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SCHOOL OF PUBLIC AFFAIRS
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Systemwide and Intervention-Specific Effects of Denver Public Schools' Portfolio District Strategy on Individual Student Achievement

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01 INTRODUCTION

Between 2008 and 2019, Denver Public Schools (DPS) implemented a comprehensive reform effort to reinvent the traditional school district. Instead of refining the century-old district model built for stability, uniformity, and centralized control, Denver's leadership implemented an alternative governance model to improve student achievement systemwide that was built for adaptation, differentiation, and continuous improvement.

Denver's comprehensive and centrally coordinated reform strategy was intended, according to the leaders who launched it, to "fundamentally change the culture and structure of public education" in the city by rejecting what they called "the 'one-size-fits-all, centralized, industrial-age' model of the past, in favor of one built to promote choice, empowerment, and equity of access for all students" (Bennet, Jupp, & DPS Board, 2007). The reforms implemented in Denver mark one of the first times in American history that an elected school board voluntarily relinquished the exclusive franchise to operate schools within its boundaries while maintaining its authority to govern all schools in the district. In doing so, the district rejected the traditional model of singularity in favor of one built for multiplicity (Baxter & Nelson, 2012).

To implement their vision, DPS leadership enacted a variety of strategic reforms over more than a decade aimed at reorganizing the district to facilitate choice for students and families, empowerment of schools and educators, and accountability for outcomes (Baxter, Ely, & Teske, 2019). Specifically, DPS leadership established a common market for public schooling that included all schools in the district—district-operated, innovation, and charter— under a common enrollment and expulsion system, a common set of performance metrics, and a common regulatory structure (Baxter et al, 2022).

Beginning in 2008, the district annually conducted an evaluation of all schools' performance, an analysis of district needs, a process for identifying and intervening in persistently low-performing schools, and a public call for new school proposals from internal and external partners (Baxter, Ely, & Teske, 2019).

Few other cities in the United States have so thoroughly altered the way they govern and deliver public education. Over 11 years, the district strategically opened more than 65 new autonomous public schools, both independently operated charter schools and semi-autonomous innovation schools and networks, and closed, restarted, replaced, or otherwise intervened in more than 35 existing schools.

The question of whether the reforms implemented in Denver increased academic achievement is one of ongoing controversy locally and nationally (Meltzer, 2023). Improvement in learning at-scale is one of the most pressing challenges we face as a society. The extent to which Denver's model offers a more effective alternative to the traditional unitary district has enormous implications.

Denver's unified regulatory and market structure and its decade of coordinated reform creates a rare opportunity to evaluate the system-level impact of the reform strategy and the effects of its constituent parts. Denver provides a context to test whether it is possible to improve public education at scale through an alternative paradigm with different operating assumptions, namely choice for families among multiple providers within a common market for publicly funded schooling that is governed and regulated for quality and equity (Baxter et al., 2022).

This study is part of a larger research effort designed to evaluate the effectiveness of Denver's portfolio district strategy (what district leaders called a "family of schools" model). In previous research, we examined the systemwide effects of Denver's reforms at the district level, using district-level data and comparing DPS to the largest and to the lowest performing districts in Colorado. We found that the reforms caused large improvements in math test scores, English Language Arts (ELA) test scores, and graduation rates for the average DPS student and for various subgroups (Baxter et al., 2022). Our prior findings provide evidence of the effectiveness of Denver's reforms for the entire population and various subgroups of students enrolled in DPS during the 11 years of implementation. It is possible, however, that the improvements we find at the system level were not entirely due to the reforms themselves but also to changes in the student population. Moreover, in conducting our prior research we did not yet have access to achievement data at the student level which made it impossible to evaluate the effects of specific reform strategies.

In this study, we use student-level data and limit our analyses to only those students who were enrolled in DPS prior to the reform efforts and who stayed in the district for their remaining academic trajectories during the reform years (see Table 2 for information about the years and grade levels included in the sample for this study). We focus first on whether Denver's reforms improved student outcomes systemwide and second on the effects of three primary reform tactics used by the district: the creation of new schools, the closure of existing schools, and district-led school turnaround. Given that DPS portfolio reform was a multi-year initiative that was implemented over time, we estimate the systemwide effects by year between the start of reform in the 2008-09 school year and the last year of available data for our sample of students, the 2014-15 school year. Students enrolled in DPS for at least two years before the start of reform experienced different numbers of years of reform. In addition to the yearly effects, we calculate cumulative effects of DPS reform for students in the sample based on the years that they were exposed to the reforms.

Table 1 summarizes the overall systemwide and intervention-specific effects identified in this study. At the system-level, the effect of DPS portfolio reform was negligible for students in math performance and positive and statistically significant in ELA performance in the first year of reform, the 2008-09 school year. However, the annual effects grew as DPS interventions were implemented over time and more widely. In the final three years of data for students in our sample, the annual effect of DPS reform on math performance was between 0.134 and 0.226 standard deviations and for ELA performance it was between 0.197 and 0.293 standard deviations. The estimates indicate that the cumulative effects were smaller for students in the early cohorts who had less exposure to DPS portfolio reform and more substantial for students who experienced most of their Kindergarten through 8th grade academic trajectory in DPS during the reform years. Across the cohorts in the sample, students performed between 0.175 and 0.978 standard deviations above the academic performance in ELA that would have been expected over the 7 years of reform in this study. The intervention-specific effects show that the impact of two of the three primary reform efforts, attending a new school that opened during reform and leaving a school closed during reform, were positive in math performance. Attending a new school also had a positive effect on ELA performance, while the effect of leaving a closed school during reform was negligible in ELA performance. The effects for students in schools identified for turnaround were negative in both math and ELA. Across the systemwide and intervention analyses, the results were consistent for math and ELA performance as well as most subgroups.

Table 1. Summary Systemwide and Intervention-Specific Effects

	Math z-score	ELA z-score
Systemwide effects		
Yearly		
2008-09 (Cohorts 3-7)	-0.000	0.059**
2009-10 (Cohorts 4-8)	0.038	0.116***
2010-11 (Cohorts 5-9)	0.062*	0.115***
2011-12 (Cohorts 6-9)	0.079*	0.181***
2012-13 (Cohorts 7-9)	0.134**	0.197***
2013-14 (Cohorts 8-9)	0.138**	0.191***
2014-15 (Cohort 9)	0.226***	0.293***
Cumulative		
Cohort 4 (grade 7 in 2009, grade 8 in 2010)	0.038	0.175***
Cohort 5 (grade 6 in 2009, grade 7 in 2010, grade 8 in 2011)	0.100	0.290***
Cohort 6 (grade 5 in 2009, grade 6 in 2010, grade 7 in 2011, grade 8 in 2012)	0.179	0.471***
Cohort 7 (grade 4 in 2009, grade 5 in 2010, grade 6 in 2011, grade 7 in 2012, grade 8 in 2013)	0.313	0.668***
Cohort 8 (grade 4 in 2010, grade 5 in 2011, grade 6 in 2012, grade 7 in 2013, grade 8 in 2014)	0.452**	0.800***
Cohort 9 (grade 4 in 2011, grade 5 in 2012, grade 6 in 2013, grade 7 in 2014, grade 8 in 2015)	0.640**	0.978***
Intervention effects		
Attending a new school	0.068***	0.030***
Leaving a closed school	0.081***	0.026
Leaving a closed school and attending a new school	0.094***	0.028
Attending a school identified for turnaround	-0.092***	-0.055***
* p<0.10, ** p <0.05, *** p <0.01		

02 DENVER'S PORTFOLIO DISTRICT STRATEGY

Historically and still today, public education in the United States has been provided by local school districts—geographically defined, special purpose governments, created by states to finance, govern, and operate public schools. School districts were created to be the sole providers of public education within their boundaries. Under the unitary model, a board of directors governs the school system and is responsible for collecting taxes from residents, employing administrators and teachers, building and operating schools, and educating students. Resources are controlled and allocated centrally, teachers are paid on a standardized scale irrespective of where or what they teach, and students are assigned to schools based on where they live. The goal is to provide schools of uniform character and quality to all children in a community (Cubberley, 1922, Cushman, 1951, Tyack, 1974).

The unitary district model was built and is maintained on the assumption that the effective and efficient delivery of public education requires centralized control and supervision, direct employment of all educators, and direct operation of all schools. The idea was that "regulation, bureaucratization, and centralization would equalize education by standardizing it, delegate decision making to experts, and 'Americanize' a diverse population" (Tyack, 1993, 3).

