# Leading Indicators of Long-Term Success in Community Schools: Evidence from New York City

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## ABSTRACT

Community schools offer supports such as health and social services, extended school days, and family education, to improve the performance of students whose learning may be disrupted by challenges related to poverty. In 2015, the New York City Community Schools Initiative was implemented in conjunction with the NYC Renewal Schools program to turnaround the city's lowest performing schools. Using a novel method that combines multiple rating regression discontinuity design with machine learning techniques, we estimate the causal effect of this effort on elementary and middle school student attendance and academic achievement. We find an immediate reduction in chronic absenteeism of 5.6 percentage points, which persists over the following three years. We also find large improvements in math and ELA test scores – an increase of 0.26 and 0.16 standard deviations by the third year after implementation – although these effects took longer to manifest than the effects on attendance. Our findings suggest that improved attendance is a leading indicator of success of this turnaround model and may be followed by longer-run improvements in academic achievement.

## INTRODUCTION

Students from impoverished backgrounds face a myriad of challenges that disrupt their ability to be successful in school and contribute to persistent socioeconomic inequities in educational outcomes. Community schools seek to alleviate these challenges by partnering with community based organizations (CBOs) to better meet the needs that are prerequisite to student academic success.

Although community schools have existed in some capacity since the turn of the 20th century, the model has seen a resurgence in recent decades. In 2016, the Coalition for Community Schools estimated that community schools serve over 5 million students across 5,000 schools in the United States and this number is expected to grow. Notably, the U.S. Department of Education recently announced updated and expanded grant support for full-service community schools (FSCS), suggesting that community schools will continue to expand (U.S. Department of Education, 2022). Our study makes a timely contribution to this context by providing the first rigorous causal evidence of community school programs brought to scale. We study the largest system of community schools in the country – the New York City Community Schools Initiative (NYC-CS) – which was largely implemented in tandem with the NYC Renewal Schools (RS) program -- to turnaround the city's lowest performing schools. To do so, we combine machine learning (ML) techniques with a regression discontinuity design (RDD) to estimate the effects of NYC-CS/RS on elementary and middle school student outcomes. Across the four years that we observe (2015/2016 through 2018/2019), we find a reduction in chronic absenteeism as large as 11.4 percentage points, and improvements in math and ELA test scores as large as 0.26 standard deviations (SD) and 0.16 SD, respectively.

Prior research on the effects of community schools on student academic achievement finds mixed null and positive results. In a comprehensive review of 143 studies, Maier et al. (2017) conclude that there is sufficient evidence that community schools improve student outcomes to justify expansion of the model. In a separate report, Moore et al. (2017) review 19 experimental or quasi-experimental studies of integrated student supports and find that all yield positive or null results.[2] One prior evaluation of the first three years of implementation of the NYC-CS program completed by researchers at the RAND Corporation found null effects on reading achievement and a positive effect on math achievement only in the third year of implementation (Johnston et al., 2020). Given the wraparound nature of the community schools model, researchers are interested not only in student academic outcomes but also in other, more holistic indicators of success which also tend to show positive impacts on school climate, student engagement, and behavior.[3]

2 For example, Evaluations of Boston's City Connects program found higher report card grades and improved middle school math and ELA test scores across all students (Walsh et al., 2014) and narrowed achievement gaps for immigrant English Language Learners (Dearing et al., 2016). Randomized control trials of the Communities in Schools program in Austin, TX, Wichita, KS, and Jacksonville, FL found some positive impacts on math and reading test scores, but the results were inconsistent across study sites (ICF International, 2010a, 2010b, 2010c). A study of two FSCS in Iowa found some improvements in math and English grades, but no evidence of improved test scores (LaFrance Associates, 2005). Finally, evaluations of the Tulsa Area Community Schools Initiative found overall null effects of the program on test scores, but effects appeared to vary by level of implementation and the long-term implementation of the Tulsa model was disrupted by other aspects of the district context (Adams, 2010; Adams, 2019).

3 For example, community schools have been found to improve access to services for families while improving family engagement and reducing family stressors (Arimura & Corter, 2010; Olson, 2014); improve school climate and adult-student relationships (Olson, 2014; LaFrance Associates, 2005; Johnston et al., 2020); and improve student attendance or reduce chronic absenteeism (Dobbie & Fryer, 2011; ICF International, 2010b; Kemple, Herilhy, & Smith, 2005; Arimura & Corter, 2010; Olson, 2014; Johnston et al., 2020). The evidence on disciplinary and behavioral outcomes is less conclusive, with some studies finding a reduction in behavioral issues (Dearing et al., 2016; Walsh et al., 2014; Dobbie & Fryer, 2011; Johnston et al., 2020) and others finding null effects (ICF International, 2010a, 2010b, 2010c).

With the 2015 passage of the Every Student Succeeds Act, states were granted substantially more flexibility in their approach to turning around low-performing schools than ever before, as long as their approach was supported by an evidentiary basis (Duff & Wohlstetter, 2019). The growing body of evidence supporting FSCS along with this increased flexibility created an opportunity for community schools models to be implemented as a turnaround strategy. This is quite different from NCLB-era school turnaround strategies, which largely focused on disruptive practices such as staffing changes, charter or state takeover, or school closure, which in some cases were found to be effective (for recent reviews of the turnaround literature, see Henry & Harbatkin, 2020, and Redding & Nguyen, 2020). However, other evidence suggests that obtaining buy-in from diverse stakeholders, especially community members, is essential to fostering an environment in which comprehensive changes can be made (Schueler, 2019; Glazer & Egan, 2018). Because school turnaround is quite labor intensive, researchers have also found that effective reforms require more than just school staff to develop and implement the organizational infrastructure necessary to do the work (Henry et al., 2018).

Overall, this research suggests that the NYC-CS/RS program has the potential to positively impact students and their families by implementing community schools programming to turnaround the city's lowest performing schools in a more holistic approach than other common turnaround models. However, the current body of evidence comes from evaluations of a small number of schools, and there is much less evidence on the effects of bringing this model to scale as a turnaround strategy. Though some previous studies do implement experimental or quasi-experimental designs on small samples of schools, our context and methodology allow us to bring rigorous causal inference to the largest community schools program in the country. While many rigorous impact studies are only able to study educational interventions under ideal circumstances, the NYC-CS/RS program presents a rare opportunity to study the causal effects of such an intervention as actually implemented at scale in a large urban district. One prior study of NYC-CS was conducted by researchers at the RAND Corporation, using a difference-in-differences with a matched control group (Johnston et al., 2020). We build upon this prior work in terms of both methods and data. We implement a novel methodology that combines ML techniques with a fuzzy multiple rating regression discontinuity design (MRRDD), which eliminates the need for parallel trends assumption of the difference-in-differences method. Furthermore, we examine annual effects over a longer period than the previous study, which provides further insight into how NYC-CS/RS impacts students as the program matures and as students experience increased exposure to the model over time.

We find that NYC-CS/RS led to an immediate reduction in chronic absenteeism among elementary and middle school students of 5.6 percentage points in the first year of implementation (2015/2016) which persisted at 7.6, 11.4, and 8.0 percentage points across the following three school years. Consistent with a reduction in chronic absenteeism, we find improvements in overall attendance rates of approximately 1 to 2 percentage points across all years. Improvements in academic outcomes, as measured by school-level standardized test scores, are quite large but take longer to manifest. In the first year of implementation (2015/2016), we find point estimates on academic achievement that are positive but not statistically significant. In the second year of implementation (2016/2017), we find a positive effect on math scores of 0.11 standard deviations and null effects on ELA (though the point estimate remains positive and increases in magnitude). In the third year (2017/2018), we find large positive effects on both math and ELA of 0.26 and 0.16 SD, respectively. And finally, in the fourth year (2018/2019), we find that these large effects on achievement level off, and we estimate moderately sized effects of 0.18 SD in math and 0.08 SD in ELA although the effect on ELA is statistically insignificant.[4]

Overall, our results suggest that implementing community schools programming as a turnaround strategy has the potential for large, positive impacts on elementary and middle school student attendance and academic achievement. Such large impact estimates warrant further investigation, and we therefore conduct supplementary analyses to explore heterogeneity by grade level and the grade span served by the school (K-5, K-8, and 6-8). These supplementary analyses show that effects on academic achievement are concentrated in the elementary grades and are consistently larger for math than ELA. We also find that effects on attendance are driven by early childhood and middle school-aged students. Additionally, grade-by-year analyses consistently show that effects grow over time, suggesting that long-term investment is critical to program success, and evaluations of these programs must be ongoing in order to capture the dynamic impact of the model over time.

In what follows, we first provide background on the development and implementation of community schools over the last century. We then provide details on the NYC-CS program and the RS program. After describing our data and measures, we describe our methodology and how it is aligned to the NYC-CS/RS setting. The next sections describe our main results and the results of our supplementary analyses of heterogeneous treatment effects. We discuss our results within the NYC-CS/RS policy context and the national policy context of an expanding

[4] To categorize effect sizes as large, moderate, or small, we apply the benchmarks proposed by Kraft (2020) in which less than 0.05 SD is considered small, between 0.05 SD and 0.20 SD is considered medium, and greater than 0.20 SD is considered large. Kraft's (2020) benchmarks are based on the distribution of 1,942 effect sizes from 747 randomized control trials that evaluated educational interventions and used standardized test scores as the outcomes.

community schools sector. Finally, we conclude with a summary of our contributions and recommendations for future evaluations of community school initiatives.

## BACKGROUND

In this section, we describe the origins of the community schools model and how it has developed across the country over the past century. We then describe the New York City Community Schools Initiative, New York City Department of Education's Theory of Change guiding the initiative, and the New York City Renewal Schools Program.

### Community Schools Model

A full-service community school (FSCS) is a school that offers a variety of non-traditional services in partnership with community based organizations (CBOs) in order to better meet the comprehensive needs of its students and the community in which the school is located. Though the specific design and implementation of each FSCS initiative is context dependent, most community schools share four core features that are foundational to the model: (1) integrated student supports, (2) expanded learning time and opportunities, (3) family and community engagement, and (4) collaborative leadership and practice (Maier et al., 2017). Integrated student supports ensure that mental and physical health services and other social services are available in schools to those who need them. Expanded learning time and opportunities may include an extended school day or year and additional opportunities for academic intervention and enrichment. Family and community engagement ensures that parent and community voice is included in decision-making. Finally, collaborative leadership and practices build a culture of shared responsibility and collective trust among leadership, teachers, and CBOs.

Schools first began utilizing this model at the turn of the 20th century when socioeconomic developments such as industrialization, urbanization, and immigration rapidly changed the needs of the urban poor. Schools became the institution that reformers turned to as the venue for offering health and social services and building shared values across diversifying communities. Over the past century, community school initiatives have come in waves, largely driven by social crises that increase the demand for such services. After the initial introduction of the model at the turn of the century, there was a resurgence in the 1930s in response to the Great Depression and again in the 1960s and 70s in response to desegregation (Maier et al., 2017). Beginning in 2008, the federal government started a FSCS grant competition to fund expansion of this model across the country. Since then, five more rounds of grants have been awarded, and the Department recently announced plans for a sixth round of funding including a national randomized control trial of grantees to evaluate program effectiveness (U.S. Department of Education, 2022).

**New York City Community Schools Initiative**

To date, the largest implementation of a community schools initiative is in New York City. The initial program was funded by an attendance-improvement and drop-out prevention grant but is now supported by a variety of sources including city, state, and federal funding (NYC Department of Education, n.d.). In 2014/2015, the first year of implementation, a cohort of 45 schools was gradually onboarded and built partnerships with lead CBOs (for this reason, we consider 2015/2016 to be the first year of full implementation for our analysis). For the 2015/2016 academic year, the Office of Community Schools (OCS) was established to centralize organization and support of the growing initiative. Since then, it has steadily expanded to now include over 300 schools across the city. Though the initiative includes schools serving students across grades K-12, due to the small number of high schools, we limit our analytic sample to schools serving elementary and middle school students.

