning gap in math in kindergarten. The Latino-white gap remains very close to zero after accounting for the effects of school racial composition; therefore, racial composition does not appear to affect Latino-white math learning differences in kindergarten.

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[13] Calculated by (-0.0108*9.5 months)/0.079 = -1.29. Where: -0.0108 is the black coefficient in Model K-2M and 0.079 is the mean math learning rate in kindergarten.

During the summer (S-1M), black-white and Latino-white math learning rate gaps are not significant. Blacks and Latinos learn math at the same rate as whites during the summer, and school racial composition does not significantly affect learning rates (S-2M). In first grade, both black-white and Latino-white learning rate gaps are not significant. Therefore, black and Latino first graders gain math skills at the same rate as white students, after accounting for all other factors in Model F-1M. Once I control for school racial composition (Model F-2M), the black learning rate disadvantage becomes slightly smaller while Latinos gain a slight advantage, but the coefficients remain non-significant. In first grade, school racial segregation does not appear to play a meaningful role in black-white or Latino-white math inequality.

Examining learning rates across the three time periods reveals an interesting pattern in black-white learning rates. In kindergarten, black students gain math skills significantly more slowly than white students. However, in the summer, black and white students learn at similar rates. Therefore, while they are in kindergarten, when both school and non-school experiences drive learning, black students gain skills more slowly than white students. However, during the summer, when primarily non-school factors shape learning, black and white students learn at a similar. This means that black students have non-school experiences and resources that promote math learning at least as fast as white students, but the school environment promotes faster learning for whites in kindergarten than it does for blacks. This pattern suggests that schooling itself might not boost black students' learning as much as it does for whites, at least for kindergarten math learning. However, the racial composition of the schools that black students attend

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does not appear to be a major factor in this pattern of black disadvantage. In contrast, Latinos and whites learn math at similar rates during the school year and the summer, and school racial composition appears to have no influence on Latino-white learning differences.

Does Racial Segregation Affect Math Learning Net of School Socioeconomic Context?

Mathematics

Table A.3 presents the results of school socioeconomic context on math learning rates during kindergarten, summer, and first grade. Table A.3 includes the second set of models from Table A.2 for easy comparison. To preserve space, I do not present controls for age, school exposure, or family background. The second set of models in Table A.3 (K-3M, S-3M, F-3M) includes measures of school socioeconomic composition. Results indicate that school socioeconomic composition is not significantly associated with math learning rates in kindergarten, summer, or first grade. Accounting for school socioeconomic composition strengthens the finding that attending a racially integrated school during kindergarten significantly increases math learning rates during the following summer (after controlling for school socioeconomic composition, the coefficient for integrated schools becomes significant at the p<.05 level). Similar to Table A.2, after changing the reference category from white-segregated to racially-integrated schools, students who attended white-segregated and minority-segregated schools learn math at significantly slower rates during the summer. Again, this suggests that students attending minority-segregated and white-segregated schools have non-school environments that do not promote learning as quickly as those of students

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attending racially-integrated schools. Finally, school socioeconomic composition has almost no effect on racial gaps in math learning rates compared to models that only control for racial composition. In sum, when looking at math learning in early grades, socioeconomic composition does not appear to have a meaningful effect on learning, net of school racial composition.

Does Racial Segregation Affect Average Learning Rates? Reading

Table A.4 adopts the same structure as Table A.2; reading learning rates are regressed on individual- and school-level variables for each time period. The first set of models (K-1R, S-1R, F-1R) includes all student-level control variables and a control for school sector, and the second set of models (K-2R, S-2R, F-2R) adds measures of school racial composition. Many of the student-level variables demonstrate similar relationships with reading learning rates as they did with math learning rates. Students in lower socioeconomic quintiles learn to read at significantly faster rates during kindergarten (first, second, and third quintiles) and first grade (first, second, third, and fourth quintiles) than students in the highest socioeconomic quintile. During the summer, students in the lowest socioeconomic quintile gain reading skills at a significantly slower rate than students in the highest quintile. As with math, these findings suggest that schools equalize reading learning skills across socioeconomic groups. Males and females gain reading skills at similar rates during kindergarten and summer, but like math, females gain reading skills at significantly slower rates during first grade. School absences and living with married biological parents have no effect on learning rates during any time period. Living in a non-English speaking home and having a larger number of siblings

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significantly increase reading learning rates during kindergarten and first grade, but have no effect on summer learning. Repeating kindergarten and being older at the beginning of the school year significantly decrease learning rates during kindergarten and first grade, but not in summer. Attending a full-day kindergarten is associated with faster reading learning during kindergarten but slower learning during the summer.

In kindergarten and summer (K-2R, S-2R), school racial composition does not significantly affect reading learning rates. During first grade, students in minority-segregated schools gain reading skills significantly slower than children in white-segregated schools (F-2R).[14] Over the course of first grade, students attending minority-segregated schools gain 0.85 months less reading skills than students in white-segregated schools.[15] This finding can be understood in multiple ways. First, it could mean that students in minority-segregated schools gain skills more slowly than students in white-segregated schools because minority-segregated schools are less enriching learning environments. Alternatively, students in minority-segregated schools could gain reading skills more slowly because they have unobserved disadvantages that have little or nothing to do with the school itself. Of course, both processes could occur simultaneously to produce the reading learning rate disadvantage in minority-segregated schools. Later, I assess these explanations.

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[14] Additional models with an alternate reference category indicate that students in racially integrated schools and minority-segregated schools learn at similar rates.

[15] Calculated by (-0.0079*9.5 months)/0.091 = -0.825 where: -0.0079 is the minority-segregated school coefficient in model F-2R and 0.091 is the mean reading learning rate in first grade.

Does Attending a Racially Segregated School Increase Racial Learning Rate Gaps?

Reading

As with the models predicting math learning, I examine how the race coefficients change after controlling for school racial composition to assess whether school racial composition affects racial learning gaps in reading. If school racial segregation is driving black-white or Latino-white learning rate gaps, then we would expect black and Latino coefficients to shift toward zero after controlling for school racial composition. Results show that blacks gain reading skills significantly slower than whites during kindergarten while Latinos gain reading skills at approximately the same rate as whites (K-1R, K-2R). The black reading learning rate disadvantage relative to whites is approximately 0.20 standard deviation units, and blacks gain approximately 1 month fewer reading skills than whites over the course of kindergarten. After accounting for the effects of racial segregation in K-2R, the black-white learning rate gap becomes more negative (moving from -0.0099 to -0.0108 Theta points per month).[16] This shift is in the opposite direction from what we would expect if racially segregated schools were responsible for the black-white learning rate gap in reading. As with kindergarten math learning, this suggests that the racial composition of schools does not explain the black-white learning rate gap in kindergarten reading. Latinos gain reading skills at similar rates as whites during kindergarten, and controlling for school racial composition has little effect.

During the summer (S-1R), the black-white reading learning rate gap is statistically significant (p<.05) and unexpectedly positive. Blacks gain reading skills

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[16] Calculated by (-0.0108*9.5 months)/0.095 = -1.07. Where: -0.0108 is the black coefficient in Model K-2R and 0.095 is the mean reading learning rate in kindergarten.

0.146 Theta points per month, or 0.15 standard deviation units, faster than whites. Latinos also gain reading skills faster than white students during the summer (approximately 0.10 standard deviation units), however Latino-white difference is marginally significant (p<.10). The black and Latino summer learning advantages are virtually unchanged after controlling for the racial composition of the schools that students attend during kindergarten (model S-2R). To put the black and Latino summer reading advantage into perspective, over a 2.5 month summer, black and Latino students gain approximately one-third of a month more reading skills than whites at the average kindergarten learning rate.[17] This finding is surprising because prior research would predict that white students learn faster than or at the same rate as black and Latino students. Net of family background and the racial composition of the schools that students attend during the year, black and Latino students gain reading skills faster than whites in the summer between kindergarten and first grade.

During the first grade, black students have a small but significant reading learning rate disadvantage in reading (coefficient= -0.0049; p<.05) compared to white students (F-1R), however that disadvantage is reduced to non-significance after school racial composition is included (F-2R). This contrasts findings in kindergarten math and reading, where controlling for school racial composition slightly exacerbates black-white gaps (suggesting that the racial composition of schools is not driving the black-white gap). The findings in Table A.4 suggest that racial segregation explains part of black first

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[17] Blacks: Calculated by (0.0148*2.5 months)/0.095 = 0.39. Where: 0.0148 is the black coefficient in Model S-2R and 0.095 is the mean reading learning rate in kindergarten. Latinos: Calculated by (0.0111*2.5 months)/0.095 = 0.29.

graders' slower learning rates in reading compared to whites. In other words, if black and white first graders were in the same schools, there might be a smaller black disadvantage in first grade reading learning. Latino and white students gain reading skills at roughly the same rate, and controlling for school racial composition has very little effect on Latino-white learning rate differences.

In sum, black students learn reading skills significantly faster than whites during the summer, but learn reading significantly slower than whites during kindergarten and first grade (see F-1R). However, the black-white gap in first grade disappears after controlling for school racial composition (F-2R). School racial composition appears to contribute to black underperformance relative to whites in first grade reading, however incorporating racial composition into the models has a fairly small effect on the black coefficient overall (reducing it by 0.002 Theta points). The reduction in the black coefficient is fairly small when compared to the black summer learning advantage or the black kindergarten disadvantage. Including the school racial composition in Model F-2R reduces the black coefficient from -0.0049 to -0.0029. This means that accounting for the effects of school racial composition reduces the black reading learning deficit in first grade from about 0.5 months relative to whites to about 0.3 months. If school racial segregation plays a role in black-white learning gaps, it is important to note that its contribution is modest in early grades.

Overall, these results demonstrate that blacks gain reading skills slower than whites while they are in school (partially due to the school racial composition in first grade but not in kindergarten) and faster than whites during the summer. Latinos gain

reading skills at faster rates than whites during the summer months, but at the same rate as whites during kindergarten and first grade. These findings suggest that during the school year, blacks and Latinos are not gaining reading skills as fast as their summer learning rates would predict. Importantly, schools may not be serving blacks as well as whites because blacks' learn at slower rates than whites during the school year but faster rates during the summer.

Does Racial Segregation Affect Learning Net of School Socioeconomic Context? Reading

Table A.5 presents the results of reading learning rates regressed on school socioeconomic context and school racial composition. As in Table A.3 for math, I reproduce the results from the second set of models in Table A.4 (K-2R, S-2R, F-2R) and present them beside the new models in Table A.5 that add controls for school socioeconomic composition (K-3R, S-3R, F-3R). School socioeconomic composition does not significantly affect reading learning during kindergarten, summer, or first grade (K-3R, S-3R, F-3R) with one exception. During kindergarten, attending a school where at least 75 percent of students receive free or reduced-price lunches unexpectedly increases reading learning by 0.009 Theta points per month (p<.10). Controlling for school socioeconomic composition does not change the non-significant influence of school racial composition on reading learning. In the first grade, attending a minority-segregated school significantly decreases reading learning. After controlling for school socioeconomic composition, the effect of attending a minority-segregated in first grade school becomes stronger and more negative (model F-3R), and the negative effect of attending of a racially integrated school becomes marginally significant (p<.10). This

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result is surprising since I expected the effect of racial composition to diminish after accounting school socioeconomic composition. The results for first grade reading do not support the hypothesis that school socioeconomic composition is the true cause of learning rate disadvantages that might occur in minority-segregated schools.

Summary of Multilevel Model Findings

Given the two dependent variables and three learning periods of interest, I summarize the significant findings from the third set of multilevel models from Tables A.3 and A.5 in Table A.6. Table A.6 includes my main variables of interest at the individual-level - being black or Latino - and at the school-level - school racial and socioeconomic composition. Values of zero represent non-significant coefficients. Minus signs represent significant negative coefficients and plus signs represent significant positive coefficients at the p<.05 level or below. Coefficients significant at the p<.10 level are noted as well.

To summarize the findings, blacks learn at significantly slower rates in both reading and math in kindergarten compared to whites. During the summer between kindergarten and first grade, blacks gain reading skills at significantly faster rates than whites. Latinos and whites learn at the same rate during all times periods for reading and math with one exception: Latinos gain reading skills faster than whites during the summer. There are a few significant findings among the school-level variables as well. Students in minority-segregated and racially integrated schools gain reading skills slower in first grade compared to students in white-segregated schools. Also, students that attended racially-integrated schools in kindergarten gain math skills faster in the summer

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compared to students that attended white-segregated schools in kindergarten. Finally, in kindergarten, students attending high-poverty schools (where at least 75 percent of students receive free or reduced-price lunches) gain reading skills at faster rates than students in low poverty schools (where less than 25 of students receive free or reduced-price lunches).

It is worth emphasizing that black students gain skills at least as fast as or faster than white students during the summer, but they often gain skills slower than whites during the school year. Latino students also gain skills as fast as or faster than white students during the summer, but Latinos and whites gain skills at the same rate during the school year. For scholars debating whether schools are compensatory or reproductive of the stratification system, this pattern suggests that schools may not be compensatory across racial groups in these early grades. It is possible that these racial inequalities are partially mitigated by the compensatory effects that schools provide for socioeconomically disadvantaged students; however student race remains a salient factor shaping education outcomes - even if school racial composition were to have no influence.

How do racial learning rate gaps in math and reading vary by racial composition of schools?

The prior tables demonstrate how the racial composition of schools affects learning rates for all students as well as black-white and Latino-white gaps in learning rates. However, it remains unclear whether racial gaps in learning vary within specific school contexts or what racial gaps would look like in each context after accounting for

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summer learning. For example, is the black-white learning gap larger in minority-segregated versus white-segregated schools? Furthermore, how large are black-white and Latino-white gaps in each school context after accounting for summer learning rates? To answer these questions, I analyze racial learning rate gaps within schools that have different racial compositions. This allows me to compare the learning rates within different school settings, and it partially controls for the unobserved processes that led to students attending schools with various racial compositions. In analyses not shown, I also examined the entire sample using cross-level interactions between student race and school racial composition, but the interactions were not significant.[18] I present separate models for each school context for ease of interpretation.

Table A.7 presents the effects of race on math (panel A) and reading (panel B) learning rates in kindergarten, the summer, and first grade by school racial composition: minority-segregated schools (i.e. 75 percent or more racial minorities), racially integrated schools (i.e. 25 to 75 percent racial minorities), and white-segregated schools (i.e. less than 25 percent racial minorities). Models included all control variables discussed in prior models, but only the race results are presented for simplicity.[19]

Tables A.8 and A.9 reinterpret the results in Table A.7 to illustrate how schools impact racial learning gaps. One way to understand how schools affect educational inequality is to calculate how much they increase learning rates for each racial group, net of non-school learning. For example, if black students gain skills as fast as white

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[18] In an alternative specification model that used minority-segregated schools as the reference group, one cross-level interaction was significant. A cross-level interaction of black race and white-segregated school for first grade reading was negative (-0.0134) and marginally significant (p<.10).

[19] My conclusions do not meaningfully change if my models include or exclude measures of school socioeconomic composition.

students during the summer but gain skills slower than white students during the school year, then one might conclude that black students are underperforming in schools when compared to their potential learning ability and non-school learning resources. This could be because schools are not providing black and white students with equally enriching learning experiences (net of socioeconomic and family background).

Table A.8 quantifies the extent to which each school context provides favorable or unfavorable learning experiences to racial minorities relative to whites by combining summer and school year learning into a single number. This number, which I label the *School Discrimination Score* (hereafter SDS), offers a simple way to assess whether schools serve racial minorities and white students equally.

To obtain the SDS, I first average the coefficients of kindergarten and first grade learning rates for each racial group, producing a mean school-year learning rate gap for each racial group relative to whites. I then subtract the summer learning rate coefficient for each racial group from the mean school-year learning rate gap. This produces the SDS, and it represents racial differences in the amount that schools increase learning rates (net of summer learning). In principle, by subtracting the summer learning rate gap from the average school-year learning rate gap, I remove the influence of the non-school environment from the school-year learning rate gap.

School Discrimination Score = [(Kindergarten Racial Gap +First Grade Racial Gap)/2]

– Summer Gap

A positive SDS indicates that schools reduce minority-white gaps by enabling racial minorities to gain skills faster relative to white students than their non-school

learning rates would predict.[20] On the other hand, a negative SDS indicates that schools exacerbate minority-white gaps because minorities would be learning slower relative to whites than their non-school learning rates would predict.[21] To illustrate this, consider the following example: if Latinos learn 0.14 Theta points per month slower than white students during kindergarten (i.e. the coefficient for Latinos is -0.14) and 0.16 Theta points per month slower in first grade, their mean school-year learning is -0.15 Theta points relative to whites ((-0.14+-0.16)/2= -0.15). During the school year, school and non-school factors work together to produce a learning disadvantage for Latinos compared to whites.

If Latino students learn 0.30 Theta points faster than white students during the summer (the coefficient for Latinos in the summer is 0.30), it means that primarily non-school factors yield a learning advantage for Latinos compared to whites and that Latinos have more enriching non-school environments. By subtracting the Latino-white gap in summer learning (0.30) from the mean Latino-white gap in school-year learning (-0.15), I remove the influence of the non-school environment. This produces a school discrimination score (SDS) of -0.45 [-0.15 - 0.30 = -0.45]. This negative score indicates schools do not serve Latinos as well as whites because, net of non-school influences, Latinos gain skills slower than we might expect compared to whites during schooling an based on Latino summer learning rates. In other words, schools boost Latino average learning rates 0.45 Theta points *less* than they boost white learning rates.

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[20] Positive scores might also indicate that white students' learning rates are slower than would be expected based on their non-school learning rates.
[21] Negative scores might also indicate that white students' learning rates are faster than would be expected based on their non-school learning rates.

Returning to Table A.7, a complex picture of racial learning gaps emerges across seasons and school racial contexts. The coefficients for black and Latino students in both math (Panel A) and reading (Panel B) indicate that racial learning rate gaps vary across the three schooling contexts. For example, black kindergarteners gain math skills significantly slower than white kindergarteners in minority-segregated and racially-integrated schools; however black and white kindergarteners gain math skills at similar rates in white-segregated schools in kindergarten. During the first grade, black and white students learn math at similar rates in both minority-segregated and white-segregated schools. Black-white learning rate gaps in kindergarten and first grade are -0.006 Theta points in minority-segregated schools, -0.003 Theta points in racially integrated schools, and -0.004 Theta points in white-segregated schools, suggesting that black students gain skills at about the same average rates as whites in these settings.[22] These school-year results suggest that schools with different racial compositions have about the same effect on black-white learning gaps. However, these results do not take into account the contribution of the non-school environment (i.e. non-school learning estimated via summer learning rates).

After subtracting the summer coefficients, (e.g. how much blacks learn during the summer compared to whites) I find that white-segregated schools have the largest black-white math gaps (SDS = -0.014) compared to minority-segregated schools (SDS = -0.001) and racially integrated schools (SDS = -0.001). Using SDS to account for the effects of the non-school environment, the black disadvantage relative to whites in math

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[22] Authors calculations based on Table 6. Minority-Segregated: ((-.0162+.0052)/2 = -.006))
Integrated: ((-.0074+.0023)/2 = -.003) White-Segregated: ((-.0052+.0032)/2 = -.004))

learning is the largest among students attending white-segregated school. Rather than discuss and interpret each coefficient in Table A.7, I focus on the School Discrimination Scores (SDS) in Table A.8.

