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Inequities and Impacts of Investments in New School Facilities

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Abstract

There is growing evidence that investment in school facilities, and new school construction in particular, can improve K-12 student outcomes, particularly for low-income students. Funding for school infrastructure, however, is inequitably distributed. Moreover, given a lack of national data on school facilities, researchers have focused on specific states or districts, leaving contextual variation understudied. This paper contributes to the literature on equity and impact of new school construction spending by examining New York City (NYC), which spent $12 billion on new K-12 school seats between 2005 and 2019. In contrast to prior studies, students who attend new school buildings in NYC, which were built to alleviate overcrowding, are disproportionately high-performing and White, and less low-income. Using detailed student and school building-level data, and a difference-in-differences estimator, I find that attending a new school building causes a statistically significant but small 1 percentage point improvement in attendance rate each year after three years in the new facility. Results for math and ELA scores are also small, but imprecise. These findings suggest policymakers should consider equity with respect to student poverty and performance in school infrastructure allocations, as this may in turn affect the return on investment.

Keywords: school finance, school infrastructure, academic outcomes

Inequities and Impacts of Investments in New School Facilities

National data on school infrastructure is limited, but suggests there is a significant need to both improve facility quality and expand facility capacity. Surveys in 2020 and 2023 found 50% of public school districts need to update/replace multiple building systems, and the average school building is 49 years old (Government Accountability Office [GAO], 2020; National Center for Education Statistics [NCES], 2024). The same surveys found significant space constraints: 31% of schools are using temporary classroom space—the same portion as in 2013 (GAO, 2020; NCES, 2024). In addition, students from low-income backgrounds are less likely to attend school in a building that is in “excellent” or “good” condition, attend older school buildings, on average, and are more likely to attend a school using non-permanent buildings (Alexander & Lewis, 2014; NCES, 2024).

Given the lack of comprehensive national data on K-12 public school infrastructure, research has focused on specific states or districts to explore the relationship between school infrastructure and student outcomes. There is growing evidence that investment in school facilities, and new school construction in particular, can improve student outcomes, especially for low-income and low-performing students (Jackson & Mackevicius, 2024; Lafortune & Schönholzer, 2022), with implications for how scare capital resources should be targeted. Additional research is necessary to confirm whether these findings hold across differing contexts and student populations. This paper provides estimates on the returns to new school construction in New York City (NYC), which spent $12 billion on new K-12 school seats between 2005 and 2019.

Using student-level data from NYC and a difference-in-differences design, I estimate the impact of attending a new or significantly renovated school facility on student outcomes. Students who attend a new school building experience a statistically significant but small 1 percentage point (pp) improvement in attendance rate each year after three years in a new school facility. Results for math and ELA scores are imprecise and should be interpreted with caution, but benchmarking against previous research and an informal cost-benefit analysis suggests the effect size is small.

Rather than implying that facilities do not matter for student learning, in context with the broader literature, these findings suggest that impacts differ depending on who benefits. Students who attend new school buildings in NYC are disproportionately advantaged because new construction is specifically targeted to alleviate overcrowding, and overcrowded schools in NYC have more high-performing and fewer low-income students (as well as more White students). This research adds to recent literature showing spending on education infrastructure remains an area of inequity in K-12 school finance (Blagg et al., 2023).

I also find that, despite the intended goal of new school construction in NYC, students who switch to new school buildings experience an increase in crowding, as measured by the district. They experience a decrease in crowding, however, as measured by enrollment per square foot, as well as a decrease in the pupil-teacher ratio. That is, it is possible both improvements in facility quality and reduced crowding contribute to positive effects on student outcomes. These findings also suggest the district's crowding measure may not capture the tangible ways in which crowding negatively impacts students' education, and raises questions about whether it is an appropriate metric to use for targeting resources.

NYC may wish to reprioritize investments in school infrastructure, given the limitations of their crowding measure and its correlation with characteristics of advantaged students. If the district believes that persistent crowding is harmful to students, declining enrollment post-COVID may allow the district to address capacity challenges using non-construction solutions.

Infrastructure investments in NYC could instead be targeted toward students and projects that research suggests have a higher return on investment: low-performing, low-income students; low-quality facilities; and/or facilities lacking modern building components such as HVAC systems (Biasi et al., 2023). Other districts and states may also want to consider equity in school infrastructure allocations, as this may in turn affect the return on investment.

Literature Review

School Infrastructure Impacts on Students

Research on the effect of school infrastructure spending has historically found small or null effects on student outcomes (Baron, 2022; Brunner et al., 2022; Cellini et al., 2010; Conlin & Thompson, 2017; Goncalves, 2015; Hong & Zimmer, 2016; Martorell et al., 2016; Rauscher, 2020). Voters must approve bond issues (and the accompanying tax increases) to finance school construction in many districts. Therefore, much of this research uses a regression-discontinuity (RD) design, comparing outcomes in districts where the bond issue is just approved to outcomes in districts where the bond issue just fails. While the RD design allows for strong causal claims, data in these studies are mostly at the district, not student, level and lack information on the specific size and scope of investments.[1] In an exception to the latter limitation, a recent study using district-level data from 28 states and the RD design finds the specific type of investment matters, which may explain the mixed findings of prior studies: Spending on infrastructure renovation and upgrades (such as HVACs, roofs, and improvements related to health and safety) raises student achievement while other types of school infrastructure spending, such as athletic fields, land purchases, and transportation vehicle purchases, has no effect (Biasi et al., 2023).

[1] The studies that do not use an RD design are Goncalves (2015) and Brunner et al. (2022a), both of which used an event study design, and Conlin and Thompson (2017), which uses an instrumental variables design (based on the same policy change in Ohio studied by Goncalves). All use district-level data except Martorell et al. (2016), who use student-level data.

Biasi et al. (2023) also find that districts with more low-income students and more Black and Hispanic students benefit more from school infrastructure investments. This echoes findings in another RD study using California data that finds impacts are larger in low-income districts (Rauscher, 2020), and could again explain the mixed findings of other state- and district-specific studies: impacts may depend on which students benefit. Biasi et al. (2023) hypothesize that investments in school facilities are particularly beneficial for students from disadvantaged backgrounds because these students receive smaller private educational investment and attend districts with lower total spending (p. 23). In addition, disadvantaged students attend districts where facilities are in worse conditions, and impacts of infrastructure spending on student outcomes are higher in districts with lower capital stock (Biasi et al., 2023).

Two studies on the effect of new school buildings, specifically, that use student-level data and an event study (dynamic difference-in-differences) design also find significant positive effects on student outcomes. Lafortune and Schönholzer (2022) use data from Los Angeles, California, which built 114 new school campuses at a cost of $10 billion (as reported) from 2002-2017. The construction campaign was successful in both improving facility quality and eliminating overcrowding. They find a 0.1 standard deviation (sd) improvement in math and 0.05 sd improvement in ELA after four years in a new school building; positive effects are concentrated among students with below-median prior achievement, and effects are negative or null for students with above-median prior achievement (although higher-performing students who switch into new school buildings also come from relatively newer facilities, and therefore see less facility improvement). Somewhat similarly, Neilson and Zimmerman (2014) use data from New Haven, CT, which undertook a school construction/renovation campaign specifically to improve facility conditions (no additional capacity was created), completely rebuilding or significantly renovating 37 school buildings from 1998 to 2014 at a cost of $1.4 billion. They find a 0.15 sd improvement in reading scores six years after building occupancy. Though they do not explore heterogeneity in impacts by student characteristics, students in the district are predominantly poor (80% free and reduced-price lunch eligible) and low performing (only 42% are proficient on state exams, relative to 69% statewide). See Appendix A for further details on these two studies to facilitate their comparison to this study of NYC.

A meta-analysis that considers both these two studies of new school construction and the other set of studies on all school infrastructure spending[2] finds the effects of capital spending on student outcomes are positive and significant 5-6 years after projects are funded, or 3-4 years after projects are likely completed (Jackson & Mackevicius, 2024; see Appendix A for additional details on the timing of impacts). As might be expected from the literature this meta-analysis is based on, it finds that effects are larger for less-economically-advantaged populations: test score impacts are about two times as large, on average, for low-income students.

Mechanisms for School Infrastructure Impacts on Students

There are a variety of hypothesized mechanisms for the impact of school infrastructure improvements, and new school buildings in particular, on student outcomes. There is fairly robust evidence that air quality (Gilraine, 2023; Persico & Venator, 2019); temperature (Park, 2020; Park et al. 2020); toxic chemicals (Gazze et al., 2022; Ramadan, 2022); noise pollution (Jaramillo, 2013; Zhang & Navejar, 2015); and access to natural light and green space (Kweon et al., 2017; Li & Sullivan, 2016; MacNaughton et al., 2017) affect student outcomes. Improved facility quality is likely to affect some or all of these environmental conditions. Indeed, as Biasi et al. (2023) demonstrate in their heterogeneity analysis of the impacts of various types of capital

[2] The Jackson and Mackevicius (2024) meta-analysis does not include Biasi et al. (2023). However, as Biasi et al. (2023) note, their findings are similar (see p. 4).

projects, installations and repairs of HVACs and infrastructure spending on the health and safety standards of schools have significant positive impacts of student achievement.[3]

Upgrades in facility quality may also lead to improved student perceptions of self-worth or motivation (see environmental psychology literature, such as Maxwell, 2016 and Syeed, 2022), teacher working conditions (Horng, 2009; Uline et al., 2010), and access to specialized instructional/communal spaces (e.g., gymnasiums, see Taras, 2005; or cafeterias, see Koch et al., 2020), though there is less evidence demonstrating a causal link between these mechanisms and student outcomes.

When new school building construction is undertaken to increase capacity, it may also affect student outcomes through reductions in overcrowding, though there is almost no research on overcrowding *per se*. Reduced overcrowding may affect student outcomes through similar mechanisms as facility improvements, by positively affecting student self-worth, teacher working conditions, and/or expanding access to specialized instructional space or other communal spaces. As an example, Prescott et al. (2022) find overcrowding is related to school meal take-up (though the direction of the relationship differs by school level). Some of the specific consequences of overcrowding—increased class size (Lafortune & Schönholzer, 2022), use of temporary classrooms (Alexander & Lewis, 2014; Haimson, 2014), and use of a split schedule (Lenard et al., 2020; Lafortune & Schönholzer, 2022), where the school day or school year is staggered so that not all students in a school are physically present at the same time—may negatively impact student performance. Overcrowding may also directly affect a buildings' state

[3] Biasi et al. (2023) find positive impacts of most types of infrastructure spending on student achievement, with the exception of athletic facilities, land purchases, and transportation vehicle purchases. Impacts are largest, however, for spending on HVACs (0.17 sd), renovations of plumbing systems, roofs, and furnaces (0.13), STEM equipment (0.12 sd), and health and safety improvements (0.11 sd). Impacts are also significant for construction, renovation and expansion of buildings and classroom space (0.07 sd) and spending can be classified in multiple categories, so the same bond election may contribute to multiple estimates (p. 22).

of repair (i.e., cause facility quality to deteriorate more quickly). However, the evidence on these mechanisms is mixed or limited.