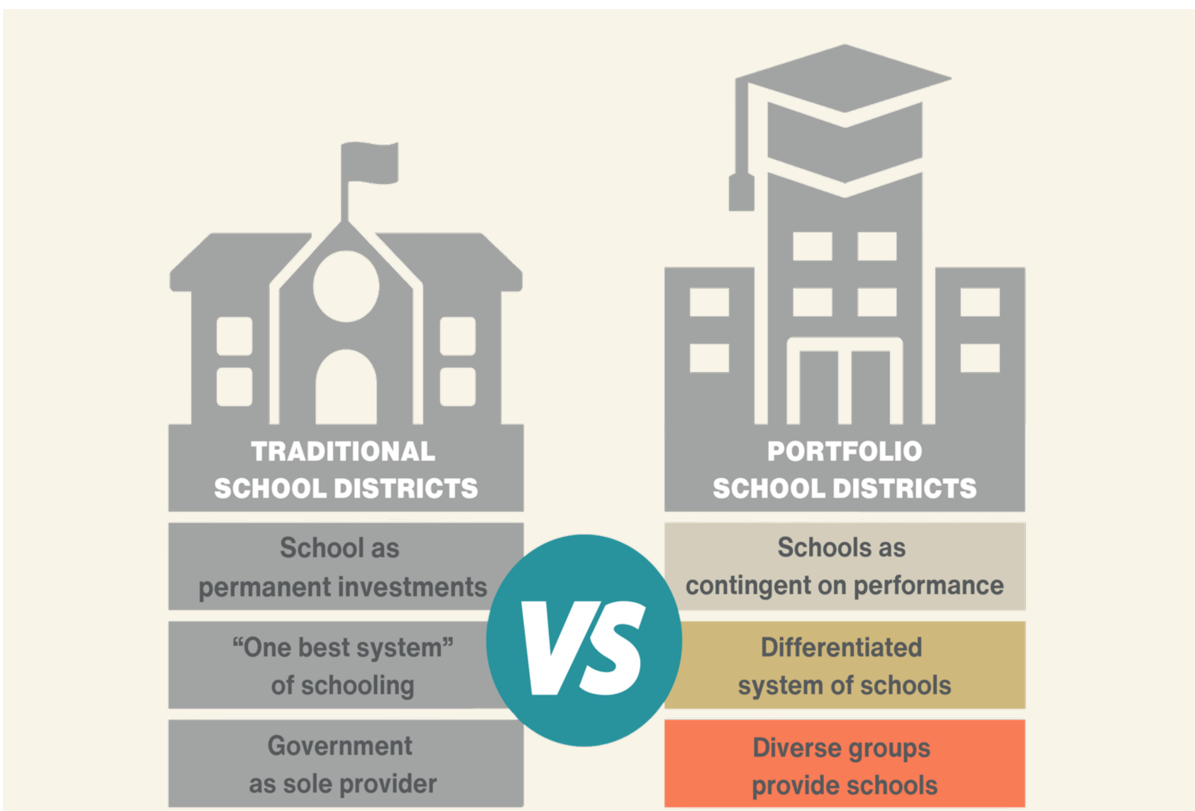
For more than a century, the unitary district was the exclusive governance model and operating structure for public education in the United States. Under this 'one best system,' "elected school boards, often with a collective bargaining agreement between the district and a local teachers' union, oversaw a set of neighborhood schools assigned to families based on geographic residence. The majority of these schools were overseen by a central office with highly bureaucratized rules and procedures dictated by both district and collective bargaining policies. While a small minority of schools had discretion over aspects of their school programs...district and union leaders retained control over the educational programs and staffing procedures for most schools" (Bulkley, Marsh, Strunk, Harris, & Hashim, 2021, 21).

Although a wide variety of publicly funded education alternatives have emerged over the last three decades, including charter schools, magnet schools, virtual schools, and—increasingly—private school tuition vouchers, tax credits and savings accounts, the traditional unitary district is still the model in use by nearly every one of the nation's more than 14,000 school districts which continue to enroll the vast majority of U.S. students (Bulkley et al., 2021). Indeed, the unitary district model is so much a part of American life and culture that prominent education scholars and political activists often argue that it is the only legitimate form of public education (Ravitch, 2010, Berkshire & Schneider, 2024).

Although the unitary model has always had detractors, efforts to provide a comprehensive alternative have been rare (Bulkley et al., 2021). One of the few exceptions is the portfolio district strategy (also called the portfolio management model or PMM). Developed and refined by Paul T. Hill at the turn of the 21st century, the portfolio district strategy combines three primary levers for education improvement: choice for families, autonomy for providers, and accountability for student outcomes (Hill, 1997, Hill, 1999, Hill, 2003, Hill, 2006, Hill, Pierce, & Guthrie, 1997, Hill & Celio, 1998, Hill, Campbell, & Harvey, 2000, Hill, Lake, & Celio, 2002. In contrast to other improvement strategies that employ these elements in isolation, either within the unitary district model or by disruption from the outside, the portfolio district strategy is a comprehensive effort to fundamentally reinvent the traditional unitary model itself (Figure 1).

The portfolio district strategy principles are intentionally different from those underlying the traditional unitary district model. Specifically, the portfolio district is organized to manage performance and encourage differentiated quality at scale by giving all schools autonomy over time, people, and money, and holding all schools accountable for student achievement.

Figure 1. A Different Vision of The School District



Whereas in a traditional district, public education resources-- instructional support, facilities, transportation, technology, classroom supplies, even textbooks and teachers—are all controlled and allocated centrally, the goal in a portfolio district is to differentiate resources based on need and to maximize control over resources at the school level so that educators may determine how best to meet the needs of the students they serve. Whereas in a traditional district the existence of schools not directly operated by the district is perceived as a threat to the district, a core component of the portfolio strategy is a focus on creating high-quality educational opportunities regardless of provider. Portfolio districts manage a portfolio of schools, operating some schools in the traditional way, contracting with external providers to run others as charter and contract schools, and holding all schools accountable under the same performance standards. A portfolio district collaborates and partners with whomever can help it best achieve its mission (Hill, 2006, Baxter & Nelson, 2012).

The portfolio district strategy is not intended to improve the traditional district model but to replace it with an entirely new approach to the governance and operation of public education. Unlike efforts to perfect existing systems which take the unitary model for granted and try to improve educational quality without challenging its fundamental assumptions, structures or operations, and unlike efforts aimed at bypassing and in some cases undermining existing systems in favor of unregulated private provision, the portfolio strategy is an attempt to reinvent existing systems from the ground up with new assumptions, structures and operations (Hill, Pierce, & Guthrie, 1997, Hill, 2006). The strategy does not include any particular curriculum or prescribe any set of programs. Its creators call it a problem-solving approach providing the district a set of tools system leaders can use to create better alignment between district goals and the incentives and capacities of educators and schools to meet them. In contrast to reform efforts that attempt to systematize quality through centralization of authority and standardization of content and instruction across all schools in the district, the portfolio district strategy is “focused on ends but flexible about means” (Hill, 2006, 17). In theory, the strategy enables school boards to govern a system of diverse schools in which educators customize their schools to meet the needs of the students and communities they serve and are accountable for student learning. In contrast to efforts which aim to improve schools within the paradigm of the unitary district and unlike efforts to bypass the district model altogether, the portfolio strategy is intended to replace the traditional model with an entirely different approach to public education (Hill, 2003, 2006).

The strategy is built around a set of interlocking policy levers—conditions, structures, and incentives—intended to drive more efficient, effective, and equitable performance systemwide.

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The exact list of components differs slightly among scholars and advocates of the strategy, but in general it includes the following core features:

- Public Coordination, Planning and Oversight of Multiple Education Providers
- A Multiplicity of Educational Opportunities with Informed Choice for Families and Protections for Equitable Access
- Flexibility for Schools on Curriculum, Staffing, Operations and Weighted Funding
- Public Accountability for Student Learning through Ongoing Evaluation and Intervention
- Educator Recruitment, Development and Support from Multiple Sources.

These elements are interlocking and not intended to be used in isolation (Bulkley et al., 2021). Indeed, Hill (2006, 16), warned that "if only a few reform elements are adopted, or the main supports of the existing system are left in place, so-called portfolio management experiments will join the litany of unsuccessful education reform fads." Hill and his colleagues have also long emphasized that the elements of the strategy cannot be implemented by districts alone and instead require the efforts of entire cities and communities (Hill, Campbell, & Harvey, 2000). Working together, these elements are intended to improve student learning at scale by enhancing "alignment between the goals of those governing a system and those doing the daily work of educating students" (Bulkley et al., 2021, 20). The idea is to improve the whole system by empowering and supporting educators to design and operate a diverse array of schools, giving families equitable choice from among them, and holding educators and schools accountable for student learning.

When Denver Public Schools first launched its own version of the portfolio strategy in the fall of 2007, the district, then Colorado's second largest, was among the bottom 10 districts in the state in ELA and math performance on state standardized assessments, ranking below the 5th percentile of districts. The district's 4-year-graduation rate was 40 percent. In an effort to dramatically improve student achievement systemwide, DPS launched one of the most ambitious and comprehensive district reform initiatives in American history. In announcing their plan for improvement, DPS and city leaders explicitly rejected the traditional unitary district model and embraced the portfolio district strategy (Bennet, Jupp, & DPS Board, 2007).

Specifically, Denver's strategy included the following components:

- A shift from direct management and operation of a single set of schools to the governance and oversight of a variety of school options.
- A systemwide focus on ensuring equitable access to quality education for all students while allowing for flexibility and innovation in how education is delivered.
- A framework for public reporting and accountability with a common set of performance metrics for all schools.
- A unified enrollment and expulsion system for all schools.
- A strategic focus on recruiting, developing, empowering, evaluating and supporting teachers and school leaders.
- A flexible funding model which allocates dollars based on student need.
- An annual process for creating new schools and intervening in persistently low-performing schools with internal and external partners.
- Support for a citywide education ecosystem through public transparency, cross sector collaboration, and decentralized authority with shared responsibility for the success of all students (Baxter et al., 2022, Baxter, Ely, & Teske, 2019

DPS went on to implement its version of the portfolio strategy for more than a decade, and its components were applied to all publicly funded schools in the city. Unlike other large school systems that have implemented the strategy in pieces and at various times, and unlike those cities that were unable to sustain the strategy or coordinate it among multiple providers or across jurisdictional lines, Denver's implementation of the district portfolio strategy was comprehensive across its entire system of traditional, innovation, and charter schools and employed the three core elements—choice, autonomy, and accountability—in an interconnected way and sustained its reforms for a decade, including four school board elections (Baxter et al., 2022, Baxter, Ely, & Teske, 2019).

Beginning in 2008 and continuing through 2019, DPS conducted an annual evaluation of all schools in the district, an annual public request for new school proposals from internal and external providers, and an annual process for intervening in persistently low-performing schools through closure, replacement, and district-led school turnaround. Each winter, following a districtwide school choice process, the district conducted a comprehensive analysis of all educational opportunities in the city and created a snapshot of the district and its geographic regions. The district used the Strategic Regional Analysis to inform an annual request for new school proposals and to signal its needs to educators. The Call for Quality Schools was then issued each spring outlining the needs of the district and inviting proposals from educators from outside and inside the district to create new schools to help meet them. Then, each fall, after months of evaluation and a series of public meetings, the DPS Board voted on recommendations from DPS administrators to approve interventions in schools deemed low performing and on which new school applicants to approve based on each proposal's likelihood of success and fit with identified district needs. Although these elements of the annual cycle were refined over time, each was present from the beginning.