Panel A of Table A.8 presents SDS results for math learning for each school racial composition group. In math, white-segregated schools have the largest SDS (-0.014) for both black and Latino students, while SDS in minority-segregated and integrated schools are essentially zero (-0.001 in each school). In other words, when including racial gaps in both school year and summer learning to account for the contributions of school and non-school environments, the black-white gap is the largest for students attending white-segregated schools. Panel B of Table A.8 provides the results for reading. Similar to results for math, black and Latino students in white-segregated schools have the largest SDS disadvantage of the three school settings (-0.040 for black students and -0.014 for Latino students), meaning that the black-white and Latino-white gaps in the amount that schools increase learning rates appear to be the largest in white-segregated schools. In analyses not shown, I constructed a dependent variable in a similar fashion as I constructed SDS (i.e. averaging the kindergarten and school year learning rates of each student and subtracting their summer learning rate) and re-ran the multilevel models. I found that the black-white reading gap in white-segregated schools was significant at $p<.05$, but no other gaps were significant for blacks. Reading and math gaps were non-significant for Latinos in all racial contexts.

Finally, I use the grand mean learning rates in Table A.7 and the SDS scores in Table A.8 to calculate the amount that schools increase math and reading learning rates

(a.k.a. learning rate boost from school) in each school setting for white, black, and Latino students. These results are presented in Table A.9 along with SDS values for black and Latino students. Similar to SDS, I calculated the average monthly learning rate for whites by averaging the grand mean (which is equivalent to the intercept) for kindergarten and first grade, then I subtracted the grand mean of summer learning for each schooling context. This produced average learning rate boost from schooling for white students. For each school context, I calculated values for black and Latino students by adding their SDS score to the white learning rate boost (just described).

In math (Panel A), minority-segregated and racially integrated schools boost learning rates the most for white, black, and Latino students, while white-segregated schools provide the smallest learning rate boost for all of these racial groups. Surprisingly, even whites in white-segregated schools appear to experience the smallest learning rate boost in math compared to whites in other schooling contexts. In reading (Panel B), the pattern is less clear. White students appear to benefit the most from white segregated schools because that is where they receive the largest learning rate boost from schooling. On the other hand, blacks appear to benefit the least from white-segregated schools, while they experience similar learning rate boosts in racially integrated and minority-segregated schools. Finally, if anything, Latinos experience the smallest reading learning rate boost in racially integrated schools, and they appear to benefit approximately equally from minority-segregated and white-segregated schools.

Together, the average learning rate increases in each school setting and the SDS scores suggest that white, black, and Latino students experience the smallest learning rate

benefits and have the largest racial inequality in white-segregated schools in math. Minority-segregated and racially integrated schools seem to offer greater racial equality and greater overall increases to math learning rates (net of summer learning). For reading, white-segregated schools appear to boost learning the most for whites and the least for blacks, and that fact likely produces the large SDS score for blacks in that school setting.

The results in Table A.8 and A.9 suggest that, if anything, minority-segregated and racially integrated schools produce little racial inequality in kindergarten and first grade, and they also appear at least as effective as white-segregated schools at boosting average learning rates. These results indicate that white-segregated schools produce black-white inequality in reading, and that blacks benefit the least when they attend these schools. The black-white SDS difference in reading in white-segregated schools is particularly large (-0.040), while other differences are likely to be non-significant. Latinos also experience the largest SDS in white-segregated schools in both math and reading, but supplemental analyses indicate that these differences are not significant. Overall, SDS results suggest that school racial composition plays no role in Latino-white educational gaps, but white-segregated schools might unexpectedly produce meaningful black disadvantage among the students who attend those schools in early grades.

At this point, it is important to summarize the differences between the findings of the models from Tables A.2 through A.6 and the SDS results and to remember that they have different dependent variables. When examining the effects of school racial composition on learning rates within each season, summarized in Table A.6, school racial

composition rarely affects overall learning rates (with the exceptions of racially integrated schools increasing summer math learning and racially integrated and minority-segregated schools decreasing first grade reading learning compared to white-segregated schools). Furthermore, the racial composition of schools has little effect on black-white or Latino-white gaps - with the exception of reducing the black-white reading learning rate gap to non-significance in first grade. In contrast, SDS assesses whether schools with specific racial compositions serve minorities and whites similarly after considering how much students learn during the school year *and* the summer. SDS is the learning that is attributable to schooling because it subtracts summer learning from the school year learning for each student. The SDS analyses show that blacks attending white-segregated schools experience a meaningfully smaller learning rate boost in their reading learning rate from schooling compared to whites in those schools. However, Latino and white students experience similar learning rate increases from schooling, regardless of school racial composition. Taken together, this study has three main findings: (1) the racial composition of schools rarely affect overall learning rates during kindergarten, the summer, or first grade, (2) The racial composition of schools also rarely affects the size of the black-white or Latino-white gap in learning in kindergarten, summer. or first grade with the important exception that school racial composition does reduce the black-white learning rate gap to non-significance in first grade reading, and (3) when looking at how much schooling boosts learning rates (net of summer learning), results suggest that minority-segregated and racially integrated schools have almost not net effect on black-white and Latino-white differences in reading and math because they boost learning rates

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similarly across these racial groups. However, white-segregated schools provide a smaller reading learning benefit to black students than to white students in kindergarten and first grade.

The multilevel models and SDS findings might tell a more complete story of the importance of school racial composition. As noted, racial composition significantly affects average reading learning in the first grade as well as black-white gaps in reading learning rates. However, when I use SDS to account for summer learning (a proxy for effects of the non-school environment on learning), minority-segregated schools do not appear to be particularly harmful for blacks (or Latinos).[23] This pattern suggests that it is not the minority-segregated schools themselves that promote slower reading learning rates on average, and it is not the minority schools that promote black reading learning disadvantages relative to whites. Rather, it is more likely that factors associated with attending minority-segregated schools lead to slower average learning rates in these schools and this may explain why accounting for the effects of racial composition reduces the black-white gap in first grade reading learning to non-significance.

Alternative Specifications

In general, these findings show that school racial and socioeconomic composition do not have large effects on learning, but it is possible that the way racial composition is specified could change the results. To assess this possibility, I tested several alternative specifications of school racial and socioeconomic composition and the findings do not substantively change. For example, I tested whether continuous measures of school

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[23] It is worth noting that I am discussing only first grade reading, but SDS includes kindergarten reading. In analysis not shown, I calculated SDS with only 1st grade learning rates minus summer learning rates, and my conclusions are the same.

racial and socioeconomic composition affect learning rates (measured as 0-100 percent black/Latino and 0-100 percent received free/reduced lunch, respectively) as well as the squared terms of those variables. These were generally non-significant, with the exception of a model with a continuous measure of percent minority (but excluding the squared term) for first grade reading. Percent minority was negative (b = -0.0001) and significant (p<.05), consistent with my categorical findings for that outcome in first grade. I also used different "cut-points" of racial and socioeconomic composition, including more than 85 percent minority / 15 to 85 percent minority / less 15 percent minority; more than 90 percent minority /10-90 percent minority / less than 10 percent minority. In addition, I disaggregated the racially-integrated category of schools (0-25 percent minority / 25-50 percent minority / 50-75 percent minority /75-100 percent minority; 0-20 percent minority / 20-40 percent minority / 40-60 percent minority / 60-80 percent minority / 80-100 percent minority). I find similar patterns of learning for each racial group by school type (white-segregated, integrated, and minority-segregated) regardless of cut-point.[24] Furthermore, the findings of the multilevel models were substantively the same: attending a racially-integrated school significantly increases the math learning rate during the summer, and attending a racially integrated school or minority-segregated school significantly decreases the reading learning rate during first grade. These robust findings suggests two things: 1) that attending racially segregated

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[24] Table A.6 shows that attending a racially integrated school (25-75% minority) increases summer math learning. When racially integrated is specified at 25-50% minority and 50-75% minority, 50-75% minority is statistically significant and positive but 25-50% minority is not. In Table A.6, racially integrated schools significantly decrease first grade reading when specified as 25-75% minority. When specified as 25-50% and 50-75% minority, only schools with 25-50% minorities are negative and statistically significant. When racially-integrated is specified as 20-40%, 40-60% and 60-80% minority, 60-80% minority is positive and significant for summer math learning and 20-40% minority is negative and significant for first grade reading.

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racial and socioeconomic composition affect learning rates (measured as 0-100 percent black/Latino and 0-100 percent received free/reduced lunch, respectively) as well as the squared terms of those variables. These were generally non-significant, with the exception of a model with a continuous measure of percent minority (but excluding the squared term) for first grade reading. Percent minority was negative (b = -0.0001) and significant (p<.05), consistent with my categorical findings for that outcome in first grade. I also used different "cut-points" of racial and socioeconomic composition, including more than 85 percent minority / 15 to 85 percent minority / less 15 percent minority; more than 90 percent minority /10-90 percent minority / less than 10 percent minority. In addition, I disaggregated the racially-integrated category of schools (0-25 percent minority / 25-50 percent minority / 50-75 percent minority /75-100 percent minority; 0-20 percent minority / 20-40 percent minority / 40-60 percent minority / 60-80 percent minority / 80-100 percent minority). I find similar patterns of learning for each racial group by school type (white-segregated, integrated, and minority-segregated) regardless of cut-point.[24] Furthermore, the findings of the multilevel models were substantively the same: attending a racially-integrated school significantly increases the math learning rate during the summer, and attending a racially integrated school or minority-segregated school significantly decreases the reading learning rate during first grade. These robust findings suggests two things: 1) that attending racially segregated

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schools rarely affects learning rates or that its effects are generally small, and 2) when the

racial composition of schools does affect learning, the findings are robust against

alternate specifications.

One might ask, what is the theoretical difference between a school with 24

percent minority students and 27 percent minority students? Such a small change in the

racial composition of schools likely does not make a substantive impact on student

experiences or student learning, but this paper places these schools in different categories.

In reality, these cut-points are somewhat arbitrary and chosen based on commonly used

cut-points in prior research. However, the main goal of this paper is examine whether

highly-segregated contexts - either highly segregated schools that are mostly comprised

of minority students or white students - affect learning and inequality. My cut-points

capture those highly-segregated contexts, and changing the cut-points does not alter my

findings.

Little is gained by changing the cut-points 5 or 10 percent in one direction or the

other. Out of the 270 schools that students attend in the fall of kindergarten, 18 percent

are minority-segregated when measured as 75 percent or more students are minorities.

Importantly, 12 percent of all schools are comprised of 95-100 percent minority students

(or two-thirds of my minority-segregated schools). If I change the categorization to 85

percent or more minority-students, I include 15 percent of the schools in my sample.

White-segregated schools are even more pervasive: 32 percent of schools have between 0

and 5 percent minority students and 54 percent of schools have between 0-25 percent

minority students. If I include 0-35 minority students, I capture only an additional 7.5

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percent of schools. Highly segregated schools may have a slightly larger influence on learning, (either white-segregated or minority-segregated), but in general school racial segregation does little to shape student learning.

Conclusion and Discussion

This study examines how school context, in particular school racial composition, affects math and reading learning rates. Most education scholars believe that schools with larger proportions of racial minorities promote slower learning rates, especially for black and Latino students, and that the racial segregation of schools contributes to persistent racial gaps in academic skills. These results indicate that the effects of school racial segregation are sparse in kindergarten and first grade, and they are not as predictable and unidirectional as prior research suggests.

In mathematics, school racial composition has virtually no effect on average learning rates, and controlling for school racial composition has little effect on racial learning rate gaps. In reading, students attending minority-segregated schools have significantly lower learning rates in the first grade compared to white-segregated schools. Additionally, blacks gain reading skills at slower rates than whites in first grade, but the black-white gap is reduced to non-significance after controlling for school racial composition. This means that if blacks and whites attended similar schools, black and white first graders might gain reading skills at more similar rates. Contrary to my expectations, school socioeconomic composition (as measured by the share of students who receive free or reduced-price lunches) does not have a strong independent effect on learning net of school racial composition and student background. This means that when

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school racial composition affects learning it is not due to the socioeconomic composition of schools.

When examining white-segregated, racially integrated, and minority-segregated schools using SDS, I found that black students in white-segregated schools experience a smaller reading learning rate boost from schooling compared to white students in those schools, but this difference was not present in any other school contexts. Latinos in white-segregated schools experience the largest disadvantages relative to whites, but supplemental analyses (not shown) suggest that these differences are not statistically significant. Black and Latino students in racially integrated schools and minority-segregated schools experience small or negligible differences relative to white students in math and reading. Supplemental analyses (not shown) suggest that these differences are non-significant. SDS results, which take into account school year and summer learning simultaneously, indicate that racially integrated and minority-segregated schools promote skills growth equally for white, black, and Latino students in kindergarten and first grade. This stands in contrast to the general opinion that minority-segregated schools produce worse outcomes for black students compared to whites – especially in reading. These results suggest that predominantly white/Asian schools may fail to support the learning of black students as well as they support the learning of white students. However, the results also generally suggest that Latino-white learning gaps are not heavily influenced by school racial composition.

Regardless of the racial and socioeconomic composition of schools, it appears that black students are disadvantaged relative to whites in the early stages of the schooling

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process. Black students tend to gain skills slower than white students during the school year, but they gain skills at least as fast as whites, or faster, during the summer. Latinos also experience this pattern but it is less pronounced. Why do racial minorities, especially blacks, appear to be disadvantaged by schooling? One explanation for this pattern is that schools fail to adequately support the learning capabilities of black and Latino students. If school racial composition plays a role in educational gaps across racial groups, its contribution appears to be quite small compared to the broader disadvantages that minorities experience in school.

There are other plausible explanations for these results. For example, in schools with few minority students, it is likely that there would also be few minority teachers and staff. Student-teacher racial mismatch for minority students would be more likely in these schools, and prior research finds that white teachers may judge the behavior of racial minorities more harshly than the behavior of white students (Alexander, Entwisle, and Thompson 1987; Downey and Pribesh 2004; Mickelson 2001; Roscigno 1998). Racial mismatch may contribute to social-psychological difficulties and lower academic achievement for minority students. Furthermore, relative deprivation theory would argue that minorities in white-segregated schools may compare themselves to their white peers and perceive worse treatment from teachers or peers, less social support, and access to fewer resources, and this could have harmful psychological effects and possibly hamper their learning (Davis 1966).

Prior research suggests that differences in linguistic patterns between African Americans and whites might have particularly harmful effects on black students' ability

to learn to read (Entwisle and Alexander 1994). These results are consistent with that explanation. Blacks experience the largest reading learning rate disadvantage compared to whites (in terms of SDS) in and the smallest learning rate boost when they attend in white-segregated schools - where white linguistic patterns are likely privileged. Minorities experience almost no reading learning rate disadvantage in predominantly minority schools (see Tables A.7 and A.9).

Finally, minorities may experience greater discrimination in ability group placement within white-segregated schools and this could hinder their learning. Ability group discrimination is likely to be more common in schools with few racial minorities, a setting in which stereotypes about racial minorities could be more salient, compared to schools comprised of larger proportions of racial minorities. Condron (2009) found that ability group placement was a key component of growing black-white gaps in reading in first grade.

This study provides more nuanced findings about the effects of school racial and socioeconomic composition on racial inequalities in learning, and it uses a more sophisticated and appropriate modeling strategy than prior research. However, there are several limitations to this study. First, this study examines only two years of schooling due to data limitations, kindergarten and first grade, and therefore is not generalizable to the entire academic career. Second, the sampling strategy of the ECLSK reduced the available sample of students by more than 70 percent, and many students were removed from the analytic sample due to missing test information. This resulted in a much smaller analytic sample, approximately 20 percent of the original dataset, and in a relatively

small sample of blacks and Latinos. The number of schools was also reduced dramatically. Such sample reductions reduced the statistical power of my models and made it less likely that I would obtain statistically significant results. Therefore, findings of marginal significance (p<.10), especially at the school level, should be cautiously considered. This caution is particularly important when thinking about summer learning. Standard deviations for summer learning were substantially larger than they were for school-year learning. As a result, effect sizes had to be larger during the summer to achieve statistical significance.

Another limitation of this paper is that it examines only standardized math and reading scores as a way to measure learning and assess the impact of the racial segregation of schools. While math and reading skills are certainly important, and they build the foundation for later school success, students learn other skills, both in schools and out of schools, that are important (and could potentially advantage white students over Latino and black students). For example, non-cognitive skills, such as social skills and self-control, and cultural capital are all taught in and out of schools and have important implications for academic and labor market outcomes (c.f. Duncan et al. 2007; Heckman, Stixrud, and Urzua 2006; Jennings and DiPrete 2010). Furthermore, if schools are supposed to prepare students for college and labor market success, exposing students to diverse peers may be critical for future success. The ability to appreciate and work productively with a diverse group of individuals is an important skill that students may not develop if they attend highly-segregated schools.

Finally, this research focuses on short-term outcomes and ignores the long-term effects of school racial composition on educational and labor market outcomes, and it does not carefully explore the mechanisms that might lead to educational disadvantages for blacks and Latinos in certain schooling circumstances. As the *Brown v. Board* decision stated and many scholars have repeated, school integration is not intended to simply reduce test score gaps. Rather, it was intended promote social equality more broadly.

It is worth highlighting again that black and Latino students have a reading learning advantage over whites (and math learning equality) in the summer between kindergarten and first grade. This is somewhat puzzling. Black and Latino students enter kindergarten with lower skills than whites on average, and this is likely due to their disadvantages in the non-school environment. An important question, then, is what changed before the start of kindergarten and after the end of kindergarten that would flip racial patterns of non-school learning? There are plausible explanations for this pattern. First, Downey et al. (2004) estimated that black and Latino students gained skills at about the same rate as whites in the months immediately preceding kindergarten, but black and Latino students had significantly fewer skills than whites at the start of kindergarten. It is possible that black and Latino skill disadvantages emerged much earlier in early childhood, but at some point white, black, and Latino children began to gain skills at the same rate (i.e. white students are ahead, but not learning *faster* when kindergarten starts). If this is true, it would make the black and Latino summer advantage seem less striking. Also, it is possible that black and Latino parents are not fully aware of their children's

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cognitive skill deficits until their children start schooling. It is possible that black and Latino parents make extra efforts to educate their children in the summer after kindergarten to help their children reach appropriate skill levels. One or both of these processes could explain the puzzling pattern in non-school learning, but future seasonal comparison research should carefully examine these issues.

Future research should explore the mechanisms that produce the learning disadvantages that black and Latino students experience in schools populated by few minorities. Also, seasonal comparison research should be applied to later grades in the academic career to determine whether the effects of racial segregation are similar. Schooling in the early grades may be more equitable across racial groups than schooling in later grades - before the disadvantages and discrimination both inside and outside of school produce wide skill differences with which teachers and schools must cope.

School racial composition has been widely studied, and scholars generally believe that minority-segregated schools widen racial achievement gaps. This research does not support that conclusion – at least not in kindergarten and first grade. In fact, using seasonal comparison techniques and SDS to compare minority-segregated, integrated, and white-segregated schools, I find that white-segregated schools probably contribute to racial inequality more than minority-segregated schools. Racial segregation should not be considered a trivial factor in racial inequality, but the effects of segregation appear small compared to the more general disadvantage that minority students experience in the schooling process. This research suggests that efforts to reduce racial discrimination in schooling must extend beyond efforts to desegregate schools and also

emphasize equalizing school experiences of minorities and non-minorities who share the same schools and classrooms.

Chapter 3: Reassessing Learning Inequalities in Catholic Versus Public Schools

Abstract: Catholic schools are the largest single source of private education in the US, and voucher programs make them inexpensive or free in many places. Researchers have examined Catholic secondary schools in depth for the past 30 years, but far less attention has been paid to Catholic primary schools. With data from the ECLSK, this study uses seasonal comparison techniques, propensity score matching, and multilevel modeling to compare how much Catholic and public schools increase math and reading learning rates net of summer learning – which I call *Student Impact*. These analyses show that Catholic schools are no more effective than public schools at increasing reading learning rates for first graders, while they are less effective at increasing math learning rates, especially in urban areas. This research indicates that the *Student Impact* for math and reading for blacks students is smaller in Catholic schools than public schools. Furthermore, the black-white gap in math and reading is larger in Catholic schools than public schools. In contrast, Catholic schooling appears to be beneficial for Latino students in reading but not math since Latinos have a significantly higher *Student Impact* in Catholic schools compared to public schools. Additionally, socioeconomically disadvantaged students experience a significantly higher *Student Impact* than socioeconomically advantaged students in public schools but not Catholic schools.