School Infrastructure Financing

While there has been significant progress in improving the equity of education spending, particularly through school finance reforms (Lafortune et al., 2018), inequity in school infrastructure spending, specifically, remains. An analysis of data from a 2013 national survey of school building conditions finds students from low-income backgrounds were less likely than other students to attend school in a building that is in "excellent" or "good" condition (Alexander & Lewis, 2014). More recent evidence also suggests low-income students are more likely to be in districts that underinvest in school infrastructure relative to districts with more high-income students (Blagg et al., 2023).

This inequity may be driven, in part, by how school infrastructure investments are financed. State contributions for school capital spending vary widely (Syverson, 2021): capital outlay revenue comes largely from local governments and some states do not offer any substantial support for new and renovated buildings (Blagg et al., 2023). States that fund a higher share of school infrastructure costs have lower levels of this spending on average but also a smaller gap in school infrastructure spending between wealthy and poor districts within the state (Biasi et al., 2021; Brunner et al., 2023; Filardo et al., 2010).

Inequities may also be driven by the specific financing scheme (which is also affected by state policy). A typical way to finance school infrastructure investments is through bond referenda: Almost all states—47—require voter approval to issue bonds (Biasi et al., 2021). The need to put budgeting questions to a vote and higher passing thresholds for public referenda on spending (e.g., requiring 66% or 60% approval rather than 50%) reduces the level of spending

(Funk & Gathmann, 2011; Grosz & Milton, 2022), suggesting a divergence in preferences between voters and elected officials. Biasi et al. (2023) find larger impacts of per capita spending on test scores in states than require a supermajority because, despite spending less *overall* on school infrastructure, these states are more likely to finance the specific types of school infrastructure spending that raise test scores. They similarly find that districts in states with debt limits see larger impacts of school infrastructure spending on student outcomes than those in states without debt limits. That is, in districts where state policy constrains spending through supermajority requirements and debt limits, districts may "prioritize projects that are useful for students and ultimately only pass those that are truly essential" (Biasi et al., 2023, p. 28). While these state policy constraints may positively impact the efficiency of spending, it is unclear if they produce optimal levels of spending or how they impact equity of spending.

Regardless of state policy, low-income districts may struggle to issue bonds for school infrastructure investments given lower credit ratings and higher interest rates (Backer, 2022). In general, higher interest rates lead to lower local infrastructure spending (St. Clair, 2022) and states with policies to improve districts' credit ratings (and, in turn reduce district bond interest rates) have higher per-student capital spending (Yang, 2023). That is, infrastructure spending may be lower than optimal in districts serving high numbers of low-income students with challenges to accessing financing, contributing to inequities across districts.

There are other mechanisms for financing school infrastructure investments, but little evidence on how these alternate financing schemes might affect the level, equity, and impact of such investments. For example, even in states that require voter approval to issue bonds, fiscally dependent districts seek appropriations for the local municipal entity, which in some cases—including Chicago, Boston, and NYC—are allocated by elected officials without voter approval and not directly tied to tax increases.[4] There is also little evidence on intra-district allocation of school infrastructure spending—that is, whether infrastructure spending *within* districts is equitable—and if and how this is also affected by state (or district) policy and/or the financing scheme.

Summary & Contribution

There is compelling evidence that school facilities are an important component of the education production function, particularly for low-income students. However, these may be the students least likely to receive school infrastructure investments, depending on state and district financing schemes. Because there is no national data on school infrastructure, whether the benefits of school infrastructure investments are consistent across contexts, types of investments and financing schemes, and the population of students who benefit, is underexplored (with the exception of Biasi et al., 2023, as discussed). This study contributes to the literature on school infrastructure investments by using data on new school construction in NYC to estimate impacts on student outcomes. This study also contributes understanding about the *intra*-district allocation of school infrastructure investments in a large, urban district, which I document in my discussion of the NYC context.

NYC Context

New School Construction in NYC

Though the NYC school system is a single district, serving 1.1 million public school students, for administrative and planning purposes it is divided into 32 geographic community

[4] Still other financing schemes exist. For example, Miami-Dade has a certificates of participation (COP) fund financed by a discretionary property tax that does not require a referendum. Windfall revenue is sometimes used for school infrastructure investments (e.g., Brunner et al., 2022; Marchand & Weber, 2020). This is typically seen as better practice than using one-time revenue for ongoing operating costs, which would subject districts to a so-called fiscal cliff (as has happened with some federal education funding in the wake of COVID-19, see Sharps & Champeny, 2023). However, when windfall revenue is spent, the abundance of funds and/or urgency to use the funds may lead to inefficient allocations.

school districts (CSDs) ranging in size from 6,600 to 64,000 K-12 students (as of the 2019-20 school year, not including charter school students; see NYC Department of Education [DOE], 2020). Serious concerns about overcrowding in NYC schools date to the 1990s (Fernandez & Timpane, 1995; Rivera-Batiz & Marti, 1995), and have persisted. As such, between 2005-2019, NYC spent $12 billion (2019$) on K-12 school construction projects, the vast majority of which—$11.1 billion—went to building *new* K-12 seats to address overcrowding (with the remaining going to facility replacement). Because the focus was on creating additional capacity, this construction took many forms—some projects were additions to existing school buildings, some were renovations of existing buildings bought or leased by NYC DOE, and some were ground-up new construction. Renovations of existing buildings varied in their intensity—for example, a number of renovations were to former Catholic school buildings (Jorgensen, 2020) that did not require significant changes to be suitable for use as a public school.

In NYC, school infrastructure spending levels and allocations are set by the mayor and approved by the City Council (bond financing is not subject to voter approval; that is, the approval process for the capital budget is the same as the expense budget). The NYC DOE and School Construction Authority (SCA) identify areas needing new school construction. From 2005-2019, the NYC DOE and SCA determined an identified seat need for sub-districts (geographic areas of the City within CSDs) based on overcrowding levels, new housing starts, and adjusted qualitatively based on other contextual information. In practice, overcrowding levels were the primary driver of identified seat need because forecast housing starts used for identified seat need were artificially low (Douglas et al., 2018).

Overcrowding in NYC

NYC's current measure of school crowding is called utilization: a school's enrollment divided by its capacity as defined in the NYC DOE's annual Enrollment, Capacity, and Utilization Report, also known as "the Blue Book" (Appendix B provides a more detailed description of the utilization measure and data, also discussed further in Data and Measures). Overcrowding is concentrated in specific CSDs, rather than citywide. Areas of overcrowding, and therefore areas of new school construction, have been largely consistent since the early 2000s. Figure 1 shows the number of K-8 seats needed according to each of the last three five-year capital plans and the actual number of K-8 seats constructed, by CSD (seats for Grades 9-12 are not included, because high school seat need is identified at the borough level). Comparing the figures showing identified seat need and actual seats constructed, it is clear that in some CSDs, constructed seats have fallen short of identified need. This is primarily due to limited funding, though there are other challenges to constructing schools: locating suitable sites for new school buildings in an already densely built city, the capacity of the construction industry itself to take on city contracts, and site-specific conditions where new construction is happening (e.g., environmental remediation, which may prolong the typical construction timeline) (Douglas et al., 2018).

Despite the significant investment in in new school seats and steady or declining enrollment in NYC public schools over this period, significant overcrowding (as measured by the district) persisted: from 2008 to 2019, the average utilization rate experienced by K-8 students increased from 88% to 94%, and the portion of K-8 students in overcrowded buildings increased from 26% to 39% (increases are similar when considering students in all grades K-12). This was due to two factors: (1) the net increase in K-12 school seats is less than the number of new seats built becuase some schools and seats came offline and (2) mismatches in school capacity and school enrollment in particular grades and geographic areas. New construction does not reflect the net change in available seats because the school system also lost seats over this period. Capacity was lost due to buildings leaving the system: for example, 78 buildings with a total of 22,754 K-12 seats in 2008 were no longer used as public schools as of 2019. Capacity was also lost due to changes in capacity in existing buildings, particularly after the introduction of universal pre-K in 2015, which reduced K-12 capacity. Despite capacity loss offsetting some of the capacity growth from new school construction, citywide there are more than enough K-12 seats for all students, and have been over the entire time period for which data are available (since at least 2008). However, the location of school seats may not match where students live or where there is demand (Edwards, 2019). For example, in 2019, four CSDs in Queens (CSDs 20, 25, 26, and 28) had average utilization rates at or above 100%, while four districts in central Brooklyn (CSDs 16, 18, 23, and 32) had utilization rates at or below 65%. In addition to geographic mismatch, there can be grade level mismatch between available seats and the need for seats; citywide, schools serving elementary grades have higher utilization rates than middle schools and high schools. NYC has been reluctant to use non-construction strategies such as capping enrollment or rezoning to alleviate overcrowding (Douglas et al., 2018; Edwards, 2019). That is, capacity as calculated by NYC is not binding; many schools have enrollment greater than capacity.

Overcrowding: Implications for Schools and Correlates

In practice, NYC schools handle overcrowding in a variety of ways. NYC uses trailers—called transportable classroom units (TCUs)—as educational space on some overcrowded campuses. In 2007, approximately 11,000 students were educated in 400 TCUs citywide (O'Hagan et al., 2018), though this had decreased to less than 400 students across 200 TCUs as of 2019 (NYC SCA). Parents, school staff, and advocates allege that instruction is held in nonpedagogical spaces such as hallways and closets in particularly overcrowded schools (Haimson, 2014), though there is no quantitative data on the prevalence of these practices. In addition, schools can set their own daily schedules, and high schools in particular often use staggered start times ("multi-session" schools: for example, some students may attend class from 7:00am-1:00pm while other students attend class from 10:00am-4:00pm.). However, the most obvious implication of overcrowding is higher class size, and overcrowded schools do have higher pupil-teacher ratios as shown in Figure 2A (though other factors—particularly operating funding for teachers—also affect class size). It is also possible that for some schools, the NYC DOE's capacity formula does not accurately capture the capacity of their space and the school is able to operate safely and effectively at a utilization rate above 100% (the NYC DOE's capacity and utilization measures are discussed further in Data and in Appendix B).