School turnaround interventions were incorporated into the DPS portfolio strategy and were driven by requirements in federal and state law (Denver Public Schools, 2016, Colorado Department of Education, n.d.). This study includes schools in DPS, both district-managed schools and charter schools, which were identified for improvement by the district and state under the federal School Improvement Grants (SIG) program authorized by Title I, Section 1003(g) of the No Child Left Behind Act of 2001, as modified by American Recovery and Reinvestment Act of 2009 (ARRA) (SIG Cohorts I and II). Intended to spur dramatic improvement in the persistently lowest performing schools in each state, the SIG program required states and districts to identify the lowest-performing five percent of schools and to implement in those schools one of four improvement models:

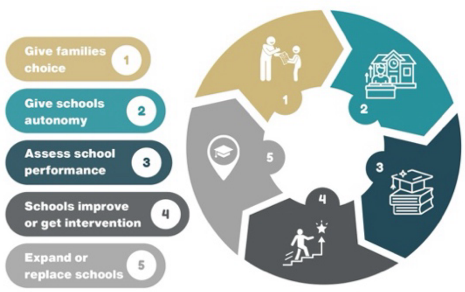
- **Restart model:** Reopen the school under the management of a charter school operator, a charter management organization, or an education management organization.
- **School closure:** Close the school and reassign students to higher-achieving schools.
- **Transformation model:** Replace the principal, develop a teacher- and leader-evaluation system that takes student progress into account, introduce significant instructional reforms, increase learning time and provide flexibility and support.
- **Turnaround model:** Replace the principal and no less than 50 percent of the staff, introduce significant instructional reforms, increase learning time, and provide flexibility and support (Hurlburt, Therriault, & Le Floch, 2012, Denver Public Schools, 2016).

In a portfolio district, the role of the school board shifts from overseeing the direct operation of all schools to determining the needs of the district, creating and maintaining a portfolio of diverse schools to meet those needs, evaluating schools and providing public information about all schools' performance, closing and replacing schools that consistently fail their students, and ensuring fair access to quality opportunities (Hill, 1999).

The theory of action of the portfolio strategy and of Denver's implementation of it holds that it is possible to create a cycle of continuous improvement in the district by empowering educators to operate and families to choose schools, evaluating all schools based on performance, intervening in persistently low-performing schools either through closure or turnaround, and the regular creation of new schools where needed (Figure 2).

Figure 2.

The continuous improvement cycle of the portfolio strategy I Center on Reinventing Public Education I 2012



Denver Public Schools I Strategic Regional Analysis I 2019

Over 11 years, Denver's implementation of the portfolio district strategy dramatically changed public education in the city. Overall, more than 65 new schools were created and more than 35 were closed, restarted, or replaced. Today, over half of the 200 public schools in the city are district-authorized charter schools or semi-autonomous innovation schools. During the reform period, the percentage of Denver students enrolled in charter schools doubled from just above 10 percent to above 20 percent. More than 80 percent of families participate in the district's school choice system, and more than 40 percent choose a school other than their geographic assignment. All schools participate in the district's unified enrollment and expulsion system, and each is subject to oversight from the DPS Board of Education.

The changes that created Denver's new system were and remain highly controversial. Their interlocking and mutually reinforcing nature, and that they change the allocation of power and control in the district, has made the reforms resilient in the face of political change (Baxter & Gottlieb, 2022). Nonetheless, the question of whether Denver's reforms helped or hurt students, particularly low-income students and students of color who make up a large majority of the district, is a matter of intense public debate (Meltzer, 2023). Considering the stakes, it is vital that this debate be informed by empirical evidence.

03 EVALUATING THE EFFECTS OF DENVER'S STRATEGY OVERALL AND OF THREE PRIMARY INTERVENTIONS

There is an extensive body of research examining the individual components of Denver's strategy, particularly charter schools, school closure, and so-called turnaround reforms. Existing research has generally focused on these reform components independently of each other and of the larger education systems in which they are embedded. Much less is known about how these reforms might operate together as part of a coordinated strategy to improve an entire school system. Research on the reforms implemented following Hurricane Katrina in New Orleans is the exception (Harris & Larsen, 2023, Bross, Harris, & Liu, 2023).

This study is part of a larger research effort designed to evaluate the effectiveness of Denver's portfolio district strategy as it was implemented between 2008 and 2019. In this study, we focus first on whether Denver's reforms improved student outcomes systemwide and second on the effects of three primary reform tactics used by the district: the creation of new schools, the closure of persistently low-performing schools, and federally defined turnaround options, including turnaround, restart, and transformation.

In previous research, we examined the system-level effects of Denver's reforms at the district level, using district-level data and comparing DPS to the largest and to the lowest performing districts in Colorado (Baxter et al., 2022). We found that the reforms caused large improvements in math test scores, English Language Arts (ELA) test scores, and graduation rates for the average DPS student and for various subgroups. Our prior findings provide evidence of Denver's reforms' effectiveness for the entire population of students enrolled in DPS and various subgroups during the 11 years of implementation. It is possible, however, that the improvements we find at the system level were not due entirely to the reforms themselves but also to changes in the student population. Moreover, in conducting our prior research we did not yet have access to achievement data at the student level which made it impossible to evaluate the effects of specific reform strategies.

In this study, we address these limitations by using student-level data and focusing exclusively on the effects of the reforms on cohorts of students who were enrolled in DPS for at least two years prior to the reforms and at least two years during the reforms. We use propensity score matching to select a comparison group of students from 11 surrounding districts who share the same demographic characteristics and academic backgrounds as the DPS students in our dataset. We then use a difference-in-differences model to estimate the average system-level effect of DPS reform on students who were enrolled in DPS before and after the onset of the reforms in the 2008-09 school year, comparing DPS students to the matched comparison group.

In addition to the systemwide effects of the reform strategy, we evaluate the effects of three primary interventions employed by DPS as part of its strategy—new schools, school closures, and district-led school turnaround—using student fixed effects models, comparing the achievement of students during reform to their own achievement before reform. These methods allow us to isolate and identify the systemwide effects of Denver's reform strategy and the effects of three of its primary mechanisms on the achievement of students who were enrolled in the district before they began and during their implementation.

04 DATA

The Colorado Department of Education (CDE) provided student-level panel data for the 2004-05 through 2018-19 school years for a subset of students in DPS and 11 comparison districts that were selected for their geographic proximity to DPS. The CDE data included information on school and district enrollment, grade level, race/ethnicity, gender, special education, English Learner, migrant, homeless, state assessment achievement test scores for grades 3 through 8 in math and English Language Arts (ELA), high school graduation, and college matriculation. CDE did not provide free- or reduced-price lunch (FRL) data for students. We collected school-level FRL percentage ranges from publicly available CDE data to include in our analyses.

We requested a subset of data for students in DPS and 11 comparison districts that included students in grade levels that aligned with nine cohorts who were enrolled in DPS for a minimum of two years prior to the reform efforts that began in the 2008-09 school year and a minimum of two years post reform. The cohort sample design means that we do not have access to all student data for students in DPS or the 11 comparison districts across the years in the study. However, the sample design lets us examine the impact of DPS reform efforts on students enrolled in the district before the start of reform, thus isolating the impact of DPS reform on student outcomes.

Table 2 shows the grade levels by year for each cohort, highlighting the year of the start of reform in gray outline, the baseline years for each cohort in gold, the grade levels by year included in the grades 4 through 8 analyses in gold plus green (grade three included as prior achievement for 4th graders in the model), and 12th grade in sandstone where we analyze graduation and matriculation. The baseline years were established as the first year of available state assessment data for each cohort of students who have a minimum of two years before and two years during DPS reform. We used the baseline years to match students for the analyses. Each cohort includes only those students who were matched in the baseline year.

For example, cohort 1 includes students who were in grade 6 in the 2004-05 school year and tracks them until grade 12 in the 2010-11 school year. Cohort 9 includes students who started Kindergarten in 2006-07, tested in grade 3 in the 2009-10 school year, and entered grade 12 in the 2018-19 school year.

Table 2. Sample cohort matrix

Cohort	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019
1	6	7	8	9	10	11	12								
2	5	6	7	8	9	10	11	12							
3	4	5	6	7	8	9	10	11	12						
4	3	4	5	6	7	8	9	10	11	12					
5	2	3	4	5	6	7	8	9	10	11	12				
6	1	2	3	4	5	6	7	8	9	10	11	12			
7	K	1	2	3	4	5	6	7	8	9	10	11	12		
8		K	1	2	3	4	5	6	7	8	9	10	11	12	
9			K	1	2	3	4	5	6	7	8	9	10	11	12

Outlined box indicates the first year of DPS reform
Gold indicates the baseline year for each cohort
Gold + green indicates the grades and years included in the 4-8 analyses
Sandstone indicates grade 12 for the graduation and matriculation analyses

Table 3 presents descriptive statistics for the sample of students in the cohorts in the baseline years, before matching. We standardized the state assessment data using the grade- and year-specific state-wide means and standard deviations of the test scores. This transforms the scale of the test score to standard deviation units, or z-scores, where the state mean is 0 and standard deviation is 1. A z-score of 0 indicates that the student is performing at the state mean relative to other students in the same grade and year. The standard deviations for the z-scores are not exactly 1 in Table 3 because the standard deviations presented are for the samples of DPS students and comparison district students instead of the state population sample.

Compared to the comparison districts, lower percentages of students in DPS were White and Asian and higher percentages of students in DPS were Hispanic and Black. Higher percentages of students in DPS were designated as special education and English Learner and there was a higher percentage of migrant students. A lower percentage of DPS students were designated as gifted and talented. A higher percentage of DPS students attended schools where the FRL range was 50%-75% and 75%-100%. DPS students had lower achievement levels in both math and ELA.