Introduction

The US public school system has often been touted as the primary avenue for making the "American Dream" accessible to everyone. Public schools are now a source of concern because of the US's middling ranking on international achievement tests (Peterson et al. 2011) and the fact that socioeconomic and racial inequalities in education have persisted for decades (Farkas 2004). Racial minorities and children from low socioeconomic backgrounds are less likely to complete high school and college (NCES 2012a) causing them to earn lower wages and experience less upward mobility (Haskins 2007). These inequalities continue to concern scholars and policymakers, and many critics argue that public schools are failing students by allowing these educational inequalities to continue. Some proponents of school reform argue that parents should have the choice to send their children to better schools - whether public or private. But, are alternatives to public schools really better for students? Can they improve academic outcomes and reduce educational inequalities?

Catholic schools present a useful comparison to public schools since they are often an alternative to "failing" public schools, and many cities and states provide school vouchers that allow students to attend religious private schools (Hochschild and Scovronick 2003). Catholic schools educate nearly 2 million of the approximately 55 million students in preK-12 schools, making them the largest private sector education provider (NCES 2011a; NCES 2011b). For decades, education scholars have studied the differential effects of Catholic and public high schools. Many studies found that Catholic high schools were more effective than public high schools and produced better academic

achievement and graduation rates than public schools (Coleman, Hoffer and Kilgore 1982a, 1982b; Coleman and Hoffer 1987; Hoffer, Greeley and Coleman 1985; Willms 1985). More recent studies have also identified benefits of Catholic high schools on academic achievement (Morgan 2001; Grogger and Neal 2000). Other researchers, however, concluded that Catholic high schools did not provide advantages to the average student compared to public schools (Alexander and Pallas 1983, 1985; Noell 1982; Goldberger and Cain 1982). One important finding that emerges from the Catholic school research is that Catholic secondary schools produce better academic outcomes than public schools for racial minorities, socioeconomically disadvantaged students, and students in urban areas (Hoffer et al. 1985; Morgan 2001; Neal 1997; Lee and Stewart 1989; Grogger and Neal 2000). If that is true, then Catholic schools could serve to reduce persistent educational inequalities, even if they are not more effective overall.

While the majority of prior research on Catholic schools focuses on high schools, some scholars have examined the effects of Catholic schools earlier in the schooling process. This is a valuable extension of Catholic schools research since high school outcomes result from years of cumulative learning experiences. It is also well known that racial and socioeconomic gaps in achievement are present early in the academic career, so it is important to understand the effects of Catholic schools in these critical elementary school years (c.f. Downey, von Hippel, and Hughes 2004; Lee and Burkam 2002; Fryer and Levitt 2004, 2006). If Catholic schools can ameliorate some of these early educational inequalities, then it is possible that they could help close gaps in critical educational milestones such as high school graduation.

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Results from research on Catholic primary schools are somewhat mixed, but the majority of this research suggests that Catholic primary schools are no more effective than public schools. Lee and Stewart (1989) found that Catholic school 3rd graders outperform their public school counterparts on NAEP math assessments, and they also found evidence black and Latino students as well as socioeconomically disadvantaged students enjoyed particularly large benefits from attending Catholic schools. However, most research indicates that Catholic and public schools produce similar achievement outcomes for students, and Catholic schools might even perform slightly worse in math (Elder and Jepsen 2013; Jepson 2003; Lubienski and Lubienski 2006; Reardon, Cheadle and Robinson 2009). Unlike Catholic secondary schools, the effects of Catholic primary schools likely do not vary by student race, socioeconomic status, or school locale (Lubienski and Lubienski 2006; Jepsen 2003), although Lee and Stewart (1989) found that Catholic schools have smaller racial and socioeconomic gaps than public schools in 3rd and 7th grades.

One challenge of studying the effects of Catholic versus public schools is the fact that students attending Catholic schools are likely different from students attending public schools in observed and unobserved ways. While we can observe that students attending Catholic schools are more likely to be white and from high socioeconomic backgrounds, it is difficult to use traditional statistical methods and large secondary datasets to capture the values and motivations of parents who send their children to Catholic schools. These parents may value the social, religious, disciplinary, or academic characteristics of Catholic schools, but these values are difficult to quantify as is their

effect on student outcomes. Morgan (2001) and Reardon et al. (2009) advanced Catholic

school research by using propensity score matching techniques that take into account the

observed characteristics that shape self-selection into Catholic schools. However,

propensity score matching does not fully account for the unobserved factors that might

shape Catholic school attendance and academic achievement.

Another important limitation of prior research on Catholic versus public schools

is that researchers estimated the effect of Catholic schools on student achievement tests

without accounting for the fact that students learn at different rates during the school year

and the summer (c.f. Downey et al. 2004; Entwisle and Alexander 1992; Heyns 1978).

To accurately estimate the effect of Catholic or public schools on learning outcomes,

models must account for summer learning rates to minimize bias in estimates of school-

level effects. No existing research on Catholic school effects has addressed this issue.

This study uses data from the Early Childhood Longitudinal Study – Kindergarten

Class 1998-1999 (ECLSK) to answer the following research questions: Are Catholic or

public schools better for elementary school students' math and reading learning? Does

the effect of attending a Catholic or public school vary by race, socioeconomic status, or

school locale? Following Morgan (2001), who argues that propensity score estimates

should be used to supplement regression analysis, I use propensity score matching and

multilevel modeling to estimate the effect of Catholic and public schools on first graders'

learning and on learning gaps among different racial groups, socioeconomic groups, and

school locales. Importantly, to account for different school and summer learning rates as

well as unobserved non-school factors that affect learning, and to isolate the unique

contribution of schools to learning, I use *Student Impact* scores – students' school year learning rate minus their summer learning rate – in first grade math and reading as the dependent variables.

Literature Review

In the early 1980s, James Coleman and his colleagues started a long-running scholarly debate about the relative effectiveness of Catholic and public schools. Early studies analyzing the *High School and Beyond* data indicated that Catholic high school students scored higher on standardized tests and experienced greater achievement growth in math and vocabulary skills than public school students (Greely 1982; Coleman, Hoffer, and Kilgore 1982a, 1982b; Hoffer, Greeley, and Coleman 1985; Coleman and Hoffer 1987). Many scholars critiqued these studies, arguing that Coleman and colleagues did not appropriately control for students' prior achievement and family background, which would reduce the Catholic school effect (Alexander and Pallas 1983, 1985; Noell 1982; Goldberger and Cain 1982).

In response to these criticisms, scholars developed more complete statistical models and applied more advanced statistical techniques to study school effects. For example, Willms (1985) included measures of school socioeconomic status and racial composition in addition to student-level family background and still found a small but significant Catholic school advantage in math and reading achievement. Bryk and colleagues applied multilevel modeling techniques and found a positive Catholic school effect (Bryk, Lee and Holland 1993; Lee and Bryk 1989). Studies using more recent data and advanced modeling strategies also found positive effects of Catholic high schools

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(Lee and Stewart 1989; Gamoran 1996; Morgan 2001; Grogger and Neal 2000; Carbonaro and Covay 2010).

Catholic schools are believed to be more effective than public schools for several reasons. First, Catholic schools have strong academic programs. Few Catholic schools have vocational programs, so all students are exposed to a college-prep curriculum (Bryk et al. 1993). Second, Catholic schools have stricter disciplinary environments with more authority to punish or expel students than public schools, which some argue creates better learning conditions (Hoffer et al. 1985). Finally, Catholic schools are sometimes built around close-knit religious communities, and while many non-Catholic students attend Catholic schools, it is argued that the environment promotes a deep commitment to the school by teachers and administrators and close connections to parents (Bryk et al. 1993). This closeness builds social capital that increases students' educational achievement (Coleman 1988). These processes enable Catholic schools to produce better academic outcomes, even after student background is taken into account.

Although extensive research found that Catholic secondary schools generally produce better academic outcomes for students than public secondary schools, the limited research on Catholic primary schools indicates that Catholic school benefits may not exist in earlier grades. From kindergarten to eighth grade, Catholic schools and public schools produce similar math and reading achievement outcomes for students (Jepson 2003; Lubienski and Lubienski 2006; Carbonaro 2006; Hallinan and Kubitschek 2012). Using data from the ECLSK, Reardon, Cheadle, and Robinson (2009) as well as Elder and Jepsen (2013) concluded that Catholic schools are no more effective, and perhaps less

effective, than public schools in promoting mathematics skill growth in primary school, while Catholic and public schools are equally effective in promoting reading skill growth during this period. These studies indicate that Catholic schools are no more effective than public schools in early grades, but there is evidence that suggests otherwise. Lee and Stewart (1989) analyzed the National Assessment of Educational Progress (NAEP) scores of 3rd and 7th graders and found that students in Catholic schools outperformed students in public schools in math (but not science). It is possible that the benefits of attending a Catholic primary school take time to emerge. Students who attended Catholic elementary and middle schools for 8 years had higher math and reading test scores in the 10th grade, but this positive effect was not found for students who attended between 1 and 7 years of Catholic schooling prior to high school (Sanders 1996). However, for education scholars and reformers, the overall effectiveness of Catholic schools in terms of achievement or learning is not the only concern. It is also important to understand whether Catholic schools are able to reduce educational gaps across socioeconomic and racial groups.

Who Benefits the Most from Catholic Schools?

An important finding of the Catholic schools literature is that Catholic secondary schools are especially beneficial for disadvantaged students, such as racial minorities, low socioeconomic students, and students living in urban areas that might have low performing public schools. The original studies on Catholic schools conducted by Coleman and colleagues found that the benefits of attending Catholic schools were larger for racial minorities and socioeconomically disadvantaged students (Coleman et al.

1982b; Greeley 1982; Hoffer et al. 1985). Furthermore, Catholic secondary schools

reduce racial and socioeconomic inequalities in achievement outcomes over time (Lee

and Stewart 1989; Bryk et al. 1993). Morgan (2001) found that the students who were

the least likely to attend Catholic schools (e.g. students from low socioeconomic

households with low educational expectations) benefited the most from a Catholic

education. These benefits may extend to earlier grades as well. A study of Chicago

middle schools found that Catholic schools reduced racial and socioeconomic gaps in

reading but exacerbated gaps in math (Hallinan and Kubitschek 2012).

The equalizing benefits of Catholic high schools extend beyond test scores.

Catholic schools significantly improve the high school graduation rates and college

attendance rates of racial minorities and students from urban areas, while they provide

little or no benefit to whites or suburban students (Neal 1997; Grogger and Neal 2000).

Attending a Catholic high-school boosted high school graduation and college attendance

rates of urban minorities by approximately 25 percent compared urban minorities in

public schools (Grogger and Neal 2000). But, Catholic schools have little or no effect on

these outcomes for urban whites, and mixed effects on suburban students (Grogger and

Neal 2000).

As with the overall Catholic school effect, there is some debate about whether

Catholic schools are better for minorities and socioeconomically disadvantaged students.

Jencks (1985) argued that the findings of Coleman and colleagues were suggestive at

best. Hoffer (1997) analyzed data from the *National Educational Longitudinal Study of*

1988 and found that Catholic schools conferred the same educational benefits to high

school students from advantaged and disadvantaged backgrounds, after controlling for middle school achievement. Finally, research examining Catholic primary schools indicates that they do not improve the skills of racial minorities, socioeconomically disadvantaged, or urban students any more than other students (Lubienski and Lubienski 2006; Jepsen 2003). Given these mixed findings, it remains unclear whether Catholic schools are more beneficial for disadvantaged students.

Arguments for why Catholic schools are more effective than public schools (or appear to be more effective) for disadvantaged students tend to follow four main narratives, succinctly outlined by Morgan (2001). First, the *common school narrative* explains that Catholic schools distribute opportunities in more virtuous and egalitarian ways than public schools, similar to the original *common schools* in the United States. This produces more equal academic outcomes than public schools offer (Coleman et al. 1982b; Bryk et al. 1993). Second, the *differential-sacrifice narrative* argues that families of disadvantaged Catholic school students must make meaningful sacrifices to allow their children to attend Catholic schools. Their children work harder than advantaged students because they recognize this familial hardship (Morgan 2001). Third, the *better alternatives narrative* argues that disadvantaged students who attend Catholic schools have poor alternative schooling options, especially students who cannot afford to live in areas with better public schools (Neal 1997). The final narrative is the *binding-constraint narrative*, and it states that there may be a selection effect of disadvantaged students who attend a Catholic school since attending Catholic schools is expensive. Disadvantaged families only send children who are likely to succeed to Catholic schools while more

advantaged families can afford to send all of their children including those who are less

likely to succeed (Morgan 2001). In other words, lower income families must cream-

skim their most promising children into Catholic schools because of resource constraints,

but higher income families do not have to make that choice. Catholic school research has

not sorted out the real mechanism(s) behind the possible advantage of attending of

Catholic schools for disadvantaged students. These narratives, while mostly conjecture,

provide insights into why Catholic schools reduce (or appear to reduce) educational

inequalities, and they raise important questions about how selection and family

background intersect with schooling choices to produce differential outcomes for

students.

One limitation of these narratives and prior research on Catholic schools is that

they tend to view racial minorities as one monolithic group, ignoring the fact that

Catholic schools may have different effects on certain racial groups. It is possible that

Catholic schools may be even more beneficial for Latino students than African American

students because Latinos are more likely to be Catholic. While few Latino or black

children attend Catholic schools on average (in the fall of 2009, 2.4 percent and 1.8

percent, respectively compared to 5 percent of white students (NCES 2012b), Latino

children are far more likely to be Catholic.[25] It is estimated that between 50 and 90

percent of Latinos in the United States are Catholic (Perl et al. 2006), compared to fewer

than 10 percent of blacks (Sherkat 2002). According to data from the 2000 General

Social Survey, 67 percent of Latinos are Catholic compared to 8 percent of blacks and 23

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[25] Latinos are also the fastest growing population in Catholic schools (McDonald 2003).

percent of whites (author calculations). Similar to the idea that minority students may benefit from having a same race teacher (Dee 2004, 2005; Downey and Pribesh 2004), in Catholic schools, students could gain additional benefits from being Catholic. Catholic students in a Catholic school may be a part of a closely-knit community built around school and church. This could provide students with additional opportunities for students to interact with teachers, for parents to interact with school employees, and for parents to interact with other parents, all of which could increase social capital and learning in schools (Coleman 1998). Since Latinos are more likely to be Catholic, it is possible that attending Catholic schools provides Latinos with unique benefits.

In sum, existing literature suggests that Catholic secondary schools are superior to public schools on average, even after accounting for differences in student background and initial achievement. Catholic schools may be the most beneficial for disadvantaged students (i.e. racial minorities and students from low socioeconomic backgrounds) and in urban locales. However, our understanding of Catholic school effects in earlier grades is limited, and the conclusions of prior work are questionable because of the methodological challenge of assessing the effects of student self-selection into Catholic schools.

Self-Selection, Non-School Learning, and Methodological Challenges in Catholic School Research

Students are not randomly assigned to Catholic and public schools. Rather, parents choose whether to send their children to Catholic schools, and this self-selection process presents several challenges for researchers hoping to isolate the effect of

attending a Catholic school on academic outcomes. Since Catholic schools charge

tuition, students from high socioeconomic backgrounds are more likely to attend them.

For example, in this study, almost 23 percent of children from households in the top two

socioeconomic quintiles attend a Catholic school in first grade, while only 3.5 percent of

students from households in the bottom two socioeconomic quintiles attend these schools.

White students are more likely to attend Catholic schools than racial minorities, which is

due in part to the strong association of race and socioeconomic status. In this study,

whites are twice as likely to attend Catholic schools as blacks and Latinos. Given the

complex Catholic school selection process, researchers examining Catholic schools face

important and difficult methodological challenges.

Self-selection bias is a critical issue that school sector researchers must address.

School selection decisions are based on numerous observed and unobserved

characteristics, and this makes it difficult to estimate the unique effect of Catholic

schools. Numerous techniques have been employed to reduce estimation biases

associated with self-selection. Regression models with covariate adjustment (i.e.

controlling for observed student and family characteristics) are commonly used to

estimate the effects of Catholic schools (c.f. Coleman et al. 1982a; 1982b), but this

approach does not account for unobserved differences between Catholic and public

school students. For example, early studies of Catholic schools by Coleman and

colleagues used covariate adjustment, and they were critiqued for not adjusting for

students' prior achievement (Goldberg and Cain 1982; Alexander and Pallas 1983, 1985).

Achievement growth models are often used to address this shortcoming. Achievement growth models, where academic achievement at an earlier time period is used to predict outcomes at a later time period (c.f. Jepson 2003), reduces estimation bias by accounting for initial ability - which is likely to be correlated with observed and unobserved characteristics and can capture some of those effects. However, these models are not able to fully account for unobserved student characteristics, nor can they fully account for learning that occurs outside of schools. For example, if students who attend Catholic schools learn more outside of school than students who attend public schools, then achievement growth models would upwardly bias estimates of Catholic school effects.

Several other advanced techniques have been employed to isolate Catholic school effects. Economists have used instrumental variables, which reduce omitted variable bias by accounting for the correlation between observed variables and the error term, to provide estimates of the effect of attending Catholic schools (Neal 1997; Grogger and Neal 2000). However, these studies have been criticized because the instrumental variables were poor quality (Altonji, Elder, and Taber 2005). Multilevel modeling is used to account for the nesting of students within schools. This modeling strategy can generate appropriate standard error estimates for student- and school-level data, and it allows the researcher to independently estimate student-level and school-level effects (c.f. Bryk et al. 1993; Lubienski and Lubienski 2006). However, multilevel modeling in itself does not address unobserved variable biases.

Morgan (2001) argued that regression modeling, especially multilevel modeling, should remain the "workhorse" of school effects research because it generates estimates of the effects of covariates and tends to be robust to biases, but this modeling strategy does not account for selection bias. Propensity score matching can partially account for selection bias, and Morgan claimed they should be presented as a supplement to regression analysis. Propensity score matching uses observed variables to match students who have similar propensities for seeking a treatment (e.g. attending a Catholic school), but this technique also does not account for unobserved variables or non-school learning. Finally, Reardon et al. (2009) used fixed effects modeling in conjunction with propensity score matching to address self-selection bias associated with the quality of local public schools. However, this approach does not address unobserved characteristics of the individual students that might affect Catholic school attendance and academic outcomes. In addition, their analyses are similar to the achievement growth models described above, so their results cannot separate school year and summer learning.

Prior research is often vulnerable to self-selection bias, unobserved variable bias, an inability to separate school learning from summer learning, or a combination of these limitations. Additional research on Catholic school effects that addresses these limitations is needed to strengthen our understanding of this important educational context. Seasonal comparison techniques provide a tool to overcome these challenges.

Using Seasonal Comparison Techniques to Study School Effects

The essential feature of seasonal comparison analysis is that it allows the researcher to measure separately school year learning and summer learning, and that

feature offers important advantages over many of the methods that are typically used to measure school effects. This technique is valuable because schools are one of many sources of enrichment and experiences that promote learning. During the school year, school factors (e.g. teacher experience, funding for facilities) and non-school factors (e.g. parental engagement, community resources) shape learning. During the summer, non-school factors dominate learning, and estimating summer learning offers researchers a tool to isolate the contribution of the non-school environment to learning (Heyns 1978; Downey et al. 2004; Entwisle and Alexander 1992). These separate "seasonal" estimates can then be used to examine how quickly students learn, how quickly skill gaps expand or contract, and how important characteristics such as race, class, and gender shape those patterns. The difference between school year learning and summer learning represents a reasonable estimate of the "impact" of schools, independent of non-school factors (Downey et al. 2008). Unfortunately, high quality seasonal data are rare, and this technique is applied far less often than the techniques discussed above.