Intuition may suggest school overcrowding signals poor school quality or an underprivileged student population, due to the potential negative effects of overcrowding on a student's physical environment and educational experience. Indeed, in Los Angeles the student population in the overcrowded area of the school district was predominantly low-income and Hispanic (Lafortune & Schönholzer, 2022). However, overcrowding in NYC schools often results from high demand (Edwards, 2019). As a result, in NYC the student body in overcrowded schools is less free- or reduced-price lunch (FRPL)-eligible (Figure 2B), more White (Figure 2C) and Asian (Figure 2D), and less Black (Figure 2E; there is no correlation with the percentage of Hispanic students, see Figure 2F). In addition, average performance on state math and ELA exams is higher in overcrowded schools (Figure 2G and 2H).

School Choice and Assignment to New School Buildings in NYC

That high demand results in overcrowding also reflects that NYC is a choice-rich public school district. While 72% of kindergarten students attended a zoned school (i.e., their neighborhood elementary school) in 2008, by 2017 that portion had decreased to 60% (Mader et al., 2018).[5] NYC has had centralized middle school choice since 2012, but the choice options and processes differ across CSDs (Hemphill et al., 2019). Only 54% of K-8 students attended a zoned school from 2010-19 (the years in which there is available student-level data on zoned schools), and all eighth-grade students participate in a citywide high school choice process.

Because of the myriad choice options and localized crowding conditions, students end up in new school buildings in a variety of ways. New school buildings may house entirely new schools; enrollment in these new schools may be based on zoning and/or some degree of choice (for example, a lottery with priority given to certain students based on their socio-economic characteristics or location). When new school buildings house entirely new schools, they may add one grade at a time (for example, a new elementary school will first enroll a class of kindergarten students, and the following year will enroll a new class of kindergarten students while the first cohort advances to first grade, etc.). Alternatively, existing schools may move into new school buildings. New students may choose to attend (again, based on rezoning and/or choice) while existing students may switch schools because of the change in location. An existing school moving into a new building may also involve a grade truncation or expansion (i.e., a K-5 school becoming a K-8 school or vice versa). See Appendix C for two specific examples of how students and schools were assigned to the new school buildings considered in this study. Because students are not randomly assigned to schools (or school buildings) I consider how the choice to attend a new school building might introduce selection bias in the methods and discussion.

[5] Community Education Councils (CECs), education policy advisory bodies with elected parents and politically appointed representatives, approve elementary and middle school zone lines for their respective CSD.

NYC School Facility Quality

While new school construction in NYC is not intended to improve facility quality, it is likely that students who attend new school buildings see a significant improvement in facility quality. As shown in Figure 3, the median age of a NYC school building is 65 years, and as shown in Figure 4, 56% of NYC school buildings are in “fair” condition, the middle rating on a five-point scale. Though I don’t observe data on the quality of new school construction in NYC, new school buildings will have features that positively affect student outcomes as discussed in the literature review. For example, all newly constructed school buildings have central air conditioning, and will not have building materials now known to cause health problems.

Implications from Prior Research & the NYC Context

Overall, the research suggests that if students who attend new school facilities in NYC see significant improvements in facility quality and/or reductions in crowding, they may also experience improved outcomes. However, because overcrowding persisted, and because the students who benefit from new school facilities in NYC are less low-income and higher performing than students who benefit in settings of prior research, it is also possible that attending a new school building has no effect on these students’ outcomes, or smaller effects.

Data, Measures, and Sample

Data

This analysis relies on multiple data sources on NYC students, schools, and school buildings. Student-level administrative data from the NYC Department of Education (DOE) for the academic years 2006-2019 (academic years are referred to by the Spring calendar year) include students’ gender, race/ethnicity, free and reduced-price lunch eligibility, English-language learner status, grade level, school, a residential move indicator, and unique identifier (ID) so they can be tracked over time. I consider three student outcomes: attendance rate, which is available for students in all grades, and state math and ELA test scores (standardized to mean zero, standard deviation one, by subject-grade-year), which are available for students in Grades 3-8. The student-level data are supplemented with publicly available school-level data from the New York State DOE, and include information on teacher experience and qualifications: the portion of teachers teaching out of certification, as well as pupil-teacher ratios. Student-level data are also aggregated to obtain school-level measures of student demographics.

I also assemble a novel data set on NYC school buildings using information from the NYC School Construction Authority (SCA) and a variety of publicly available sources. Annual Enrollment, Capacity, and Utilization reports (known as the "Blue Book") identify each school and each building's total capacity, total enrollment, and utilization (enrollment divided by capacity). See Appendix B for a more detailed discussion of these utilization and capacity data.[6] Crucially, the Blue Book data also provide a school-building crosswalk, because the student-level data only identify a student's school, not the building they attend. However, most students in traditional public schools—85%—are in schools located entirely within one building.[7] Data on new school construction comes from the closeout reports of the three most recently completed NYC DOE Five-Year Capital Plans, which cover the fiscal years 2005-2009, 2010-2014, and 2015-2019 (fiscal years run from July 1-June 30, so they align with academic years). The Five-Year Capital Plans include the address of new capacity projects (used to match the projects to the appropriate school building ID), the projects' total cost, and construction timeline. I also have cross-sectional data from NYC DOE on school buildings that includes the school building ID,

[6] I am aware of only one published study that used these data (Prescott et al., 2022), and they only use cross-sectional utilization data for 2014.
[7] For schools with multiple buildings with unique building IDs, students are assumed to attend the building with the largest share of enrollment for the school. The 85% figure reflects the percentage of all K-8 student-year observations from 2008-2019 in a traditional public school (i.e., not a special education only school, charter school, or alternative high school) that are located entirely in one building.

year constructed, and total square footage as of 2015. Some of these data are missing (including for buildings added to the system after 2015), so where necessary, I supplement them using publicly available information from the 2020 Building Condition and Assessment Survey (BCAS), a detailed annual survey of school facility conditions.[8]

Overcrowding Measures

Capacity is based on the size and functions of rooms in a building, using information reported in an annual survey (the Principal Annual Space Survey, or "PASS") that is verified by the School Construction Authority. Therefore, utilization can change within the same building over time due to both changes in enrollment and changes in capacity. Most notably, the introduction of universal pre-K in 2015 led to a decline in K-12 capacity in buildings that opened more pre-K classrooms, since this space was no longer available to serve K-12 students. Capacity can also change due to internal reconfiguration of space and/or changes in the grade levels served, since expected class sizes differ by grade.

Given the many factors that can contribute to a change in capacity as calculated by NYC DOE, I also operationalize crowding as students per square foot, using building-level enrollment data from the Blue Book and building square footage from the building-level data. The correlation between building-level utilization and students per square foot is only 0.59. This is likely due to the many non-instructional spaces that add to a school buildings' square footage but that do not contribute to NYC DOE's calculation of capacity: gymnasiums, cafeterias, auditoriums, libraries, offices for the principal, guidance counselor, and/or nurse, and rooms less than 240 square feet. The number and size (and even existence) of these facilities varies widely

[8] The BCAS is an annual survey conducted by architects, electrical engineers, and mechanical engineers to determine the baseline condition of all of the DOE's facilities. Buildings' main systems and components are each rated on a scale of 1 ("good") to 5 ("poor"). At any point in time only the most recent BCAS reports are publicly available online as PDFs.

across school buildings.

Sample

To identify the effects of attending a new/significantly renovated school building on student outcomes, new school buildings with only minor renovations[9] and school campuses with a new addition[10] are excluded. Blue Book data are only available for the years 2008 on, so this analysis focuses on 63 new/extensively renovated school buildings that open between 2008 and 2019 and serve students who switch into the new building in Grades 3-8 (though student data from 2006-2007 are still used to establish pre-trends). The locations of the projects considered in this analysis are mapped in Figure 5, along with 2008 utilization rates by CSD, reflecting the expected correlation between overcrowding and new school construction. The map also reflects the geographic mismatch between K-12 school seat supply and demand previously discussed.

Table 1 shows details of these construction projects, disaggregated between new construction and major renovations.[11] New construction projects cost more overall and per square foot, create a larger number of seats, and take longer to complete. However, the average investment for major renovations is still significant (an average cost of $36 million overall, and $534 per square foot). For all 63 projects, the average time from design to completion is approximately four years and the average cost per seat (2019$) is $117,814. This is higher than the cost of previously studied new school construction projects: the average cost per seat in LA

[9] Projects that cost less than $10 million, create less than 100 seats, or projects in buildings where the BCAS does not list any major renovation are excluded. Many of these less-intensive renovations are former Catholic school buildings that required relatively little infrastructure investment to be suitable for NYC DOE use, and therefore may lack the major facility upgrades through which new school facilities positively impact student outcomes.
[10] I define additions as buildings that do not receive a unique building ID. Students who attend a school in a building with an addition may be educated in the old (main) building, the new building (the addition), or both.
[11] These 63 projects have a total cost of $5.5 billion, and account for approximately 55% of spending in the 2008-2019 period on completed projects; as a reminder, high schools, additions, and projects that did not involve major renovations are excluded from the sample.

was \$96,247, and the average cost per seat in New Haven was \$91,634 (2019\$).[12] It is unclear why per seat spending is higher in New York City. Research on transportation infrastructure costs suggests differences in labor and procurement costs drive part of the cost differential for infrastructure in NYC (Goldwyn et al., 2023); it is possible these factors similarly affect the cost of new school construction. In addition, these costs include the cost of land acquisition (this cost is not disaggregated at the project level). I return to the issue of the cost of new school construction in NYC in the discussion.

The student sample consists of students in traditional public schools with outcome measures for at least two years, since the analysis relies on within-student changes. Students who switch into a new school building must have the outcome measures for the year before treatment (since this is the base year for estimates; see Methods) and at least one year after the switch to be included in the sample. Students who attend an excluded building (i.e., a building that receives a minor renovation or addition) or multiple new school buildings are excluded from the sample. In addition, because information on charter school facilities is only available if the charter school is located in an NYC DOE-owned or leased building, charter school students are excluded from the sample. Finally, students in other non-traditional public schools, such as special education only schools, are excluded from the sample; these schools are often located across many school buildings and these students' educational programs differ significantly from the average student.

Table 2 presents the sample, which includes students in Grade 3-8, the grades with subject-grade state standardized tests in NYC.[13] Column 2 reports means for students who never

[12] The cost per seat in LA and New Haven is taken from Jackson and Mackevicius (2024) and converted to 2019 dollars.
[13] Results for attendance rate, which is available for all students in Grades K-12, are very similar when using a larger sample of students in all grades K-12 (see Appendix Figure E2). Therefore, results for all outcomes are presented for the Grade 3-8 sample since these are the grades for which there are grade-standardized tests in math and ELA.

attend a newly constructed school building during the sample period (“never-treated”) across all student-year observations and Column 3 reports means for students who switch into a newly constructed school building at some point during the sample period (“ever-treated”) for the year before the switch.