There are six key components of the NYC-CS model that are present in every school: (1) partnerships with CBOs, (2) real-time data use, (3) family engagement, (4) attendance-improvement strategies, (5) extended learning time, and (6) health services. The NYC-CS Theory of change combines these six components with four core capacity domains: (1) continuous improvement, (2) coordination, (3) connectedness, and (4) collaboration (Johnston et al., 2017; Johnston et al., 2020; NYC Department of Education, n.d.). This approach moves beyond simply adding the components into schools and acknowledges the capacity building required to effectively implement the components and sustain the model in the long run. Figure 1 visually depicts our adaptation of the NYC-CS Theory of Change, which offers a streamlined version of the details provided in the program's Strategic Plan and demonstrates how the theory is aligned to the leading and lagging indicators analyzed in this study – attendance and test scores, respectively. The theory posits that OCS provides the resources, support, organization, and infrastructure for community schools, which lays the groundwork for the schools to simultaneously develop core capacities and implement the model. The theory suggests a feedback loop between capacities and components such that improved capacity will improve program implementation, which in turn might further develop capacity. In the short-run, these capacities and components  may increase student and family engagement and shared responsibility, which in the longer-run can lead to improved student and school outcomes on both academic and non-academic indicators.

There are several other features of NYC-CS in addition to the core components of the model (Johnston et al., 2020; Johnston et al., 2017; NYC Department of Education, n.d.). Each school has a Community School Director – a full-time staff member dedicated to assessing needs, securing resources, and ensuring targeted services are provided. All NYC-CS implement a three-tiered model of mental health programming in which Tier 1 offers preventative and universal services, Tier 2 offers early interventions for students identified at-risk, and Tier 3 offers targeted treatment for students with diagnosed mental health disorders. Each school is assigned a School Mental Health Manager who supports school staff with implementing the three-tiered model and monitors progress within their assigned schools. Though every NYC-CS is expected to implement all core components of the model, the Strategic Plan specifies sufficient flexibility for the components to be implemented in the best way to meet each community's unique needs. The ability for each school to customize their approach is central to this model, and therefore, it is best to think of NYC-CS as a strategy rather than a program (Johnston et al., 2017; NYC Department of Education, n.d.).[5]

*Renewal Schools*

During the implementation of NYC-CS, another school improvement initiative called the Renewal Schools (RS) program was also introduced. The RS program was first implemented in 2014 by Mayor Bill de Blasio to identify and turnaround the city's lowest performing schools within a three-year period. Key aspects of the RS program included professional development for staff, coaching for principals, increased oversight by superintendents, additional academic interventions, and extended learning time. Another key part of the RS program was an individualized plan to incorporate each RS school into the NYC-CS program (NYC Office of the Mayor, 2014), and in the 2015/2016 school year, all 94 RS were added to NYC-CS, providing

[5] Because implementation details vary by individual schools, we hesitate to assert more implementation details beyond the scope of the framework within which all NYC-CS have the flexibility to implement the six components. For more information on variation in implementation of NYC-CS, see Johnson et al., 2017.

them with all services offered to NYC-CS in addition to the academic and operational supports provided to RS (NYC Department of Education, n.d.). The RS program complements the flexible social, community, and health components of NYC-CS as a turnaround strategy in that it's focus is on improved academic achievement and increased oversight and accountability for academically underperforming schools (Johnston et al., 2017).

Because nearly all the initial cohort of community schools were also Renewal Schools and because the two programs' goals and approaches were similar, we do not attempt to disentangle the impact of the RS program from the NYC-CS program and instead consider them as working in conjunction as a turnaround initiative that offers community schools programming to all the city's lowest performing schools.[6] A prior study of the implementation of NYC-CS describes NYC-CS and RS as "a concurrent school-improvement initiative" (Johnston et al., 2017, p. xiii). The RS program was gradually phased out beginning in 2019, but all RS were allowed to keep their community school designations (Zimmerman, 2018). For this reason, we refer to our treatment as NYC-CS/RS.

Though our strategy for identifying the causal effect of this treatment is detailed in the Methods section below, we note here that there were other concurrent education reform policies implemented by Mayor DeBlasio over the study period that may have potentially impacted both treated and counterfactual schools in the neighborhood of the cutoff for NYC-CS/RS eligibility including universal Pre-K, expansion of after school programming, and a citywide mental health program called ThriveNYC (Johnston et al., 2017). However, because expanded learning opportunities and mental health programming are key components of the NYC-CS/RS treatment,

[6] As discussed more below, our identification strategy relies on the existence of thresholds to determine whether a school qualified to be a RS, which is another reason why we do not attempt to disentangle the two programs.

the extent to which these supports were also more widely available in control schools during the study period would only downwardly bias our estimates of the differences between NYC-CS/RS and untreated schools.

## DATA AND MEASURES

Data for this project is provided by the New York City Department of Education and is supplemented with publicly available information from NYC Open Data. Within the Department, OCS provided information for us to identify NYC-CS across all study years, as well as additional information about the resources provided to NYC-CS and the programming being implemented in these schools. They also provided information on the criteria that collectively determined whether a school was classified as a Renewal School or not, namely the proportion of students proficient in ELA and math in 2011/2012, 2012/2013, and 2013/2014 as well as a continuous "Beat the Odds" measure, which reflects the adjusted growth percentile values of students at the school in 2013/2014. Annual School Quality Reports are publicly available through NYC Open Data, which we use across years 2015/2016 through 2018/2019 for school-level measures of attendance and student demographics. Other publicly available reports from NYC Open Data provide attendance and average state assessment test scores by grade and year, which we use in combination with distributional information from NYSED annual technical reports to create standardized measures of academic performance.

### Outcome Measures

Academic achievement is measured by third through eighth grade math and ELA end-of-year state assessment test scores. Our analyses are conducted at the school-level, and therefore, we use publicly available information on the distributions of math and ELA scores from annual

NYSED technical reports to convert grade-by-school average scale scores from NYC Open Data into standard deviation units, both for all students within each school (weighted by grade-level enrollment), and by grade-level. As such, test-score outcomes are defined as the average (standardized) score by grade, year, and subject.

Our measures of attendance include chronic absenteeism and average daily attendance. Chronic absenteeism is defined as the proportion of students within a school who are absent for 10% or more of the school year. Average daily attendance is defined as the percentage of school days present for all students. In the same manner as our measures of academic achievement, data on student attendance and chronic absenteeism is averaged at the school level to accommodate our school-level analysis. Average daily attendance and chronic absenteeism by grade-level are only available in NYC Open Data for years 2016/2017 through 2018/2019, and therefore, our grade-level analysis of attendance omits the first treatment year (2015/2016).

### Student and School Characteristics

Annual School Quality Reports from NYC Open Data also include the demographic composition of each school including race and ethnicity, economic disadvantage, temporary housing, ELL, and SWD. We use these variables, along with lagged values of the outcomes and the selection criteria (described above), to improve precision of the estimates.

## METHODS

Our empirical approach combines a multidimensional regression discontinuity design, often referred to as a multiple rating regression discontinuity design (MRRDD), with a machine learning technique known as ridge regression to precisely estimate the causal effect of the NYC-CS/RS initiative at a multidimensional boundary. While others have similarly used regression discontinuity to evaluate school turnaround programs (e.g., Atchison, 2019; Dragoset et al., 2019; Henry & Harbatkin, 2020), combining this approach with machine learning allows us to improve the precision of the estimator. Our approach differs from the more commonly used techniques in causal machine learning (e.g., Wager & Athey, 2018; Chernozhukov et al., 2017; Athey, Tihshirani, & Wager, 2019; Hahn et al., 2018), in that these techniques rely on estimating how likely each unit is to be treated, i.e., the propensity score, which is not applicable in a regression discontinuity setting. In this section, we describe the theoretical motivation for MRRDD and how ridge regression can be used to improve the precision of the estimator. We then describe how this framework can be extended to a fuzzy MRRDD to accommodate imperfect compliance and how this methodology applies to our study context of NYC-CS. Finally, we address the additional considerations made in determining our preferred estimation strategy.

**Multiple Rating Regression Discontinuity Design with Machine Learning**

The MRRDD framework builds on Rubin's potential outcome notation and pre-supposes that school $i$ would have an outcome of $Y_i(1)$ if it is treated ($T_i = 1$) and an outcome of $Y_i(0)$ if it is not treated ($T_i = 0$). The casual effect of the treatment on school $i$ can then be defined as $\tau_i \equiv Y_i(1) - Y_i(0)$. The difficulty in estimating this effect is that we do not observe both $Y_i(1)$ and $Y_i(0)$, and instead only observe $Y_i = T_i \cdot Y_i(1) + (1 - T_i) \cdot Y_i(0)$.

In a sharp univariate RDD, there is a cutoff – denoted $Z_c$ – of some running variable which determines treatment, i.e., $T_i = 1$ if $Z_i < Z_c$ and $T_i = 0$ otherwise. Extending this traditional sharp univariate RDD to the multivariate case, that treatment is instead determined by $j$ running variables such that $T_i = 1$ if $Z_{i,j} < Z_{c,j}$ for all $j$ and $T_i = 0$ otherwise (i.e., if $Z_{i,j} >$

$Z_{c,j}$ for at least one $j$). While one could, in theory, estimate a MRRDD by extending the univariate RDD methods, e.g., local linear or non-parametric regressions, to multiple dimensions; due to the curse of dimensionality doing so is usually infeasible given the limited sample size around the boundary. This has led researchers to propose a variety of approaches one can use to estimate MRRDDs, each of which have advantages and disadvantages (e.g., Reardon & Robinson, 2012; Wong, Steiner, & Cook, 2013; Papay, Murnane, & Willett, 2011). Our approach is to combine the multidimensional vector of $j$ running variables into a single dimensional running variable by measuring the distance of each observation to the multidimensional threshold.

One advantage of this approach is that we can then treat the new variable – denoted as $M_i$ – as a running variable using techniques used in traditional univariate RDDs. Just as a unidimensional RDD estimates the average effect of schools at the cutoff, this approach to a multidimensional RDD gives the average effect of schools at the multidimensional boundary. We show this formally in the Technical Appendix, which is available online. This is true even when the boundary does not completely determine treatment and instead there is merely a discontinuous increase in the probability of treatment at the multidimensional boundary. As is commonly done in the unidimensional setting, we can use the boundary as an instrument for treatment status after collapsing the multidimensional vector into a single running variable.

A drawback to this approach is that collapsing multiple running variables into a single dimension involves discarding potentially valuable information about the value of each of the $j$ different measures that combine to determine treatment. With large sample sizes this is unimportant, but with smaller samples sizes it reduces the precision of the resulting treatment effect estimates. We therefore augment our MRRDD approach by using a ridge regression to initially residualize the outcome.

Our approach is theoretically motivated by two important facts. First, under the common assumptions required in an RD setting, we can replace the outcome ($Y_i$) with a residualized outcome $Y_i - g(X_i)$ for *any* continuous function $g$ and exogenous covariates $X_i$ and still obtain a consistent estimate of the average treatment effect on the compliers at the boundary. Second, the asymptotic variance of the resulting treatment effect estimates is proportional to the variance of $Y_i - g(X_i)$. We prove those two facts in the Technical Appendix. We will refer to $Y_i - g(X_i) + \overline{g(X_\iota)}$ as the regression-adjusted outcome. Note that by adding the average of the predictions – denoted $\overline{g(X_\iota)}$ – to the residuals shifts all schools' outcome in the same way and so has no effect on the estimates; we do so because it means that the regression-adjusted outcomes have the same overall outcome as the raw outcomes $Y_i$ and the conditional means are easier to interpret.