Downey, von Hippel, and Hughes (2008) introduced *Impact* as a counterfactual tool to examine school effects using seasonal learning estimates. In simple terms, *Impact* is the difference between the school-year learning rate and the summer learning rate of students within a school. This difference represents the change in students' learning rate that can be attributed to the school. *Impact* is a school-level measure that addresses some of the major limitations that are present in prior research on Catholic schools. First, *Impact* does not simply compare achievement scores of Catholic and public school students at a given time point, so school effects estimates are not determined by

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educational experiences that were prior to the study period. *Impact* includes skill

assessment scores from the beginning and the end of the study period, therefore school

effects are only calculated for the observed assessment period. Second, unlike typical

achievement growth models, *Impact* incorporates separate estimates of summer learning

and school year learning. Therefore, school effects estimates are not comingled with

summer learning driven by non-school factors. Recent research found that value-added

models based on the nine-month school year correlate at approximately .50 with models

based on annual data (Attebury 2012). This moderate correlation indicates that value-

added models relying on yearly data produce substantial bias because they fail to

properly account for summer learning. Third, as Downey et al. (2008) note, students

spend the majority of their time out of school, even during the school year. Even the best

covariate adjustment is unlikely to properly capture all of the non-school factors that

affect learning. Failure to account fully for the non-school environments and experiences

that shape student learning may result in unobserved variable bias and inappropriate

conclusions about school effects. *Impact* accounts for the effects of the non-school

environment that shape student learning, and it eliminates the possibility that unobserved

variables associated with academic achievement and Catholic school attendance will bias

the results.

Like all evaluation techniques, *Impact* relies on important assumptions. First, it

assumes that there is little "contamination" of summer learning by school-based

processes. This is a difficult assumption to test with confidence, but the little information

that exists suggests that this is not an unreasonable assumption. For example, some

schools in the ECLSK send children home after kindergarten with book lists and other

school-related assignments, and these school practices might contaminate summer

learning estimates. It turns out, however, that these practices are unrelated to students'

summer learning (Downey et al. 2008). Second, *Impact* assumes that summer learning is

the best estimate of how students' non-school environments matter during the school

year. For example, if parents of children attending Catholic schools provide better non-

school environments during the summer, the model assumes that these advantages persist

at the same magnitude during the school year. It is unclear the extent to which these

assumptions are reasonable, but proponents of the *Impact* measure point out that all

modeling strategies employ assumptions and that those related to *Impact* are arguably the

least onerous (Downey et. al. 2008).

   To address the challenges present in estimating Catholic school effects, this study

will use propensity score matching and multilevel modeling to examine math and reading

Student Impact in Catholic and public schools. These techniques overcome the

limitations of prior research that examined Catholic school effects, and together they will

yield the best estimates of Catholic school effects on math and reading learning at the

beginning of elementary school.

Data and Measures

   This research uses data from the Early Childhood Longitudinal Study –

Kindergarten Class 1998-1999 (ECLSK), a nationally representative sample of more than

21,000 students in more than 1,300 schools who started kindergarten in the 1998-1999

school year. The ECLSK collected information on student characteristics, student

achievement, school characteristics, and many other important measures between kindergarten and eighth grade. These data are ideal for this research because of the quality and scope of data collected as well as its national representativeness. The ECLSK also provides student skill assessment data for math and reading from the three time points that are necessary for the seasonal techniques employed in this research - near the end of kindergarten, near the beginning of first grade, and near the end of first grade.

I restrict my analytic sample to include only those students who took math or reading assessments at all three testing periods, as is necessary for the analytic approach. This reduces the analytic sample substantially because only 30 percent of the participating schools were randomly selected to participate in the fall-of-first-grade assessment period. I omit students who attended year-round schools because they are not appropriate for seasonal comparison. I exclude students who changed school-sectors (e.g. moved from a public school to a Catholic school) during the first grade because it is not possible to precisely estimate the contributions that each school made to student learning. I also exclude students who attended non-Catholic private schools, because these schools fall outside of the scope of this research. Finally, I exclude a small number of students with missing race and gender information.

I use multiple imputation to address missing values for my explanatory variables (Rubin 1987). I use Stata's *ice* command to create 5 versions of the data. I include all cases in the imputation model but exclude cases from my analyses if they have missing values on the dependent variable (Allison 2002; von Hippel 2007). This approach, known as multiple imputation then deletion, allows me to use the information that is

available in cases with missing Y values to impute missing X values in other cases, while

it eliminates the risk of including poorly imputed Ys in my analysis. I impute student

level data and school level data separately and then combine the student level and group

level data to ensure that each student within a given school or location had the same

values for group level characteristics (Downey et al. 2008). When imputing missing

values for student level data, I account for the clustering of students within schools by

using wide-format data with student-level and school-level variables on a single row and

include school identification number as a predictor variable in my imputation model.

This allows both school level and student level variables to predict missing values for

each student (see Allison 2002; Downey et al. 2004). The final analytic sample is

approximately 4030 students within 390 schools, which represents 19 percent of the

original sample of kindergarteners. Approximately 3510 of those students attended

public schools and 520 attended Catholic schools.[26]

Dependent Variables

      The dependent variables in this research are schools *Impact* on student math and

reading learning rates in the first grade. This is a student-level measure that I refer to as

Student Impact, which differs from Downey et al.'s (2008) school-level measure called

Impact.[27] *Student Impact* is derived from standardized math and reading achievement

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[26] I round all sample sizes to the nearest 10 to protect student confidentiality as required by the National Center for Education Statistics.

[27] Downey et al. (2008) introduced *Impact* as a more valid measure of school effectiveness. They compared *Impact* to other measures of school effectiveness (e.g. achievement test scores) to illustrate how our understanding of schools can change when we apply different measures of school effectiveness. *Impact* for Downey and colleagues was a school-level measure, and that was appropriate for their analysis. I adapt *Impact* into student-level measure, *Student Impact*, and that allows me to assess how student and school characteristics affect individual student learning. If I were to use the school-level measure, I would be unable to assess effects of individual level characteristics on learning.

tests that were administered to students in near the end of kindergarten, near the

beginning of first grade, and near the end of first grade. The calculation of *Student

Impact* is discussed in detail in the next section.

Math assessments included the following concepts: knowledge of one-digit

numbers and shapes, knowledge of relative size of numbers and objects, reading two-

digit numerals and recognizing the next number in a sequence, and solving simple

problems in addition, subtraction, multiplication, and division. Reading assessments

included the following concepts: identifying letters, associating letters with sounds at the

beginning and end of words, recognizing common "sight" words, and reading words in

context.[28]

From these assessments, the ECLSK calculated Theta scores for each student in

math and reading using Item Response Theory (IRT) techniques. IRT uses patterns of

correct and incorrect answers to assign a score, Theta, that can be used order test-takers

by ability as well as compare students' results from different tests (Sijtsma and Molenaar

2002). This research uses Theta scores to address some of the limitations that are

presents in the scale scores that are also available in the ECLSK, especially the positive

skew of scale scores and their non-normal distribution (LoGerfo, Nichols, and Reardon

2005). I construct *Student Impact* from these Theta scores.

Constructing Student Impact

Student Impact is calculated by subtracting each student's monthly learning rate

during the summer before first grade from his or her first grade monthly learning rate.

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[28] For more details on these assessments, see the Early Childhood Longitudinal Study – Kindergarten Class of 1998-1999 *User's Manual*.

Student Impact is an estimate of the effect of schooling on learning rates, net of the

effects of the non-school environment, and it can be understood as the change in a

student's monthly learning rate that is attributable to schooling (also referred to as a

learning rate boost). I construct *Student Impact* for each student in two steps. First, I

calculate the monthly learning rate for the first grade and the summer preceding first

grade for each child. Second, I subtract the summer learning rate for each child from

her/his first grade learning rate.

> *Student Impact$_{ij}$ = Monthly School Year Learning Rate$_{ij}$ − Monthly Summer Learning Rate$_{ij}$*

$$Monthly\ School\ Year\ Learning\ Rate\ ij\ =\ \frac{(\theta ijt3 - \theta ijt2)}{(tj3 - tj2)/30.42}$$

$$Monthly\ Summer\ Learning\ Rate\ ij\ =\ \frac{(\theta'ijt2 - \theta'ijt1)}{(t'j2 - t'j1)/30.42}$$

The monthly school year learning rate of student *i* in school *j* is the difference

between spring and fall assessment scores in the first grade (θ_{ijt3}- θ_{ijt2}) divided by the time

in months between assessments during the school year ((t_{j3}-t_{j2})/30.42).

The monthly summer learning rate of student *i* in school *j* is calculated in a

similar fashion, however the summer learning rate is the difference between each

student's extrapolated Theta scores on the last day of kindergarten and the first day of

first grade ($\theta'ijt2 - \theta'ijt1$) divided by the time in months between the last day of

kindergarten and the first day of first grade $(t'j2 - t'j1)/30.42$.[29]

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[29] Details about calculating extrapolated Theta scores for spring of kindergarten and fall of first grade are available in Chapter 2.

Independent Variables

The independent variable of interest, school sector, is a dummy-coded measure that identifies whether a school is Catholic or public. Catholic schooling is the treatment condition (equals 1) and public schooling is the control condition (equals 0) in the propensity score matching analysis described below. At the school level, I include dummy coded variables to account for school locale type. Large and midsized cities (as determined by the NCES) are defined as urban locales. Large and midsize fringe cities are defined as suburban locales. Finally, large towns, small towns, and rural areas are defined as rural locales. Urban locales are the omitted reference category.

On the student level, I control for several observable student characteristics that might affect learning. I account for student race with four dummy variables representing black, Latino, Asian, and "other race" students. Students defined as "other race" include Native American, Native Alaskan, Native Hawaiian, Pacific Islander, and non-Hispanic multi-racial students. Non-Hispanic whites are the reference group. I include a dummy variable to control for student gender (1=female). I account for family socioeconomic status using the socioeconomic quintile measure provided in the data. Family socioeconomic status was calculated by the NCES using a combination of parent/guardian education, parent/guardian occupation, and household income. I include four dummy variables representing socioeconomic quintiles (normed internally by the ECLSK) and use the highest (5th) quintile as the reference group. I also include controls for family background including: parents' marital status (0=married biological parents; 1=unmarried biological parents), the primary language spoken at home (0= English is the

primary language at home, 1=English is not the primary home language), and a continuous measure of the number of siblings of the respondent child.

I include a control for whether the child is repeating kindergarten during the study period (1=kindergarten repeater). In multilevel models only, I also include continuous measures of student age (in years) at the beginning of the first grade that is grand mean centered and the number of absences the student had during the first grade. In propensity score matching models only, I include a continuous measure of the number of absences the student had in kindergarten, because it might predict the probability of attending a Catholic school in the first grade.

Finally, in propensity score matching models only, I include dummy coded measures of whether students' families are religiously active. In kindergarten, students in religiously inactive families have parents who never, almost never, or several times per year speak with their children about religion. Students in religiously active families have parents who speak with their children about religion several times per month or several times per week. In the first grade, students in religiously inactive families have parents who attend religious services never, almost never, or several times per year. Students in religiously active families have parents who attend religious services a few times per month, once a week, or multiple times per week.[30] In both variables, inactive families are the reference category. The ECLSK does not offer data on religious affiliation (e.g. whether the family is Catholic), but these controls will partially measure family religiosity and partially predict the probability of Catholic school attendance. A

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[30] Table B.1 provides a list of variables and their application by modeling strategy.

summary of each of the variables included in the propensity score matching models and multilevel models is detailed in Table B.1.

Analytic strategy

This research uses two separate techniques to answer the research questions. I use propensity score matching to examine the effect of attending Catholic and public schools on *Student Impact* in math and reading, and I use multilevel models to assess whether the effects of attending Catholic schools vary by student race, socioeconomic status, and school locale. Both of these techniques use math and reading *Student Impact* as the dependent variable.

Propensity Score Matching

To analyze *Student Impact*, I first employ propensity score matching (Rosenbaum and Rubin 1983). Propensity score matching (PSM) is a statistical technique that compares students who have similar propensities of seeking treatment (e.g. attending a Catholic school) to estimate the effect of obtaining that treatment. PSM falls within the broad counterfactual framework. The central tenant of this framework is that it is impossible to observe a person in both treatment and control conditions simultaneously; therefore we cannot directly estimate treatment effects (Winship and Morgan 2007). In this analysis, students cannot be simultaneously observed in the treatment condition (attending a Catholic school) and the control condition (attending a public school).[31] This

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[31] In this dissertation, I apply propensity score matching to the Catholic versus public schools analysis here but not to the racial segregation analysis in Chapter 2 because I do not believe it is appropriate to "match" similar students in white-segregated versus minority-segregated schools given that the racial composition of schools is not a choice for many students and parents. While middle-class parents can choose to send their kids to a school with few minorities and few poor students, lower class parents are unlikely to have that option. Especially at the time the ECLSK were collected (1998) when school choice was not widely

means that it is not possible to directly estimate the effects of attending a Catholic school for each individual, and I must estimate the average effects of receiving treatment. I could obtain average treatment effects by estimating and comparing the means math assessment scores between students that attended Catholic schools and public schools, but this comparison is problematic.

Average treatment effects are biased by the fact that students are not randomly assigned to Catholic and public schools. Rather, students and parents choose Catholic schools based on a variety of factors, including the expected benefits of attending a Catholic school and their available resources to pay for Catholic schooling. The average treatment effect is likely the result of student and parent characteristics (e.g. socioeconomic status) that affect both treatment selection (i.e. the decision to attend a Catholic school) and educational outcomes.

One way to address this problem in observational data like the ECLSK is to "match" members of the treatment group to members of the control group with similar probabilities (or propensities) of seeking treatment. The matched cases from the treatment and control groups have similar aggregate values for observable characteristics that are expected to affect seeking treatment. Therefore the treatment itself is assumed to be the only variable affecting differences in outcomes. Importantly, the seasonal

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available, attending a different elementary school would likely require a residential relocation or perhaps a move to another city (e.g. city to suburb). Simply, the racial composition of a public school is something that many students/parents (especially disadvantaged parents) must accept, and there are not necessarily alternative treatment options for them to seek. On the other hand, Catholic schools, where they are available, are an alternative treatment that might be within the grasp of students/parents from all backgrounds - especially if those schools offer scholarships or vouchers. Therefore, I decided that PSM was an ideal tool to apply to the research questions in Chapter 3 but not Chapter 2.

comparison techniques used in this research take into account many of the observed and

unobserved factors associated with Catholic school attendance and learning. PSM is

valuable because it also allows me to account for other factors that might affect Catholic

school attendance but might not be appropriate as controls in a multilevel model

predicting first grade learning – such as religiosity. In addition, Morgan (2001) argues

that if the people who are likely to receive the most benefit from Catholic schooling are

also those who are most likely to self-select into Catholic schooling, this could bias

treatment effects estimates in regression models. PSM allows me to compare students

with similar propensities of attending Catholic schools, thus eliminating that potential

source of bias.

To obtain a propensity score for each student, I first run a logistic regression

model that predicts Catholic school enrollment based on a set of observed covariates that

affect the probability of enrollment. Each student's propensity score is determined by

their unique set of covariates and the estimated effect of each covariate on Catholic

school enrollment.[32]

> *Pr[Catholic School=1| Socioeconomic Quintile + Student Gender + Student Race +*
> *Language Spoken at Home + Parents Marital Status + Number of Siblings + Number*
> *of Absences in Kindergarten + Kindergarten Repeater Status + Religiously Active in*
> *Kindergarten + Religiously Active in First Grade + School Locale]*

Treatment is defined as attending a Catholic school in first grade. The probability of

attending a Catholic school in the first grade is predicted by numerous covariates,

including student socioeconomic status, race, and gender, as well as family structure,

school locale, and whether parents are religiously active.

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[32] I use the *pscore* command suite in Stata 12 to obtain propensity scores and estimate treatment effects.

Once propensity scores are assigned to each student, I restrict the sample to those students who fall within the region of common support. The region of common support is the range of propensity scores of control group members (Morgan and Winship 2007). Focusing only on cases within this region limits the matching sample to those students for whom a reasonable match exists. Students outside this region are poor matches for other students because they have relatively extreme propensity scores.[33]

I employ kernel matching to create comparable treatment and control samples. Rather than matching each treatment group member to its best control group match(es) using nearest neighbor or caliper matching, kernel matching uses all of the control cases in region of common support, and assigns weights to the control cases based on their closeness to a given treatment case. Higher weights are assigned to control cases that are the closest matches (i.e. have the most similar propensity score) for each treatment case (Heckman, Ichimura, and Todd 1998; Morgan and Winship 2007).[34] Other matching techniques may require the researcher to establish an acceptable range of propensity values that might be considered a match (i.e. caliper), or they simply match the cases that are closest together. This can result in bad matches and is also likely to result in dropped cases. Kernel matching uses all available data to generate treatment effect estimates, and it eliminates the possibility of defining overly broad matching parameters or matching students who are not necessarily comparable (Heckman, Ichimura, and Todd 1998).

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[33] Findings from the propensity score models are the same when I apply the region of common support constraint and when I do not apply this constraint. Treatment group sample sizes are the same with and without the region of common support constraint; however control group samples are 2 to 45 percent smaller when I constrain my matched samples to the region of common support compared to the unconstrained sample. The small effect of substantial sample changes is likely driven by the matching technique I employ (kernel matching) which down-weights poor matches.

[34] I complete 500 repetitions of bootstrap estimation to generate standard errors for each model because they are not automatically generated with kernel matching.

Finally, the stable unit treatment value assumption (SUTVA) is important in counterfactual modeling. In simple terms, SUTVA says that the treatment effect is stable, regardless of the number of people seeking treatment (Winship and Morgan 2007). For Catholic schooling, this assumption is implausible (Morgan 2001; Winship and Morgan 2007). If the number of students attending Catholic schools suddenly increased tenfold, then this would probably affect the observed effects of Catholic schooling since existing schools would serve more students. In contrast, the number of people taking aspirin to prevent heart attacks, for example, would not alter the effect of aspirin on heart attacks for any individual seeking treatment. The estimates produced by PSM in this study represent the average treatment effect on the treated (ATT) because of the likely violation of SUTVA. ATT estimates are the average effect of attending Catholic schools rather than public schools for the students who are likely to attend Catholic schools. In the counterfactual tradition, ATT is the hypothetical result that would arise if one could observe the learning outcomes of a randomly selected Catholic school student in both the public school and Catholic school setting (Winship and Morgan 2007). The propensity matching estimates in this research must be narrowly interpreted as ATT estimates, therefore the policy implications of the PSM estimates must carefully understood based on this narrow interpretation.

To understand how the ATT of Catholic schooling varies, I perform separate PSM analyses on several subsamples of the full dataset. These subsamples fall within the following groups: (1) School Locale – urban, suburban, and rural (2) Race – white, black, Latino, and (3) Socioeconomic Status – bottom two socioeconomic quintiles, top two

socioeconomic quintiles. The eight subsamples within these three groups allow me to understand how the effect of Catholic schools on *Student Impact* varies within group, but these analyses alone cannot determine if Catholic schooling differently affects racial or socioeconomic gaps more than public schooling. Other analyses are needed to make those determinations.

Multilevel Modeling

I also use multilevel models to further examine Catholic schooling effects on *Student Impact*. As noted previously, Morgan (2001) argued that using techniques like PSM alongside regression techniques is a better approach to assessing school effects. PSM techniques are not ideal for assessing whether Catholic schools narrow educational gaps more than public schools across racial and socioeconomic groups. This is because PSM uses covariates to predict propensity scores, but PSM does not generate estimates of the effects of each covariate on the outcome. Those coefficients are needed to compare Catholic and public schools' effects on racial and socioeconomic gaps.