Ever-treated students are more likely to be White, less likely to be FRPL-eligible, and have significantly higher math and ELA scores (as well as higher attendance rates) than never-treated students. These differences in demographics and outcomes are expected based on the correlates of overcrowding previously discussed: the students in overcrowded K-8 schools are more likely to have a new school building open near them, and therefore more likely to switch into a new school building. Indeed, ever-treated students are more likely to be in an old and more crowded building in the year prior to their switch, as measured by both utilization and enrollment per square foot. Somewhat surprisingly, ever-treated students are actually less likely to attend a school building with a TCU (trailer classroom) in the year before they attend a new school building, or be in a school that is co-located. Finally, students who switch into a new school building are 1.7 pp more likely to make a residential move in the year before the switch than the average annual percentage of residential moves among never-treated students. I consider how this may affect results in the robustness checks.

Methods

The challenge in determining the impact of attending a new school facility on student outcomes is that students systematically sort between schools based on differences in preferences that are correlated with student characteristics and outcomes, particularly in a choice-rich district. I use a student fixed effect to eliminate biases due to time-invariant differences between students who attend different schools (and school buildings).

To start, I estimate impacts using a dynamic difference-in-differences model:

$$y_{it} = \alpha_i + \gamma_g + \delta_t + \sum_{k=\underline{K}}^{\overline{K}} \beta_k \mathbb{1}(t = t_i^* + k) + \varepsilon_{it} \quad (1)$$

where y_{it} is outcome *y* for student *i* in year *t*, α_i is a student fixed effect, γ_g is a grade fixed effect, δ_t is a year fixed effect, and β_k captures the effect of attending a new school building *k* years relative to the first year a student attends, t_i^* (*k* = 0 in a student's first year attending a new school building).[14] Effects are measured relative to year *k* = -1, which is excluded. Event time *k* is not binned, in line with recent research that suggests this may bias event study estimates (Sun & Abraham, 2021). In practice, most students do not have data for more than three years before attending a new building and do not attend a new school building for more than three years (*k* = [-3, 2]).[15] However, because prior literature has typically reported effects after three years in a new school building, I present results for *k* = [-3, 3], though the results for β_3 are less precise and based on a smaller subset of the ever-treated students. Standard errors are clustered at the student level, to account for correlations in outcomes. Improvements in school facility quality are likely to affect a student in all years they are in the new facility, so the dynamic estimator captures changes in achievement that accumulate over time with additional exposure.

The contemporary difference-in-differences (DID) literature has now established that when there are multiple periods and variation in treatment timing (staggered implementation)—as is the case in this study—traditional event study models may include forbidden comparisons: the use of early-treated units as controls for later-treated units (see Roth et al., 2023 for a

[14] The inclusion of time-varying controls can introduce bias in a traditional event study model (Goodman-Bacon, 2021). I test the sensitivity of the model to including time-varying student controls and find the results are almost identical (see Table 4).
[15] Each event from *k* = -3 to *k* = 2 has at least 10,000 observations, and this set of observations (*k* = [-3, 2]) are 96% of all observations for treated students. See Appendix D for the number of observations, by event time, for each outcome.

review).[16] To estimate effects without these forbidden comparisons, I use the event study estimator proposed by Callaway and Sant'Anna (2021), which estimates a group-time average treatment effect on the treated, denoted ATT(g, t).[17] In this case, the first year in which a student attends a new school building determines their group (g = {2008, . . . , 2019}) and the times are the years before and after treatment (t = {2006, . . . , 2019}). The estimator uses all sensible two-by-two difference-in-difference comparisons.[18] As an example, ATT(2008, 2008) is estimated by comparing outcome changes for students first treated in 2008 (relative to the year before treatment, 2007) to outcome changes between 2007 and 2008 for students never treated.[19] Though the panel is unbalanced, because students are not observed (or may not have outcome data) for every year, each ATT(g, t) is estimated using only observations that are pairwise balanced (i.e., students observed with the outcome of interest in both *t* and the year before treatment, *g*-1).

The advantage of the Callaway and Sant'Anna estimator is that it makes transparent exactly which units are being used as the control group—in this case, never treated students. In addition, the weights used to aggregate heterogeneous effects are explicitly specified, rather than determined by OLS; in this case, the group-time treatment effects are aggregated to generate a

[16] This creates a conceptual problem, in that the control group is not transparent and estimates cannot be straightforwardly interpreted. Specifically, in this case, our counterfactual is not actually students who attend an old school building, because students who switch into a new school building in the earlier years of the data (e.g., 2008) will be used as controls for students who switch into a new school building in later years of the data. Empirically, this can result in treatment effects for some units and time periods receiving negative weights, which can attenuate effect estimates or even reverse their sign in extreme cases. Even in the absence of negative weights, the aggregation of treatment effects when there are heterogeneous effects across treated groups is not transparently determined (Roth et al., 2023).

[17] This estimator is implemented using the csdid command in Stata (Rios-Avila et al., 2021).

[18] In this case, there are 156 (13 x 12) potential comparisons: 13 years (2006-2019, with the year before treatment serving as the reference year) and 12 groups (2008-2019). Fewer comparisons are actually estimated because most students are observed for a maximum of six times (once each in Grades 3-8), so data will not be available, for example, to estimate treatment effects in 2019 for the group of students first treated in 2010.

[19] I use the "long2" option to set the year before treatment (event time -1) as the comparison year for pre-treatment estimates, so that pre-treatment estimates are constructed symmetrically to post-treatment estimates, which is comparable to traditional dynamic difference-in-differences estimators (Roth, 2024).

weighted average of the treatment effect for each event time, comparable to the interpretation of estimates from the dynamic two-way fixed effects model (Model 1). For example, the weighted average of the treated effect for event time 0 (the first year a student is in a new school building) includes ATT(2008, 2008), ATT(2009, 2009), ATT(2010, 2010), etc.

Identification of the effect of attending a new school building is based on the parallel trends assumption: the outcome change would have been the same for treated and untreated students in the absence of treatment (Roth et al., 2023). The counterfactual is attending an old school building. That is, the assumption is that the outcome trends for students that switch into new school buildings would have followed a parallel path to outcomes trends for students who do not, after accounting for fixed differences between students, grades, and years.

It is common to gauge the plausibility of the parallel trends assumption by assessing whether outcome trends in the years before treatment are parallel between treatment and control groups. That is, for $k < 0$, nonzero coefficients would suggest differing trends prior to treatment, and the counterfactual is not valid. Even in the absence of differing pre-trends, if there are contemporaneous shocks also correlated with outcomes when a student switches into a new school building (i.e., time-varying selection), effect estimates may be biased. For example, if students sort into new buildings after residential moves that also affect outcomes, estimates will capture both the effect of the residential move and the new school building. In Appendix E, I present a number of robustness checks, addressing this concern about time-varying selection (and other issues); these are also discussed further in Results.

Results

Mechanisms: Changes in School Crowding?

To descriptively show how students' schools change after they switch into a new school building, I estimate the event study (equation 1) with building and school characteristics as the outcome. Table 3 presents these estimates for event time 0-2 (after students have been in a new building for three years) for ease of presentation, and to capture effects that may take a few years to materialize. As expected, students see a significant decline in building age of 61 years as soon as they switch into a new school building. Students who switch into a new school building see a decline in crowding in the first year, as measured by both utilization and enrollment per 1,000 square feet. However, after this, utilization increases, and after three years in a new school building utilization is 6.3 pp *higher* than the utilization of a student's building prior to the switch. In contrast, enrollment per 1,000 square feet remains lower than in the building they left (by about one student per 1,000 square feet, a 10% decline from the baseline average of 10). Many non-instructional spaces add to a school buildings' square footage but do not contribute to NYC DOE's calculation of capacity: gymnasiums, cafeterias, auditoriums, libraries, offices for the principal, guidance counselor, and/or nurse, and rooms less than 240 square feet. Therefore, the decline in enrollment per square foot after attending a new school building, despite the increase in utilization, may reflect more and/or larger non-instructional spaces (e.g., gymnasiums, auditoriums, cafeterias). Students who switch into a new school building also see a statistically significant 0.7 reduction in the pupil-teacher ratio (PTR) after three years in a new school facility (this is a 5% reduction in the baseline PTR of 14.6). This reduction in crowding as measured by enrollment per square foot, and the similar reduction in PTR, may contribute to positive effects on student outcomes.

There are also some changes to teachers and peers when students switch to new school buildings. The percentage of teachers teaching out of certification increases.[20] The percentage of

[20] Many of the students who switch into new school buildings are switching in Grade 6, some into middle schools, which in NYC have higher portions of teachers teaching out of certification than both elementary (e.g., K-5) and K-8 schools (O'Hagan et al., 2024, p. 13).

White students in the school increases by approximately 2 pp and the percentage of FRPL-eligible students decreases by 2-4 pp. It is possible these changes in peers also contribute to effects on student outcomes.

Overall, the results suggest it is possible that both improvements in facility quality and changes in crowding drive positive effects of attending a new school building. In addition, the inconsistency of increasing utilization and decreasing enrollment per square foot and PTR highlights the limitations of the NYC DOE's crowding measure to capture the tangible ways in which crowding might negatively impact students' education, and raises questions about whether it is an appropriate metric to use for targeting resources.

Effects on Attendance and Test Scores

Figure 6 and Table 4 show the results from both the event study specification and the Callaway and Sant'Anna (C&S) estimator for all three outcomes considered. Overall, the results from the two estimators are fairly similar, though there are larger differences between the two for the test score results than the attendance results, particularly for event time 3 (after four years of attending a new school building). This is expected because fewer students are observed at this event time (see Appendix D for the number of observations by event time).

The attendance rate results (Figure 6a) suggest attending a new school building causes a small but statistically significant increase in student attendance rate of 1 pp after three years in a new school building. Though there is a statistically significant pre-trend in the Calloway & Sant'Anna estimate, it is small (approximately 0.1 pp) and steady or declining before the move to a new school building, so the positive impact on attendance reflects a break in the pre-trend. While the estimates suggest attending a new school building improves attendance, this effect size is smaller than the effect estimated in the LA context, where students attended six additional days of school per year after four years in a new school building (Lafortune & Schönholzer, 2022); a 1 pp increase in attendance in a 180-day school year is less than two days.