Given that the DPS students were different from the full set of students in comparison districts in the cohorts in the baseline years, we use propensity score matching (PSM) to identify a matched sample of students from the comparison districts for the analyses. We use a matched comparison group for the systemwide analyses, but only DPS students for the intervention analyses.

Table 3. Descriptive statistics at baseline year pooled across cohorts

	DPS Students			Comparison District Students		
	# Stud.	Mean	Std Dev	# Stud.	Mean	Std Dev
Demographics						
White	52,458	0.197	0.398	181,389	0.580	0.494
Hispanic	52,458	0.586	0.493	181,389	0.293	0.455
Black	52,458	0.172	0.377	181,389	0.069	0.254
Asian	52,458	0.034	0.180	181,389	0.048	0.213
American Indian or Alaska Native	52,458	0.012	0.108	181,389	0.010	0.099
Female	52,458	0.492	0.500	181,389	0.489	0.500
Special Education	52,458	0.127	0.333	181,389	0.102	0.302
English Learner	52,458	0.350	0.477	181,389	0.195	0.396
Gifted and Talented	52,458	0.013	0.334	181,389	0.068	0.251
Homeless	52,458	0.015	0.121	181,389	0.015	0.122
Migrant	52,458	0.036	0.186	181,389	0.009	0.092
FRL Range 0%-25%	52,458	0.090	0.286	181,389	0.488	0.500
FRL Range 25%-50%	52,458	0.158	0.365	181,389	0.208	0.406
FRL Range 50%-75%	52,458	0.245	0.430	181,389	0.166	0.372
FRL Range 75%-100%	52,458	0.507	0.500	181,389	0.138	0.344
Achievement						
Math z-score	49,421	-0.533	1.046	171,855	-0.018	1.009
ELA z-score	49,733	-0.481	1.078	172,525	-0.034	0.974

Our research team completed primary data collection to gather school-level DPS reform related data for the study years from publicly available sources. The data include year-by-school flags that indicate new schools, school closures, and schools identified for turnaround. See Table A1 in the Appendix for the count of DPS students in the sample by each of the reform interventions by year.

05 EMPIRICAL FRAMEWORK

The primary methodological challenge to measuring the impact of DPS reforms (or any educational intervention) is that it is impossible to know what would have happened to DPS students had the reforms not been implemented. Consequently, we must estimate the effects by comparing the academic outcomes of DPS students (i.e., the treatment group) to a control group of non-DPS students.

Given our study design, the findings in this study are generalizable to the subset of students who were enrolled in DPS for at least two years prior to the start of reform and stayed in the district for at least two years after the start of reform. We employ three statistical techniques, described in the following sections, to address this challenge: Propensity Score Matching, Systemwide Difference-in-Differences Estimation, and Intervention Student Fixed-Effects.

06

PROPENSITY SCORE MATCHING

The gold standard method for overcoming the lack of a counterfactual is to conduct an experiment, whereby students are randomly assigned to a treatment and control group. Random assignment ensures that all student characteristics (observed and unobserved) that influence student achievement independently of their school district are probabilistically the same for the treatment and control groups. Satisfying this condition ensures that we do not incorrectly attribute the differences in outcomes between treatment and control to the DPS reforms when they stem from differences in the characteristics of DPS and non-DPS students. Choosing school districts is not a random process and it is reasonable to expect that some of the factors that determine where families enroll their students may also influence their students' academic performance (e.g., parent education level).

Given that students are not randomly assigned to DPS, we rely on a statistical technique known as Propensity Score Matching (PSM) to select a control group of students from the 11 surrounding districts who share the same demographic characteristics and academic backgrounds as the DPS students in our dataset. This matching process is designed to balance the student characteristics between the treated and control groups, thus mimicking a randomized experimental design.

There are two steps to PSM. Step one is to estimate each student's probability of being enrolled in DPS based on their demographic characteristics and prior achievement levels. These values are referred to as propensity scores. We estimate the propensity scores separately for each cohort of students in their respective baseline years, which are the first year they show up in our dataset with state test scores. For most cohorts, propensity scores are based on third grade data (see Table 2). Propensity scores are calculated for DPS students and all non-DPS students in the surrounding 11 districts.

The propensity score for each student is estimated using a logistic regression model:

$$logit\left(\hat{e}\left(DPS_{igt}\right)\right) = b_0 + b_x\boldsymbol{X}_{igt-1} + b_A A_{igt-1}$$

where the outcome, $\mathrm{DPS_{igt}}$, is a binary indicator equal to one if student i in grade g and year t is enrolled in DPS. The outcome is regressed on a set of student characteristics X that includes special education, English Learner, migrant, homeless, gifted and talented, Black, Hispanic, White, and Asian. A is a vector of two prior achievement measures: math and ELA state test z-scores from the baseline year and grade.

The second step is to match each DPS student to a non-DPS student who has the closest propensity score to their own. This is known as nearest neighbor matching. By design, students who share the same propensity score will be similar on the characteristics included in the logit model.

To test the balance of the treatment and control group we divided students into five blocks according to their propensity scores. We ensured that treated and control units within each block had similar propensity scores, which enhances the balance between the groups. The common support condition was enforced to exclude observations where there was no overlap between the treatment and control groups in terms of their propensity scores.

Table 4 presents the balance test statistics for the variables used in the PSM. There are several variables where the difference is statistically significant, but overall the groups are well balanced.

Table 4. Comparison of means on matching variables

	DPS	Matched Control	Difference
White	0.197	0.194	0.003
Hispanic	0.587	0.587	0.000
Black	0.171	0.176	-0.005*
English Learner	0.346	0.347	-0.001
Special Education	0.119	0.118	0.001
Gifted	0.134	0.099	0.035***
Homeless	0.014	0.015	-0.001
Migrant	0.036	0.016	0.020***
Math Score (z)	-0.529	-0.513	-0.016*
ELA Score (z)	-0.478	-0.470	-0.008
N count	49,298	49,298	
* p<0.05, ** p<0.01, *** p<0.001			

Our analyses restrict the sample to students enrolled in DPS or the control districts for all years observed in the district panel where they were matched at baseline. Records for students who switched from DPS to other districts or from the control districts to other districts are excluded from the model.

We examined attrition for DPS and matched control district students for students in grades 3 through 8 and students in grades 8 through 12 (see Table A2 in the Appendix). First, we examine a persistence rate, which is defined as the percentage of students who stayed in the district they

were enrolled in during the baseline year used for matching. For students in grades 3 through 8, there were similar levels of persistence in their baseline district through 8^{th} grade (69.5 percent for DPS students and 68.9 percent for control district students). For students in grades 8 through 12, students in the control districts were slightly more likely than DPS students to persist to 12^{th} grade in the same district that they were enrolled in during the baseline year (59.7 percent versus 55.9 percent, respectively).

Second, we examined the difference-in-differences in math and ELA z-scores between students who persisted and attritted between DPS and the control districts, controlling for student demographics. For the students in grades 3 through 8, we find no statistically significant differential for 3^{rd} grade ELA z-scores, but a statistically significant negative differential for 3^{rd} grade math z-scores, indicating that students with higher test scores in DPS were more likely to stay in the district. For the students in grades 8 through 12, we find the opposite. There were negative statistically significant differentials in 8^{th} grade ELA z-scores, while differentials in 8^{th} grade math z-scores were statistically insignificant. Overall, the findings suggest that there were some differences between the students who stayed in DPS compared with matched students in the control districts. The statistically significant differentials in 3^{rd} grade math and 8^{th} grade ELA suggest that our findings may be biased upward.

07

SYSTEMWIDE MODELS

Consistent with prior studies of school choice reforms (Betts, 2009, Bross, Harris, & Liu, 2023, Chen & Harris, 2023, Cordes, 2018, Harris & Larsen, 2023, Jabar, et al., 2022, Waddington & Berends, 2018, Zimmer et al., 2021) we use a difference-in-differences model with student-level panel data to estimate the systemwide, average effect of DPS reform on students who were enrolled in DPS before and after the onset of the reforms in the 2008-09 school year. While the DPS reform efforts began in the 2008-09 school year, the portfolio reform interventions were implemented over time, in effect building up over time. We use the following model specification to estimate yearly overall systemwide DPS reform effects for students in grades 4 through 8:

$$Y_{igt} = b_1 ReformYear_{it} + b_2(DPS_i * ReformYear_{it}) + \mathbf{X}_{it} + \mathrm{A}_{it} + \gamma_t + G_{gt} + (\gamma_t * G_{gt}) + \delta_i + \varepsilon_{igt}, \quad (1)$$

where $\mathrm{Y_{igt}}$ is the state test z-score in math or ELA for student i, grade g, and year t. $\mathrm{ReformYear_{it}}$ is a binary indicator for each of the reform-era years. It takes a value of zero for all students (DPS and matched non-DPS students) during the pre-reform years (2005-06 to 2007-08), and a value of one for all students in each of the reform years (2008-09 to 2014-15). The coefficient $\mathrm{b_1}$ is an estimate of the mean difference in all students' test scores from before and after the start of the reforms in the 2008-09 school year. $\mathrm{DPS_i}$ is a binary indicator equal to one if the student was enrolled in DPS. The interaction of $\mathrm{DPS_i}$ with $\mathrm{ReformYear_{it}}$ yields the difference-in-differences coefficients for each year, $\mathrm{b_2}$. These coefficients are an estimate of the average difference in the change in test scores of DPS students and non-DPS students' from before and after the start of DPS's reforms, by reform year. $\mathrm{X_{it}}$ is a vector of the following student characteristics that vary within students over time: special education, English Learner, homeless, migrant, gifted and talented, and student mobility (first year in the school). $\mathrm{A_{it}}$ is a vector of two prior achievement measures: math and ELA state test z-scores from the previous school year. The model also includes year and grade fixed effects, $\mathrm{\gamma_t}$ and $\mathrm{G_{gt}}$, and their interaction to control for unobserved effects that are constant to all students within the same grade and year. Student fixed effects, $\mathrm{\delta_i}$, are included in the model to control for all observed and unobserved time-invariant student characteristics. Standard errors, $\mathrm{\varepsilon_{igt}}$, are adjusted to account for the clustering of students within districts.