Multilevel modeling is ideal for this research because it allows me to separately estimate the effects of student-level characteristics and school-level characteristics on an outcome, and it properly accounts for the clustered nature of the data (Raudenbush and Bryk 2002). Several student characteristics are included in the student level (Level 1), such as student race, gender, and socioeconomic quintile. At the school level (Level 2), I account for school sector and school locale type (urban, suburban, or rural). This allows me to determine if Catholic schooling has a significant and meaningful effect on *Student Impact*, net of individual characteristics and school locale. I next stratify the sample by

school sector and examine the effects of student socioeconomic status and race on *Student Impact.* This allows me to assess size of racial and socioeconomic gaps in Catholic and public schools separately.

Model 1, Level 1

$$Student\ Impact_{ij} = \beta_{0j} + \beta_{1j}SES\ Quintile + \beta_{2j}Gender + \beta_{3j}Race + \beta_{4j}English\ Language$$
$$Home + \beta_{5j}\ Marital\ Status + \beta_{6j}\ Number\ of\ Siblings + \beta_{7j}Absences + \beta_{8j}School\ Year$$
$$Repeater + \beta_{9j}Age\ at\ School\ Start + r_{ij}$$

Model 1, Level 2

$$\beta_{0j} = \Upsilon_{00} + \Upsilon_{01}Suburban + \Upsilon_{02}Rural + \mu_{0j}$$

In Model 1, *Student Impact* for student i in school j is a function of the average *Student Impact* of school j (β_0) plus the additive effects of student socioeconomic quintile, gender, and race (β_{1j}, β_{2j}, and β_{3j}), family characteristics (β_{4j}, β_{5j}, and β_{6j}), school exposure (β_{7j}, β_{8j}), student age (β_{9j}) and student level random variation from the school mean (r_{ij}) on Level 1. On Level 2, the average *Student Impact* in school j, (β_{0j}), is a function of the grand mean *Student Impact* rate (Υ_{00}), school locale (Υ_{01}, Υ_{02}), and a school-level random effect (μ_{0j}).[35]

Model 2, Level 2

$$\beta_{0j} = \Upsilon_{00} + \Upsilon_{01}Suburban + \Upsilon_{02}Rural + \Upsilon_{03}Catholic + \mu_{0j}$$

Model 2 adds school sector (Υ_{03}) to Level 2 of the model predicting the average *Student Impact* in school j, (β_{0j}). Model 2 allows me to assess relationship between school sector and *Student Impact.* Level 1 remains as it was in Model 1.

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[35] All analyses are conducted with unweighted data to produce more accurate standard errors (see Winship and Radbill 1994).

<u>*Models 3 and 4 (School Sector Subsamples)*</u>

$$\beta_{0j} = \Upsilon_{00} + \Upsilon_{01}Suburban + \Upsilon_{02}Rural + \mu_{0j}$$

In Models 3 and 4, I stratify the sample by school sector, and run separate

analyses on each subsample. This allows me to assess how the effects student- and

school-level characteristics vary by sector. To perform this analysis, I remove school

sector (Υ_{03}) from Level 2 and only control for school locale (Υ_{01}, Υ_{02}). Level 1 remains

the same as in Model 1.

Results

Table B.2 presents descriptive statistics of the dependent and independent

variables. Nearly 13 percent of the students in the analytic sample attend Catholic

schools, while 87 percent attend public schools. Students in the bottom socioeconomic

quintile are slightly underrepresented, at 17.6 percent of the sample, while students in the

top socioeconomic quintile are slightly overrepresented, at 22 percent. The mean *Student*

Impact is 0.055 in math and 0.087 in reading.

Are Catholic Schools More Effective than Public Schools for Student Impact?

Tables B.3 and B.4 display the results from propensity score matching analyses

for math and reading respectively. Each table provides results for the full sample, as well

as results from subsamples relevant to the research questions. For the full sample, all

students in the analytic sample are eligible to be matched, while in each subsample, only

those students who meet the noted selection criteria (e.g. they are black, they are in an

urban area) are eligible to be matched with one another. The coefficients should be

interpreted as the average effect of attending Catholic schools among students who are

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likely to attend Catholic schools. Positive coefficients in these tables indicate that Catholic schools increase learning rates more than public schools for students likely to attend Catholic schools.

Starting with the full sample, Table B.3 shows that Catholic schools have a significant negative treatment effect on math learning. This means that the math learning rates of students attending Catholic schools increased less than they would have if the students had attended public schools. This statistically significant treatment effect is only 0.1 S.D. units.[36] The full sample results in Table B.4 indicate that there is no treatment effect of Catholic schooling for reading. The next step is to consider Catholic school treatment effects for specific sub-groups of the population, to determine if Catholic school treatment effects vary across these groups.

Does School Locale Affect Catholic School Treatment Effects?

Prior research on Catholic high schools suggests that Catholic schools are particularly effective for learning in urban areas relative to public schools, but not necessarily in suburban areas. These results do not support that claim. The results for the urban subsample (in Table B.2) indicate that urban Catholic schools have a significant (p<.01) negative treatment effect in math that represents almost 0.2 S.D. units. However, the Catholic school treatment effect in math is not significantly different from zero for the suburban and rural subsamples. In reading, Table B.3 indicates that Catholic treatment effects are not significant among urban or suburban subsamples. However, Catholic schools in the rural subsample have a positive, marginally significantly (p<.10) *Student*

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[36] Standard deviation unit calculated by math ATT coefficient (-0.013) divided by the standard deviation of Student Impact in math (0.133).

Impact in reading. These results suggest that in urban areas, students in Catholic schools experience a significantly smaller math learning benefit than students in public schools. In suburban areas, Catholic and public schools produce similar *Student Impacts* in math and reading. In rural areas, Catholic schools increase *Student Impact* in reading more than public schools, but the result for rural schools is marginally significant and should be interpreted cautiously. In sum, these results indicate that urban Catholic schools are not a superior learning environment for first graders overall, in contrast to some findings on Catholic secondary schools.

Are Catholic School Effects Larger for Blacks and Latinos than for Whites?

Prior research indicates that racial minorities experience particularly large benefits from Catholic schooling. To examine whether this is true for *Student Impact* in math and reading, I estimate the Catholic school treatment effects for white, black, and Latino student subsamples.[37] Among white students, Catholic schooling has a non-significant treatment effect in both math and reading. For black students, Table B.3 indicates that the Catholic treatment effect is negative (-0.043) and statistically significant (p<.05) *Student Impact* in math. Similarly, Table B.4 indicates that the Catholic treatment effect is negative (-0.050) and significant (p<.01) in reading. In both math and reading, the learning rates of black students in Catholic schools would have increased more if they had attended public schools. The treatment effect of Catholic school attendance is moderately strong for black students, at -0.33 S.D. units in math and -0.40 S.D. units in reading. Finally, Latinos attending Catholic schools experience no net

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[37] I did not analyze race specific subsamples for Asian students or other race students, however those students are represented in all other analyses presented in Tables B.3 and B.4.

benefit from Catholic schooling in math, but their reading learning rate increases significantly more in Catholic schools than if they had attended public schools. The positive effect of Catholic schooling on Latino reading skills growth is the only statistically significant benefit of Catholic schooling among the racial sub-samples, and it is thus far the only evidence suggesting that Catholic schools might close achievement gaps between students from advantaged and disadvantaged groups.

Are Catholic School Effects Larger for Socioeconomically Disadvantaged Students?

Finally, PSM is applied to a subsample of students from the bottom two socioeconomic quintiles and a subsample of students from the top two socioeconomic quintiles. These subsamples broadly represent students from disadvantaged and advantaged socioeconomic backgrounds. In Tables B.3 and B.4, the results indicate that the treatment effect of attending a Catholic school is positive but non-significant in both math and reading for disadvantaged students. This suggests that attending a Catholic school does not boost learning for socioeconomically disadvantaged students more than attending a public school. For socioeconomically advantaged students, however, Table B.3 indicates that the Catholic school treatment effect for math is negative (-0.017) and statistically significant (p<.05). Socioeconomically advantaged students in Catholic schools actually experience a smaller math learning rate boost in first grade than if they had attended public schools. In reading, there is no effect of attending a Catholic school for socioeconomically advantaged students.

In sum, PSM is intended to compare students who are as similar as possible in their propensities to seek treatment (here, a Catholic education). Among students who

have similar propensities to attend Catholic schools in first grade, these results suggest that Catholic schooling rarely produces superior *Student Impact* in math or reading compared to public schooling. Only for Latinos and students in rural locales do Catholic schools produce a significantly larger *Student Impact* than public schools, and this is true in reading but not math. These results also indicate that black students as well as students in urban areas are significantly worse off in math if they attend Catholic schools rather than public schools. Black students are also experience significantly smaller *Student Impact* in reading in Catholic schools. Surprisingly, socioeconomically disadvantaged students experience no net benefit or harm from attending Catholic schools, however, socioeconomically advantaged students experience a significantly smaller increase in their math learning rate if they attend Catholic schools instead of public schools.

The subsample findings using PSM allow me to examine the effect of attending a Catholic school for members of certain groups, but they do not tell me much about whether Catholic schooling can narrow gaps between whites and minorities or gaps between socioeconomically advantaged and disadvantaged students. To explore these effects, I use multilevel models that examine how Catholic and public schools affect racial and socioeconomic differences in learning. There are limitations that go along with estimating average treatment effects using these models, as described above, but this modeling strategy is necessary to examine these group differences in greater detail.

Catholic Schooling Effect on Racial and Socioeconomic Gaps – Math Student Impact

Models 1 through 4 in Table B.5 examine the effect of student-level and school-level characteristics on *Student Impact* in math. On the student level, I present only the

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results for student socioeconomic quintile, gender, and race, however the models also control for several other factors such as family structure, number of absences, student age, and whether the student repeated kindergarten. Model 1 includes student-level controls and school-level controls for school locale, while Model 2 adds a control for school sector (Catholic versus. public) on the school level. Model 1 indicates that students from the lowest (1st) socioeconomic quintile have a marginally significant (p<.10) *Student Impact* advantage over students from the top (5th) socioeconomic quintile, while students from the fourth socioeconomic quintile have a significant (p<.05) *Student Impact* advantage over student from the top socioeconomic quintile. The effect size of being in the 1st or 4th quintile is only 0.1 S.D. units. The results also indicate that black and Latino *Student Impact* in math is similar to that of white students (the reference group). Finally, looking at school locale, Model 1 indicates that suburban schools have a significantly (p<.05) lower *Student Impact* on math learning rates than urban schools. The effect being a suburban school is also small at 0.1 S.D units.

Model 2 in Table B.5 adds a control for school sector. Model 2 indicates Catholic schools' effect on math *Student Impact* is not significantly different from public schools, after controlling for all other factors. Accounting for school sector has very little effect on student-level coefficients. Overall, these results suggest that students who are at the bottom of the socioeconomic distribution may experience large math learning benefits from schooling, while blacks' and Latinos' *Student Impact* in math is neither advantaged nor disadvantaged by schooling relative to white students. Suburban schools actually increase math learning rates significantly less than urban schools. However, Catholic and

public schools have similar effects on math learning overall, and accounting for school sector has very little effect on socioeconomic or racial gaps.

In Models 3 and 4 of Table B.5, the sample is stratified by school type and reanalyzed. Model 3 examines Catholic school students only and Model 4 examines public school students only. In Model 3, among Catholic school students, student socioeconomic status is generally negatively associated with math *Student Impact*, but the effect is never statistically significant. Black students in Catholic schools have lower math *Student Impact* than whites (approximately 0.3 S.D), and this disadvantage is marginally significant (p<.10). Latinos' math *Student Impact* is not significantly different from whites. School locale does not significantly affect learning outcomes in Catholic schools. These results suggest that Catholic schools produce similar math *Student Impacts* for students from various locales and socioeconomic quintiles. While Latino and white students in Catholic schools experience a similar increase to math their learning rates, black students in Catholic schools experience a significantly smaller increase to their math learning rates (p<.10) compared to white students. In math, Catholic schools do not appear to be compensatory for disadvantaged students.

Model 4 in Table B.5 examines math *Student Impact* among public school students. Notably, students from the bottom socioeconomic quintile as well as students from the fourth socioeconomic quintile experience a significantly higher math *Student Impact* than students from the top socioeconomic quintile. This indicates that the most socioeconomically disadvantaged students in public schools experience larger math learning rate benefits from schooling than students from the top socioeconomic quintile.

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Students from the fourth socioeconomic quintile similarly experience this benefit in public schools but not Catholic schools. The effect size is for 1st and 4th quintile students relatively small, however, at about 0.15 S.D. units. In addition, black and Latino students' math *Student Impact* is not significantly different from that of white students in public schools. Finally, students in suburban public schools experience significantly lower math *Student Impact* ($p<.05$) than students in urban areas. This result is surprising because urban public schools are often labeled as ineffective, especially when they are evaluated using average achievement levels. Additionally, the non-significant findings for blacks and Latinos are surprising given the persistent black-white and Latino-white achievement score gaps in math. However, these findings suggest that public schools produce similar learning rate benefits in first grade math for whites, blacks, and Latinos - net of summer learning.

Cross-level interaction models (not shown) indicate that the effect of being in the bottom socioeconomic quintile on *Student Impact* does not significantly vary by school sector, and the effect of race does not significantly vary by school sector. In addition, interactions of school sector and school locale (not shown) are not significant, which indicates that school sector effects do not significantly vary by school locale. Although the effects of school sector do not appear to vary by socioeconomic quintile or race, it is clear that Catholic schools are not more compensatory for disadvantaged students than public schools when schools are evaluated by their effects on math learning rates.

Catholic Schooling Effect on Racial and Socioeconomic Gaps – Reading Student Impact

Table B.6 presents the multilevel model results for reading *Student Impact*. In Models 1 and 2, the results indicate that students from the bottom (1st) socioeconomic quintile experience a significantly (p<.001) higher reading *Student Impact* than students from the top socioeconomic quintile. This *Student Impact* advantage is equivalent to approximately 0.25 S.D. units, and suggests that schooling narrows reading skills gaps between top and bottom socioeconomic quintiles relative to what those gaps would have been without schooling. In addition, black students experience significantly lower reading *Student Impact* (p<.05) than white students, while there is no significant difference between Latino and white students. On the school-level, rural schools are have a significantly higher (p<.05) reading *Student Impact* than urban schools, but the effect size is small at 0.15 S.D. units. Finally, in Model 2, the effect of Catholic schools on reading *Student Impact* is not significantly different from public schools, and including school sector in the model has almost no effect on student-level coefficients.

Models 3 and 4 in Table B.6 analyze Catholic and public school students separately. Once again, Catholic school students from different socioeconomic quintiles have similar reading *Student Impact* rates, but this is not true in public schools. In public schools (Model 4), students in the bottom socioeconomic quintile and the fourth socioeconomic quintile experience a significantly greater reading learning rate boost from schooling than students in the top socioeconomic quintile. A cross-level interaction of school sector and socioeconomic quintile (not shown) indicates that the effect of

socioeconomic status on reading *Student Impact* does not significantly vary by school sector.

Black students in Catholic schools experience a significantly lower (p<.01) reading *Student Impact* than white students in Catholic schools. In Catholic schools, black students' reading *Student Impact* is approximately 0.45 S.D. units lower than that of white students. In public schools, the reading *Student Impact* of blacks does not significantly differ from whites. In contrast to the reading disadvantage experienced by blacks in Catholic schools, Latino students in Catholic schools experience greater reading *Student Impact* than white students, and this effect is marginally significant (p<.10). This Latino *Student Impact* advantage relative to white students is equivalent to 0.25 S.D. units. In public schools, there is no Latino-white difference in reading. Cross-level interactions (not shown) indicate that the effect of school sector varies significantly by student race. Black students' reading *Student Impact* is significantly less than white students in Catholic schools, and as opposed to the non-significant black-white gap in public schools. The cross-level interaction for Latinos is marginally significant (p<.10), indicating that school sector effects vary significantly for Latinos compared to whites. Finally, there is no effect of school locale on reading *Student Impact*, nor does the effect of school sector vary by school locale.

The racial and socioeconomic gaps in each school setting describe the inequality within them, but these results do not fully describe how Catholic and public schools influence educational inequality for all students (especially white versus black and Latino, and high socioeconomic versus low socioeconomic). In other words, even if

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inequality is higher within Catholic schools, these schools might still reduce global inequality by boosting the learning rates of disadvantaged students more overall than public schools. To understand this, I calculated the total *Student Impact* for Catholic and public school students in Table B.7. This table illustrates the total increase in student learning rates for the bottom and top socioeconomic quintiles, as well as for white, black, and Latino students. These results allow me to determine if Catholic or public schools serve their students better, regardless of socioeconomic or racial gaps within schools. The values presented in Table B.7 are based on models that included cross level interactions of school sector with student race and student socioeconomic status (not shown). In math and reading, I added the appropriate coefficients (e.g. the coefficient for black) to the grand mean *Student Impact* to calculate the values shown in Table B.7. Recall that the grand mean *Student Impact* is the average *Student Impact* for students in my reference category (i.e. white students in the top socioeconomic quintile who attend public schools).

Panel A in Table B.7 provides the results for math. Public school students in the top and bottom socioeconomic quintiles gain math skills faster than Catholic school students. In both school sectors, students in the bottom socioeconomic quintile experience a larger learning rate increase from schooling than students in the top socioeconomic quintile, but the low socioeconomic advantage is larger in public schools. Notably, in public schools, the black-white difference in *Student Impact* is trivial, and blacks in Catholic schools have a much lower *Student Impact* than blacks in public schools (0.0065 and 0.0512 respectively). The *Student Impact* in math for Latino

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students is not affected by whether they attend public or Catholic schools, while white students experience a larger average increase in math learning rates in public schools compared to Catholic schools (0.0518 and 0.0407 respectively). In math, one could not argue that Catholic schooling reduces overall skills inequality more than public schooling because disadvantaged groups are at least as well off or better in public schools compared to attending Catholic schools.

Panel B of Table B.7 provides the results for reading. Looking at the top and bottom socioeconomic quintiles, school sector has very little effect on overall *Student Impact* or socioeconomic differences in *Student Impact*. Catholic and public schools appear to have compensatory effects with respect to socioeconomic status, because both school sectors provide larger learning rate boosts to bottom socioeconomic quintile first graders compared to top quintile first graders. Looking at race in Panel B, *Student Impact* in reading is very similar for white students in Catholic and public schools. Black first graders experience a smaller *Student Impact* in Catholic schools than in public schools (0.0171 and 0.0589 respectively), and black-white gaps are smaller in public schools compared to Catholic schools. Finally, Latinos in Catholic schools experience a higher overall reading *Student Impact* than Latinos in public schools (0.0987 and 0.0639 respectively).

Overall, the results in Table B.7 indicate that black first graders are better off in relative terms (i.e. smaller black-white gaps) and in absolute terms (i.e. higher total *Student Impact)* in math and reading when they attend public schools rather than Catholic schools. Latino first graders, on the other hand, are better off in relative and absolute

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terms if they attend Catholic schools rather than public schools, but this is only true in reading. There is no difference in math for Latinos. Finally, the results for socioeconomic status offer evidence that students from low socioeconomic backgrounds are better off in math in absolute terms if they attend public schools in first grade, but they likely experience no absolute benefit from public or Catholic schooling in reading.

The results presented in Tables B.5, B.6, and B.7 suggest that Catholic schools are not better learning environments for math and reading than public schools for low socioeconomic students or for black students, and they are probably worse for these disadvantaged groups in first grade. When using *Student Impact* as the measure of school effects on learning, these analyses indicate that socioeconomically disadvantaged students experience greater increases to their math and reading learning rates relative to students in the top socioeconomic quintile in public schools. This is not true in Catholic schools. Similarly, these results indicate that black students in Catholic schools experience lower math and reading *Student Impact* relative to whites, but this is not true in public schools. Latino first graders present a noteworthy exception to this pattern in that their reading learning may be boosted more by Catholic schools than it is by public schools. Finally, school locale never significantly shapes Catholic school effects in math or reading, however, suburban public schools provide a significantly smaller learning rate increase to their students in math than urban public schools.