These twin facts are important, as they imply that the optimal choice of function $g$ is simply the one that best predicts $Y_i$ at the multidimensional boundary. This is precisely what ML techniques are designed to do, to optimize outcome prediction, and so we can use existing "off-the-shelf" ML techniques. In this paper, we use a ridge regression since it ensures that the resulting function $g(X_i)$ is continuous. Furthermore, it allows for all of the covariates to be associated with the outcome (rather than implicitly assuming a sparse model as is the case with a LASSO model), while not overfitting the model as might be the case if we used a traditional OLS regression. However, we get almost identical results when using a LASSO regression or an elastic net, and the resulting estimates hold for any value of the penalty term; this includes an infinite penalty term, which is equivalent to include no controls, and a zero penalty term, which is equivalent to estimating it via a traditional OLS regression. Furthermore, as we show in the appendix if we use a relatively large bandwidth to estimate the ridge regression, we can ignore uncertainty in the estimation of $g(X_i)$ when conducting inference on the resulting treatment effect estimates.

In short, our methodological approach consists of three steps:

1. Run a ridge regression of the outcome on a vector of exogenous covariates $X_i$ using a relatively large bandwidth.
2. Residualize the outcome, i.e., calculate $r_i = Y_i - \hat{g}(X_i) + \overline{\hat{g}(X_i)}$, for each observation.
3. Run a traditional univariate using the distance from the boundary as the running variable and the residual $r_i$ as the outcome.

**MRRDD in the NYC-CS/RS Context**

As noted above, in the first full year of implementation of NYC-CS (2015/2016), all schools designated as RS were added to the NYC-CS. RS are identified for additional academic and instructional interventions along nine criteria. The nine criteria include seven continuous test-score based criteria (the school falls in the bottom quartile of percent proficient in math and ELA across years 2012, 2013, and 2014; the school is not in the top quartile of student growth in 2014) and two categorical criteria (the school has a recent NYCDOE Quality Review rating below “Well Developed”; the school is designated as Focus or Priority by the NY State Department of Education). A school must meet all nine criteria to be designated RS, and the Chancellor has the discretion to add or remove schools from the list (to which he added four schools in the first year of implementation). Therefore, all schools identified by these criteria as RS are also included in NYC-CS, but there are other NYC-CS schools that do not meet these criteria and are therefore not part of the RS program.

Given that the two categorical criteria for RS are very coarse, it is not possible to determine the similarity of schools along those criteria any more precisely than whether they fall into the same category. Therefore, we limit our analysis to schools that meet these categorical conditions, and generate the multidimensional boundary based on the seven continuous test-score based criteria. Because all seven of the continuous selection criteria are expressed in NYC-wide percentiles, we combine them into a single dimension "binding score" by taking their maximum (Reardon & Robinson, 2012). For the ridge regression we include all schools within 25 percentiles of the nearest cut-off. For the univariate RD, we limit the analytic sample to a bandwidth of 10 percentile points. The precision gained by the ridge regression allows us to detect effects in these small samples that would not be detectable otherwise.

As noted above, all schools identified for the RS program are automatically included in the NYC-CS, but there are other NYC-CS that are not part of the RS program. Therefore, there is perfect compliance with treatment below the multidimensional boundary, but imperfect compliance above it, because some schools above the boundary do receive NYC-CS treatment. Figure 2 shows the probability of treatment within 20 percentile points of distance from the boundary and demonstrates that there is a 100% chance of treatment below the boundary and a small chance of treatment above the boundary that ranges from approximately 0 to 20%. For this reason, we implement a fuzzy MRRDD as described in the section above. We use distance from the boundary to predict the probability of being included in the treatment, and then regress outcomes on the predicted probabilities of treatment in the final stage of estimation. Table 1 shows the mean characteristics of schools above and below 20, 10, and 5 percentile points of the boundary in 2013/2014 (prior to treatment). As expected, schools above the boundary tend to have higher academic achievement and attendance, on average, but these gaps narrow as the bandwidth narrows towards the boundary.

**Additional Considerations**

Given that our methodology requires us to make multiple modeling decisions that may impact our estimates, this section describes the additional considerations made in determining our preferred estimation strategy.

*Optimal Bandwidth*

While it is possible to estimate the ridge regression and treatment effect in a single step, a benefit to running the ridge regression as an additional step is that different bandwidths can be chosen for the ridge regression and the RD. As outlined in the Technical Appendix, choosing a wider bandwidth for the ridge regression than for the RD is useful, as it allows us to ignore estimation error in the ridge regression when calculating the standard error of the resulting treatment effect estimates. Furthermore, since bias is less problematic in the ridge regression than for the RD it is likely that a wider bandwidth also improves the precision of the estimates, although we leave a more detailed analysis of the optimal bandwidth for the ridge regression to future work. Choosing a bandwidth of 25 percentile points for the ridge regression allows us to use more information to optimize predictions at the boundary, which lowers the residual variance within the RD bandwidth, thereby improving precision. To ensure that our RD results are indeed driven by schools that either barely qualified for or barely missed out on NYC-CS/RS, we limit our RD bandwidth to schools within a 10 percentile point distance from the boundary. Given that we have multiple observations per school (one for each year), the traditional equations for the optimal bandwidth do not apply. Because of this, we checked that our choice of bandwidth and kernel weights do not affect our results.

*Sloping Regression Lines*

A common approach in RDD is to allow the regression line on either side of the boundary to slope (and to allow the slope to vary on either side). Unlike most RDDs, we control for a range of covariates in the ridge regression (including those that comprise the multidimensional running variable), which minimizes the sensitivity of our predictions to the values on the x-axis. For this reason, we do not allow our regression lines to slope. We verify this decision through simulations which suggest that allowing the regression lines to slope causes an overfitting of the data. These simulations also confirm that our estimates are similar regardless of whether we allow the lines to slope, and we therefore retain our preferred model in which the lines do not slope.

## RESULTS

In this section we present the results from the analyses described in the previous section. We first present the average treatment effects on attendance and academic achievement overall and treatment effects by year. We then discuss heterogeneity by grade and grade span served by the school. Finally, we explore the extent to which heterogeneous effects by year are attributable to increased student exposure to the model over time versus program maturity.

### Main Results

*Attendance*

We find that NYC-CS/RS improved student attendance and lowered the rates of chronic absenteeism. The first row of column 1 of Table 2 shows an overall increase in student attendance of 1.61 percentage points in NYC-CS/RS as compared to schools that barely missed eligibility for the program. Panel A of Figure 3 plots this residualized discontinuity in attendance within our chosen bandwidth of 10 percentage points around the cut-off. It shows that treated schools have a regression-adjusted average attendance rate of just over 92%, while untreated schools have a regression-adjusted attendance rate of between 90 and 91%.

The second row of Table 2 shows effects on the rate of chronic absenteeism. Consistent with the effects on attendance rates, row 2 of column 1 shows an overall decrease in chronic absenteeism of 8.1 percentage points. Panel B of Figure 3 shows the residualized discontinuity in chronic absenteeism within 10 percentage points of the boundary, where the fitted lines represent the regression-adjusted average proportion of chronically absent students in treated versus untreated schools. After controlling for a range of covariates, between 27 and 28 percent of students in NYC-CS/RS schools were chronically absent over the years we observe, as compared to about 35 percent of students in control schools.

*Academic Achievement*

We find large effects of NYC-CS/RS on end-of-year math standardized test scores, as shown in row 3 of Table 2. We find an overall effect of 0.15 SD when pooling all grades and years. Panel C of Figure 3 shows the discontinuity in math scores for treated versus untreated schools within 10 percentile points of the cutoff. The regression-adjusted average performance of treated schools is about 0.44 SD below the mean for all NYC schools, while it is closer to 0.59 SD below the mean in the untreated schools.

While we do find positive impacts of NYC-CS/RS on end-of-year ELA standardized test scores as well, they are not as large in magnitude as the effects we find for math. ELA results are shown in row 4 of Table 2. We find an overall improvement of 0.08 SD, as shown in column 1. Panel D of Figure 3 plots the discontinuity in ELA scores at the treatment cutoff. After adjusting for a range of covariates, treated schools perform, on average, at about 0.36 SD below average for all schools in NYC, and untreated schools perform at about 0.44 SD below average.

**Changes in Effects over Time**

The pooled results reported in the previous section mask substantial heterogeneity by year. In this section, we present results demonstrating treatment effects across each of the four years included in our study (2015/2016 to 2018/2019) for both attendance and academic achievement, which provides insight into how program effects develop and evolve over time. Across all outcomes, effect sizes increase incrementally in the first three years of implementation, are largest in magnitude in the third year of implementation (2017/2018), and level off in the fourth year of implementation.

*Attendance*

Row 1 of Table 2 shows the annual effects of NYC-CS/RS on student attendance over the four years we observe across columns 2 through 5. The program had a positive impact on student attendance over all observed years and was largest in magnitude in the third year of implementation at 2.09 percentage points. These results translate to increased attendance in the range of 2 to 4 days per school year. Row 2 of Table 2 shows the effects of NYC-CS/RS on chronic absenteeism over all observed years across columns 2 through 5. Effects are consistent with those for attendance rates and show a consistently significant reduction in chronic absenteeism over all four years, which is largest in magnitude in the third year of implementation at 11.4 percentage points.

*Academic Achievement*

Rows 3 and 4 of Table 2 show the effects of NYC-CS/RS on math and ELA achievement, respectively, over the four years we observe across columns 2 through 5. Row 3 shows that the effect on math achievement is consistently positive but becomes statistically significant in the second year of implementation at 0.11 SD and is largest in magnitude in the third year of implementation at 0.26 SD. Effects remain large in 2018/2019, the fourth year of implementation, at 0.18 SD, but have somewhat leveled off from the prior year. Row 4 shows that the pattern for ELA achievement is consistent with math in that the point estimates are consistently positive and increasing in magnitude until the third year of implementation in which they are largest at 0.16 SD, leveling off in the following year. However, effects for ELA are only statistically significant in the third year of implementation.

**Heterogeneity by Grade**

To check for underlying heterogeneity in treatment effects by grade, we run our analyses separately for grades 3 through 8. We note here that our grade-level and grade-span analyses are primarily motivated by the fact that NYC-CS/RS services are customized to each school, and therefore, it is reasonable to assume that students across different grade-spans may be receiving different sets of services, if services differ by the age of students. Differential services, in turn, may lead to differential impacts on attendance and test scores. We speculate more on this in in the discussion section. Given the varying annual treatment effects reported in the previous section, we also conduct our grade-level analysis by year. To further probe heterogeneity by grade, we disaggregate our analyses by the grade span served by the school (elementary, elementary/middle, and middle). The results of these supplementary analyses are reported in the sections that follow.

*Attendance*

The first row of Table 3 shows treatment effects on attendance rates by grade across grades 3 through 8. We find positive effects across all grades that are largest in magnitude and only statistically distinguishable from zero in grade 8 at 2.4 percentage points. The second row of Table 3 shows results for chronic absenteeism, which mirror those of attendance in that we find consistent reductions in chronic absenteeism across all grades, effects are largest in grade 8 at a reduction in 10.2 percentage points, and the point estimates are only statistically significant in grade 8.

Given that our exploration of annual treatment effects showed that effects were largest in the third year of implementation (2017/2018) across all outcomes, we also report grade-by-year effects on attendance rates and chronic absenteeism in panels A and B of Table 4. Results across these panels are largely null, but consistent with our findings that effects are largest in 2017/2018 and largest for grade 8. We find an increase in attendance rates for grade 8 of 3.4 percentage points in 2018 and a reduction in the rate of chronic absenteeism for grade 8 of 13.8 percentage points in 2018.