Equation 1 estimates year-specific DPS reform estimates for students in grades 4 through 8. The cohorts of DPS students in our sample were each exposed to a different number of years of the DPS portfolio reform. For example, cohort 4 was exposed to two years of reform prior to 8th grade testing (2008-09 and 2009-10) while cohort 8 was exposed to six years of reform before 8th grade testing (2008-09 through 2014-15). To understand the cumulative overall systemwide effect of the DPS reform efforts on students, we sum yearly estimates from the panel data in equation 1 for the years that each cohort was exposed to DPS portfolio reform.

We also examine the effect of DPS reform efforts on high school graduation and college matriculation at the system-level. We are not able to use the same difference-in-difference analytic model because we do not observe high school graduation or college matriculation before DPS reform in the 2008-09 school year. Additionally, high school graduation and college matriculation are one-time events. As a result, we use the following linear probability model to estimate the overall DPS reform impacts on graduation and matriculation:

$$Y_i = b_1 DPS_i + \mathbf{X}_i + \mathrm{A}_i + \gamma_t + \varepsilon_i, \qquad (2)$$

where $\mathrm{Y_i}$ is either graduation or college matriculation for student i. $\mathrm{DPS_i}$ is a binary indicator equal to one if the student was enrolled in DPS. $\mathrm{X_i}$ is a vector of the following student characteristics in grade 12: gender, race/ethnicity, special education, English Learner, homeless, migrant, gifted and talented, and student mobility (first year in the school). $\mathrm{A_i}$ is a vector of a student's math and ELA state test z-scores from grade 8. γ_t are year fixed effects and ε_i is the error term. In the results, we present the overall predictive margins for DPS and control group students, which is the average predicted probability of graduation and college matriculation.

08 INTERVENTION MODELS

Separately, we examine three DPS intervention strategies—new schools, school closures, and identifying schools for turnaround—using student fixed effects models (Imberman, 2011, Nicotera, et al., 2010, Sass, 2006, Winters, 2012, Zimmer et al., 2009). This analytic approach allows us to examine changes in school intervention enrollment on student achievement. It should be noted that students may have experienced more than one DPS reform intervention. We estimate the intervention models separately such that students may be included in more than one DPS reform intervention model. Additionally, given that the control districts did not implement the intervention strategies employed by DPS, we restrict the intervention analyses to DPS students in our sample cohorts.

We use the following student fixed effects model to estimate the average effect of attending a new school opened during the DPS reform years:

$$Y_{igt} = b_1 NewSchool_{it} + \mathbf{X}_{it} + \mathrm{A}_{it} + \gamma_t + G_{gt} + (\gamma_t * G_{gt}) + \delta_i + \varepsilon_{igt}, \qquad (3)$$

where $\mathrm{Y_{igt}}$ is the state test z-score in math or ELA for student i, in grade g, and year t. $\mathrm{NewSchool_{it}}$ is a binary indicator equal to one in each year that the student attended a new school opened during the DPS reform years. $\mathrm{X_{it}}$ is a vector of the following student characteristics that vary within students over time: special education, English Learner, homeless, migrant, gifted and talented, and student mobility (first year in the school). There may be a concern that the inclusion of a flag for student mobility would capture some of the negative effect of the disruption from moving schools and thus upward bias the intervention-specific estimates. We estimated the models with and without the student mobility variable and the estimate for new schools remained the same. $\mathrm{A_{it}}$ is a vector of two prior achievement measures: math and ELA state test z-scores from the previous school year. The model also includes year and grade fixed effects, $\mathrm{\gamma_t}$ and $\mathrm{G_{gt}}$, and their interaction to control for unobserved effects that are constant to all students within the same year and grade. Student fixed effects, $\mathrm{\delta_i}$, are included in the model to control for all observed and unobserved time-invariant student characteristics. $\mathrm{\varepsilon_{igt}}$ is the error term. We also examine a modified version of equation 3 where we estimate the effect of attending a new school in each of the first five years that the new school was open during DPS reform.

To examine school closures during the DPS reform years, we use the following student fixed effects model to estimate the average effect of leaving a closed school:

$$Y_{igt} = b_1 PostSchoolClosure_{it} + \mathbf{X}_{it} + \mathrm{A}_{it} + \gamma_t + G_{gt} + (\gamma_t * G_{gt}) + \delta_i + \varepsilon_{igt}, \qquad (4)$$

where $\mathbf{PostSchoolClosure_{it}}$ is a binary indicator equal to one for student i in all years t after the student left the school that closed. All other variables are the same as equation 3. In this study, we estimate the effect of all school closures and closures resulting from a turnaround decision in the district. We also examine a modified version of equation 4 where we estimate the effect of attending a new school opened during DPS reform after leaving a school that was closed during DPS reform.

For our turnaround analyses, we use the following student fixed effects model to estimate the average effect of attending a school identified for turnaround:

$$Y_{igt} = b_1 Turnaround_{it} + \mathbf{X}_{it} + \mathrm{A}_{it} + \gamma_t + G_{gt} + (\gamma_t * G_{gt}) + \delta_i + \varepsilon_{igt}, \quad (5)$$

where $\mathbf{Turnaround_{it}}$ is a binary indicator equal to one for student i in all years t that the school was identified for turnaround strategies. The turnaround estimate compares the academic performance of students when the school is identified for turnaround to the academic performance of the same students before the school was identified for turnaround. All other variables are the same as equation 3.

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09 Results

Systemwide Results

Table 5 presents the overall systemwide effects of DPS reform efforts by year for students in grades 4 through 8 who had been enrolled in the district for at least two years prior to the start of reform in 2008-09. The effect of DPS portfolio reform was negligible for students in math and positive and statistically significant in ELA in the first year of reform, the 2008-09 school year. However, the annual effects increased as DPS interventions were implemented over time and more widely. In the final three years of data for students in our sample, the annual effect of DPS reform on math was an increase between 0.138 and 0.226 standard deviations and for ELA it was an increase between 0.197 and 0.293 standard deviations. We present the full set of yearly estimates for subgroups in Table A3 in the Appendix. In general, the yearly effects were positive in ELA for all subgroups except for English Learner students where the ELA results were statistically insignificant in all years except for the last when the results were positive. The findings suggest that English Learner students performed the same as they would have without DPS reform until the last two years in our study. The yearly effects for math were statistically insignificant for most subgroups in the first two years of reform and then were positive for the subsequent five years of reform, except for Native American students where the math results were negative in all years.

Table 5. Difference-in-differences overall estimates for DPS reform, by year

School Year	Math	ELA	Treatment N	Control N
2008-09	-0.000	0.059**	19,197	18,889
	(0.022)	(0.020)		
2009-10	0.038	0.116***	19,849	19,484
	(0.034)	(0.030)		
2010-11	0.062*	0.115***	20,880	20,475
	(0.033)	(0.032)		
2011-12	0.079*	0.181***	16,391	16,042
	(0.044)	(0.047)		
2012-13	0.134**	0.197***	11,972	11,528
	(0.051)	(0.053)		
2013-14	0.138**	0.191***	7,901	7,718
	(0.050)	(0.058)		
2014-15	0.226***	0.293***	3,921	3,597
	(0.049)	(0.086)		

* p<0.10, ** p <0.05, *** p <0.01

Table 6 presents the overall systemwide effects of DPS reform efforts by cohort for students in grades 4 through 8 who had been enrolled in the district for at least two years prior to the start of reform in 2008-09. The cohort estimates were smaller for students in the early cohorts who had less exposure to DPS portfolio reform and more substantial for students who experienced the majority of their Kindergarten through 8th grade academic trajectory in DPS during the reform years. Students performed between 0.175 and 0.978 standard deviations above what would have been the expected ELA performance for students over the 7 years of reform in this study. This means that the average DPS student in the sample cohorts received the equivalent of at least an additional six (6) months and as much as an additional 27 months of schooling compared to their matched peers in surrounding districts (Baird & Pane, 2019, Evans & Huan, 2019, Hanushek, Woessmann, & Peterson, 2012, Bloom et al., 2008). The effect of DPS reform on math was statistically insignificant for cohorts 4 through 7, suggesting that student performed the same in math during DPS reform, but the effects of DPS reform on math were positive and statistically significant for cohorts 8 a nd 9. These are the effects for the average DPS student in the sample irrespective of whether they experienced any of the reforms directly. The results hold for nearly every subgroup, except for Native American students who experienced negative cumulative effects in math and English Learner students who experienced statistically insignificant cumulative effects in ELA (see Table A4 in the Appendix).

Table 6. Difference-in-differences cumulative overall estimates for DPS reform, by cohort

	Math	ELA	Treatment N	Control N
Cohort 4	0.038	0.175***	3,045	3,035
	(0.055)	(0.049)		
Cohort 5	0.100	0.290***	3,008	2,939
	(0.086)	(0.082)		
Cohort 6	0.179	0.471***	3,443	3,447
	(0.130)	(0.128)		
Cohort 7	0.313	0.668***	3,546	3,385
	(0.176)	(0.180)		
Cohort 8	0.452**	0.800***	3,638	3,583
	(0.203)	(0.219)		
Cohort 9	0.640**	0.978***	3,921	3,597
	(0.216)	(0.273)		

* p<0.10, ** p <0.05, *** p <0.01

Tables 7 and 8 provide the probability of high school graduation and college matriculation, respectively, for DPS students and matched control students during the DPS reform years. Overall, DPS students had a higher probability of graduating during the reform years and the difference with the matched control students was statistically significant. Hispanic and English Learner students, as well as students who attended schools with FRL ranges of 0-25%, 50-75%, and 75-100% in DPS also had a higher probability of graduation than the matched control students. American Indian or Alaska Native and special education students, as well as students who attended schools with FRL ranges between 25-50%, in DPS had a lower and statistically significant difference in the probability of graduation than matched control students during the DPS reform years.