Results for math and ELA test scores are less conclusive. There is a decline in performance when students first switch to a new school building, though this is expected based on prior research—the disruption of switching school buildings may negatively impact student outcomes in the short-term (Lafortune & Schönholzer, 2022). Point estimates suggest improvements of 0.008-0.08 sd in math and 0.01-0.06 sd in ELA after 3-4 years in a new school building. However, these results are not statistically significant. Given the imprecision, I cannot rule out effects similar in magnitude to that found in prior research: a 0.1 sd improvement in math, and a 0.05 or a 0.08 sd improvement in reading after four years in a new school building (Lafortune & Schönholzer, 2022 and Neilson & Zimmerman, 2014, respectively). I also cannot rule out null effects. It is possible estimated null effects could result from ceiling effects (students who switch into a new school building are higher performing on average). However, the distribution of test scores for never treated and ever treated students are very similar (see Appendix Figure E3 for comparisons of the math and ELA test score distributions), so it seems unlikely that ceiling effects would drive a null result. Lack of precision, as well as variations in findings in my robustness analyses (discussed below), caution against drawing strong conclusions regarding the impact of new school facilities on student math and ELA performance in NYC. I revisit the interpretation of these inconclusive test score results in the discussion.

Heterogeneity

Because the prior literature on new school construction suggests that effects may be larger for low-income students, I estimate the effects on student outcomes separately for students who are always, ever, or never FRPL-eligible to see if this is also true in the NYC context. This follows the taxonomy of students as persistently, transitorily, or never disadvantaged in Michelmore & Dynarski (2017), based on the idea that the portion of years a child is eligible for FRPL is a better proxy for income and reflection of economic disadvantage than point-in-time FRPL-eligibility (FRPL-eligibility itself is widely acknowledged to be a limited approximation for income or socio-economic status, see Fazlul et al., 2023). The results are presented in Figure 7. Attendance rate effects are driven by always- and ever-FRPL-eligible students, who have a 0.9 pp higher attendance rate after three years in a new school building, compared to 0.5 pp for never-FRPL-eligible students. Results for math and ELA remain imprecise, the subsamples show differing pre-trends, and the point estimates do not consistently suggest that effects are larger for ever-FRPL-eligible students.

Robustness

Appendix Figure E1 shows each group-time average treatment effect (the ATT(g,t)) from the main attendance rate results for event times [-3, 3]. For each group, effect estimates post-treatment (after switching into a new school facility) are positive, suggesting the results are not driven by one group of students.

Appendix Figure E2 shows results for a number of different samples for each of the three outcomes: attendance (Panel A), math scores (Panel B), and ELA scores (Panel C). Confidence intervals on the robustness estimates are not shown for clarity, but estimates and standard errors are also presented in Appendix Table E1. I review each robustness check below.

The first robustness check only applies to attendance rate, and estimates results using a larger sample that includes students in Grades K-12 (the main estimates are limited to the tested gades, Grades 3-8). Results are largely consistent with the main findings: relatively small or null pre-trends, and an approximately 1 pp improvement in attendance rate 3-4 years after attending a new school facility (event time 2-3).

To address concerns that results may be affected by different students contributing to different event time estimates, Appendix Figure E2 and Table E2 present results using a balanced subsample of ever-treated students who I observe in all event times two years before and three years after attending a new school building (i.e., $k = [-2, 2]$), compared to all never treated students. The results are similar.

Students who switch to a new school building are 1.7 pp more likely to have made a residential move in the year prior to the switch than the average rate of residential moves among the comparison group. This will bias results if the residential move is correlated with changes in outcomes independent of attending a new school facility. As a robustness check, I drop students who switch to a new school building who also made a residential move in the year prior or in the same year (i.e., I drop students who made a residential move in $k = -1$ or $k = 0$). Appendix Figure E1 and Table E2 present the results, which are very similar for all student outcomes.

The trends of students who will, but have not yet, switched into a new school building might be theoretically more appropriate counterfactuals for the outcome trends of students who switch into a new school building. Appendix Figure E2 and Table E2 presents results for a sample limited to ever-treated students. The attendance rates results for event times 0 and 1, after 1-2 years in a new school building, are similar to the main results. However, estimates for event times 2 and 3 are very noisy and go to zero. This is because there are very few not-yet treated students in a given year and grade to use as comparison for the treated students in event times 2 and 3.[21] The imprecision in estimates for event time 2 and 3 are therefore expected. Math and

[21] As an example, estimating the ATT(2010, 2012), which is the treatment effect after three years in a new school building for students first treated in 2010, requires using a comparison group of not-yet treated students observed in

ELA results are similarly noisy, and reflect much more significant pre-trends than the main results, again suggesting caution in interpretation.

We might expect that students who switch into a new school building in the first year it is open are less likely to have contemporaneous shocks that affect their outcomes independently of the new school building. For example, students who switch into a new school building after it has been open for a few years may be selecting into that new school building based on a change in outcome *trends* (e.g., the students' performance is declining, and the parent, noticing the benefits of attending a new facility, believes their student will particularly benefit). In this case, effect estimates may be biased because the trends of students who did not switch into a new school building are not an appropriate counterfactual. Put differently, estimates may reflect that the students who switch into a new school building would have had different trends post-switch regardless of the new school building (e.g., if they were trending down, an upward trend after the switch may reflect regression to the mean). Appendix Figure E2 and Table E2 also present result estimates limited to a sample of students who switch into a new school building in the year the building opens. This sample of treated students is smaller (recall many new school buildings open to one grade at a time, meaning fewer students in tested grades attend a new school building the first year it is open). While the results are similar for the attendance rate outcomes, trends prior to attending a new school facility for students who switch are higher (by approximately 0.25 pp) than the comparison group of students who do not switch into a new school building, and statistically significant. That is, there may have been *more* time-varying

both 2009 (the year before treatment) and 2012 (i.e., these comparison students must be treated in 2013 or later). Because I only observe students for six years (Grades 3-8) if they make standard academic progress, most of the ever-treated students I observe in 2009 have already been treated by 2012, and therefore cannot be used in the comparison group (note these are exactly the type of "forbidden" comparisons a traditional dynamic DID estimator makes).

selection into these buildings among the students who switch into a new school building in the first year it is open. Math results show similarly noisy pre-trends, though also positive effects after 3-4 years in a new school building. ELA results using this sample of treated students show flat pre-trends and positive effects after three years in a new school building.

Students are assigned or choose to attend new school buildings in a variety of ways. The majority of students (85% in the main sample) also switched into a new school when they moved to a new building. This may raise the concern that observed effects are due to students switching schools, rather than the new school building itself (though never treated students in the comparison group will also be switching schools, from, for example, an elementary school in an old school building to a middle school in an old school building). I estimate a placebo event study, where the treatment is school switches that don't involve attending a new school building. Results, presented in Appendix Figure E3, show that switching schools has a significant, persistent negative effect on students' outcomes, consistent with the literature on school mobility (Welsh, 2017). These results also suggest that the estimates of attending a new school building in NYC may be negatively biased by the counteracting negative effects of switching schools. That is, the effect of attending a new school facility for students who *always* attend a new school building might be larger. However, it is difficult to identify the causal impact of new school facilities on always-treated students, given the previously discussed selection issues.

Discussion

Expected Impacts of New School Facilities

Given the findings regarding the effects of attending a new school facility in New York City on math and ELA scores are inconclusive, I compare my estimates to expected effects based on the prior literature. In Appendix F, I detail calculations for expected cost-effectiveness of the new school construction projects I examine, as well as conduct an informal break-even analysis.

I find the expected test score impact based on prior literature: 0.14 sd, is outside of my 95% confidence intervals for effects on both math and ELA scores. In addition, the effect size needed for the projects to break even, in terms of impacts on test scores, is 0.11 sd—at or just above the upper limit of my 95% confidence intervals of impacts after four years in a new facility.

While this study focused on three specific student outcomes, new school facilities encompass a component of school quality that may not be fully captured by these metrics. Research on school infrastructure investments has established that even when these investments do not have significant impacts on student achievement, they are nonetheless valued by the school community. For example, Cellini et al. (2010) find school capital improvements cause an increase in home prices in California, only a small portion of which is explained by the impact on test scores; Lafortune and Schönholzer (2022) similarly find only 24% of each dollar capitalized in housing prices is due to test score improvements. Neilson and Zimmerman (2014), Goncalves (2015), and Conlin and Thompson (2017) also find improvements in school facilities raise housing prices. Th disconnect between capitalization into housing prices and impacts on test scores may be because achievement scores do not fully capture benefits to students: Jackson & Mackevicius (2024) find that school spending impacts on educational attainment are larger than impacts on test scores. It may also be because there are broader benefits than those that accrue directly to students served during the school day. School buildings are social infrastructure (Klinenberg, 2018): they provide sites for after school and summer school programming, playgrounds and recreational spaces for the community, and serve other community purposes such as voting sites and emergency shelters.

Biasi et al. (2023), who also find that school infrastructure investments increase housing prices by more than would be expected based on test score effects, suggest the discrepancy is due to state aid, not inefficiencies in the level of spending. That is, because many school infrastructure projects are financed partially by state grants, they raise the benefit of spending without raising costs to the local taxpayer. Biasi et al. (2023) also suggest the disconnect between improvements in test scores and housing prices may be due to the type of infrastructure spending: The types of infrastructure investment that raise student test scores do not necessarily raise house prices, and vice versa. School investments that carry an amenity component and that are more visible, such as an athletic field, increase housing prices despite having no effect on student outcomes. As previously mentioned, students may still benefit from these facilities in ways that are not captured by test scores. On the other hand, investments that are not "visible " to taxpayers without school-age children but improve student outcomes, such as HVACs, do not increase housing prices.

In summary, my finding that impacts on student outcomes are smaller than expected does not contradict the broader literature which finds that school buildings are an important component of the education production function. However, this paper does add to the growing literature documenting inequities in funding for school facilities, inequities than have direct implications for the efficiency of these investments, since we expect larger impacts for low-income and lower-performing students.

Implications for the Current NYC Policy Context

In terms of infrastructure investment, NYC spent an average of $900 million (2019 dollars) annually on new K-12 school seats between 2005 and 2019. NYC's spending on new K-12 school seats in fiscal years 2020-2022 as part of the 2020-2024 Five-Year School Capital Plan has been similar, averaging $954 million (2019 dollars). This may seem surprising, given enrollment levels were already declining before the COVID-19 pandemic, and the pandemic also led to enrollment loss (Roy & Guarda, 2022). However, many of the projects were already in-progress and budgeted total spending over the five-year plan is significantly lower than initially proposed. The city also has new pressure related to school capacity from recently passed state legislation requiring lower class sizes, which must be fully phased in by September 2028 (Amin & Zimmerman, 2022). As documented in this paper, NYC DOE's measure of utilization is positively correlated with measures student income, student performance, the portion of students who are White and Asian, and pupil-teacher ratio. Therefore, it is unsurprising that analyses of the potential impacts of the new state class law find that White students, Asian students, and students from high-income families would be much more likely to see their class sizes reduced under the state law (Chingos & Meltzer, 2023; Guarda & Subraminian, 2023).