To further explore heterogeneous treatment effects on attendance by grade, we also disaggregate our analyses by the grade span served by each school. We observe three types of schools in our data: those serving grades K-5 (elementary schools), those serving grades K-8 (combined elementary and middle schools), and those serving grades 6-8 (middle schools). The results of this analysis are shown in rows 1 and 2 of Table 6. While we do find a large decrease in chronic absenteeism in middle schools of 10.8 percentage points, we find the largest and most significant improvements in attendance in schools serving grades K-5. This is inconsistent with our previous finding that effects on attendance are largest in grade 8, which we examine further below. We find an improvement in attendance of 2.9 percentage points and a reduction in chronic absenteeism of 14.1 percentage points for elementary schools.

Interpretation of our grade span analysis must take into consideration that schools serving elementary grades also serve grades K-2, which contribute to the school-level attendance outcome measures. To explore the extent to which the lower grades are driving our finding that effects on attendance are largest in elementary schools, we plot the average attendance rates and chronic absenteeism by grade in treated versus control schools in Figure 4. The u-shaped nature of attendance patterns across grades confirms our suspicion that attendance effects are driven by the lowest and highest grades, as opposed to the middle grades, and much of the large effects of NYC-CS on improving attendance in elementary grades is driven by the youngest students.

*Academic Achievement*

Results by grade for math and ELA achievement are shown in rows 3 and 4 of Table 3. We find that effects are largest in grade 4 for both math and ELA scores, at 0.26 and 0.17 SD, respectively. We find smaller, but significant effects on math scores in grades 5 and 6 of 0.19 and 0.16 SD, but null effects in grades 3, 7, and 8. Similarly, we also find a smaller positive effect on ELA scores in grade 5 of 0.13 SD, with null effects across all other grades. Effects on academic achievement appear to be concentrated in the elementary grades, especially in grades 4 and 5.

Because we find that treatment effects are largest in the third year of implementation across all outcomes, we also disaggregate our grade-level analysis by year which we report in Table 5. Panel A shows the results for math scores by grade and year where we find very large effects in grades 4 and 5 in 2017/2018 of 0.39 and 0.34 SD, respectively. Effects are smaller, but still quite large in grade 6 at 0.22 SD. Grade 4 is the only grade where we see significant effects on math scores in years other than 2017/2018, as results are also positive and significant in 2015/2016 and 2016/2017, but null in 2018/2019. These results suggest that math effects are largest in grade 4 across all years and are largest in 2017/2018 across all grades.

Panel B of Table 5 shows the results for ELA scores by grade and year, which tell a similar story to math but are less consistent. Like math, effects on ELA scores are largest for grade 4 in 2017/2018 at 0.31 SD. We find smaller positive effects for grade 4 in 2015/2016, grade 5 in 2016/2017, and grade 7 in 2018/2019. Though the point estimates are consistently largest and positive in 2017/2018 across all grades, we find them to be null aside from those just noted.

Results by grade span are reported in Table 6 and are consistent with our grade-level analysis in that we find largest effects in schools serving only the elementary grades. We find an improvement in math scores of 0.29 SD for elementary schools, 0.16 SD for elementary/middle schools and null effects for middle schools. We find a positive effect on ELA of 0.23 SD for elementary schools, but null effects on elementary/middle and middle schools. This analysis confirms that the NYC-CS/RS program has the largest effects on attendance and academic achievement in the elementary grades, and that effects on math are larger and more consistent than those found on ELA.

**DISCUSSION**

Overall, we find large positive impacts of NYC-CS/RS on student attendance and end-of-year standardized test scores. We consistently find that effects are larger for math than ELA, are largest in the third year of implementation of the program and are largest for students in the elementary grades. Our findings contribute to the literature on FSCS initiatives by providing rigorous causal evidence of positive impacts of the largest community schools program in the country over a four-year period. However, because we are unable to identify the specific programs and practices occurring within NYC-CS/RS schools, we are limited in our ability to draw conclusions as to why these large positive impacts occurred. We also make a methodological contribution to applied education research by introducing a novel methodology that can be implemented in future studies.

**Overall and Annual Results**

While there are likely many positive impacts of NYC-CS/RS on students, families, and communities, in this paper, we focus on attendance as a leading indicator of success and academic achievement as a longer-run indicator of success. We consider attendance to be an important leading indicator if it signals increased student and family engagement in the school community, which in turn increases the exposure that students and families have to the programmatic offerings of NYC-CS/RS. This can lead to longer-run improvements in student academic achievement. Several other shorter-run evaluations of small community schools initiatives have also found improvements in student attendance, which is consistent with our findings that effects on attendance show up immediately. Other studies related to such student supports have also found that effects on academic achievement take time to manifest (e.g., Dearing et al., 2016; Johnston et al., 2020), also consistent with our findings. While our results provide evidence in support of our theory that attendance is a leading indicator of long-run academic outcomes, we are unable to prove empirically that attendance is the main mechanism by which academic achievement improves in the long-run, and there may be other plausible explanations for why these outcomes tend to appear sequentially.

While we show that the effectiveness of NYC-CS/RS increases over time, we are unable to determine precisely why this occurs. In particular, the increase over time could either be driven by increased exposure of students to the program in the latter years or simply due to the program becoming more effective as it matures and can incorporate early feedback into its design. In theory, we can shed light on this question by looking at how the effect evolves differentially over time for different grade levels. For example, we could examine how the program's effectiveness evolves over time for sixth-grade students (many of whom will be exposed for only one year, regardless of how long the program has been in place) to eighth-grade students (whose exposure will depend on how long the program has been in place). When implementing this approach using all grade-levels, we find suggestive evidence that the main driver of increased effectiveness is due to the program maturity, rather than increased student exposure; however, the results are quite imprecise and so the result should be viewed as speculative, rather than definitive. This suggestive evidence is consistent with prior research on the early implementation of NYC-CS, which found increasing levels of implementation of the core features of the model over time (Johnston et al., 2017). We also believe this to be consistent with the NYC-CS Theory of Change, which emphasizes building core capacities in tandem with implementing the core features of the model.

Regardless of the mechanisms, our results provide strong evidence that it takes time to realize the academic benefits of full-service community schools as a turnaround strategy, and that sustaining program success long-term will require ongoing commitment, resources, and evaluation.

**Heterogeneity by Grade**

Our exploration of heterogeneity by grade and grade span consistently shows that effects are largest in the elementary grades, particularly grades 4 and 5, and appear to be driven by schools serving only elementary-aged children. While there may be a variety of reasons why NYC-CS/RS program effects are largest in elementary schools, we suspect that this is in part because younger children necessitate greater family engagement in school than older children. Parents of elementary-aged children (especially in NYC where there is no yellow bus transportation) are more likely to interface with the school at pick-up and drop-off than parents of older children who often transport themselves to school, which provides more opportunities for parents to build relationships with school staff and be exposed to information regarding programmatic offerings, many of which are specifically designed for parents. This can create a positive feedback loop in which more engaged parents receive more information regarding program offerings, which leads to further opportunities for engagement.

Additionally, a key feature of NYC-CS/RS is extended school days and extended school years. This provides childcare support for working families, which may improve attendance. If parents lack sufficient childcare options, they may be forced to keep their child home from school if the school schedule is not aligned with their working schedule. Longer school days and years can relieve a childcare burden on working parents while also providing students with greater exposure to NYC-CS/RS programming. Older students that do not require adult supervision when not in school will not pose the same childcare challenges for their families and may therefore have less incentive to participate in before- or after-school activities, thereby receiving less exposure to NYC-CS/RS programming.

**CONCLUSION**

It is widely recognized that, although the primary objective of public schools is to provide students with the academic skills that are necessary to be productive members of society, many students require non-academic supports in order to be able to effectively learn. Full service community schools are a comprehensive strategy that aim to provide supports that are tailored to the needs of students in a particular community, including behavioral, mental health, social or academic scaffolding (Maier et al., 2017)

Evaluating the impact of the community school strategy is difficult for many reasons. Many districts only implement the community school strategy in a small number of schools, which makes it difficult to determine whether any observed gains are due to idiosyncratic factors. More importantly, community schools are often identified by unknown or non-random reasons, which creates difficulty for researchers to identify a valid counterfactual for evaluation purposes. Fortunately, the NYC strategy for designating community schools, which we leverage in this paper, avoid these pitfalls. In NYC, we have a sufficiently large program to rule out idiosyncratic differences as the cause of an estimated community school impact, and an easily interpretable cutoff for treatment, which makes possible the use of one of the most trusted methods – regression discontinuity -- for reliably detecting a programmatic impact. We extend current regression discontinuity methods for use in a situation such as this, in which the running variable is complex (i.e., multidimensional), and discuss how to make full use of additional covariates to gain the necessary precision to rule out the possibility that the impacts are only chance differences between community and non-community schools. In doing so, we create an analytic method that can be used to evaluate other programs that use algorithms to determine eligibility and have moderate sample sizes.

Using this method, we are able to establish that a comprehensive community school turnaround model is responsible for immediate improvements in attendance, which over time leads to the program having a positive impact on math and ELA standardized test scores. We show that these effects appear to continue into the fourth year following implementation, although there is some evidence that the effects do not continue to grow larger after the third year. Our results confirm the importance of ongoing, rigorous evaluations of FSCS that account for changes in treatment effects over time, especially as the model continues to expand across the country.