Conclusion and Discussion

This research examined whether Catholic schools have higher math and reading *Student Impact* (school year learning rates net of summer rates) than public schools for

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first grade students. The results indicate that Catholic schools generally do not have higher *Student Impact* than public schools, and Catholic schools have lower *Student Impact* than public schools in math. In addition, Catholic schools are not broadly more compensatory for socioeconomically disadvantaged students, blacks, or students in urban areas. First, Catholic schools do not reduce black-white gaps more than public schools. ATT estimates from propensity matching models indicate that black first graders in Catholic schools experience significantly smaller learning rate increases from schooling than they would in public schools in math and reading, and multilevel models present similar results (Table B.7). Multilevel models indicate that black first graders in Catholic schools lose ground relative to their white counterparts in math and reading, but this is not the case in public schools. This suggests that black students are worse off if they attend Catholic schools in terms of math and reading learning because they experience smaller learning benefits in the Catholic school context.

Second, students from the bottom socioeconomic quintile do not benefit more from schooling overall and compared to their more advantaged peers if they attend a Catholic school. The ATT estimates among students from the bottom two socioeconomic quintiles indicate that the treatment effects of attending a Catholic school are non-significant in math and reading. This suggests that socioeconomically disadvantaged first graders experience no net benefits in math or reading from attending a Catholic school rather than a public school, and this is true even when Catholic school student outcomes are compared to their matched public school counterparts. Multilevel model results similarly show that public school students from the bottom socioeconomic quintile

experience a significantly larger learning rate boost from schooling than students from the top socioeconomic quintile, but this is not the case among Catholic school students.

Third, the ATT for Catholic schools in urban locales is significant and negative in math, but non-significant in reading. Urban Catholic schools perform worse than public schools overall in math but not in reading, suggesting that Catholic schools do not provide a better alternative to supposedly low-quality urban public schools for first grade students. In suburban areas, the ATT for Catholic schools is not significantly different from zero, while in rural areas, Catholic schools have a positive and marginally significant ($p<.10$) ATT in reading but not in math.

Finally, results for Latino students are an important exception to other findings. The Latino subsample in the propensity matching models indicates that Latino students in Catholic schools experience a significantly larger reading learning rate boost than Latinos in public schools. Multilevel models also show that in reading Latino first graders in Catholic schools experience a marginally significant reading *Student Impact* advantage over whites in Catholic schools, but this is not true in public schools. Overall, Latinos experience a larger reading learning rate increase when they attend Catholic schools in the first grade. Both of the estimation strategies take into account whether English is the primary language spoken at home, so that cannot be a spurious factor driving these results.

This research offers mixed support to the small number of studies examining Catholic primary schools. Prior research generally indicates that Catholic schools are no more effective than public schools for learning or student achievement scores, and these

results support that position. Reardon et al. (2009) find some evidence that Catholic schools are less effective at boosting student math skills than public schools, while they more confidently conclude that Catholic schools and public schools are equally effective in boosting reading skills. This research supports both of those conclusions. However, only a few studies actually examined whether the effects Catholic primary schools vary across racial and socioeconomic groups as well as school locales. Prior studies indicated that Catholic primary school effects did not vary across these groups (Lubienski and Lubienski 2006; Jepsen 2003), or that Catholic primary schools are somewhat compensatory for racial minorities and socioeconomically disadvantaged students (Lee and Stewart 1989). This research indicates that Catholic schools produce worse outcomes than public schools for black students in reading and possibly math by boosting black student learning rates significantly less than public schools. However it is likely that Catholic schools are especially effective for Latinos in reading. These analyses also provide some evidence that public schools are more compensatory than Catholic schools across socioeconomic groups, however, non-significant cross-level interactions weaken this finding.

While Lee and Stewart (1989) found that Catholic primary schools were compensatory for racial minorities, this study finds this to be true only for Latinos. It is possible that Latino first graders learn more in Catholic schools than public schools because they are likely to be Catholic (as noted before, 67 percent of Latinos are Catholic compared to 8 percent of blacks; author calculations from the 2000 General Social Survey). Being Catholic and attending a Catholic school may provide additional benefits

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to students because they may also attend the church associated with their school and frequently encounter teachers and other school officials inside and outside of school. Parents may also have increased interactions with other parents and teachers through church, and this could build the network of adults involved in students' lives and bolster their social capital (Coleman 1988). Being a Catholic student in a Catholic school may also provide a Catholic school specific cultural capital (Lareau 1987) and improve student-teacher interactions and understanding, which could improve learning. It is also possible that since so few African Americans are Catholic, blacks in Catholic schools are numeric minorities in terms of race and religion. Therefore black students could experience a racial mismatch with teachers as well as a religious mismatch, which could be psychologically harmful or at least limit the benefits that black students receive from Catholic schooling (Alexander, Entwisle, and Thompson 1987; Downey and Pribesh 2004; Davis 1966). Unfortunately, the ECLSK does not provide data on religious affiliation of study participants, so I cannot directly assess the influence that being Catholic has on learning in Catholic schools or whether it explains the advantages Latinos gain from attending Catholic schools.

This study finds that Catholic elementary schools generally do not have higher *Student Impact* than public elementary schools for first graders. These findings are consistent with the majority of prior research on Catholic elementary schools. Yet, these findings diverge in a few important ways, which is due to the superior methodological approach used in this study. Lubienski and Lubienski (2006) and Jepsen (2003) examine whether the effect of Catholic schools varied by student race, socioeconomic status, or

locale type, and they did not find any significant differences between groups. This research does find such differences (as discussed above). Although my broad conclusions are similar to prior work, my findings provide additional nuances to these relationships. I argue that the methodological approach used in this study is a better way to estimate Catholic school effects.

The findings of prior work may have been biased because the methods previously employed failed to fully account for selection bias, unobserved variable bias, and/or summer learning. This study addressed those issues by applying seasonal comparison techniques, propensity score matching, and multilevel modeling. In particular, this research uses *Student Impact* as the dependent variable to account for summer learning and non-school factors that affect learning. Analyzing the effects of Catholic schooling on this outcome allows me to understand its unique effects on learning. Future research of Catholic school effects on achievement tests should continue to use *Student Impact.* This would be especially fruitful in studies of Catholic secondary schools - which prior research has often judged as more effective than public secondary schools. Future research should use the *Student Impact* measure to reassess whether Catholic secondary schools are more effective than public secondary schools and whether Catholic school effects vary by race, socioeconomic status, and school locale type.

If Catholic schools are generally not more effective for learning outcomes than public schools, then why do parents continue to pay tuition to send their children to Catholic schools? Some parents might value the religious education and closely-knit community provided by Catholic schools, while others see the benefits of the rigorous

academic curriculum offered in Catholic schools (Bryk et al. 1993). Parents who send

their kids to private schools value academic quality and rigor over convenience (Coulson

1999). Many parents, Catholic and non-Catholic, might be motivated by the reputation of

Catholic schools. It is doubtful that most parents closely follow the scholarly debates

about the effectiveness of Catholic schools, but conventional wisdom may hold that

Catholic schools are better than public schools. Parents choose schools based on

perceived academic quality, and studies show that parents are as good as experts in

assessing school quality at the local level (Bast and Walberg 2004; Hoxby 2001).

Additionally, even if parents do not have complete information on the quality of schools,

some evidence suggests that parents tend to choose "better" schools that are

predominately white and middle-class if given the chance, and they construct narratives

about how these schools are higher quality (Holme 2002). For parents who have the

financial resources to send their children to Catholic schools, they are likely to do so for

religious reasons or because of the perceived quality of Catholic schools compared to

public schools, regardless of evidence about their effectiveness.

This research employed rigorous methods to produce the results from which these

conclusions were drawn. However, these results have notable limitations. First, due to

data limitations, the research focused only on the first grade and the summer before the

first grade. Therefore, these results are not readily generalizable to later stages of the

academic career. In addition, I was forced to eliminate most of the available sample to

meet the data requirements of these analyses (i.e. test scores from 3 consecutive time

points). This means that some of the subsamples in these analyses, such as Latinos in

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Catholic schools and low socioeconomic students in Catholic schools, are small. As a result, relatively large, but marginally significant or non-significant coefficients and treatment effect estimates should be interpreted with caution. Future research would benefit from a larger sample of students who are followed and appropriately assessed for multiple years. This would allow researchers to determine whether these results are reasonable, and it would also allow researchers to understand how Catholic school effects vary throughout the academic career.

I must limit the interpretation and application of results from PSM models to students who are likely to attend Catholic schools because this research likely violates the stable unit treatment value assumption (SUTVA). This narrows the real-world implications of PSM results. The violation of SUTVA means that it is inappropriate for policy-makers to use PSM results to decide whether to expand voucher programs that allow students to attend Catholic schools.

This research only looks at test scores among young students, and it cannot inform our understanding about how Catholic schools affect larger academic milestones, such as high school graduation and college enrollment. While academic achievement in early years has lasting effects on important academic milestones, many other factors will affect students' likelihood of achieving them. The research also does not address other important benefits that schooling can provide, such as cultural and social capital. Cognitive skills are one of many factors that shape academic and life outcomes, therefore, future research on Catholic schools should examine the other skills taught by schools if we hope to fully evaluate their role in the stratification system.

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Finally, building on Downey and colleagues (2008) who created a school-level measure of *Impact,* this study creates and analyzes an individual-level measure of *Student Impact* to assesses the influence that schooling has on each students' school year learning, net of summer learning. I argue that *Student Impact* is a superior approach to evaluating school effects given its ability to account for the educational contributions of the non-school environment. However, this approach may bias results in an unpredictable way. If non-school contributions to learning vary between the school year and the summer, then incorporating the summer learning rate into the dependent variable might over- or under-correct for the non-school environment. While this is a reasonable caution for interpreting these results, it is very likely that estimation bias is smaller with this technique than it would be if I had used techniques employed in prior research.

This research shows that Catholic elementary schools do not provide better learning opportunities for students, and Catholic schooling generally does not reduce educational inequality among first graders. Catholic schools are particularly ineffective in promoting math skills to black students and in urban areas compared to public schools. This seems particularly relevant because STEM jobs are an important and growing segment of the labor market. As policy makers attempt to improve academic achievement and reduce academic inequalities, pulling students out of public schools through school choice and voucher programs is probably not a good solution - especially if it reduces funding for public schools.

Chapter 4: Conclusion

This dissertation asks the question: how does school context affect educational inequality? To answer it, I use seasonal comparison techniques that take into account the potentially divergent ways that school and non-school environments influence learning. I find that the context of schooling shapes educational inequality between advantaged and disadvantaged students in meaningful and sometimes unexpected ways. The analyses in Chapter 2 indicate that the racial composition of schools does not significantly affect math learning in kindergarten, first grade, or the intervening summer. Racial composition does explain a portion of black-white differences in first grade reading learning, and minority-segregated schools have slower average reading learning rates in the first grade than white-segregated schools. However, using the *School Discrimination Score* I find that black students in white-segregated schools experience the largest learning disadvantages relative to whites in those same schools, while minority-segregated and racially integrated schools produce almost no racial inequalities among their students in math or reading. With the methodological approach I used, our views of how school racial composition matters change in important ways. My results suggest that segregated schools are not as harmful as previously thought, at least in terms of math and reading learning rates in kindergarten and first grade.

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In Chapter 3, I find that Catholic primary schools offer a less compensatory learning environment than public schools for black and low socioeconomic first graders. Public primary schools have compensatory effects for low socioeconomic students, and these effects are not present in Catholic primary schools. Black students in Catholic primary schools are significantly worse off relative to whites in reading and math, but this is not the case for blacks in public schools. In contrast, Latinos benefit from attending Catholic schools in reading. Catholic schools have a reputation of egalitarianism and helping disadvantaged students, but these results do not consistently support that reputation.

These findings suggest that there are reasons to be both optimistic and pessimistic about the effects of schools. Public schools are *not* "failing" students from the bottom of the socioeconomic distribution. Public primary schools reduce math and reading inequality in early grades by giving low socioeconomic students a significantly larger learning rate boost than they provide to high socioeconomic students. In other words, socioeconomically disadvantaged students benefit more from public schooling than socioeconomically advantaged students. By that metric, one cannot conclude that public elementary schools are failing to properly serve students from socioeconomically disadvantaged backgrounds.

On the other hand, this research finds evidence that black students experience disadvantages in schooling, those disadvantages are not isolated to minority-segregated schools, and black disadvantages emerge immediately. Black kindergarteners gain math and reading skills at slower rates than their white counterparts. Blacks also experience a

learning disadvantage in reading during first grade (Chapter 2, Table A.4, F-R1). From the onset of formal schooling, black children's skills fall further behind those of their white counterparts. The black learning rate disadvantage in kindergarten could be driven by non-school disadvantages of black students, but schools might also contribute to this inequality. Unfortunately, these data do not allow me to fully clarify the issue.

Seasonal comparison research demonstrates how patterns of learning change when school is in versus out in order to show how schools matter, yet black-white learning gaps in the ECLSK data are not consistent during non-school periods. Black students start kindergarten with significantly lower academic skills than white students. This indicates that blacks gained skills more slowly than white students prior to kindergarten because of non-school learning disadvantages. However, during the summer after kindergarten black students gain skills at least as fast as or faster than white students. This suggests that black students have equal or better non-school learning experiences compared to whites during the summer after kindergarten. This confusing pattern of non-school disadvantage (prior to kindergarten) and later non-school advantage (in the summer after kindergarten) makes it difficult to be certain if black students learn slower in kindergarten because of school factors, non-school factors, or both. Latino students also start school with fewer skills than whites and gain skills at least as quickly as white students in the summer. However, Latino and white students gain skills at the same rates during the school year.

We would have more confidence in how schools affect the black-white gap if we had data from more summers than the single summer available in the ECLSK. A newer

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version of the data, ECLSK 2011 will soon be available, and it will provide the necessary data points to enable researchers to replicate prior seasonal research. The black-white gap during the summer should be a priority with these new data. Of course, other seasonal studies have examined the black-white gap, but their results are also equivocal because of data and methodological limitations (Heyns 1978; Entwisle and Alexander 1992, 1994). Clearly this is an area where consensus has not yet emerged.

Limitations of this Research

This dissertation has two main limitations. First, this dissertation takes a big-picture view of learning outcomes, so it is not well suited to identify the mechanisms that might shape inequality across school contexts. While I find school contexts shape learning rate inequalities across social groups, I am unable to identify the specific school and non-school mechanisms that drive these differences. For example, what processes in Catholic schools make them harmful to blacks but beneficial for Latinos? One possibility is that Latinos are more likely to be Catholic than blacks, and that allows Latinos to develop and use greater social and cultural capital within Catholic schools in addition to the potential benefit of attending Catholic churches and having greater student and parental access to teachers. Another question that arises from these findings is why would minority-segregated schools be the most egalitarian for black and Latino students compared to whites? Teacher-student racial matching could play an important role in these schools to the benefit of minorities, but we must also consider whether minority-segregated schools are less beneficial to white students rather than being especially effective for black and Latino students. Results in Table A.9 do not support such a

conclusion, however. It is also possible that the white students who attend minority-segregated schools are very different from white students in other schools, and have greater non-school disadvantages. Finally, which school and non-school factors shape the black disadvantage in kindergarten math and reading, and how much do they matter? Black kindergarten learning disadvantages could be driven largely by the same non-school inequalities that leave black children behind whites at the beginning of kindergarten. However, it is also possible that the transition into kindergarten is more difficult for black students or formal learning might be less familiar to black students. It would be informative to compare black and white kindergarteners who had similar preschool or childcare experiences, or to compare only white and black students with high skill levels to determine if they have similar kindergarten learning in these circumstances. Future research should examine how compensatory and reproductive mechanisms vary across schooling contexts to explain these results.

The second limitation of this research is its solitary focus on cognitive skills, in particular growth rates in cognitive skills, as a way to evaluate and compare schools. Cognitive skills, measured by standardized tests, are important because they are critical predictors of later academic success and eventually occupational success. Black-white gaps in first grade math and reading ability account for approximately half of black-white gaps in the 12th grade, and early childhood academic skills are highly predictive of academic skills as students approach adolescence (Duncan et al. 2007; Phillips, Crouse, and Ralph 1998). However, schools do a lot more than teach students math and reading. Schools teach students non-cognitive skills and cultural capital that shape their

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worldview and their behaviors (Bowles and Gintis 1976). Non-cognitive skills, for example, have meaningful effects on academic and labor market outcomes (c.f. Duncan et al. 2007; Heckman, Stixrud, and Urzua 2006; Jennings and DiPrete 2010). In addition, research on Catholic secondary schools suggests that those schools offer benefits such as high expectations and curricular rigor for all students. And, students may benefit from ties to high-resource networks, more extracurricular activities, and better guidance counselors in some schools versus others. Even if Catholic schools do not boost cognitive skills faster than public schools, students in Catholic schools might have higher educational attainment and better labor market outcomes as a result of their Catholic schooling experiences. Minority-segregated schools might produce similar learning rates as white-segregated or racially integrated schools in kindergarten and first grade, but students attending minority-segregated schools might experience other hurdles to completing high school and attending college that may not be present in white-segregated or integrated schools. Future research should study how school contexts either equalize or exacerbate inequalities in non-cognitive skills, cultural capital, and later educational outcomes such as college enrollment. Of course, doing so in a way that carefully separates the effects of school and non-school factors, as I did here, is a major methodological hurdle. For example, merely demonstrating that students in advantaged schools have superior non-cognitive skills or cultural capital does not constitute evidence that schools are responsible. These advantages could stem from non-school sources.

Despite these caveats, understanding gaps in cognitive skills is crucial. Teaching students cognitive skills is the accepted *raison d'être* of schools. Furthermore, the

primary focus of the US accountability system is measuring cognitive skills on standardized tests, and this is also an important way the US compares itself to other countries. It is possible that racially integrated and Catholic schools reduce inequalities in ways not assessed by this study, but it is important to acknowledge that these schools are less successful at reducing learning gaps than previously thought – at least in kindergarten and first grade. This big finding might be countered by different patterns for other skills, but because teaching students math and reading is a consensus function of schools, an argument can be made that it should be the foundation of our discussion about schools.

Can We Eliminate Educational Inequality by "Fixing" Schools?

Education researchers and policy makers tend to focus on schools as the locus of educational reform. Existing metrics of school quality - such as school report cards, achievement test scores, and graduation rates - indicate that bad teachers and failing schools are often concentrated among low-income and minority students. However, seasonal comparison research shows that schools are already playing an important role in reducing educational inequality across socioeconomic groups. This technique emphasizes the simple fact that children learn unequally when they are not in school, and children spend the majority of their time out of school. As Downey and colleagues (2008) point out, children spend only 13 percent of their waking hours in school by the time they are 18, and they spend only one-third of their waking hours at school during the school year. School evaluation metrics that do not take into account the limited amount of time that schools have students in their care will tend to favor schools that teach students

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from privileged backgrounds because those students are likely to learn faster when they are away from school.

To understand fully the time schools have access to students, we must recognize that there are four (4) distinct "periods" of learning for students. Three of these periods occur outside of school, and one period occurs in school. The first learning period extends from birth to the start of formal schooling, when the non-school environment dominates learning. Early childhood learning is shaped by many factors such as parental socioeconomic status, parenting behaviors, learning resources, and childcare (c.f. Lee and Burkam 2002). Large and lasting cognitive skill gaps emerge during this period, and schools are not presently able to eliminate them (Heckman 2006).