Since NYC DOE's measure of overcrowding is correlated with measures of student advantage but imperfectly correlated with related measures of crowding—pupil-teacher ratio and enrollment per square foot—the district might consider alternative prioritization of school infrastructure investment. If the district believes high utilization, as measured, reflects conditions in schools that harm student learning, they can explore non-construction strategies, such as capping enrollment and rezoning, to reduce crowding in schools (Edwards, 2019). Indeed, this may be an increasingly desirable strategy given the state legislation regarding class size.[22] Instead, infrastructure spending could be redirected to what research suggests may be more efficient: improving the condition of schools in the worst state of repair, modernizing schools lacking building components that likely have significant impacts on student performance (e.g., HVAC), and/or prioritizing funding for schools with high portions of low-income and low-

[22] The July 2023 draft version of NYC DOE's plan to comply with the class size law notes there are 400-500 schools that cannot meet class size mandates given existing space and enrollment configurations, and they will consider the law in determining new capacity for the forthcoming 2025-2029 Five-Year Capital Plan, which is currently being developed (NYC DOE, 2023).

performing students.

Conclusion

Though there is no comprehensive national data on school facilities, existing research using data from specific districts or states finds that investment in school infrastructure, and new school construction in particular, has a significant positive effect on student outcomes, particularly for low-income students. Despite this, low-income students may be least likely to receive school infrastructure investments, based on current state and district policies for financing school infrastructure. This study contributes to the literature by using detailed panel data on students and schools in New York City, the nation's largest school district, to estimate the impact of attending a new school facility on student outcomes. In contrast to the student populations that benefitted from new construction in previously studied contexts, students who attend new school buildings in NYC are more likely to be White and high-performing, and less likely to be low-income. This is partially because NYC has focused new school construction on areas of overcrowding, and in this choice-rich district overcrowding is the result of high demand.

I find students who move to a new school building experience significant improvements in facility quality: a decline in building age of 61 years. Using NYC DOE's own measure of crowding would suggest that students who moved to a new school building experience increased crowding, contrary to the goals of new school construction. However, students who switch to a new school building see a 10% reduction in the number of students per square foot, and a 5% reduction in the pupil-teacher ratio. This suggests the utilization measure may not fully capture school building conditions that affect student outcomes.

Students who attend new school buildings in NYC see improvements in attendance outcomes of approximately 1 percentage point, but this is smaller than effects found in prior research, where the population who benefit from new school facilities is more low-income, lower performing, and less White. Indeed, when looking at heterogeneity in effects on attendance by FRPL-eligibility, low-income students see greater benefit from attending a new school building than higher income students (though impacts are small for both groups). While the math and ELA score results are inconclusive and should be interpreted with caution, they also suggest the size of the effects for the relatively advantaged student population that attend new school buildings in NYC are smaller than effects in prior research.

These results suggest, for both NYC and all school districts, that while school infrastructure investment matters for student outcomes, how this funding is targeted is key. Targeting has implications for equity, which, in turn, affects the impact of infrastructure investments.

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Figures and Tables

Figure 1. Maps of Identified K-8 Seat Need and Constructed K-8 Seats, by Capital Plan and CSD

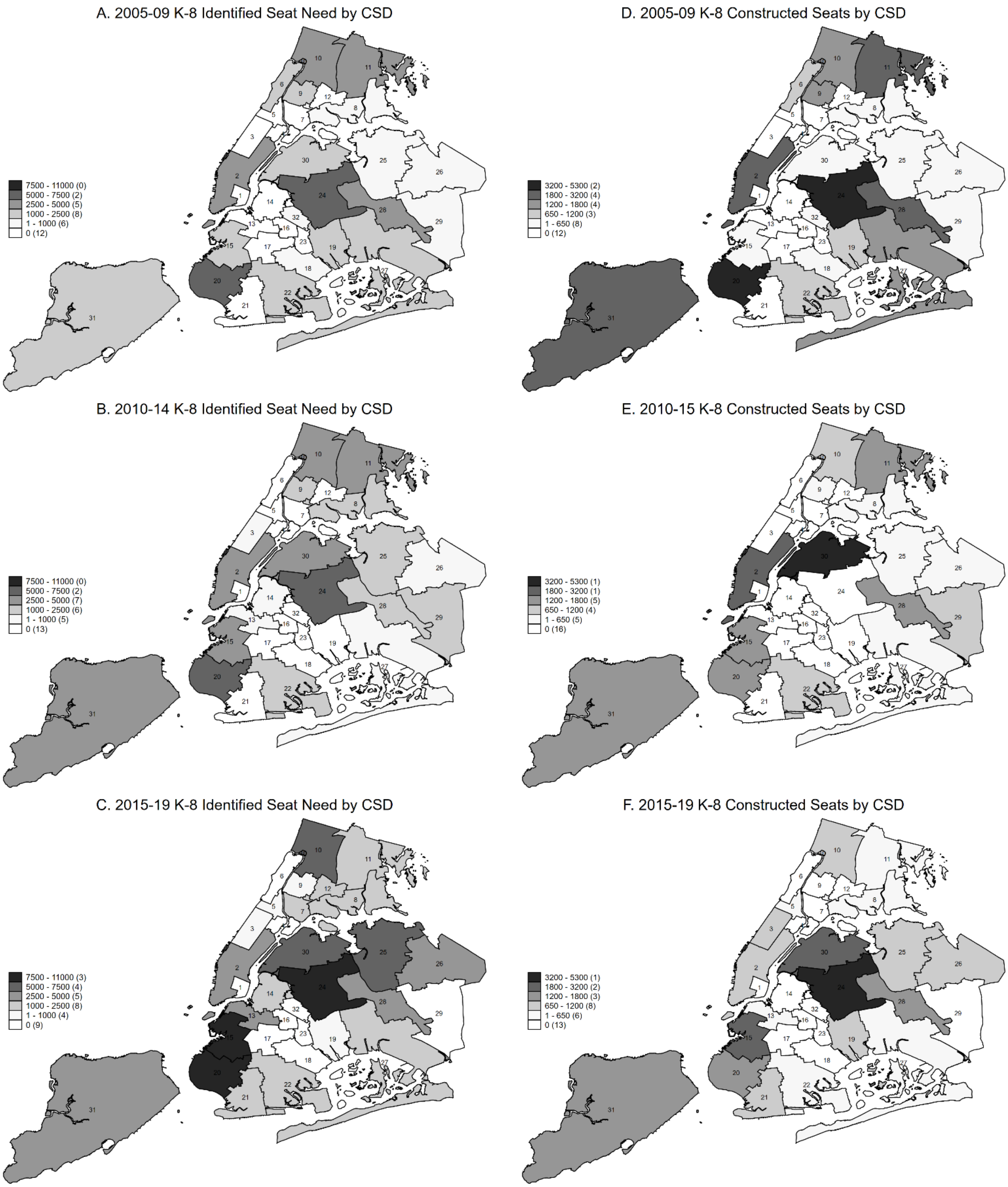


Figure 2. Correlates of Overcrowding, NYC Traditional Elementary & Middle Schools, 2008-19