Table 7. Probability of high school graduation during DPS reform, overall and by subgroups

	DPS	Matched Control	Difference
Overall	0.790	0.777	0.013***
White	0.817	0.816	0.001
Hispanic	0.777	0.756	0.021***
Black	0.805	0.806	0.000
Asian	0.866	0.873	-0.007
American Indian or Alaska Native	0.673	0.764	-0.090*
English Learner	0.811	0.767	0.044***
Special Education	0.795	0.830	-0.035***
FRL Range 0%-25%	0.970	0.849	0.121***
FRL Range 25%-50%	0.718	0.786	-0.068***
FRL Range 50%-75%	0.787	0.704	0.082***
FRL Range 75%-100%	0.796	0.754	0.042***
* p<0.10, ** p <0.05, *** p < 0.01			

Overall, there was no statistically significant difference in the probability of college matriculation between DPS students and matched control students during the DPS reform years. There were statistically significant differences for White, Hispanic, Black, English Learner, and special education students, as well as by the FRL range of the schools students attended. Hispanic and English Learner students, as well as students who attended schools with FRL ranges between 50-75% and 75-100% in DPS had a higher probability of college matriculation, while White, Black, and Special Education students had a lower probability compared with matched control students during the DPS reform years.

Table 8. Probability of college matriculation during DPS reform, overall and by subgroups

	DPS	Matched Control	Difference
Overall	0.604	0.604	0.000
White	0.608	0.633	-0.025
Hispanic	0.577	0.562	0.015*
Black	0.677	0.704	-0.027*
Asian	0.742	0.750	-0.007
American Indian or Alaska Native	0.485	0.568	-0.083
English Learner	0.629	0.608	0.021**
Special Education	0.584	0.619	-0.035*
FRL Range 0%-25%	0.627	0.677	-0.050
FRL Range 25%-50%	0.611	0.959	-0.348
FRL Range 50%-75%	0.617	0.534	0.083***
FRL Range 75%-100%	0.581	0.544	0.037***
* p<0.10, ** p <0.05, *** p < 0.01			

Intervention Results

The first DPS reform intervention that we examine is the impact of attending a new school opened during the reform years on student academic achievement. For the intervention models, we were not able to compare DPS students to the matched control students because the control districts did not implement similar portfolio reform interventions. As a result, we used student fixed effects models and compared the academic achievement of students when they experienced the DPS reform intervention to when they did not experience the intervention. For example, the new school intervention effect compares the achievement of DPS students in new schools to the same students' achievement before they attended the new DPS school.

The overall effect of attending a new school opened during the DPS reform years was positive and statistically significant in both math and ELA performance (0.068 and 0.030 standard deviations, respectively). The results were also positive and statistically significant for all subgroups, except for Black students where the results were statistically insignificant in math and negative in ELA and American Indian or Alaska Native students where the results were not

statistically significant indicating that the students performed the same in new schools as they had performed prior to attending the new school (see Table 9). Also, attending a new school during DPS reform was positive in math and ELA as early as the first year it was in operation (0.035 and 0.017 standard deviations, respectively) and the effects grew larger as the new schools were in operation longer.

Table 9. Student fixed effects estimates for attending a new DPS school, overall and by subgroups

	Math	ELA	Treatment N
Overall effect of attending a new DPS school	0.068***	0.030***	7,005
	(0.005)	(0.005)	
Overall effect of attending a new DPS school, new school's first year	0.035***	0.017**	4,346
	(0.007)	(0.007)	
Overall effect of attending a new DPS school, new school's second year	0.062***	0.032***	4,411
	(0.007)	(0.007)	
Overall effect of attending a new DPS school, new school's third year	0.095***	0.036***	4,006
	(0.008)	(0.007)	
Overall effect of attending a new DPS school, new school's fourth year	0.101***	0.013***	1,979
	(0.011)	(0.010)	
Overall effect of attending a new DPS school, new school's fifth year	0.144***	0.068***	780
	(0.017)	(0.015)	
Effect of attending a new DPS school, White	0.118***	0.076***	827
	(0.014)	(0.013)	
Effect of attending a new DPS school, Hispanic	0.072***	0.033***	4,522
	(0.006)	(0.006)	
Effect of attending a new DPS school, Black	0.017	-0.019	1,375
	(0.012)	(0.010)	
Effect of attending a new DPS school, Asian	0.163***	0.124***	192
	(0.032)	(0.029)	
Effect of attending a new DPS school, American Indian or Alaska Native	-0.012	0.030	89
	(0.046)	(0.042)	
Effect of attending a new DPS school, English Learner	0.093***	0.058***	3,661
	(0.007)	(0.006)	
Effect of attending a new DPS school, Special Education	0.089***	0.162***	979
	(0.014)	(0.012)	
Effect of attending a new DPS school, FRL Range 0%-25%	0.098***	0.185***	177
	(0.029)	(0.026)	
Effect of attending a new DPS school, FRL Range 25%-50%	0.109***	0.047**	409
	(0.023)	(0.021)	
Effect of attending a new DPS school, FRL Range 50%-75%	0.081***	0.065***	1,282
	(0.012)	(0.011)	
Effect of attending a new DPS school, FRL Range 75%-100%	0.057***	0.015**	5,719
	(0.006)	(0.005)	
* p<0.10, ** p <0.05, *** p <0.01			

Our analysis of schools closed during the DPS reform years examines the academic performance of DPS students after they left schools closed in the district compared to the same students' performance when they were attending the schools that closed. During the DPS reform years included in this study, there were schools that the district closed through the federally defined turnaround strategy as well as other schools that were closed outside of turnaround. Our first set of analyses examines all schools closed during DPS reform. The results indicate that after leaving a closed school in DPS, students performed higher in math. The results were statistically insignificant in ELA suggesting that students performed the same after leaving a closed school (see Table 10). Moreover, students who left a closed school and attended a new school opened during DPS reform experienced a larger positive effect in math scores. There were no subgroups where the effect of leaving a closed school resulted in negative and statistically significant results. There were subgroups where the effect of leaving the closed schools was positive, including White, Hispanic, English Learner, and special education students, as well as students who attended schools where the FRL range was between 0% and 25%, between 50% and 75%. and between 75% and 100%.

Table 10. Student fixed effects estimates for leaving a closed DPS school, overall and by subgroups

	Math	ELA	Treatment N
Overall effect of leaving a closed DPS school	0.081*	0.026*	493
	(0.019)	(0.017)	
Overall effect of leaving a closed DPS school for a new school	0.094***	0.028	360
	(0.022)	(0.020)	
Effect of leaving a closed DPS school, White	0.222***	0.110**	60
	(0.061)	(0.054)	
Effect of leaving a closed DPS school, Hispanic	0.059***	0.013	345
	(0.023)	(0.020)	
Effect of leaving a closed DPS school, Black	0.045	0.027	75
	(0.051)	(0.046)	
Effect of leaving a closed DPS school, Asian	--	--	<20
Effect of leaving a closed DPS school, American Indian or Alaska Native	--	--	<20
Effect of leaving a closed DPS school, English Learner	0.136***	0.071***	234
	(0.026)	(0.024)	
Effect of leaving a closed DPS school, Special Education	0.256***	0.245***	74
	(0.046)	(0.043)	
Effect of leaving a closed DPS school, FRL Range 0%-25%	0.169	0.167	30
	(0.075)	(0.070)	
Effect of leaving a closed DPS school, FRL Range 25%-50%	0.003	0.094	29
	(0.078)	(0.072)	
Effect of leaving a closed DPS school, FRL Range 50%-75%	0.103**	0.171***	81
	(0.047)	(0.043)	
Effect of leaving a closed DPS school, FRL Range 75%-100%	0.121***	0.042**	417
	(0.020)	(0.018)	
* p<0.10, ** p <0.05, *** p <0.01			

When we examine the impact for students who left schools that were closed in DPS because of a turnaround decision, the overall effect was positive and statistically significant in both math and ELA (see Table 11). The results were similarly larger for students who subsequently enrolled in a new school opened during DPS reform. Additionally, the effects of leaving a closed school due to a turnaround decision was positive and statistically significant for Hispanic, English Learner, and Special Education students, as well as students who attended schools where the FRL range was between 50% and 75% and between 75% and 100%.

Table 11. Student fixed effects estimates for leaving a DPS school closed for a turnaround decision, overall and by subgroups

	Math	ELA	Treatment N
Overall effect of leaving a DPS school closed for turnaround	0.098**	0.073***	140
	(0.034)	(0.030)	
Overall effect of leaving a DPS school closed for turnaround for a new school	0.112***	0.089**	92
	(0.039)	(0.035)	
Effect of leaving a DPS school closed for turnaround, White	--	--	<20
Effect of leaving a DPS school closed for turnaround, Hispanic	0.119**	0.081**	81
	(0.042)	(0.038)	
Effect of leaving a DPS school closed for turnaround, Black	0.053	0.069	55
	(0.059)	(0.053)	
Effect of leaving a DPS school closed for turnaround, Asian	--	--	<20
Effect of leaving a DPS school closed for turnaround, American Indian or Alaska Native	--	--	<20
Effect of leaving a DPS school closed for turnaround, English Learner	0.175***	0.083***	69
	(0.044)	(0.041)	
Effect of leaving a DPS school closed for turnaround, Special Education	0.328***	0.314***	36
	(0.066)	(0.061)	
Effect of leaving a DPS school closed for turnaround, FRL Range 0%-25%	--	--	<20
Effect of leaving a DPS school closed for turnaround, FRL Range 25%-50%	--	--	<20
Effect of leaving a DPS school closed for turnaround, FRL Range 50%-75%	0.172**	0.189*	32
	(0.077)	(0.102)	
Effect of leaving a DPS school closed for turnaround, FRL Range 75%-100%	0.153***	0.320***	120
	(0.035)	(0.120)	
* p<0.05, ** p <0.01, *** p <0.001			

Our turnaround intervention analyses examine student achievement during the years the school was identified for turnaround comparing performance for the same students in the years prior to turnaround identification (see Table 12). DPS utilized the four federally defined turnaround options: turnaround, restart, closure, and transformation. For the closure turnaround strategy, there were schools that remained open for one or more years with the turnaround designation of closure. We examine student performance in the years that the school was identified for turnaround closure before it closed. Contrary to the new school and closure intervention results, turnaround strategies during the DPS reform years had an overall negative and statistically significant effect on students who remained in the turnaround schools (-0.092 and -0.055 standard deviations in math and ELA, respectively). When we examine the separate effects of the four turnaround options, students in schools using turnaround, restart, and transformation strategies had negative math and ELA performance compared to their performance prior to the school being identified. However, students in schools identified for turnaround closure performed similar in both math and ELA compared to the years before the school was identified for closure. The overall turnaround results were negative and statistically significant for White, Hispanic, Black, American Indian, or Alaska Native, English Learner students. The overall turnaround results were negative in math and positive in ELA for special education students.