The second learning "period" occurs each school day after students leave school and every weekend during the school year. During this period, numerous factors can affect student learning - many are likely the same factors that affect students' learning from birth to age five. While schools may hope to engage students outside of school with homework, student engagement with and completion of homework is largely out of school control. Similarly, the third learning "period" is the summer between each school year. There are 12 summers during the K-12 academic career that total approximately 30 months (12 x 2.5 months). During the summer learning period, skills diverge for students from different backgrounds, as numerous seasonal comparison studies have reported (Downey et al. 2004; Entwisle and Alexander 1992, 1994; Heyns 1978).

The final learning "period" occurs at school during each school day, each school year. During this period, teachers and schools provide intense educational stimuli to

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students, and all students gain skills quickly. But, this period of time is dwarfed by the amount of time students spend out of school, and out-of-school time is critical to educational inequality. Alexander et al. (2007) concluded that the time prior to first grade and summer learning periods explained the entire gap in reading skills between socioeconomically advantaged and disadvantaged students at the beginning of high school. School periods explained none of the gap. Education researchers must continue to study school-based processes and their effects on academic inequality, but it is clear that out-of-school learning periods play a profound role in educational inequality. Fixing schools would not solve educational inequality unless we also "fix" what is happening during these out-of-school learning periods.

What Should be Done to Reduce Educational Inequality?

Seasonal comparison research generally shows that schools provide the largest benefits to disadvantaged students, particularly socioeconomically disadvantaged students. With that understanding, it is reasonable to think that expanding access to schools and school-like experiences would reduce educational inequality. This would reduce the amount of time that children experience the sizable inequalities in the non-school environment, and it would limit the skill inequality that emerges as a result of lengthy non-school learning periods.

As noted, one of the critical learning periods in which academic inequality emerges is the approximately 30 months of summer that students experience during the K-12 academic career. Summer learning is highly unequal, yet we do not have a strong understanding of the parenting behaviors or summer activities that shape summer

learning (Burkam et al. 2004). In their most comprehensive models, Burkam and colleagues (2004) explained less than 15 percent of the variation in summer learning. Why children learn at different rates during the summer is mostly unknown. If we could learn more about this process and eliminate summer learning inequality, then we could reduce overall academic inequality substantially.

Summer-school programs are often labeled ineffective because students in summer school typically gain few skills. However, summer school often lacks the rigor of regular schooling, therefore the lack of learning among summer-school students is not surprising (Heyns 1987). It is also likely that summer school feels like punishment to students, rather than an extra opportunity to learn. At best, summer school has no effect or very small benefits for students (Borman and Dowling 2006; Matsudaira 2008). However, as Heyns (1987) points out, even if summer school produces no new skills, it can minimize the "summer setback" that often harms disadvantaged students. Rather than using summer school to babysit, remediate, or punish students, it would be far better add school days to the academic calendar to increase all students' exposure to the effective education they already receive during the school year.

One obvious way to increase the amount of time children experience school and reduce exposure to unequal non-school environments is to lengthen the school year. Most children in the US attend school for about 180 days a year, significantly less than the international average (~190). The long summer vacation is a product of our agrarian past and may not be productive for our current information-based economy. Evidence from seasonal studies suggests that all children learn faster when they are in school

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versus out, and this is especially true of disadvantaged children. Increasing the number school days school would likely improve skills for all children and could reduce achievement gaps.

The biggest problem is the large skill gaps that are evident at the beginning of kindergarten. Early childhood research indicates that early learning experiences in the family are important but highly unequal. Prior to kindergarten, parenting behaviors and learning resources are central to early childhood learning, and these are often closely tied to parental socioeconomic status (Guo and Harris 2000; Hart and Risley 1995; Heath 1982; Lee and Burkam 2002). In addition, childcare arrangements and childcare quality affect cognitive development (c.f. Lee and Burkam 2002; Brooks-Gunn, Han, and Waldfogel 2002; Peisner-Feinberg et al. 2001). Middle-class parents, who tend to have more human, cultural, and financial capital, are more likely to engage in the parenting behaviors and activities that benefit their children's cognitive development. Middle-class parents are also more able to purchase high quality, center-based childcare which improves learning outcomes, rather than relying of family and friends for childcare.

President Obama recently set a goal of providing preschool to all four year olds. This admirable effort will not be enough to solve racial achievement gaps, however, because black-white cognitive inequality has been found as early as 9 months of age (Covay 2010). Universal childcare and preschool is the ideal solution to educational inequality because it could reduce skills inequality before kindergarten. It is important that these services begin as early as possible, and ideally they would be available immediately after children are born.

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If universal childcare and preschool provide stimulating activities to disadvantaged children, such as talking and reading with adults, then these programs could reduce academic inequality by boosting the skills of students from the bottom of the distribution. Studies of Head Start and the Perry Preschool show that these interventions have short term and long term benefits for disadvantaged children while high quality child care also improves academic outcomes (Currie and Thomas 1995; Lee, Brooks-Gunn, Schnur, and Liaw 1990, see Barnett 1995 for review of Head Start research). Universal childcare and preschool from birth to kindergarten would likely go a long way toward improving schooling outcomes for disadvantaged students and reducing cognitive skill inequality in early childhood.

So why do we focus so little attention and so few resources on pre-K and summer learning periods? Why do we continue to focus our attention and resources on schools as they are currently designed? The best explanations are: money, the belief that schools are supposed to be enough to fix social inequality and provide equal opportunities to all students, and because schools are perceived as easier to reform than homes. Schools represent the social institution responsible for educating large numbers of children efficiently, and many people still believe that schools are the answer to equality of opportunity to all children. Also, the United States spends billions of dollars on public education, giving policy makers control over the operation of schools, but they have very little control over what students do when they are not in school. It is much easier to try to reform schools than it is to reform parental behaviors and the myriad of social structures -

such as residential segregation, unequal access to quality healthcare, labor market discrimination, etc. – that can shape those behaviors.

School reforms might be an appealing approach to improving educational outcomes and reducing educational inequality, yet it is unlikely that reforms to the existing K-12 schooling process with its 9.5 month school year will ever eliminate educational inequalities across racial and socioeconomic groups. However, there is evidence that more intense and expensive school reforms can reduce educational inequalities. The Harlem Children's Zone (HCZ) - a nonprofit organization in Harlem that couples charter schools with community engagement programs, early childhood education, and after school programs - has made impressive gains in eliminating black-white test score gaps. Dobbie and Fryer (2011) found that the HCZ charter middle school closed the black-white achievement gap in math and while the HCZ elementary school closed black-white gaps in math and English Language Arts. This is likely due to the extended school days and longer school years of HCZ charter schools in addition to the dedicated staff and focus on community engagement and parental involvement.

So is HCZ the model of school reform the rest of the United States should use? President Obama certainly thinks so, and the US Department of Education allocated one hundred million dollars between 2010 and 2012 to develop "Promise Neighborhoods" throughout the country (US Department of Education 2013). Yet there are reasons to believe HCZ's success may be difficult to replicate. HCZ's spends more than 65 million dollars annually on charter schools and supportive services (HCZ Form 990 2013), an amount that far exceeds what will likely be available in most communities even. Also,

Harlem is a unique setting because it is a densely populated and geographically small area that is adjacent to the wealthiest individuals and corporations in the world, and they donate a large portion of HCZ's budget. These aspects of the HCZ can be found in few other places. Perhaps the key to HCZ's success is its "no excuses" charter schools, which Obama supported in his both the Promise Neighborhood initiative and his *Blueprint for Education Reform.* While HCZ's charter schools appear to be successful, research on charter schools more broadly finds that they either have no effect on the black-white test score gap (Renzulli and Roscigno 2007) or they increase the gap (Bifulco and Ladd 2007). More importantly, even if we succeeded in funding and scaling up successful programs like the Harlem Children's Zone, we would have to make them available to disadvantaged children, but deny them to advantaged ones in order to reduce achievement gaps. That is not a feasible strategy for reducing inequality.

Rather than attempt to replicate the HCZ success, all public schools could replicate part of their educational model by extending their school day and school year. This would reduce children's exposure to non-school learning periods and reduce the academic inequality that develops during these periods. Disadvantaged students would probably benefit more from extended exposure to schooling than advantaged students, and this would reduce skill gaps between these groups. Rather than investing in charter schools alone, the findings from HCZ and research on learning during early childhood and summers suggests that increasing exposure to schooling and school-like experiences (through universal childcare and preschool, longer school days and school years) is needed to achieve greater educational equality. HCZ increases that exposure for a small

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number of K-12 students in Harlem, but universal public programs are needed to meaningfully change racial and socioeconomic achievement gaps.

One could argue that another effective solution to reducing educational inequality would be to reduce economic inequality more broadly. After all, student socioeconomic background is an important predictor of academic success. Raising minimum wage or implementing a national minimum income might also reduce educational inequality by increasing incomes for families of children who would otherwise be in very poor homes. This could reduce overall income inequality as well as educational inequality. This approach would yield benefits by providing parents more resources to purchase educationally stimulating objects and experiences and allow them to provide healthier home environments and more nutritious food. However, the association of cognitive skills and poverty or low socioeconomic status is also mediated by parenting behaviors, such as frequency and style parent-child speaking and parenting style (Guo and Harris 2000; Hart and Risley 1995). These parenting behaviors would not necessarily change by simply increasing income among those who have limited financial means; therefore, this approach would likely reduce but not eliminate educational inequality.

A final consideration for this research is the fact that it focuses on cognitive skill learning rates, but people are not judged by their learning rates. People are judged by their achievements (e.g. having a college degree, attending an Ivy League university) and their relative positions on socially meaningful distributions (e.g. IQ scores; SAT scores). Even if all students learned at the same rate from the day they started kindergarten to the day they finished high school, disadvantaged students would still trail advantaged

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students because they started school with fewer skills. It would continue to be very difficult for disadvantaged students to attend the best colleges and universities, and their social mobility would continue to lag their more advantaged peers. Many scholars have labeled schools as compensatory, and learning rate estimates suggest that the label is reasonable. But, equalizing school-year learning rates does not eliminate educational inequality. For schools to be a social equalizer, efforts to address inequality in the non-school environment are necessary. Eliminating inequality in the non-school environment would be difficult, but we could limit its effects by increasing exposure to schools and school-like experiences starting from birth. Instead of working to reform our current public schools by moving to a model of charter schools or pouring money and resources into expensive new evaluation tools and education technology, I recommend that we extend all students' access to positive learning environments in school-like settings through two avenues. First, provide access to universal childcare and preschool for children starting the day they are born to minimize the educational inequalities that emerge before the kindergarten. Second, extend the length of school days and make schools year-round to increase the time children spend on structured learning and development. These two approaches, while costly, would improve learning outcomes for all students and only then can disadvantaged children have a fair opportunity to achieve the American Dream.

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Appendix A: Chapter 2 Tables

Table A.1: Descriptive Statistics		
	Analytic Sample	
Learning Outcomes	**Mean**	**Std. Dev.**
Kindergarten Math Learning Rate	0.079	0.044
Summer Math Learning Rate	0.019	0.110
First Grade Math Learning Rate	0.072	0.038
Kindergarten Reading Learning Rate	0.095	0.051
Summer Reading Learning Rate	0.004	0.107
First Grade Reading Learning Rate	0.091	0.041
Level 1 Controls		
White (Reference)	0.581	0.493
Black	0.142	0.349
Latino	0.160	0.366
Asian	0.040	0.196
Other Race	0.078	0.268
Kindergarten First SES quintile (Lowest)	0.161	0.368
Kindergarten Second SES quintile	0.184	0.388
Kindergarten Third SES quintile	0.199	0.399
Kindergarten Fourth SES quintile	0.222	0.415
Kindergarten Fifth SES quintile (Highest, Reference)	0.234	0.423
First Grade First SES quintile	0.157	0.363
First Grade Second SES quintile	0.184	0.387
First Grade Third SES quintile	0.205	0.404
First Grade Fourth SES quintile	0.212	0.409
First Grade Fifth SES quintile	0.243	0.429
Female (Yes=1)	0.501	0.500
Non-English Home Language (Yes=1)	0.110	0.314
Married Biological Parents (Yes=0)	0.374	0.484
Number of Siblings	1.457	1.132
Kindergarten Number of Absences	10.158	15.104
First Grade Number of Absences	8.209	9.643
Kindergarten Repeater (Year 1, Yes=1)	0.046	0.209
Kindergarten Repeater (Year 2, Yes=1)	0.024	0.152
Full Day Kindergarten (Yes=1)	0.574	0.495
Kindergarten Age at School Year Start (Years)	5.466	0.362
First Grade Age at School Year Start (Years)	6.463	0.362
Level 2 Controls		
Kindergarten: 75 Percent Minority School	0.184	0.388
Kindergarten: 25-75 Percent Minority	0.285	0.451
Kindergarten: 75 Percent Receive Free Lunch	0.136	0.343
Kindergarten: 25-75 Percent Free/Red Lunch	0.456	0.498
Kindergarten: Private School (1=Yes)	0.231	0.422
First Grade: 75 Percent Minority School	0.188	0.391
First Grade: 25-75 Percent Minority	0.256	0.436
First Grade: 75 Percent Receive Free Lunch	0.156	0.363
First Grade: 25-75 Percent Free/Red Lunch	0.371	0.483
First Grade: Private School (1=yes)	0.224	0.417
N Students (Analytic Sample)	4000	
N Schools (Analytic Sample)	380	

The analytic sample includes students with the following characteristics: (1) took all four assessments in reading or math, (2) went to schools with normal calendar years, (3) did not switch schools during either school year, and (4) had a valid school ID for both years.

	Kindergarten[b]		Summer[c]		First Grade[d]	
Table A.2: The Effect of School Segregation Level on Math Learning Rates[a]						
Level 1	K-1M	K-2M	S-1M	S-2M	F-1M	F-2M
Black[e]	-0.0094 ***	-0.0108 ***	0.0063	0.0083	-0.0025	-0.0016
Latino	0.0004	-0.0005	0.0082	0.0093	-0.0003	0.0003
Asian	0.0011	0.0006	0.0153	0.0131	-0.0148 ***	-0.0142 ***
Other Race	-0.0012	-0.0020	0.0020	0.0023	-0.0027	-0.0025
First SES quintile (Lowest)[f]	0.0049 x	0.0047 x	-0.0094	-0.0081	0.0107 ***	0.0107 ***
Second SES quintile	0.0019	0.0018	-0.0006	0.0003	0.0048 *	0.0048 *
Third SES quintile	0.0027	0.0026	-0.0045	-0.0042	0.0032 x	0.0032 x
Fourth SES quintile	0.0005	0.0005	-0.0040	-0.0037	0.0058 **	0.0058 **
Female (Female=1)	-0.0019	-0.0019	-0.0013	-0.0012	-0.0030 *	-0.0030 **
Non-English Home Language (Yes=1)	-0.0027	-0.0034	0.0007	0.0030	0.0074 **	0.0076 **
Married Biological Parents (Yes=0)	0.0012	0.0011	0.0009	0.0005	0.0009	0.0010
Number of Siblings	0.0024 ***	0.0023 ***	-0.0006	-0.0005	0.0011 *	0.0011 *
Number of Absences	0.0000	0.0000	-0.0004 *	-0.0004 *	-0.0001	-0.0001
Repeated Kindergarten (Yes=1)	-0.0081 *	-0.0081 *	-0.0083	-0.0096	-0.0051	-0.0051
Full Day Kindergarten (Yes=1)	0.0092 ***	0.0087 ***	-0.0065	-0.0069	N/A	N/A
Age at School Year Start	-0.0109 ***	-0.0108 ***	0.0062	0.0060	-0.0118 ***	-0.0118 ***
Level 2						
75% Minority School[g]		0.0048		-0.0123		-0.0016
25-75% Minority School		0.0017		0.0123 x		-0.0034
Grand Mean (Gamma00)	0.0720	0.0716	0.0292	0.0262	0.0694	0.0705
N	3980	3980	3980	3980	3980	3980
N-Schools	270	270	270	270	370	370

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

a. All models (including summer) also control for private school status.

b. Kindergarten models control for whether the school was full day or half day

c. Summer models control for kindergarten repeater status (year 1), number of absences during kindergarten, and full day or half day kindergarten

d. First grade models (year 2) control for whether the student repeated kindergarten. (i.e. student is in kindergarten in Year 1 and Year 2 of the study period

e. White students are the reference group

f. The fifth (highest) SES quintile is the reference group

g. Schools with less than 25 percent racial minorities are the reference group.

Table A.3: Math Learning Rates Accounting for School Racial and Socioeconomic Composition[a]					
	Kindergarten[b]		Summer[c]		First Grade[d]
Level 1	K-2M	K-3M	S-2M	S-3M	F-2M	F-3M
Black[e]	-0.0108 ***	-0.0108 ***	0.0083	0.0082	-0.0016	-0.0017
Latino	-0.0005	-0.0005	0.0093	0.0092	0.0003	0.0004
Asian	0.0006	0.0007	0.0131	0.0129	-0.0142 ***	-0.0142 ***
Other Race	-0.0020	-0.0020	0.0023	0.0025	-0.0025	-0.0026
First SES quintile (Lowest)[f]	0.0047 x	0.0045	-0.0081	-0.0076	0.0107 ***	0.0105 ***
Second SES quintile	0.0018	0.0016	0.0003	0.0009	0.0048 *	0.0045 *
Third SES quintile	0.0026	0.0025	-0.0042	-0.0037	0.0032 x	0.0029
Fourth SES quintile	0.0005	0.0004	-0.0037	-0.0035	0.0058 **	0.0056 **
Female	-0.0019	-0.0019	-0.0012	-0.0012	-0.0030 **	-0.0030 **
Level 2						
75 Percent Minority School[g]	0.0048	0.0046	-0.0123	-0.0124	-0.0016	-0.0028
25-75 Percent Minority	0.0017	0.0015	0.0123 x	0.0127 *	-0.0034	-0.0035
75 Percent Receive Free/ Red Lunch[h]		0.0010		-0.0014		0.0016
25-75 Percent Free/Red Lunch		0.0015		-0.0051		0.0033
Grand Mean (Gamma00)	0.0716	0.0710	0.0262	0.0282	0.0705	0.0689
N	3980	3980	3980	3980	3980	3980
N-Schools	280	280	280	280	370	370

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

a. All models (including summer) also control for whether English is spoken at home, parent marital status, number of siblings, number of absences during the school year, kindergarten repeater status, age at school year start, and private school status.

b. Kindergarten models control for whether the school was full day or half day are included

c. Summer models controls for kindergarten repeater status (year 1), number of absences during kindergarten, and full day or half day kindergarten on the individual level and school sector on the school level.

d. First grade models (year 2) control for whether the student repeated kindergarten. (i.e. student is in kindergarten in year 1 and year 2 of the Study

e. White students are the reference group

f. The fifth (highest) SES quintile is the reference group

g. Schools with less than 25 percent racial minorities are the reference group.

h. Schools with less than 25 percent of students receiving free or reduced lunches are the reference group.