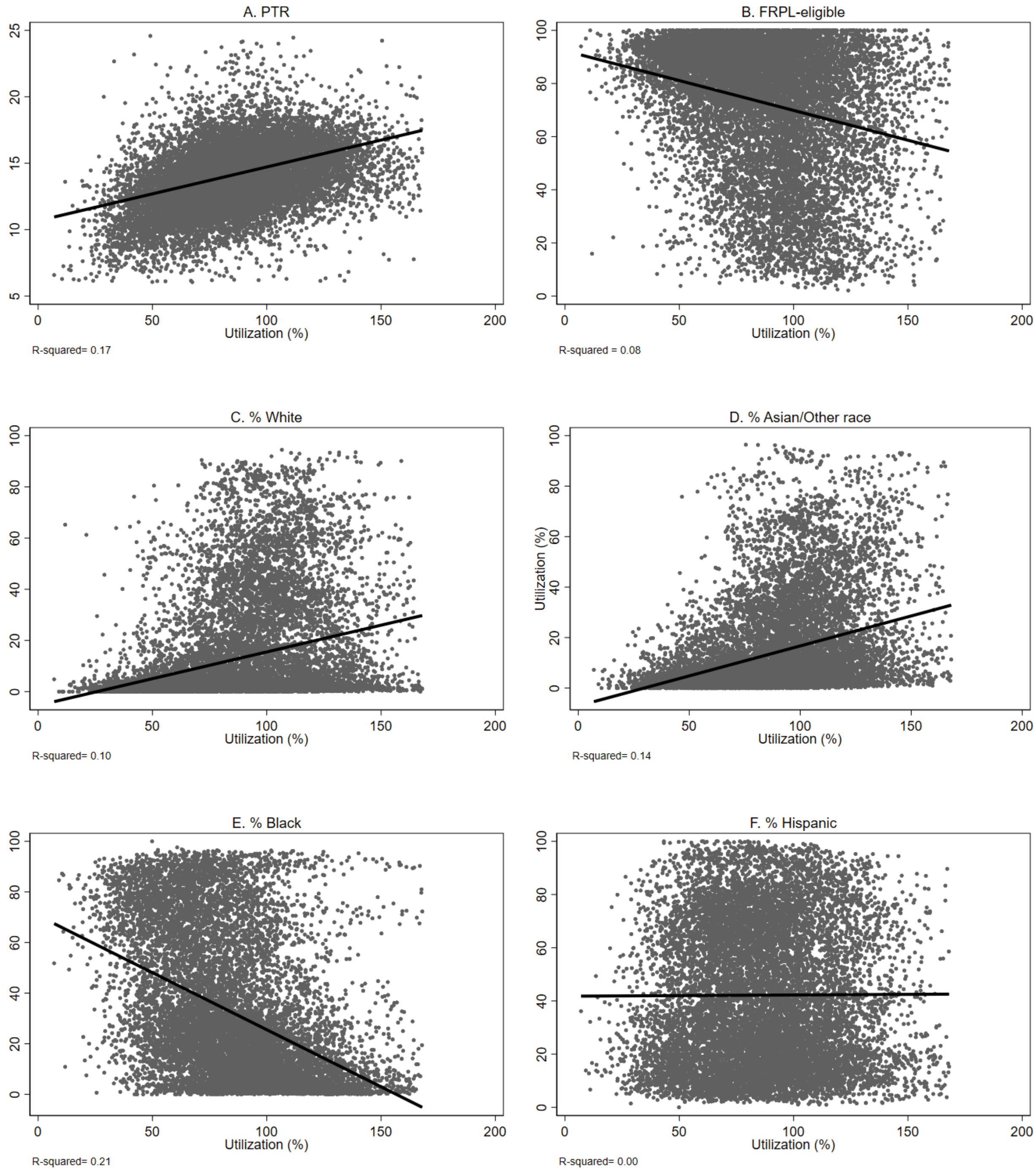
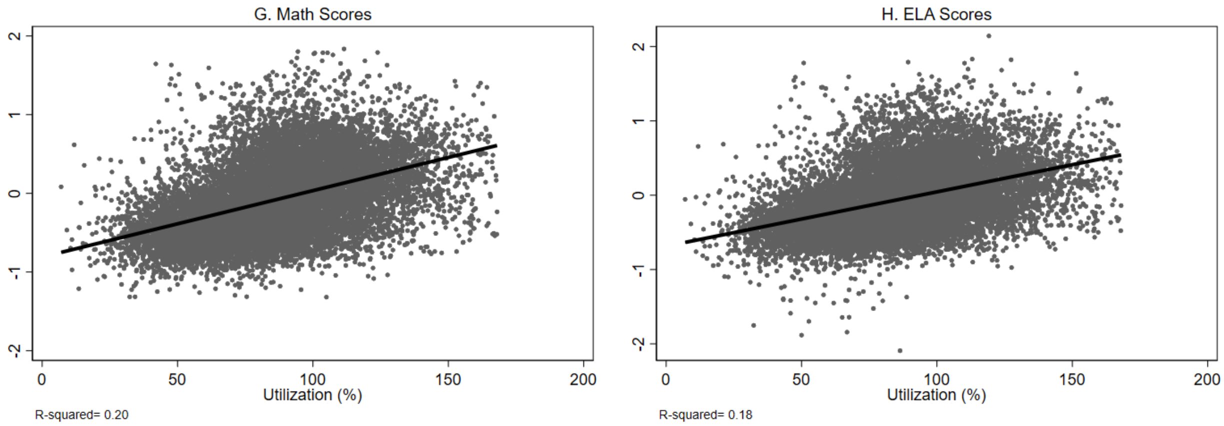
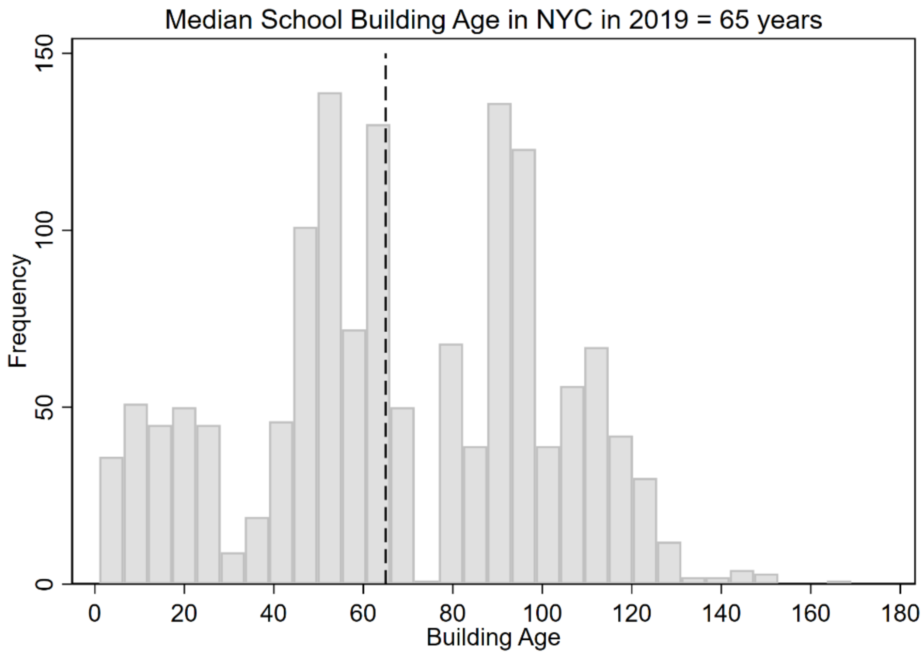


Figure 2. Correlates of Overcrowding, NYC Traditional Elementary & Middle Schools, 2008-19 (cont.)



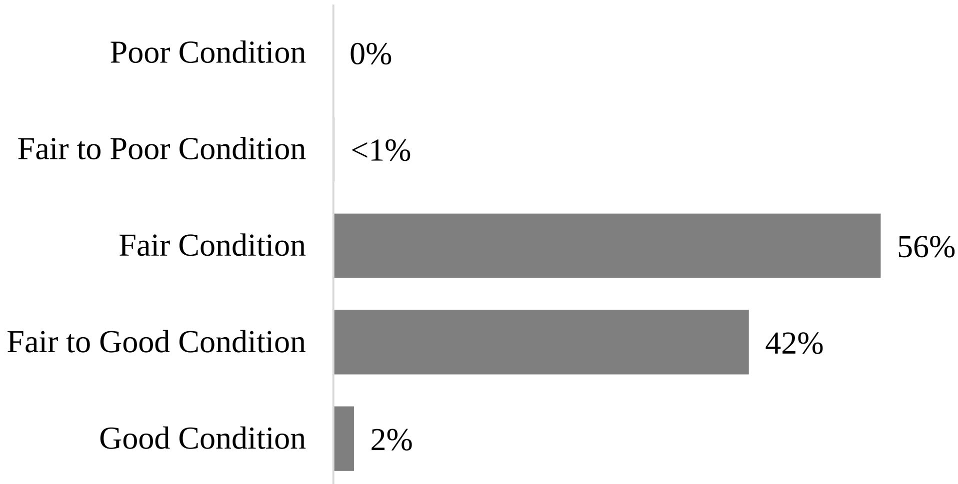
Notes: Utilization data from the annual Blue Book reflects historical school-level capacity as described in Appendix B. The count of the number of teachers used to construct the pupil-teacher ratio (PTR) comes from the NYS Report Cards. The rest of the school-level data are aggregated from student-level data. For ease of presentation, extreme outliers in the utilization data are removed (the top percentile), as are extreme outlies in the PTR data (less than 6 or more than 24), and in the math and ELA score data (less than -3 or greater than 3); this does not substantially change correlations.

Figure 3. NYC School Building Age, 2019



Notes: For buildings with major renovations reflected in both the Five-Year Capital Plan and in the building's BCAS report, age is calculated from the year of the renovation. TCUs and buildings serving only charter schools, pre-K, or 3-K are not included.

Figure 4. NYC School Building Conditions, 2019



Notes: Data from the NYC Mayor's Management Report (Fuleihan & Thamkittikasem, 2019).