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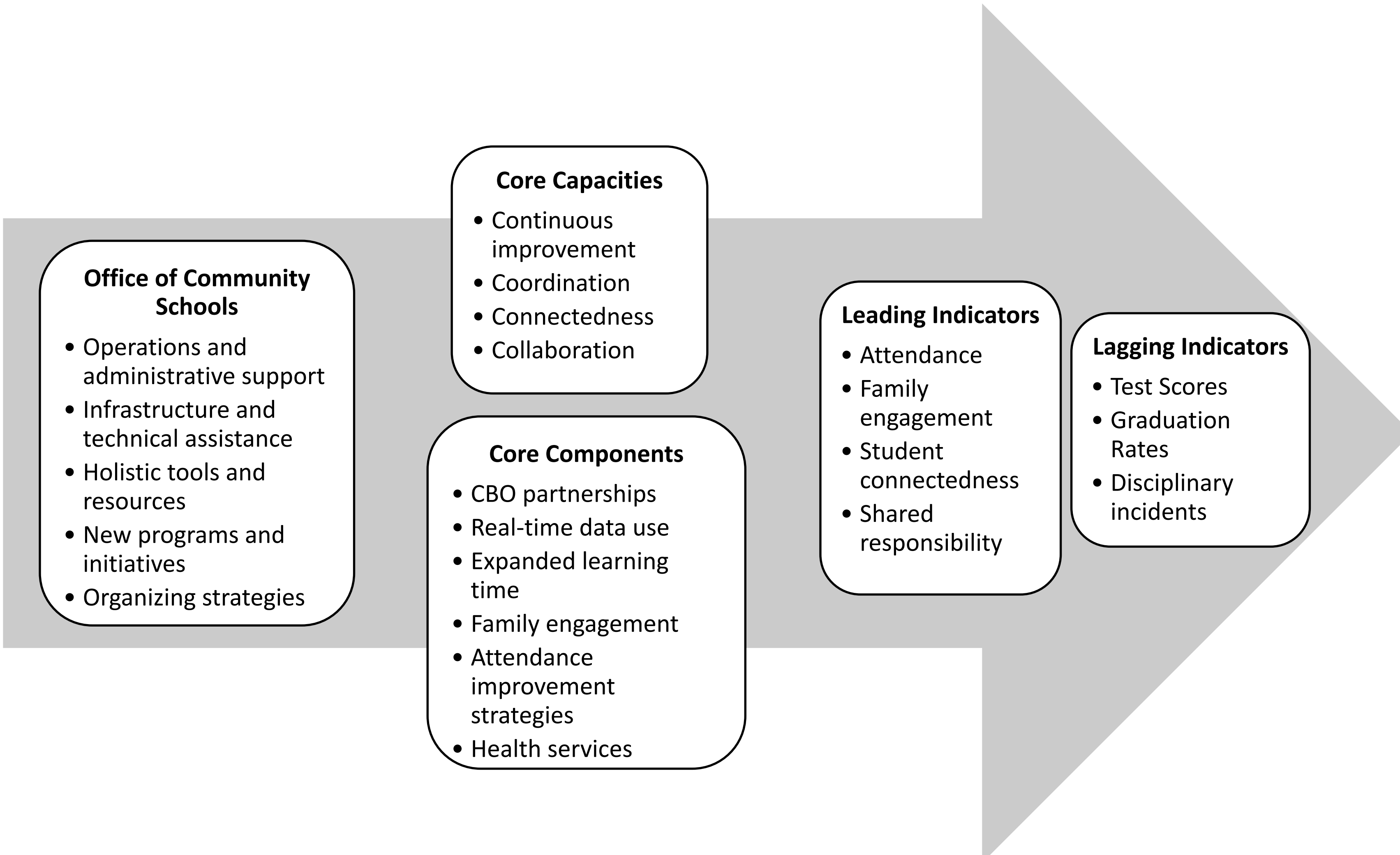
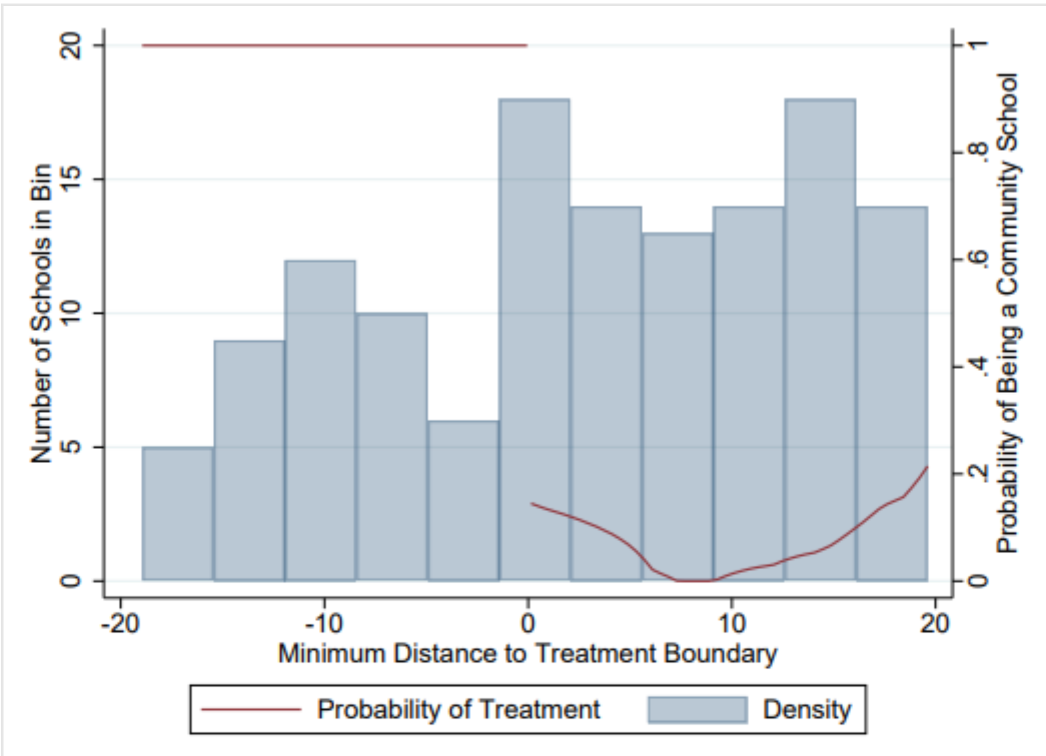
**TABLES AND FIGURES**

**Figure 1.** New York City Community Schools Initiative Theory of Change.



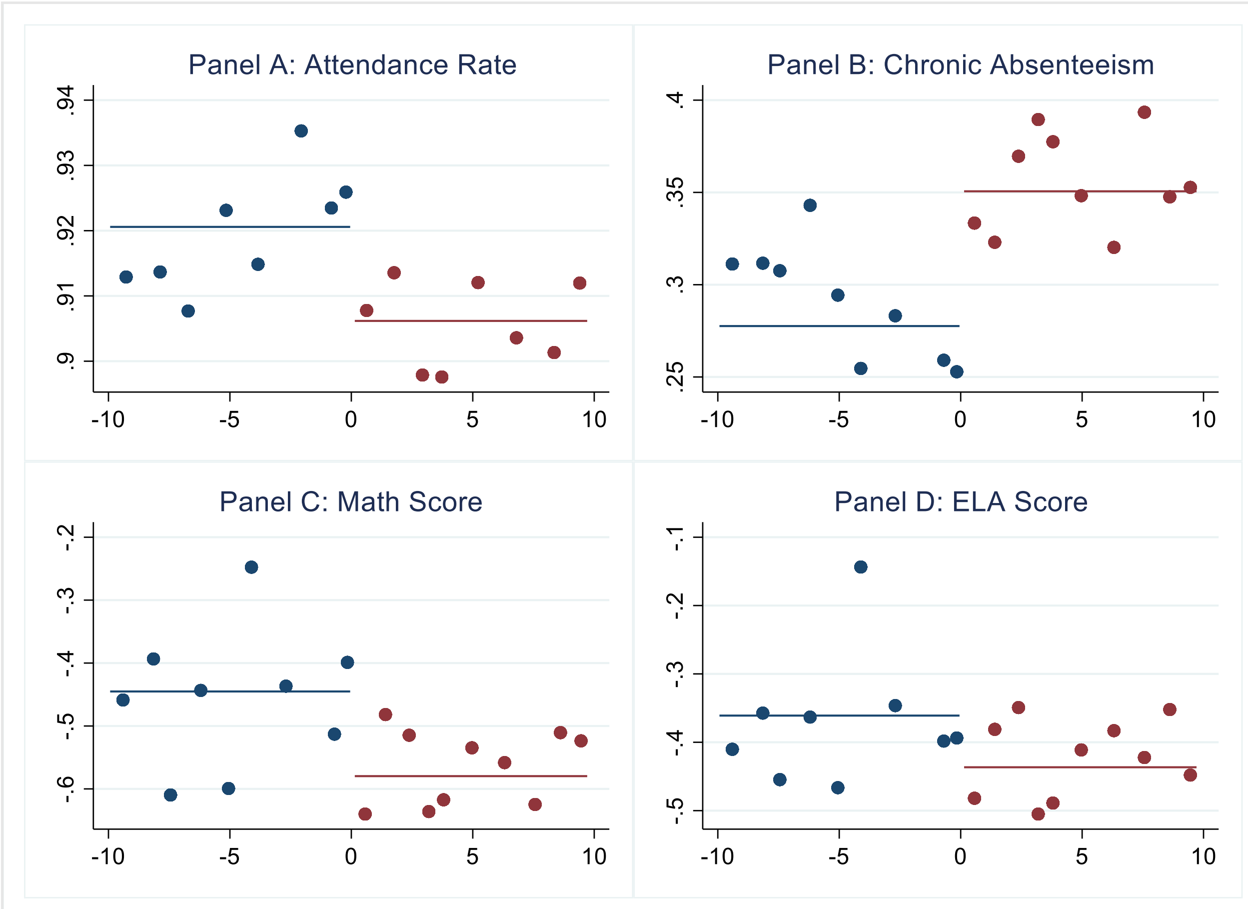

*Source:* Author's adaptation from the New York City Community Schools Strategic Plan (New York City Community Schools, n.d.)

**Figure 2.** Probability of Treatment and School Density Above/Below Cutoff



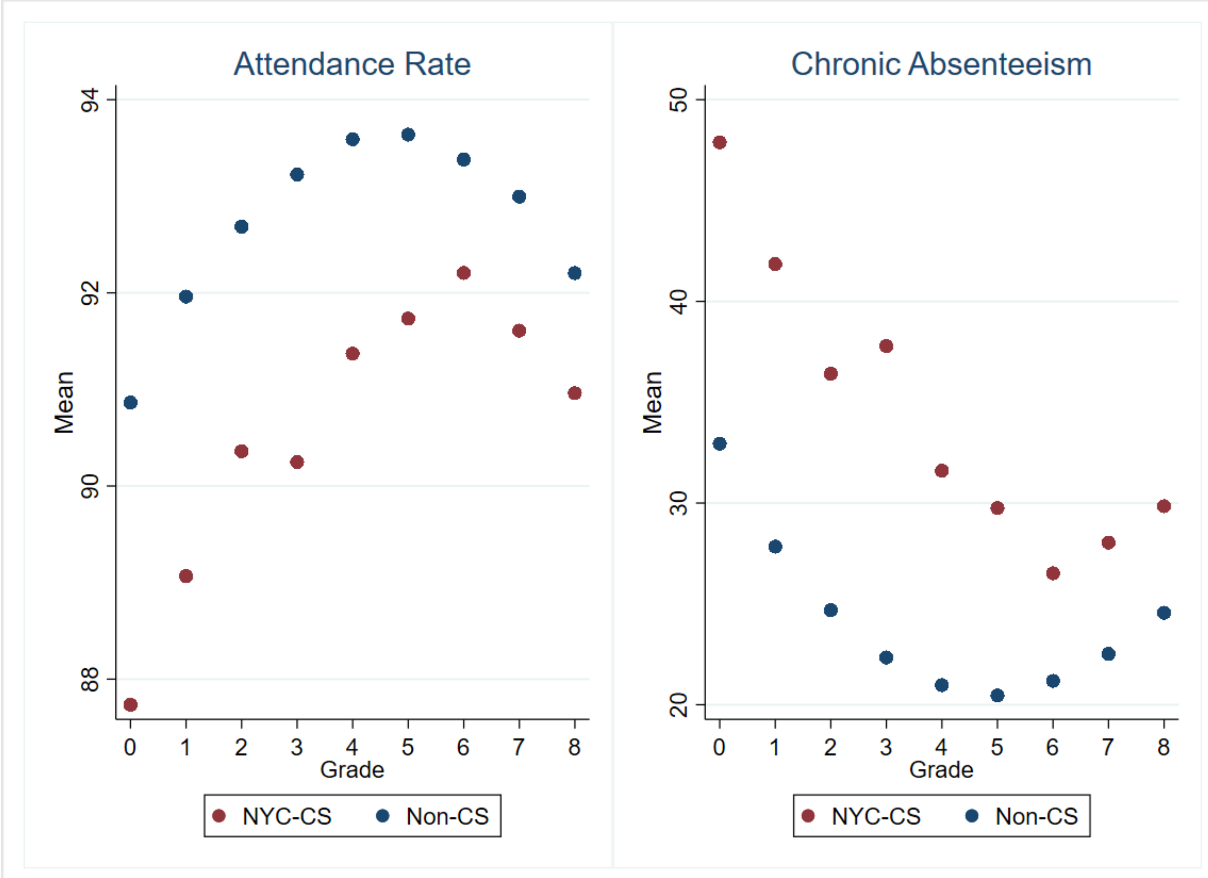

*Note.* Plot shows the density of schools within 20 percentile points of the treatment boundary and the probability of treatment on either side of the boundary.

**Figure 3.** RD Plots of NYC-CS Program Effects on Outcomes




*Note.* Plots show the discontinuity in outcomes within our preferred bandwidth of 10 percentile points around the cutoff.

**Figure 4.** Attendance Patterns by Grade




*Note.* Plots show the average attendance rates and rates of chronic absenteeism by grade and community school status. Data is pooled across the 2016/2017, 2017/2018, and 2018/2019 school years.