Table A.4: The Effect of School Segregation Level on Reading Learning Rates by Season[a]						
	Kindergarten[b]		Summer[c]		First Grade[d]	
Level 1	K-1R	K-2R	S-1R	S-2R	F-1R	F-2R
Black[e]	-0.0099 **	-0.0108 **	0.0146 *	0.0148 *	-0.0049 *	-0.0029
Latino	-0.0016	-0.0021	0.0109 x	0.0111 x	-0.0024	-0.0012
Asian	-0.0030	-0.0036	0.0171 x	0.0162 x	-0.0120 **	-0.0112 **
Other Race	-0.0001	-0.0007	0.0081	0.0080	0.0031	0.0039
First SES quintile (Lowest)[f]	0.0089 **	0.0090 **	-0.0138 *	-0.0134 *	0.0138 ***	0.0140 ***
Second SES quintile	0.0078 **	0.0078 **	0.0005	0.0009	0.0087 ***	0.0089 ***
Third SES quintile	0.0056 *	0.0057 *	-0.0011	-0.0010	0.0065 **	0.0066 **
Fourth SES quintile	0.0033	0.0034	0.0043	0.0044	0.0068 ***	0.0068 ***
Female (Female=1)	0.0020	0.0020	-0.0002	-0.0001	-0.0047 ***	-0.0047 ***
Non-English Home Language (Yes=1)	0.0120 **	0.0119 **	-0.0016	-0.0010	0.0053 x	0.0061 x
Married Biological Parents (Yes=0)	-0.0001	-0.0002	-0.0015	-0.0016	0.0011	0.0013
Number of Siblings	0.0018 *	0.0018 *	-0.0014	-0.0014	0.0019 **	0.0020 **
Number of Absences	-0.0001	-0.0001	0.0000	0.0000	-0.0001	-0.0001
Repeated Kindergarten (Yes=1)	-0.0205 ***	-0.0206 ***	-0.0050	-0.0055	-0.0110 *	-0.0112 **
Full Day Kindergarten (Yes=1)	0.0114 ***	0.0109 ***	-0.0188 ***	-0.0192 ***	N/A	N/A
Age at School Year Start	-0.0085 **	-0.0084 **	0.0060	0.0061	-0.0100 ***	-0.0100 ***
Level 2						
75% Minority School[g]		0.0023		-0.0042		-0.0079 *
25-75% Minority School		0.0034		0.0073		-0.0042
Grand Mean (Gamma00)	0.0848	0.0839	0.0139	0.0121	0.0837	0.0856
N	3790	3790	3790	3790	3790	3790
N-Schools	270	270	270	270	370	370

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

a. All models (including summer) also control for private school status.

b. Kindergarten models control for whether the school was full day or half day

c. Summer models control for kindergarten repeater status (year 1), number of absences during kindergarten, and full day or half day kindergarten

d. First grade models (year 2) control for whether the student repeated kindergarten. (i.e. student is in kindergarten in Year 1 and Year 2 of the study period

e. White students are the reference group

f. The fifth (highest) SES quintile is the reference group

g. Schools with less than 25 percent racial minorities are the reference group.

Table A.5: Reading Learning Rates Accounting for School Racial and Socioeconomic Composition[a]					
	Kindergarten[b]		Summer[c]		First Grade[d]
Level 1	K-2R	K-3R	S-2R	S-3R	F-2R	F-3R
Black[e]	-0.0108 **	-0.0110 ***	0.0148 *	0.0150 *	-0.0029	-0.0032
Latino	-0.0021	-0.0021	0.0111 x	0.0110 x	-0.0012	-0.0014
Asian	-0.0036	-0.0033	0.0162 x	0.0158 x	-0.0112 **	-0.0113 **
Other Race	-0.0007	-0.0005	0.0080	0.0079	0.0039	0.0036
First SES quintile (Lowest)[f]	0.0090 **	0.0081 *	-0.0134 *	-0.0122 x	0.0140 ***	0.0135 ***
Second SES quintile	0.0078 **	0.0071 *	0.0009	0.0019	0.0089 ***	0.0086 ***
Third SES quintile	0.0057 *	0.0051 x	-0.0010	-0.0002	0.0066 **	0.0064 **
Fourth SES quintile	0.0034	0.0031	0.0044	0.0048	0.0068 ***	0.0068 ***
Female	0.0020	0.0020	-0.0001	-0.0001	-0.0047 ***	-0.0047 ***
Level 2						
75 Percent Minority School[g]	0.0023	0.0000	-0.0042	0.0001	-0.0079 *	-0.0101 *
25-75 Percent Minority	0.0034	0.0019	0.0073	0.0100	-0.0042	-0.0049 x
75 Percent Receive Free/ Red Lunch[h]		0.0090 x		-0.0109		0.0073
25-75 Percent Free/Red Lunch		0.0063		-0.0093		-0.0008
Grand Mean (Gamma00)	0.0839	0.0813	0.0121	0.0162	0.0856	0.0856
N	3790	3790	3790	3790	3790	3790
N-Schools	280	280	280	280	370	370

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

a. All models (including summer) also control for whether English is spoken at home, parent marital status, number of siblings, number of absences during the school year, kindergarten repeater status, age at school year start, and private school status.

b. Kindergarten models control for whether the school was full day or half day are included

c. Summer models controls for kindergarten repeater status (year 1), number of absences during kindergarten, and full day or half day kindergarten on the individual level and school sector on the school level.

d. First grade models (year 2) control for whether the student repeated kindergarten. (i.e. student is in kindergarten in year 1 and year 2 of the Study

e. White students are the reference group

f. The fifth (highest) SES quintile is the reference group

g. Schools with less than 25 percent racial minorities are the reference group.

h. Schools with less than 25 percent of students receiving free or reduced lunches are the reference group.

Table A.6: Findings of Significance in Reading and Math[a,b]						
	Math			Reading		
	Kindergarten	Summer	First	Kindergarten	Summer	First
(White = ref)						
Black	−	0	0	−	+	0[c]
Latino	0	0	0	0	+[d]	0
(<25% Minority = ref)						
75% Minority	0	0	0	0	0	−
25% - 75% Minority	0	+	0	0	0	−[d]
(<25% Free/Red Lunch = ref)						
75% Free Lunch	0	0	0	+[d]	0	0
25-75% Free Lunch	0	0	0	0	0	0

a. Based on results from Model 3 for each season in math and reading.

b. Positive and negative symbols indicate statistically significant findings at $p<.05$ unless otherwise noted. Zeros indicate non-significant findings.

c. The coefficient for black is negative and statistically significant ($p<.05$) for first grade reading in Model F-1R when school racial and socioeconomic composition are not included. The black coefficient becomes non-significant after those school level controls are included.

d. These coefficients are marginally significant at the $p<.10$ level

Table A.7: The Effect of Individual and School Segregation Level on Math and Reading Learning Rates[a]								
Panel A: Mathematics	Kindergarten[b]			Summer[c]			First Grade[d]		
	>75% Minority	25% - 75% Minority	<25% Minority	>75% Minority	25% - 75% Minority	<25% Minority	>75% Minority	25% - 75% Minority	<25% Minority
Level 1									
Black[f]	-0.0162 *	-0.0074 *	-0.0052	-0.0050	-0.0011	0.0097	0.0052	0.0023	-0.0032
Latino	0.0005	0.0015	-0.0021	0.0067	-0.0011	0.0107	0.0143 x	0.0039	-0.0048
Asian	-0.0158	0.0070	-0.0005	0.0025	0.0085	0.0106	0.0046	-0.0136 *	-0.0169 **
Other Race	0.0085	0.0003	-0.0118 *	-0.0181	-0.0134	0.0205 x	0.0154 x	-0.0043	-0.0052
Grand Mean (Gamma00)	0.0869	0.0765	0.0696	0.0188	0.0199	0.0345	0.0591	0.0692	0.0691
N	740	1130	2110	740	1130	2110	680	1130	2170
N-Schools	50	80	150	50	80	150	70	110	190
Panel B: Reading	Kindergarten[b]			Summer[c]			First Grade[d]		
	>75% Minority	25% - 75% Minority	<25% Minority	>75% Minority	25% - 75% minority	<25% Minority	>75% Minority	25% - 75% Minority	<25% Minority
Level 1									
Black[f]	-0.0025	-0.0099 *	-0.0117 x	0.0041	0.0098	0.0270 *	-0.0016	0.0068	-0.0142 *
Latino	0.0098	-0.0033	-0.0029	0.0066	0.0100	0.0098	0.0100	0.0011	-0.0052 x
Asian	-0.0159	0.0049	-0.0047	0.0319	-0.0005	0.0259 x	-0.0035	-0.0089	-0.0140 *
Other Race	0.0258 *	-0.0025	-0.0051 *	-0.0142	0.0107	0.0090	0.0186	0.0076 *	0.0003
Grand Mean (Gamma00)	0.0873	0.0866	0.0802	0.0164	0.0198	0.0126	0.0693	0.0798	0.0874
N	610	1070	2110	610	1070	2110	560	1060	2170
N-Schools	50	80	150	50	80	150	70	110	190

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

a. All models (including summer) also control for whether English is spoken at home, parent marital status, number of siblings, number of absences during the school year, kindergarten repeater status, age at school year start, and private school status.

b. Kindergarten models control for whether the school was full day or half day are included

c. Summer models controls for kindergarten repeater status (year 1), number of absences during kindergarten, and full day or half day kindergarten

d. First grade models (year 2) control for whether the student repeated kindergarten. (i.e. student is in kindergarten in year 1 and year 2 of the Study

e. The fifth (highest) SES quintile is the reference group

f. White students are the reference group

Table A.8: School Discrimination Scores (SDS) for Math and Reading by School Racial Composition Level[a]

Panel A: Math	Model 1 >75% Minority	Model 2 25%-75% Minority	Model 3 <25% Minority
Black	-0.001	-0.001	-0.014
Latino	0.001	0.004	-0.014
Asian	-0.008	-0.012	-0.019
Other Race	0.030	0.011	-0.029

Panel B: Reading	Model 1 >75% Minority	Model 2 25%-75% Minority	Model 3 <25% Minority
Black	-0.006	-0.011	-0.040
Latino	0.003	-0.011	-0.014
Asian	-0.042	-0.001	-0.035
Other Race	0.036	-0.008	-0.011

[a] SDS Calculated from coefficients shown in Table 6.
For example, the Black coefficient in Panel A, Model 1 was calculated by: SDS = [(-.0162+.0052)/2] - (-.0050)

Table A.9: Learning Rate Boost from Schooling and SDS Scores by School Racial Composition [a]

Panel A: Math	>75% Minority		25% - 75% Minority		<25% Minority	
	Learning Rate Boost from School	SDS	Learning Rate Boost from School	SDS	Learning Rate Boost from School	SDS
White	0.054	N/A	0.053	N/A	0.035	N/A
Black	0.054	-0.001	0.052	-0.001	0.021	-0.014
Latino	0.054	0.001	0.057	0.004	0.021	-0.014

Panel B: Reading	>75% Minority		25% - 75% Minority		<25% Minority	
	Learning Rate Boost from School	SDS	Learning Rate Boost from School	SDS	Learning Rate Boost from School	SDS
White	0.062	N/A	0.063	N/A	0.071	N/A
Black	0.056	-0.006	0.052	-0.011	0.031	-0.040
Latino	0.065	0.003	0.052	-0.011	0.057	-0.014

a. For each racial composition group, the learning rate boost for whites is average of the grand mean learning rates for kindergarten and first grade, minus the summer grand mean of the summer learning rate   Black and Latino values are calculated by adding their respective SDS coefficients.

Appendix B: Chapter 3 Tables

Table B.1: Variables Used in Each Modeling Strategy	Propensity Score Matching	Multilevel Models
Dependent Variables		
Student Impact Math	Yes	Yes
Student Impact Reading	Yes	Yes
Level 1 Controls		
First Grade First SES Quintile (lowest)	Yes	Yes
First Grade Second SES Quintile	Yes	Yes
First Grade Third SES Quintile	Yes	Yes
First Grade Fourth SES Quintile	Yes	Yes
First Grade Fifth SES Quintile (Reference)	Yes	Yes, Reference
Female (Yes=1)	Yes	Yes
White (Reference)	Yes	Yes, Reference
Black	Yes	Yes
Latino	Yes	Yes
Asian	Yes	Yes
Other Race	Yes	Yes
Non-English Home Language (Yes=1)	Yes	Yes
Married Biological Parents (Yes=0)	Yes	Yes
Number of Siblings	Yes	Yes
First Grade Number of Absences	No	Yes
Kindergarten Absences	Yes	No
Kindergarten Repeater (Year 2, Yes=1)	Yes	Yes
First Grade Age at School Year Start (Years)	No	Yes
Kindergarten - Religiously Active (Yes=1)	Yes	No
First Grade - Religiously Active (Yes=1)	Yes	No
Level 2 Controls		
First Grade - Public School	Control Condition	Yes, Reference
First Grade - Catholic School	Treatment Condition	Yes
First Grade - Urban	Yes	Yes Reference
First Grade - Suburb	Yes	Yes
First Grade - Rural	Yes	Yes

Table B.2: Descriptive Statistics		
Dependent Variables	Mean	S.D.
Student Impact Math	0.0546	0.1331
Student Impact Reading	0.0867	0.1252
Level 1 Controls		
First Grade First SES Quintile	0.1764	0.3812
First Grade Second SES Quintile	0.1980	0.3986
First Grade Third SES Quintile	0.2042	0.4032
First Grade Fourth SES Quintile	0.2015	0.4011
First Grade Fifth SES Quintile	0.2199	0.4142
Female (Yes=1)	0.4885	0.4999
White (Reference)	0.5719	0.4949
Black	0.1532	0.3602
Latino	0.1571	0.3639
Asian	0.0416	0.1996
Other Race	0.0762	0.2653
Non-English Home Language (Yes=1)	0.1168	0.3212
Married Biological Parents (Yes=0)	0.3862	0.4869
Number of Siblings	1.4773	1.1133
First Grade Number of Absences	8.1197	9.2829
Kindergarten Repeater (Year 2, Yes=1)	0.0229	0.1496
First Grade Age at School Year Start (Years)	6.4597	0.3617
Kindergarten - Religiously Active (Yes=1)	0.6686	0.4707
First Grade - Religiously Active (Yes=1)	0.5695	0.4952
Level 2 Controls		
First Grade - Public School	0.8715	0.3347
First Grade - Catholic School	0.1285	0.3347
First Grade - Urban	0.3782	0.4850
First Grade - Suburb	0.4041	0.4908
First Grade - Rural	0.2178	0.4128
N Students	4030	
N Schools	390	

Table B.3: Average Treatment Effect on the Treated (ATT) - Student Impact Math						
Sample	ATT	SE_c	Treatment N_d	Control N	T Score	Significance
Full Sample	-0.013	0.007	520	3220	-1.97	*
Urban	-0.024	0.009	300	1200	-2.59	**
Suburban	0.003	0.010	170	1340	0.28	NS
Rural	-0.024	0.022	40	480	-1.08	NS
Whites	-0.012	0.008	390	1650	-1.54	NS
Blacks	-0.043	0.020	50	370	-2.12	*
Latinos	-0.003	0.023	50	320	-0.15	NS
Low SES Students_a	0.007	0.014	50	1000	0.47	NS
High SES Students_b	-0.017	0.008	380	1260	-2.12	*

x p<.10, * p<.05, **p<.10

a. SES quintiles 1 and 2

b. SES quintiles 4 and 5

c. Standard errors are estimated from 500 repetitions of bootstrapping.

d. All sample sizes are rounded to the nearest 10 as required by the user agreement with the NCES.

Table B.4: Average Treatment Effect on the Treated (ATT) - Student Impact Reading						
Sample	ATT	SE	Treatment N	Control N	T Score	Significance
Full Sample	0.000	0.006	520	3220	0.00	NS
Urban	-0.007	0.009	300	1200	-0.75	NS
Suburban	0.010	0.009	170	1340	1.13	NS
Rural	0.028	0.016	40	480	1.75	x
Whites	0.004	0.007	390	1650	0.67	NS
Blacks	-0.050	0.019	50	370	-2.72	**
Latinos	0.049	0.022	50	320	2.26	*
Low SES Students_a	0.003	0.016	50	1000	0.17	NS
High SES Students_b	-0.001	0.007	380	1260	-0.19	NS

x p<.10, * p<.05, **p<.10

a. SES quintiles 1 and 2

b. SES quintiles 4 and 5

c. Standard errors are estimated from 500 repetitions of bootstrapping.

d. All sample sizes are rounded to the nearest 10 as required by the user agreement with the NCES.

	Model 1	Model 2	Catholic Only Model 3 [c]	Public Only Model 4 [d]
Table B.5: School Sector Effect on Student Impact -Math [a]				
Level 1				
First SES quintile (Lowest) [b]	0.0155 x	0.0138 x	-0.0332	0.0177 *
Second SES quintile	0.0048	0.0033	-0.0105	0.0063
Third SES quintile	0.0035	0.0022	0.0010	0.0044
Fourth SES quintile	0.0148 *	0.0142 *	-0.0142	0.0215 **
Female	-0.0022	-0.0021	-0.0196 x	0.0000
Black (white=ref)	-0.0052	-0.0058	-0.0401 x	-0.0031
Latino	-0.0029	-0.0035	0.0130	-0.0055
Asian	-0.0290 **	-0.0298 **	0.0581	-0.0361 **
Other Race	-0.0077	-0.0081	-0.0273	-0.0072
Level 2				
Catholic School (Public = ref)		-0.0147		
Suburban (Urban = ref)	-0.0140 *	-0.0156 *	-0.0074	-0.0170 *
Rural	0.0067	0.0044	-0.0118	0.0050
Grand Mean (Gamma00)	0.0492	0.0538	0.0608	0.0499
N_e	4030	4030	520	3510
N-Schools	390	390	50	340

x P<.10, * p<.05, ** p<.01, *** p<.001

a. Models also control for whether English is spoken at home, parent marital status, number of siblings, number of absences during the school year, kindergarten repeater status, and age at school year start.

b. The fifth (highest) SES quintile is the reference group

c. Model includes only students who attend Catholic schools

d. Model includes only students who attend public schools

e. All sample sizes are rounded to the nearest 10 as required by the user agreement with the NCES.

			Catholic Only	Public Only
Level 1	Model 1	Model 2	Model 3[c]	Model 4[d]
First SES quintile (Lowest)[b]	0.0322 ***	0.0321 ***	0.0248	0.0350 ***
Second SES quintile	0.0084	0.0083	0.0111	0.0105
Third SES quintile	0.0093 x	0.0093	0.0100	0.0113
Fourth SES quintile	0.0079	0.0079	-0.0207 x	0.0160 *
Female	-0.0033	-0.0033	0.0001	-0.0042
Black (white=ref)	-0.0140 *	-0.0141 *	-0.0546 **	-0.0109
Latino	-0.0037	-0.0037	0.0313 x	-0.0085
Asian	-0.0257 *	-0.0257 *	-0.0187	-0.0256 *
Other Race	-0.0057	-0.0057	-0.0229	-0.0044
Level 2				
Catholic School (Public = ref)		-0.0006		
Suburban (Urban = ref)	0.0013	0.0012	0.0107	-0.0022
Rural	0.0177 *	0.0176 *	0.0336	0.0147
Grand Mean (Gamma00)	0.0717	0.0718	0.0749	0.0714
N_e	3880	3880	510	3370
N-Schools	380	380	50	330

Table B.6: School Sector Effect on Student Impact -Reading[a]

x P<.10, * p<.05, ** p<.01, *** p<.001

a. Models also control for whether English is spoken at home, parent marital status, number of siblings, number of absences during the school year, kindergarten repeater status, and age at school year start.

b. The fifth (highest) SES quintile is the reference group

c. Model includes only students who attend Catholic schools

d. Model includes only students who attend public schools

e. All sample sizes are rounded to the nearest 10 as required by the user agreement with the NCES.

Table B.7: Student Impact for Math and Reading in Catholic and Public Schools by Student Race and Socioeconomic Status [a]

Panel A: Math	Student Impact	
	Catholic	Public
Low SES	0.0457	0.0667
High SES	0.0407	0.0518
White	0.0407	0.0518
Black	0.0065	0.0512
Latino	0.0504	0.0494

Panel B: Reading	Student Impact	
	Catholic	Public
Low SES	0.1070	0.1033
High SES	0.0729	0.0708
White	0.0729	0.0708
Black	0.0171	0.0589
Latino	0.0987	0.0639

a. Values for each category are based on results from models (not shown) with cross level interactions of (1) school sector and student race and (2) school sector and student socioeconomic quintile. Each coefficient for Student Impact is calculated by adding the grand mean for each outcome (math and reading) to the appropriate coefficients (e.g. black, low SES). White students from the 5th socioeconomic quintile in public schools are the reference group.