Table 12. Student fixed effects estimates for attending a DPS school identified for turnaround, overall and by subgroups

	Math	ELA	Treatment N
Overall effect of attending a school identified for turnaround	-0.092***	-0.055***	3,821
	(0.007)	(0.006)	
Overall effect of attending a school identified for turnaround, turnaround	-0.104***	-0.103***	1,391
	(0.011)	(0.010)	
Overall effect of attending a school identified for turnaround, restart	-0.095***	-0.046***	1,115
	(0.012)	(0.011)	
Overall effect of attending a school identified for turnaround, closure	-0.021	-0.027	452
	(0.020)	(0.019)	
Overall effect of attending a school identified for turnaround, transformation	-0.069***	-0.016	1,097
	(0.012)	(0.011)	
Effect of attending a school identified for turnaround, White	-0.062**	-0.099***	220
	(0.028)	(0.025)	
Effect of attending a school identified for turnaround, Hispanic	-0.093***	-0.055***	2,990
	(0.008)	(0.007)	
Effect of attending a school identified for turnaround, Black	-0.094***	-0.044***	514
	(0.019)	(0.017)	
Effect of attending a school identified for turnaround, Asian	-0.064	0.007	64
	(0.051)	(0.046)	
Effect of attending a school identified for turnaround, American Indian or Alaska Native	-0.266***	-0.063	33
	(0.073)	(0.066)	
Effect of attending a school identified for turnaround, English Learner	-0.081***	-0.033***	2,216
	(0.009)	(0.008)	
Effect of attending a school identified for turnaround, Special Education	-0.079***	0.081***	568
	(0.017)	(0.016)	
Effect of attending a school identified for turnaround, FRL Range 0%-25%	--	--	<20
Effect of attending a school identified for turnaround, FRL Range 25%-50%	--	--	<20
Effect of attending a school identified for turnaround, FRL Range 50%-75%	-0.109***	0.002	168
	(0.035)	(0.031)	
Effect of attending a school identified for turnaround, FRL Range 75%-100%	-0.096***	-0.058***	3,781
	(0.007)	(0.006)	
* p<0.05, ** p <0.01, *** p <0.001			

10 SUMMARY AND CONCLUSIONS

Denver Public Schools' implementation of the portfolio district strategy was one of the most comprehensive efforts to redesign the delivery of public education in U.S. history. The reforms fundamentally altered the educational landscape in Denver and the allocation of power and opportunity in the city. They also serve as an example to others of what is possible. As a result, the portfolio strategy and Denver's implementation of it remain highly controversial locally and nationally (Meltzer, 2023).

Ultimately, the test of any effort to expand and improve educational opportunity is whether it improves student learning. Claims about whether the reforms in Denver helped or harmed students should be based on empirical evidence.

This study provides new evidence, based on student-level data, that Denver's reforms had a large positive effect on academic achievement for students who were enrolled in the district for at least two years before the reforms began and at least two years during implementation. We observe more than 20,000 individual students gain, on average, between 0.175 and 0.978 standard deviations improvement in ELA academic performance over the 7 years of reform in this study, meaning that the average DPS student received the equivalent of at least an additional six months and as much as an additional 27 months of schooling compared to their matched peers in surrounding districts (Baird & Pane, 2019, Evans & Huan, 2019, Hanushek, Woessmann, & Peterson, 2012, Bloom et al., 2008). The effect of DPS reform on math was statistically insignificant for cohorts 4 through 7, but positive and statistically significant for cohorts 8 and 9. These are the effects of the reforms for the average DPS student irrespective of whether they experienced any of the reforms directly. The overall systematic effects hold for nearly every subgroup, except for math for Native American students where the effects were negative and ELA for English Learner students where the effects were statistically insignificant.

In addition to this evidence of the effects of the reforms on achievement systemwide, this study provides new evidence about the impact of three of the district's primary reform tactics (new school creation, closure of persistently low-performing schools, and district-managed school turnaround, restart, closure, and transformation). We limit our analysis to students who directly experienced one or more of the DPS reforms. We find that among these three DPS reform tactics, students benefited from new school creation and the closure of low-performing schools, but not from district-managed turnaround, restart, or transformation. We find that there were positive effects for students who attended a new school created during the study period (an average of 0.068 and 0.030 standard deviations in math and ELA, respectively). We further find that there were positive effects for students after they left a school that was closed for performance during DPS reform (an average of 0.081 standard deviations in math). The effects were even larger for students who attended a school that was closed as part of a larger turnaround initiative and still larger for students who left a closed school and then attended a new school opened during DPS reform. Again, these effects hold for almost every subgroup. While the overall system-level effects were statistically insignificant in ELA for English Learner students, the DPS intervention results were positive in both math and ELA for English Learner students who experienced the specific portfolio intervention strategies of attending new schools and leaving closed schools during reform.

Improvement in learning at-scale, particularly in large school systems with high levels of need, is rare. The traditional unitary district model was built for stability and is notoriously resistant to change. Despite the expansion of a variety of alternatives over the last three decades, efforts to reinvent the unitary model are also rare (Bulkley et al., 2021, Hill, 2006, Boyd, 2003).

Denver's implementation of the portfolio district strategy creates an opportunity to evaluate whether it is possible to improve public education at-scale through an alternative paradigm with different operating assumptions, namely choice for families among multiple providers within a common market for publicly funded schooling that is governed and regulated for quality and equity. Denver's use of an annual, coordinated strategy of needs assessment, performance evaluation, intervention, and new school creation also allows us to examine whether it possible to improve student achievement by evaluating the performance of individual schools, intervening in schools that are persistently low-performing, and creating new schools to replace those that are closed.

This study, together with our earlier system-level research, provides rigorous, empirical evidence that Denver's reforms dramatically improved student achievement in the city, including for students who directly experienced the district's most controversial reforms. Not only were the reforms among the most comprehensive in U.S. history, they were also among the most effective.

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Appendix

Table A1. Number of DPS students in sample in the DPS reform interventions, by year

Year	Total DPS Students in Sample	New School	School in Year It Closes	School in Year It Closes by DPS Board Decision	School in Turnaround	School in Turnaround			
						Turnaround	Restart	Closure	Transformation
2008-09	26,265	976	115	0	147	0	0	0	147
2009-10	27,230	1,563	51	43	584	0	201	248	135
2010-11	21,852	2,583	116	0	1,382	316	971	95	0
2011-12	17,135	3,303	608	105	1,840	709	502	0	629
2012-13	12,565	4,278	193	0	1,819	1,218	131	0	470
2013-14	8,306	3,293	77	0	1,439	985	0	0	454
2014-15	4,234	1,899	56	0	674	450	0	0	224

Table A2. Attrition by DPS and control district students

	DPS	Matched Control	Persist-Attrit, DID
Grades 3-8 sample			
8th grade persistence rate	69.5%	68.9%	
Math			
3rd-grade z-score, persist	-0.012	-0.047	
3rd-grade z-score, attrit	-0.176	-0.173	
3rd-grade z-score, persist-attrit	-0.164***	-0.126***	-0.038***
ELA			
3rd-grade z-score, persist	-0.050	-0.063	
3rd-grade z-score, attrit	-0.209	-0.204	
3rd-grade z-score, persist-attrit	-0.158***	-0.141***	-0.018
Grades 8-12 sample			
12th grade persistence rate	55.9%	59.7%	
Math			
8th-grade z-score, persist	-0.073	-0.196	
8th-grade z-score, attrit	-0.206	-0.326	
8th-grade z-score, persist-attrit	-0.134***	-0.131***	-0.003
ELA			
8th-grade z-score, persist	0.004	-0.095	
8th-grade z-score, attrit	-0.134	-0.195	
8th-grade z-score, persist-attrit	-0.139***	-0.100***	-0.039***
* p<0.10, ** p <0.05, *** p <0.01			

Table A3. Difference-in-differences overall and subgroup estimates for DPS reform, by year