**Table 1.** Mean School Characteristics by Bandwidth

| | Bandwidth 20 | | Bandwidth 15 | | Bandwidth 10 | |
|---|---|---|---|---|---|---|
| | Below | Above | Below | Above | Below | Above |
| Enrollment | 391.4 | 501.5 | 392.5 | 523.9 | 425.8 | 505.3 |
| | (176.7) | (263.7) | (188.6) | (276.2) | (194.5) | (276.7) |
| ELL | 0.189 | 0.170 | 0.185 | 0.178 | 0.175 | 0.173 |
| | (0.102) | (0.113) | (0.103) | (0.115) | (0.109) | (0.0917) |
| SWD | 0.263 | 0.235 | 0.260 | 0.238 | 0.266 | 0.243 |
| | (0.0503) | (0.0510) | (0.0484) | (0.0522) | (0.0558) | (0.0513) |
| STH | 0.218 | 0.193 | 0.222 | 0.199 | 0.213 | 0.211 |
| | (0.0788) | (0.0765) | (0.0797) | (0.0805) | (0.0818) | (0.0840) |
| Economic | 0.868 | 0.857 | 0.866 | 0.860 | 0.867 | 0.854 |
| Disadvantage | (0.0627) | (0.0763) | (0.0626) | (0.0777) | (0.0530) | (0.0686) |
| Asian | 0.0221 | 0.0219 | 0.0236 | 0.0231 | 0.0269 | 0.0200 |
| | (0.0313) | (0.0322) | (0.0339) | (0.0347) | (0.0383) | (0.0235) |
| Hispanic | 0.537 | 0.561 | 0.522 | 0.563 | 0.524 | 0.573 |
| | (0.239) | (0.246) | (0.244) | (0.243) | (0.246) | (0.215) |
| Black | 0.413 | 0.386 | 0.426 | 0.383 | 0.417 | 0.380 |
| | (0.225) | (0.243) | (0.227) | (0.241) | (0.229) | (0.215) |
| White | 0.0165 | 0.0199 | 0.0167 | 0.0201 | 0.0195 | 0.0181 |
| | (0.0136) | (0.0304) | (0.0139) | (0.0308) | (0.0150) | (0.0266) |
| Math Score | -0.777 | -0.554 | -0.757 | -0.580 | -0.744 | -0.591 |
| | (0.143) | (0.153) | (0.143) | (0.153) | (0.155) | (0.151) |
| ELA Score | -0.649 | -0.490 | -0.645 | -0.520 | -0.615 | -0.521 |
| | (0.129) | (0.149) | (0.135) | (0.139) | (0.129) | (0.121) |
| Attendance | 0.892 | 0.906 | 0.893 | 0.905 | 0.895 | 0.903 |
| Rate | (0.0226) | (0.0182) | (0.0233) | (0.0189) | (0.0215) | (0.0183) |
| *N* | 329 | 602 | 273 | 462 | 189 | 294 |

*Note.* Mean coefficients; standard deviations in parentheses.

**Table 2.** Overall and Yearly Effects

| | (1)<br>All Years | (2)<br>2016 | (3)<br>2017 | (4)<br>2018 | (5)<br>2019 |
|---|---|---|---|---|---|
| Attendance Rate | 0.0161**<br>(0.00682) | 0.0111<br>(0.0069) | 0.0149**<br>(0.00757) | 0.0209***<br>(0.00744) | 0.0173**<br>(0.00685) |
| Chronic Absenteeism | -0.0814***<br>(0.0298) | -0.0555*<br>(0.0288) | -0.0760**<br>(0.0333) | -0.114***<br>(0.0341) | -0.0799***<br>(0.0305) |
| Math Score | 0.150***<br>(0.0488) | 0.0421<br>(0.0382) | 0.111*<br>(0.0571) | 0.263***<br>(0.0677) | 0.184***<br>(0.0712) |
| ELA Score | 0.0843**<br>(0.0399) | 0.0412<br>(0.0336) | 0.0583<br>(0.0435) | 0.155***<br>(0.0569) | 0.0827<br>(0.0598) |
| *N* | 560 | 144 | 144 | 144 | 144 |

*Note.* Standard errors in parentheses. The standard errors reported above were estimated while clustering the observations by school. All effects are estimated using a bandwidth of 10 percentile points on either side of the cutoff.

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

**Table 3.** Heterogeneity by Grade

| | (1)<br>Grade 3 | (2)<br>Grade 4 | (3)<br>Grade 5 | (4)<br>Grade 6 | (5)<br>Grade 7 | (6)<br>Grade 8 |
|---|---|---|---|---|---|---|
| Attendance Rate | 0.0040<br>(0.00738) | 0.0086<br>(0.0073) | 0.0077<br>(0.00697) | 0.0071<br>(0.00979) | 0.0163<br>(.01075) | 0.0244*<br>(.01139) |
| Chronic Absenteeism | -0.0326<br>(0.03547) | -0.0394<br>(0.03965) | -0.0459<br>(0.03447) | -0.0451<br>(0.04038) | -0.0753<br>(0.04725) | -0.1021*<br>(0.04601) |
| Math Score | 0.0877<br>(0.0918) | 0.256**<br>(0.0881) | 0.185*<br>(0.0809) | 0.157*<br>(0.0727) | 0.0413<br>(0.0637) | 0.0765<br>(0.0828) |
| ELA Score | 0.0667<br>(0.0937) | 0.174*<br>(0.0687) | 0.132*<br>(0.0599) | 0.0747<br>(0.0574) | 0.0363<br>(0.0464) | 0.000424<br>(0.0498) |

*Note.* Standard errors in parentheses. The standard errors reported above were estimated while clustering the observations by school. All effects are estimated using a bandwidth of 10 percentile points on either side of the cutoff. Number of observations varies based on the outcome because attendance-by-grade data is not available for 2015/2016 and there are cases in which the data is suppressed in the publicly available files due to the minimum n-size required for reporting.
* p<0.10, ** p<0.05, *** p<0.01

**Table 4.** Heterogeneity of Effects on Attendance by Grade and Year

| | (1) Grade 3 | (2) Grade 4 | (3) Grade 5 | (4) Grade 6 | (5) Grade 7 | (6) Grade 8 |
|---|---|---|---|---|---|---|
| *Panel A: Attendance Rate* | | | | | | |
| 2017 | -0.0023 | 0.0100 | 0.0039 | -0.0032 | 0.0063 | 0.0195 |
| | (0.01011) | (0.00850) | (0.00976) | (0.00991) | (0.01198) | (0.01400) |
| 2018 | 0.0148 | 0.0066 | 0.0142 | 0.0123 | 0.0162 | 0.0341** |
| | (0.00841) | (0.00772) | (0.00865) | (0.01160) | (0.01148) | (0.01245) |
| 2019 | -0.0006 | 0.009214 | 0.0054 | 0.0145 | 0.0289 | 0.0188 |
| | (0.00719) | (0.00741) | (0.00591) | (0.01294) | (0.01512) | (0.01228) |
| *Panel B: Chronic Absenteeism* | | | | | | |
| 2017 | -0.0061 | -0.0355 | -0.0436 | 0.0143 | -0.0383 | -0.0737 |
| | (0.04429) | (0.05246) | (0.05056) | (0.04576) | (0.05640) | (0.05432) |
| 2018 | -0.0900* | -0.0325 | -0.0629 | -0.0939 | -0.0658 | -0.1382** |
| | (0.04426) | (0.04309) | (0.04799) | (0.05678) | (0.04893) | (0.05090) |
| 2019 | -0.0017 | -0.0501 | -0.03183 | -0.06205 | -0.1341* | -0.0939 |
| | (0.03893) | (0.03962) | (0.03081) | (0.06186) | (0.06382) | (0.05488) |

*Note.* Standard errors in parentheses. The standard errors reported above were estimated while clustering the observations by school. The number of observations varies by grade and year due to cases where data is suppressed in the publicly available files. All effects are estimated using a bandwidth of 10 percentile points on either side of the cutoff.
* p<0.10, ** p<0.05, *** p<0.01

**Table 5.** Heterogeneity of Effects on Academic Achievement by Grade and Year

| | (1)<br>Grade 3 | (2)<br>Grade 4 | (3)<br>Grade 5 | (4)<br>Grade 6 | (5)<br>Grade 7 | (6)<br>Grade 8 |
|---|---|---|---|---|---|---|
| *Panel A: Math Score* | | | | | | |
| 2016 | -0.0103 | 0.197* | 0.0535 | 0.107 | -0.120 | 0.098 |
| | (0.0739) | (0.0994) | (0.0723) | (0.0902) | (0.0672) | (0.103) |
| 2017 | 0.119 | 0.245* | 0.151 | 0.114 | -0.0123 | -0.0956 |
| | (0.104) | (0.0966) | (0.123) | (0.114) | (0.0942) | (0.126) |
| 2018 | 0.228 | 0.392** | 0.343*** | 0.217* | 0.190 | 0.225 |
| | (0.163) | (0.137) | (0.0926) | (0.0923) | (0.0996) | (0.132) |
| 2019 | 0.0137 | 0.190 | 0.192 | 0.207 | 0.145 | 0.116 |
| | (0.135) | (0.118) | (0.102) | (0.109) | (0.118) | (0.185) |
| *Panel B: ELA Score* | | | | | | |
| 2016 | -0.0327 | 0.163* | 0.149 | 0.0720 | -0.0813 | 0.0324 |
| | (0.0707) | (0.0652) | (0.0892) | (0.0631) | (0.0679) | (0.067) |
| 2017 | 0.125 | 0.173 | 0.178* | 0.0735 | -0.0381 | -0.0829 |
| | (0.0962) | (0.0961) | (0.0766) | (0.103) | (0.0593) | (0.0805) |
| 2018 | 0.139 | 0.312** | 0.170 | 0.105 | 0.142 | 0.0421 |
| | (0.146) | (0.115) | (0.0944) | (0.0678) | (0.0753) | (0.0769) |
| 2019 | 0.0355 | 0.0499 | 0.0314 | 0.0465 | 0.164* | 0.0218 |
| | (0.145) | (0.138) | (0.110) | (0.0815) | (0.0778) | (0.0552) |

*Note.* Standard errors in parentheses. The standard errors reported above were estimated while clustering the observations by school. The number of observations varies by grade and year due to cases where data is suppressed in the publicly available files. All effects are estimated using a bandwidth of 10 percentile points on either side of the cutoff.
* p<0.10, ** p<0.05, *** p<0.01

**Table 6.** Heterogeneity by School Grade Span

| | (1)<br>K-5 | (2)<br>K-8 | (3)<br>6-8 |
|---|---|---|---|
| Attendance<br>Rate | 0.0287***<br>(0.0064) | -0.00102<br>(0.0088) | 0.0241<br>(0.0124) |
| Chronic<br>Absenteeism | -0.141***<br>(0.0361) | -0.00354<br>(0.0341) | -0.108*<br>(0.0500) |
| Math Score | 0.289*<br>(0.1230) | 0.159**<br>(0.0600) | 0.100<br>(0.0713) |
| ELA Score | 0.234*<br>(0.1190) | 0.106<br>(0.0643) | 0.0056<br>(0.0504) |
| *N* | 200 | 88 | 268 |

*Note.* Standard errors in parentheses. The standard errors reported above were estimated while clustering the observations by school. All effects are estimated using a bandwidth of 10 percentile points on either side of the cutoff.
* p<0.10, ** p<0.05, *** p<0.01

## TECHNICAL APPENDIX

In our discussion of the method in main body of the paper, we make two claims: first, that our approach to a multidimensional RDD approach converges to the average effect of the compliers at the multidimensional boundary; second, that we can residualize the outcome without changing the interpretation or adjusting inference. Here we discuss these claims in more detail, providing more precise statements of these claims and an outline of why they are true.

### From a Multidimensional RDD to a One-dimensional RDD

For a theoretical analysis of the multidimensional RDD, we start with additional details on the formal description of the problem. As stated in the main paper, we start with Rubin's potential outcome framework and the assumption that a set of running variables collectively determines treatment. We will assume that the researchers observe all $p$ running variables, which we denoted as a vector $Z_i$ and are assumed to be unaffected by the schools' treatment status $T_i$. We will denote the entire space of potential $Z_i$'s as $\boldsymbol{Z}$ and assume that: $\boldsymbol{Z}$ is endowed with a distance metric $d(Z_i, Z_j)$ and that $Z_i$ are distributed according to some continuous probability density function $f(Z)$ with $f(Z) > 0$ for all $Z \in \boldsymbol{Z}$.

As discussed in the main body of the paper, we extend the traditional univariate RDD to the multivariate case by specifying that treatment is instead determined by $j$ running variables such that $T_i = 1$ if $Z_{i,j} < Z_{c,j}$ for all $j$ and $T_i = 0$ otherwise (i.e., if $Z_{i,j} > Z_{c,j}$ for at least one $j$). This implies that we can partition of $\boldsymbol{Z}$ into two spaces, denoted $\mathcal{F}_-$ and $\mathcal{F}_+$, with individuals in $\mathcal{F}_+$ being treated and those in $\mathcal{F}_-$ not being treated. We denote the boundary between $\mathcal{F}_-$ and $\mathcal{F}_+$ as $\mathcal{F}$. In the specification above, we can then write $\mathcal{F}$ as:

$$\mathcal{F} = \left\{Z_i \in \boldsymbol{Z} \mid Z_{i,j} \leq Z_{c,j} \text{ for all } j \text{ and } Z_{i,j} = Z_{c,j} \text{ for at least one } j\right\}$$

We will then use $\mathcal{F}_+(h)$ to denote the set of treated observations within a distance $h$ of the boundary, i.e. the set $\mathcal{F}_+(h) = \{Z_i \in \mathcal{F}_+ \mid d(Z_i, W) < h \; for \; some \; W \in \mathcal{F}\}$, and define $\mathcal{F}_-(h)$ similarly.[7]

We will also assume that the observations $(Y_i(0), \tau_i, Z_i)$ are independent and that the relationship between $Y_i$ and $Z_i$ is nicely behaved. More specifically, we'll assume that $\mathbb{E}[Y_i(0) | \, Z_i = Z] \equiv g_0(Z)$ and $\mathbb{E}[\tau_i | \, Z_i = Z] \equiv \; g_\tau(Z)$ are both continuously differentiable function of $Z$. We will further assume that $Var[Y_i(0) | \, Z_i = Z] = \; \sigma_0^2 < \infty$ and that $Var[\tau_i | \, Z_i = Z] = \; \sigma_\tau^2 < \infty$, although we could allow for heteroskedasticity at the cost of additional notation, and that higher order moments exist. For notation, it will also be useful to use $\tilde{\sigma}_0^2$ to denote $\mathbb{E}[Var[Y_i(0) | \, Z_i = Z] \mid Z_i \in \mathcal{F}]$, so $Var[Y_i(0) | \; Z_i \in \mathcal{F}] = \; \sigma_0^2 + \tilde{\sigma}_0^2$. We define $\tilde{\sigma}_\tau^2$ similarly.

Note that all of these assumptions are straightforward generalizations of the traditional assumptions of a univariate regression discontinuity to a multivariate case, e.g., paragraph one assumes that there is a positive probability of observing observations arbitrarily close to the discontinuity, paragraph two assumes that boundary determines treatment, and paragraph three assumes the continuity of the potential outcomes and that the error terms are well-behaved enough for us apply the law of large numbers and the central limit theorem.

We next show that if we are willing to make these assumptions instead of running a complex multidimensional discontinuity, we can run a traditional univariate regression

[7] We could extend our model to the fuzzy multidimensional regression discontinuity by assuming that: $\lim_{h \to 0} \mathbb{E}[T_i | \, Z_i \in \mathcal{F}_+(h)] = p_h \; > p_l = \lim_{h \to 0} \mathbb{E}[T_i | \, Z_i \in \mathcal{F}_-(h)]$, without necessarily assuming that $\mathbb{E}[T_i | \, Z_i \in \mathcal{F}_+(h)] = 1$ and $\mathbb{E}[T_i | \, Z_i \in \mathcal{F}_-(h)] = 0$. It is relatively straightforward to show that the results we present here also apply to the fuzzy multidimensional RDD case, but doing so requires much additional notation (such as introducing compliers).

discontinuity using distance to the boundary as a running variable without affecting the parameter the approach converges to. To state this formally, let $D(Z_i)$ be a function from $\mathbb{R}^p$ *to* $\mathbb{R}$ defined as:

$$D(Z_i) = \begin{cases} -1 * \min_{W \in \mathcal{F}} d(Z_i, W) \; if \; Z_i \in \mathcal{F}_- \\ \min_{W \in \mathcal{F}} d(Z_i, W) \; if \; Z_i \in \mathcal{F}_+ \end{cases}$$

Our claim is then the using $D(Z_i)$ in a traditional univariate RDD design converges to the same parameter – the average effect of individuals on the boundary – as would a multidimensional RD.

This can be seen in two ways. First, it is straightforward to show that the univariate RD inherits all the important features of the multidimensional context, i.e., that (a) there is an implied probability distribution $f_D(d)$ with $f_D(d) > 0$; (b) the treatment is determined by whether $D(Z_i)$ is greater or less than zero; (c) both $\mathbb{E}[Y_i(0)|\, D(Z_i) = d]$ and $\mathbb{E}[\tau_i|\, D(Z_i) = d]$ are continuous functions; and (d) that the error terms $Y_i - \mathbb{E}[Y_i(0)|\, D(Z_i) = d] - T_i\mathbb{E}[\tau_i|\, D(Z_i) = d]$ are well-behaved and so we can apply the law of large numbers and the central limit theorem.

While this approach is useful since it allows us to be flexible in the estimation specification, we also can show directly that a traditional univariate RD – when estimated the way we do so in our baseline specification – converges to the true average treatment effect on the boundary. To do so, let $\widehat{\mathbb{E}}_n$ denote the empirical mean of random variable. A traditional sharp RD using this $D(Z_i)$ as a running variable (and no slope terms) with a bandwidth of $h$ would be equal to:

$$\hat{\tau} = \widehat{\mathbb{E}}_n[Y_i|0 < D(Z_i) < h] - \widehat{\mathbb{E}}_n[Y_i|0 > D(Z_i) > -h]$$

We next show that converges to the true average treatment effect of individuals on the boundary, i.e., to $\mathbb{E}[\tau_i\ |D(Z_i) = 0]$.

***Proposition:***
*Define the estimator as follows:*

$$\hat{\tau} = \widehat{\mathbb{E}}_n[Y_i|0 < D(Z_i) < h] - \widehat{\mathbb{E}}_n[Y_i|0 > D(Z_i) > -h]$$

*Then if* $h \to 0$ *and* $nh \to \infty$*, we have that* $\hat{\tau} \xrightarrow{p} \mathbb{E}[\tau_i\ |D(Z_i) = 0]$. *Furthermore, if* $h\sqrt{nh} \to 0$*, then we have that:*

$$\sqrt{nh} \cdot (\hat{\tau} - \mathbb{E}[\tau_i\ |D(Z_i) = 0]) \xrightarrow{d} N\left(0, \frac{2}{f_0} \cdot (2\sigma_0^2 + \sigma_\tau^2 + 2\tilde{\sigma}_0^2 + \tilde{\sigma}_\tau^2)\right)$$

***Proof Sketch:***
*First, we note that in the multidimensional framing, the estimator equals* $\widehat{\mathbb{E}}_n[Y_i|Z_i \in \mathcal{F}_+(h)] - \widehat{\mathbb{E}}_n[Y_i|Z_i \in \mathcal{F}_-(h)]$ *and* $\mathbb{E}[\tau_i\ |D(Z_i) = 0] = \mathbb{E}[\tau_i \mid Z_i \in \mathcal{F}]$ *. Next, we can re-write the estimates as:*

$$\hat{\tau} = \{\mathbb{E}[Y_i|Z_i \in \mathcal{F}_+(h)] - \mathbb{E}[Y_i|Z_i \in \mathcal{F}_-(h)]\} + \left\{\widehat{\mathbb{E}}_n[Y_i|Z_i \in \mathcal{F}_+(h)] - \mathbb{E}[Y_i|Z_i \in \mathcal{F}_+(h)]\right\} - \left\{\widehat{\mathbb{E}}_n[Y_i|Z_i \in \mathcal{F}_-(h)] - \mathbb{E}[Y_i|Z_i \in \mathcal{F}_-(h)]\right\}$$

*We first consider the third term, which we can re-write as* $\widehat{\mathbb{E}}_n[Y_i(0)|Z_i \in \mathcal{F}_-(h)] - \mathbb{E}[Y_i(0)|Z_i \in \mathcal{F}_-(h)]$*, since all individuals in* $\mathcal{F}_-(h)$ *are not treated. Next, note that the expected number of individuals included in the sample average is equal to* $n\int f(Z_i)1\big(Z_i \in \mathcal{F}_+(h)\big)dZ_i \to nh\frac{1}{2}\int f(Z_i)\delta(Z_i \in \mathcal{F})dZ_i$ *as* $h \to 0$*, where* $\delta(.)$ *is the Dirac delta function. We will let* $f_0 \equiv \int f(Z_i)\delta(Z_i \in \mathcal{F})dZ_i$ *and from the assumption that* $f(Z_i) > 0$ *for every Z, we get that* $f_0 > 0$*. Thus, if the* $nh \to \infty$*, we can apply the central limit theorem to conclude that:*

$$\sqrt{nh} \cdot \left(\widehat{\mathbb{E}}_n[Y_i(0)|Z_i \in \mathcal{F}_-(h)] - \mathbb{E}[Y_i(0)|Z_i \in \mathcal{F}_-(h)]\right) \xrightarrow{d} N\left(0{,}2\frac{\sigma_0^2 + \tilde{\sigma}_0^2}{f_0}\right)$$

*We can use a similar approach for the second term, now noting that* $\widehat{\mathbb{E}}_n[Y_i|Z_i \in \mathcal{F}_+(h)] = \widehat{\mathbb{E}}_n[Y_i(0) + \tau_i|Z_i \in \mathcal{F}_+(h)]$*, to get that:*

$$\sqrt{nh} \cdot \left(\widehat{\mathbb{E}}_n[Y_i|Z_i \in \mathcal{F}_+(h)] - \mathbb{E}[Y_i|Z_i \in \mathcal{F}_+(h)]\right) \xrightarrow{d} N\left(0{,}2\frac{\sigma_0^2 + \tilde{\sigma}_0^2 + \sigma_\tau^2 + \tilde{\sigma}_\tau^2}{f_0}\right)$$

*Finally, we can consider the first term* $\mathbb{E}[Y_i|Z_i \in \mathcal{F}_+(h)] - \mathbb{E}[Y_i|Z_i \in \mathcal{F}_-(h)] = \mathbb{E}[\tau_i|Z_i \in \mathcal{F}_+(h)] + \mathbb{E}[Y_i(0)|Z_i \in \mathcal{F}_+(h)] - \mathbb{E}[Y_i(0)|Z_i \in \mathcal{F}_-(h)]$*. From the continuity of the conditional moments, we get that* $\mathbb{E}[\tau_i|Z_i \in \mathcal{F}_+(h)] \to \mathbb{E}[\tau_i|Z_i \in \mathcal{F}]$ *and* $\mathbb{E}[Y_i(0)|Z_i \in \mathcal{F}_+(h)] - \mathbb{E}[Y_i(0)|Z_i \in \mathcal{F}_-(h)] \to 0$ *as* $h \to 0$*, which together with the above results give the* *consistency. For the limiting distribution, however, we need to go a step farther and show that:* $\sqrt{nh} \cdot (\mathbb{E}[\tau_i | Z_i \in \mathcal{F}_+(h)] - \mathbb{E}[\tau_i \mid Z_i \in \mathcal{F}]) \rightarrow 0$. *To start, note that we can re-write* $\mathbb{E}[\tau_i | Z_i \in \mathcal{F}_+(h)]$ *as* $\mathbb{E}[g_\tau(Z_i) | Z_i \in \mathcal{F}_+(h)]$ *and make a similar change for* $\mathbb{E}[\tau_i \mid Z_i \in \mathcal{F}]$. *We can then note:*

$$\sqrt{nh} \cdot (\mathbb{E}[\tau_i | Z_i \in \mathcal{F}_+(h)] - \mathbb{E}[\tau_i \mid Z_i \in \mathcal{F}]) = h\sqrt{nh} \cdot \frac{(\mathbb{E}[g_\tau(Z_i) | Z_i \in \mathcal{F}_+(h)] - \mathbb{E}[g_\tau(Z_i) | Z_i \in \mathcal{F}])}{h}$$