	2008-09			2009-10			2010-11			2011-12			2012-13			2013-14			2014-15		
	Math	ELA	Tx N	Math	ELA	Tx N	Math	ELA	Tx N	Math	ELA	Tx N	Math	ELA	Tx N	Math	ELA	Tx N	Math	ELA	Tx N
Overall	-0.000	0.059**	19,197	0.038	0.116***	19,849	0.062*	0.115***	20,880	0.079*	0.181***	16,391	0.134**	0.197***	11,972	0.138**	0.191***	7,901	0.226***	0.293***	3,921
	(0.022)	(0.020)		(0.034)	(0.030)		(0.033)	(0.032)		(0.044)	(0.047)		(0.051)	(0.053)		(0.050)	(0.058)		(0.049)	(0.086)	
White	-0.009	0.070***	3,647	-0.017	0.120***	3,895	-0.023	0.152***	4,275	-0.020	0.217***	3,379	0.017	0.252***	2,433	0.056	0.280***	1,636	0.120	0.446***	842
	(0.024)	(0.005)		(0.054)	(0.012)		(0.066)	(0.014)		(0.078)	(0.020)		(0.093)	(0.018)		(0.101)	(0.023)		(0.070)	(0.057)	
Hispanic	0.014	0.064**	11,587	0.059	0.117**	11,922	0.099**	0.103*	12,421	0.117*	0.167**	9,742	0.175**	0.171*	7,167	0.171**	0.158*	4,769	0.275***	0.259**	2,363
	(0.026)	(0.026)		(0.042)	(0.042)		(0.040)	(0.050)		(0.054)	(0.070)		(0.057)	(0.080)		(0.055)	(0.084)		(0.062)	(0.116)	
Black	-0.033	0.027*	3,132	0.036	0.105***	3,140	0.031	0.121***	3,180	0.058	0.179***	2,461	0.128***	0.221***	1,786	0.129***	0.201***	1,096	0.164**	0.187***	516
	(0.031)	(0.013)		(0.038)	(0.018)		(0.024)	(0.017)		(0.036)	(0.022)		(0.033)	(0.024)		(0.027)	(0.023)		(0.067)	(0.036)	
Asian	-0.030	0.045	649	-0.011	0.058	694	0.037	0.070	771	0.125**	0.190***	628	0.190***	0.215***	454	0.184***	0.171***	314	0.316***	0.287***	162
	(0.030)	(0.039)		(0.038)	(0.033)		(0.038)	(0.045)		(0.043)	(0.037)		(0.056)	(0.036)		(0.057)	(0.040)		(0.067)	(0.064)	
American Indian or Alaska Native	-0.152***	-0.014	182	-0.169***	0.169**	198	-0.127**	0.086	233	-0.233***	0.141**	181	-0.135***	0.105	132	-0.153**	0.122	86	-0.209*	0.411***	38
	(0.040)	(0.023)		(0.029)	(0.054)		(0.047)	(0.058)		(0.066)	(0.063)		(0.043)	(0.076)		(0.050)	(0.091)		(0.101)	(0.112)	
English Learner	0.039	-0.008	8,400	0.072*	-0.002	8,904	0.117**	-0.039	9,639	0.137**	-0.038	7,685	0.154***	-0.041	5,744	0.127*	-0.074	4,075	0.138**	0.360***	2,070
	(0.022)	(0.012)		(0.033)	(0.027)		(0.038)	(0.036)		(0.045)	(0.051)		(0.049)	(0.058)		(0.061)	(0.058)		(0.049)	(0.088)	
Special Education	-0.061**	0.041**	2,313	0.109***	0.126***	2,296	0.081**	0.088**	2,329	0.055	0.107*	1,822	0.072*	0.086	1,396	0.030	0.138*	910	0.104*	0.126	441
	(0.023)	(0.014)		(0.021)	(0.029)		(0.025)	(0.031)		(0.039)	(0.055)		(0.035)	(0.061)		(0.030)	(0.068)		(0.050)	(0.105)	
FRL Range 0%-25%	0.005	0.046***	1,412	-0.017	0.095***	1,415	-0.033	0.095***	1,444	-0.019	0.099***	983	0.024	0.231***	601	0.080	0.255***	445	0.303***	0.448***	319
	(0.027)	(0.012)		(0.052)	(0.014)		(0.061)	(0.017)		(0.080)	(0.022)		(0.093)	(0.022)		(0.095)	(0.034)		(0.048)	(0.081)	
FRL Range 25%-50%	-0.033	0.055***	3,801	0.007	0.123***	3,137	0.007	0.136***	2,342	-0.015	0.211***	2,046	0.039	0.218***	1,293	0.024	0.199***	447	-0.065	0.160***	197
	(0.025)	(0.012)		(0.032)	(0.019)		(0.028)	(0.018)		(0.036)	(0.030)		(0.057)	(0.037)		(0.076)	(0.025)		(0.083)	(0.048)	
FRL Range 50%-75%	-0.033*	0.028*	3,808	-0.027	0.046	3,034	0.028	0.112***	3,485	0.057	0.154**	3,046	0.083*	0.238**	2,426	0.152**	0.234**	2,147	0.072	0.249**	1,082
	(0.017)	(0.016)		(0.028)	(0.029)		(0.023)	(0.021)		(0.032)	(0.051)		(0.044)	(0.086)		(0.066)	(0.076)		(0.074)	(0.086)	
FRL Range 75%-100%	0.006	0.039	10,176	0.059	0.100*	12,261	0.082	0.062	13,598	0.114	0.133	10,316	0.188**	0.109	7,652	0.174**	0.087	4,862	0.328***	0.202	2,317
	(0.040)	(0.027)		(0.066)	(0.055)		(0.064)	(0.063)		(0.082)	(0.079)		(0.071)	(0.077)		(0.063)	(0.085)		(0.090)	(0.131)	

*p<0.10, **p<0.05, ***p<0.01

Table A4. Difference-in-differences overall and subgroup estimates for DPS reform, by cohort

	Cohort 4			Cohort 5			Cohort 6			Cohort 7			Cohort 8			Cohort 9		
	Math	ELA	Tx N	Math	ELA	Tx N	Math	ELA	Tx N	Math	ELA	Tx N	Math	ELA	Tx N	Math	ELA	Tx N
Overall	0.038	0.175***	3,045	0.100	0.290***	3,008	0.179	0.471***	3,443	0.313	0.668***	3,546	0.452**	0.800***	3,638	0.640**	0.978***	3,921
	(0.055)	(0.049)		(0.086)	(0.082)		(0.130)	(0.128)		(0.176)	(0.180)		(0.203)	(0.219)		(0.216)	(0.273)	
White	-0.026	0.190***	558	-0.049	0.341***	552	-0.069	0.559***	698	-0.052	0.810***	661	0.013	1.020***	713	0.151	1.346***	842
	(0.077)	(0.016)		(0.143)	(0.030)		(0.221)	(0.049)		(0.313)	(0.067)		(0.391)	(0.085)		(0.408)	(0.129)	
Hispanic	0.073	0.181**	1,857	0.172	0.284**	1,865	0.289*	0.451**	2,060	0.464*	0.622**	2,146	0.621**	0.717**	2,225	0.837***	0.858**	2,363
	(0.068)	(0.066)		(0.107)	(0.116)		(0.159)	(0.185)		(0.211)	(0.263)		(0.236)	(0.323)		(0.253)	(0.396)	
Black	0.004	0.132***	506	0.035	0.252***	461	0.093	0.431***	514	0.220	0.653***	585	0.382**	0.827***	521	0.509**	0.909***	516
	(0.068)	(0.031)		(0.091)	(0.038)		(0.126)	(0.057)		(0.157)	(0.072)		(0.382)	(0.084)		(0.177)	(0.098)	
Asian	-0.042	0.103	99	-0.005	0.173	102	0.120	0.363**	137	0.310	0.577***	124	0.525**	0.703***	139	0.852***	0.932***	162
	(0.066)	(0.070)		(0.097)	(0.114)		(0.137)	(0.150)		(0.189)	(0.181)		(0.217)	(0.184)		(0.227)	(0.212)	
American Indian or Alaska Native	-0.321***	0.155*	25	-0.448***	0.241**	28	-0.681***	0.382**	34	-0.815***	0.487**	30	-0.817***	0.624**	40	-0.857***	0.866**	38
	(0.055)	(0.071)		(0.079)	(0.095)		(0.133)	(0.140)		(0.143)	(0.192)		(0.169)	(0.223)		(0.224)	(0.296)	
English Learner	0.111*	-0.010	1,265	0.228**	-0.048	1,374	0.365**	-0.087	1,566	0.519**	-0.128	1,675	0.607**	-0.194	1,879	0.699**	-0.307	2,070
	(0.054)	(0.037)		(0.092)	(0.073)		(0.136)	(0.123)		(0.182)	(0.181)		(0.217)	(0.229)		(0.230)	(0.289)	
Special Education	0.137***	0.138***	351	0.218***	0.225***	314	0.273**	0.332**	320	0.345**	0.418**	408	0.347**	0.544**	387	0.341*	0.544	441
	(0.040)	(0.036)		(0.057)	(0.066)		(0.095)	(0.119)		(0.127)	(0.177)		(0.136)	(0.236)		(0.164)	(0.310)	
FRL Range 0%-25%	-0.012	0.141***	143	-0.045	0.236***	142	-0.065	0.335***	179	-0.041	0.566***	166	0.034	0.774***	144	0.355	1.128***	319
	(0.078)	(0.025)		(0.139)	(0.042)		(0.219)	(0.064)		(0.311)	(0.085)		(0.379)	(0.106)		(0.373)	(0.168)	
FRL Range 25%-50%	-0.026	0.177***	462	-0.019	0.314***	265	-0.034	0.524***	479	0.004	0.742***	426	0.062	0.887***	210	-0.010	0.924***	197
	(0.056)	(0.028)		(0.082)	(0.041)		(0.115)	(0.069)		(0.166)	(0.099)		(0.216)	(0.119)		(0.272)	(0.148)	
FRL Range 50%-75%	-0.060	0.074	644	-0.031	0.186**	677	0.025	0.340**	660	0.108	0.577***	760	0.293	0.783***	1,062	0.392*	0.986***	1,082
	(0.042)	(0.043)		(0.058)	(0.063)		(0.088)	(0.112)		(0.126)	(0.185)		(0.178)	(0.244)		(0.214)	(0.298)	
FRL Range 75%-100%	0.066	0.138	1,794	0.148	0.200	1,924	0.263	0.333	2,125	0.450	0.442	2,194	0.618*	0.491	2,222	0.886*	0.594	2,317
	(0.106)	(0.080)		(0.169)	(0.143)		(0.251)	(0.222)		(0.316)	(0.298)		(0.331)	(0.354)		(0.354)	(0.429)	

*p<0.10, **p<0.05, ***p<0.01