*Note, however, that the expression* $\frac{(\mathbb{E}[g_\tau(Z_i) | Z_i \in \mathcal{F}_+(h)] - \mathbb{E}[g_\tau(Z_i) | Z_i \in \mathcal{F}])}{h}$ *looks very similar to the derivative of* $g_\tau(Z_i)$ *as* $h \rightarrow 0$. *In fact, from the assumption that* $g_\tau(Z_i)$ *is continuously differentiable, we can use a Taylor expansion to show that this is finite. Thus, if* $h\sqrt{nh} \rightarrow 0$ *we get that* $\sqrt{nh} \cdot (\mathbb{E}[\tau_i | Z_i \in \mathcal{F}_+(h)] - \mathbb{E}[\tau_i \mid Z_i \in \mathcal{F}]) \rightarrow 0$. *We can use a similar approach to show that* $\sqrt{nh} \cdot (\mathbb{E}[Y_i(0) | Z_i \in \mathcal{F}_+(h)] - \mathbb{E}[Y_i(0) | Z_i \in \mathcal{F}_-(h)]) \rightarrow 0$, *which gives us the result.*

Note that (in contrast to some of the discussions surrounding the MRRDD) the dimension reduction approach outlined above does not necessarily imply that parameter the estimate converges to depends on the scaling of the covariates $Z_i$ (Reardon & Robinson, 2012; Wong, Steiner, & Cook, 2013). Let $\tilde{Z}_i$ be a re-scaled version of $Z_i$, for example; i.e., for all dimensions of $Z_i$ we have that $\tilde{Z}_{i,k} = h_k(Z_{i,k})$ for some continuous and monotonic function $h_k$. Importantly, this re-scaling also changes the regions $\mathcal{F}_-$ and $\mathcal{F}_+$ and hence the boundary $\mathcal{F}$. If we denote the new regions as $\tilde{\mathcal{F}}_-, \tilde{\mathcal{F}}_+$, and $\tilde{\mathcal{F}}$, it follows that $\{i \mid X_i \in \mathcal{F}\} = \{i \mid \tilde{X}_i \in \tilde{\mathcal{F}}\}$ and so the average treatment effect of those individuals on the boundary does not change. For example, suppose, as in our example, that the multidimensional RDD involves multiple measures and schools are more likely to be treated if the value of every measure is below 0.25. Now suppose that we multiplied one of the measures by 100. Clearly the set of schools who had a value of 0.25 on every measure before the re-scaling is precisely the same set of schools who, after the re-scaling, have value of 25 on the re-scaled measure and 0.25 on the other measures. Note, however, that in practice one needs to be thoughtful in defining the distance measure when combining multiple variables on potentially different scales.

Finally, and in many ways most importantly, we can see from the result that the dimension-reduction is not without cost. As we can see, the asymptotic variance of the estimator converges to: $\frac{2}{nhf_0} \cdot (2\sigma_0^2 + \sigma_\tau^2 + 2\tilde{\sigma}_0^2 + \tilde{\sigma}_\tau^2)$. Importantly, while the terms $\sigma_0^2$ and $\sigma_\tau^2$ correspond to variance in the outcomes conditional on what is observed, the terms $2\tilde{\sigma}_0^2$ and $\tilde{\sigma}_\tau^2$ correspond to variance that stems from the fact that reducing the dimension implies that we do not control for the covariates. Note that even if we estimate the unidimensional RD using, for example, a local linear regression we still would not be fully adjusting for variance in the outcomes that could be explained by the full set of covariates, i.e., by $Z_i$. It is this limitation that motivates the next section.

**Residualizing the Outcome**

While the above results illustrate that we can reduce the multidimensional RDD into a single-dimensional RDD, as discussed above and in the methods section doing so throws away potentially useful information. To highlight and motivate our approach, consider a set of exogenous variables $X_i$. These will likely include the covariates $Z_i$ that originally determined treatment, as well as potentially other exogenous variables such as baseline outcomes.

We can then consider what happens if we replace the outcome ($Y_i$) with a residualized outcome $Y_i - g(X_i)$, for some continuous function $g$ of the exogenous variables $X_i$. In other words, we want to understand the behavior of the following treatment effect estimate:

$$\widehat{\tau_g} = \widehat{\mathbb{E}}_n[Y_i - g(X_i) | 0 < D(Z_i) < h] - \widehat{\mathbb{E}}_n[Y_i - g(X_i) | 0 > D(Z_i) > -h]$$

for some bandwidth $h$. Using the same steps as outlined in the proof sketch above (which themselves are a standard application of non-parametric kernel estimators), it is easy to show that under the assumptions stated in the above section that as if $h \to 0$ and $nh \to \infty$, if $h\sqrt{nh} \to 0$ then regardless of $g$ we have:

$$\sqrt{nh} \cdot \left(\widehat{\tau_g} - \mathbb{E}[\tau_i \,|D(Z_i) = 0]\right) \xrightarrow{d} N\left(0, \frac{2}{f_0} \cdot (\sigma_{Y0}^2 + \sigma_{Y1}^2)\right)$$

where $\sigma_{Y0}^2 = \lim_{h \to 0^+} Var[Y_i - g(X_i)|\, D(Z_i) = h]$ and $\sigma_{Y1}^2 = \lim_{h \to 0^-} Var[Y_i - g(X_i)|\, D(Z_i) = h]$.

Note that the key point is that this hold regardless of $g$. That is, the specific function that we use does not affect the consistency of the estimator and only affects the asymptotic variance. Furthermore, the expression of the asymptotic variance also illustrates that the best way to minimize the variance of resulting estimate, i.e., of $\widehat{\tau_g}$, is to simply choose $g(X_i)$ such that it best explains the outcome $Y_i$, and hence minimizing the variance of the residuals (i.e., minimizing $\sigma_{Y0}^2 + \sigma_{Y1}^2$).

Another implication of the discussion above is that rather than doing any special analysis, one can simply use $Y_i - g(X_i)$ as the outcome in a traditional unidimensional RD. Of course, in practice researchers will likely need to estimate the function $g$, which potentially renders this conclusion invalid. In particular, one should be concerned with an approach that first estimates $\hat{g}$, uses $Y_i - \hat{g}(X_i)$ as an outcome in a traditional RD, and then reports standard errors for the traditional RD without accounting for estimation error in $\hat{g}$. However, as we show in the proposition below, this approach is in fact valid if one is willing to use a wider bandwidth to estimate $\hat{g}$ and use a linear function $\hat{g}(X_i) = \theta' X_i$ for some $\theta$.

***Proposition:***
*Consider a two-step estimator, where one first estimates $\hat{\theta}$ via on OLS regression of $Y_i$ on $X_i$ restricting attention to observations such that $-\tilde{h} < D(Z_i) < \tilde{h}$ and then estimates the treatment effect as follows:*

$$\widehat{\tau_g} = \widehat{\mathbb{E}}_n\left[Y_i - \hat{\theta}' X_i \mid 0 < D(Z_i) < h\right] - \widehat{\mathbb{E}}_n\left[Y_i - \hat{\theta}' X_i \mid 0 > D(Z_i) > -h\right]$$

*Then if $h \to 0$ and $nh \to \infty$, we have that $\hat{\tau} \xrightarrow{p} \mathbb{E}[\tau_i \mid D(Z_i) = 0]$. Furthermore, if $h\sqrt{nh} \to 0$ and $\frac{h}{\tilde{h}} \to 0$, then we have that:*

$$\sqrt{nh} \cdot \left(\widehat{\tau_g} - \mathbb{E}[\tau_i \mid D(Z_i) = 0]\right) \xrightarrow{d} N\left(0, \frac{2}{f_0} \cdot (\sigma_{Y0}^2 + \sigma_{Y1}^2)\right)$$

*where $\sigma_{Y0}^2 = \lim_{h \to 0^+} Var[Y_i - \theta_0' X_i \mid D(Z_i) = h]$ and $\sigma_{Y1}^2 = \lim_{h \to 0^-} Var[Y_i - \theta_0' X_i \mid D(Z_i) = h]$ and $\hat{\theta} \xrightarrow{p} \theta_0$.*

***Proof Sketch:***
*We can start by writing the $\widehat{\tau_g}$ as*

$$\begin{aligned}\widehat{\tau_g} = &\left\{\widehat{\mathbb{E}}_n[Y_i - \theta_0' X_i \mid 0 < D(Z_i) < h] - \widehat{\mathbb{E}}_n[Y_i - \theta_0' X_i \mid 0 > D(Z_i) > -h]\right\} \\ &- \left\{\widehat{\mathbb{E}}_n\left[\hat{\theta}' X_i - \theta_0' X_i \mid 0 < D(Z_i) < h\right] - \widehat{\mathbb{E}}_n\left[Y_i - \hat{\theta}' X_i \mid 0 > D(Z_i) > -h\right]\right\}\end{aligned}$$

*The key to showing the result is showing that:*

$$\sqrt{nh} \cdot \left\{\widehat{\mathbb{E}}_n\left[\hat{\theta}' X_i - \theta_0' X_i \mid 0 < D(Z_i) < h\right] - \widehat{\mathbb{E}}_n\left[Y_i - \hat{\theta}' X_i \mid 0 > D(Z_i) > -h\right]\right\} \xrightarrow{p} 0$$

*at which point we can refer to the proof sketch above and/or standard results in nonparametric kernel regression to conclude the result. This follows directly from the fact that the OLS parameters converge at a rate of $\sqrt{n\tilde{h}}$, since the number of observations such that such that $-\tilde{h} < D(Z_i) < \tilde{h}$ is approximately $n\tilde{h}f_0$ (where $f_0$ is defined in the above proof sketch). In particular, we can use standard results on linear regressions asymptotics to conclude that:*

$$\sqrt{n\tilde{h}} \cdot \left\{\widehat{\mathbb{E}}_n\left[\hat{\theta}' X_i - \theta_0' X_i \mid 0 < D(Z_i) < h\right] - \widehat{\mathbb{E}}_n\left[Y_i - \hat{\theta}' X_i \mid 0 > D(Z_i) > -h\right]\right\} \xrightarrow{d} N(0, V)$$

*for some variance V and $\sqrt{\frac{h}{\tilde{h}}} \to 0$ (by assumption). From this, it follows directly that:*

$$\sqrt{\frac{h}{\tilde{h}}} \sqrt{n\tilde{h}} \cdot \left\{\widehat{\mathbb{E}}_n\left[\hat{\theta}' X_i - \theta_0' X_i \mid 0 < D(Z_i) < h\right] - \widehat{\mathbb{E}}_n\left[Y_i - \hat{\theta}' X_i \mid 0 > D(Z_i) > -h\right]\right\} \xrightarrow{p} 0$$

Loosely speaking, the result above relies on two important assumptions: that our estimates of $\hat{g}(X_i)$ are root-n consistent (where "n" here is the effective sample size of the ridge regression) and that the bandwidth of ridge regression is (at least asymptotically) much larger than the bandwidth of the RD analysis. As discussed in Noack, Olma, and Rothe (2021), these conditions can be relaxed if one is willing to employ sample splitting.