Figure 5. New School Buildings in Sample and 2008 Utilization by CSD

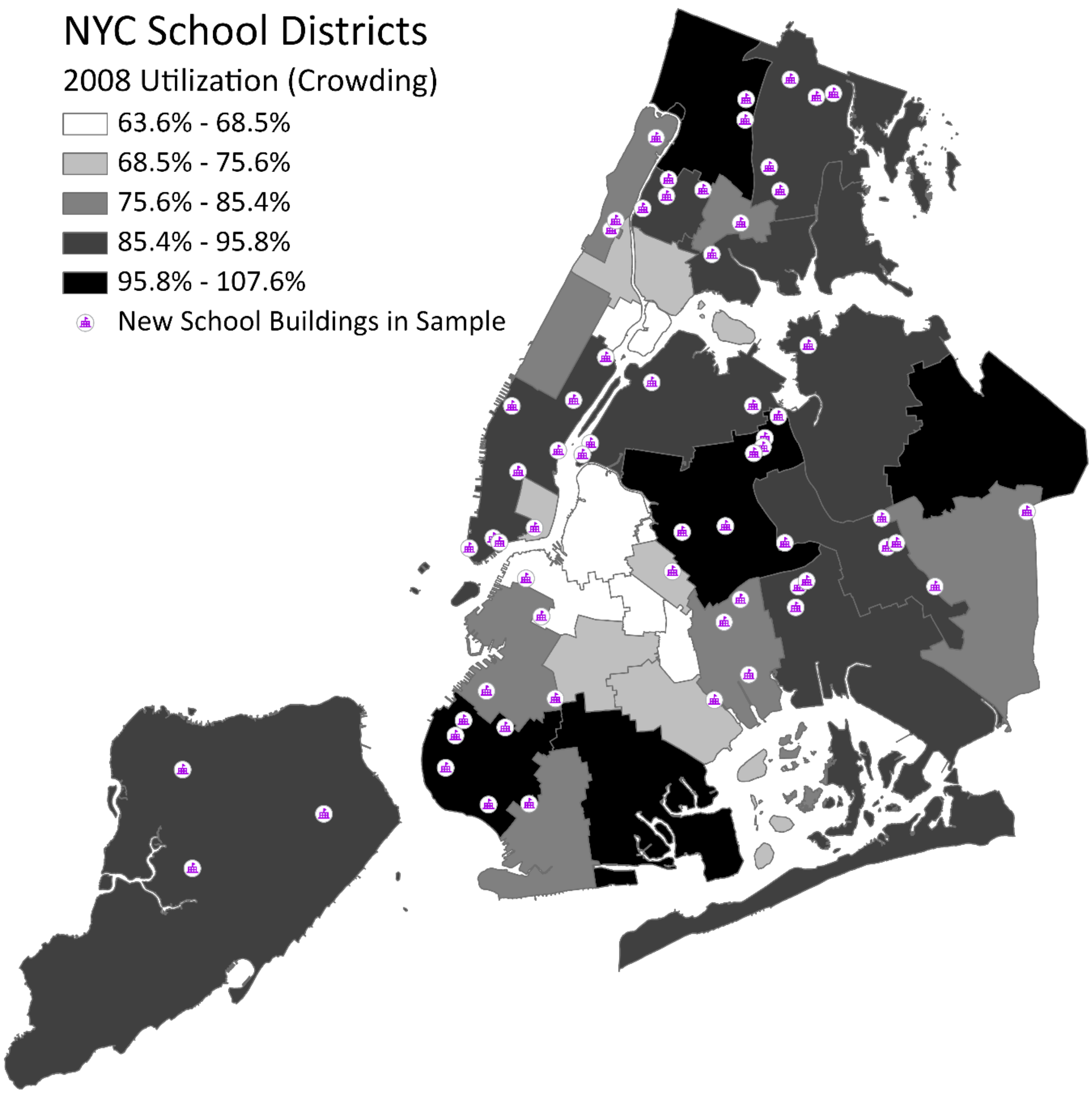
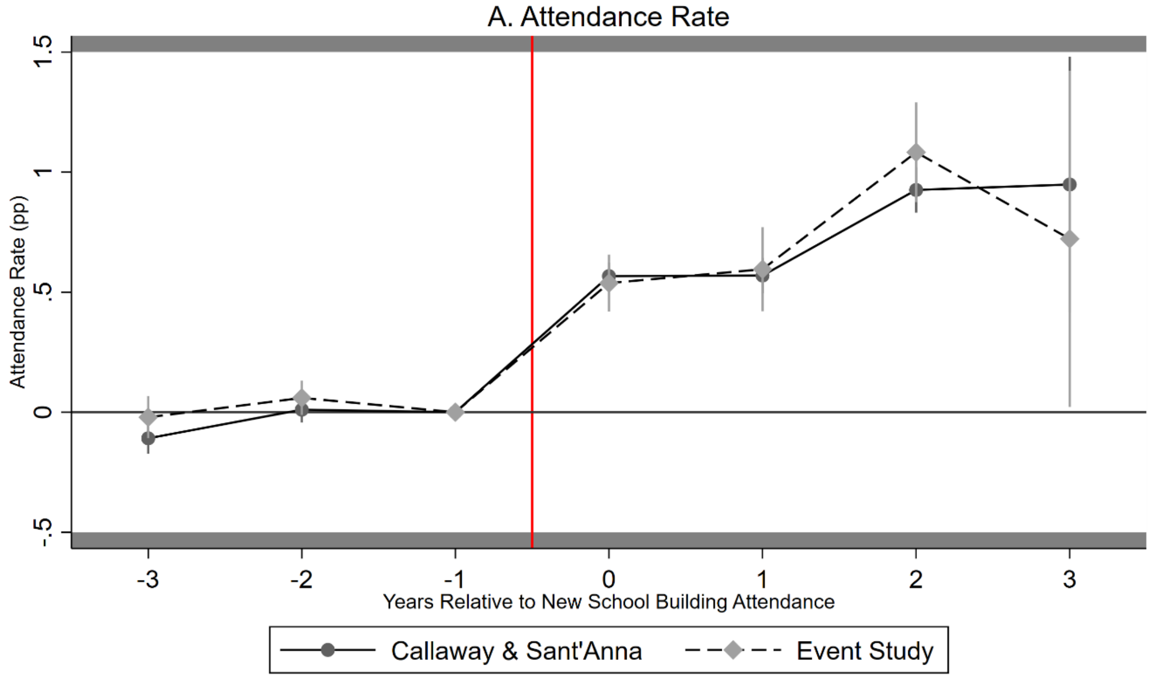
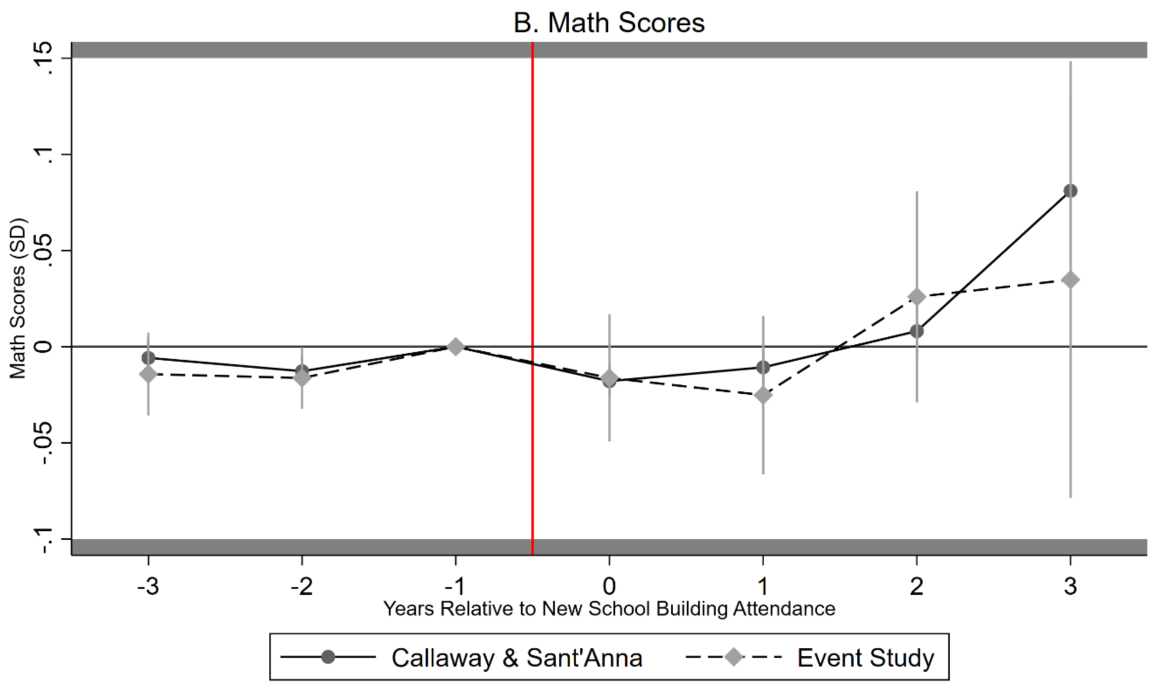
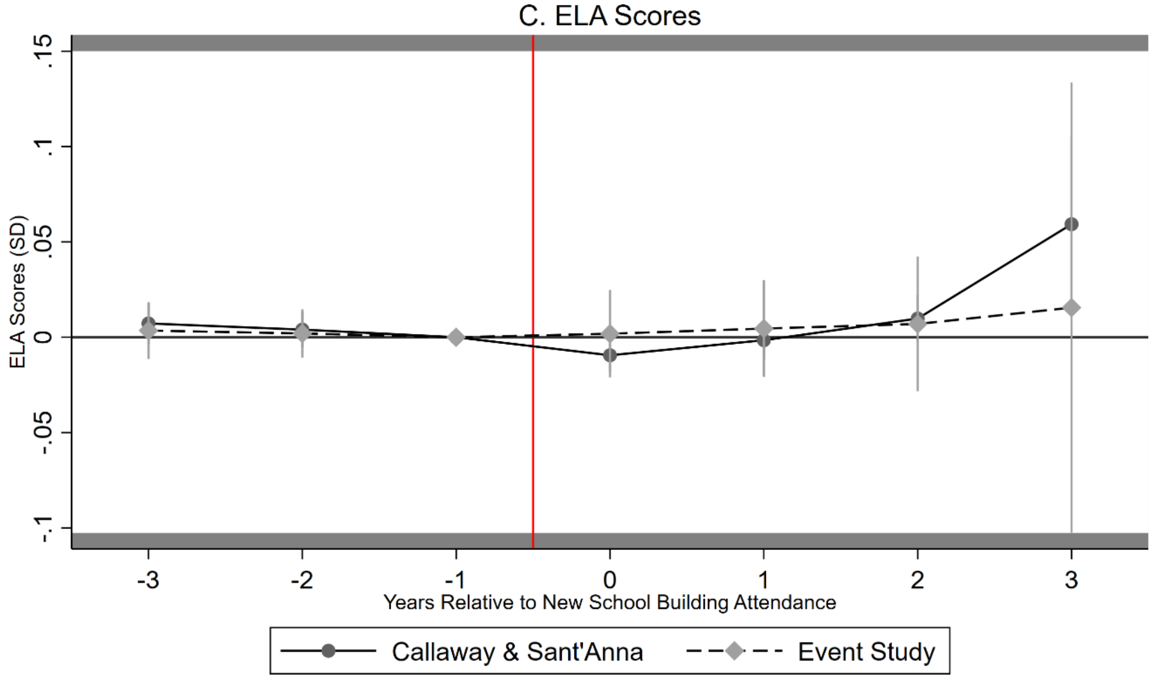


Figure 6. Results: Effect of Attending at New School Building on Student Outcomes

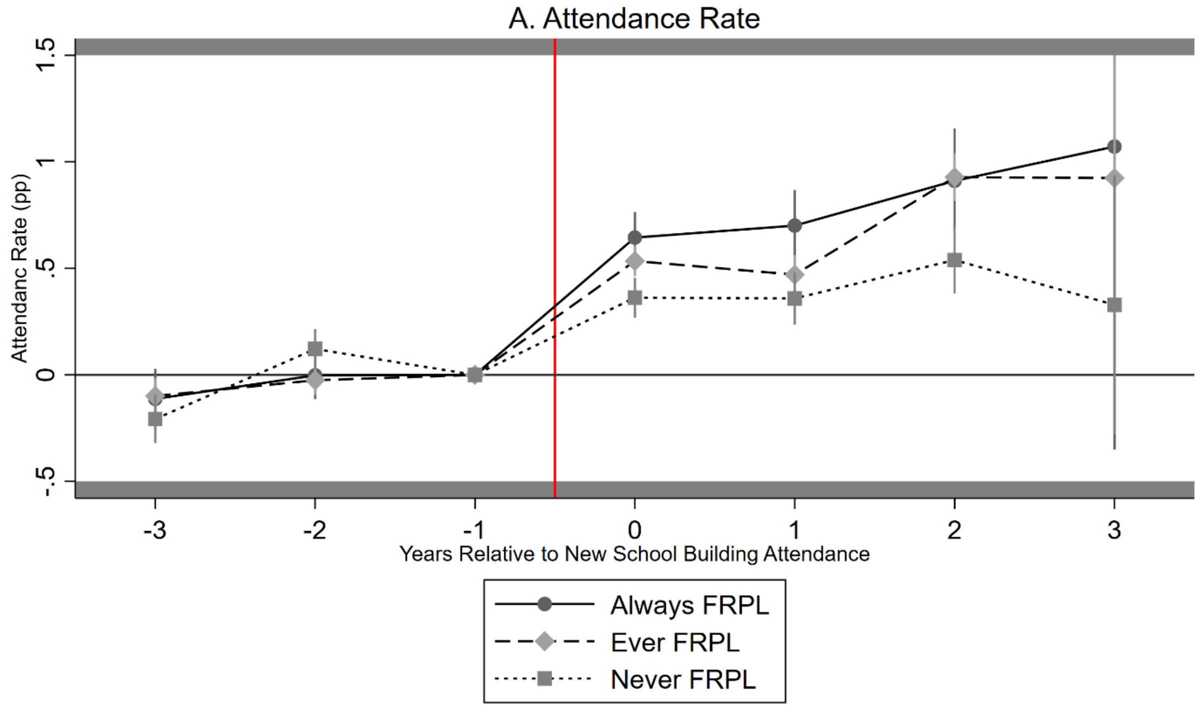


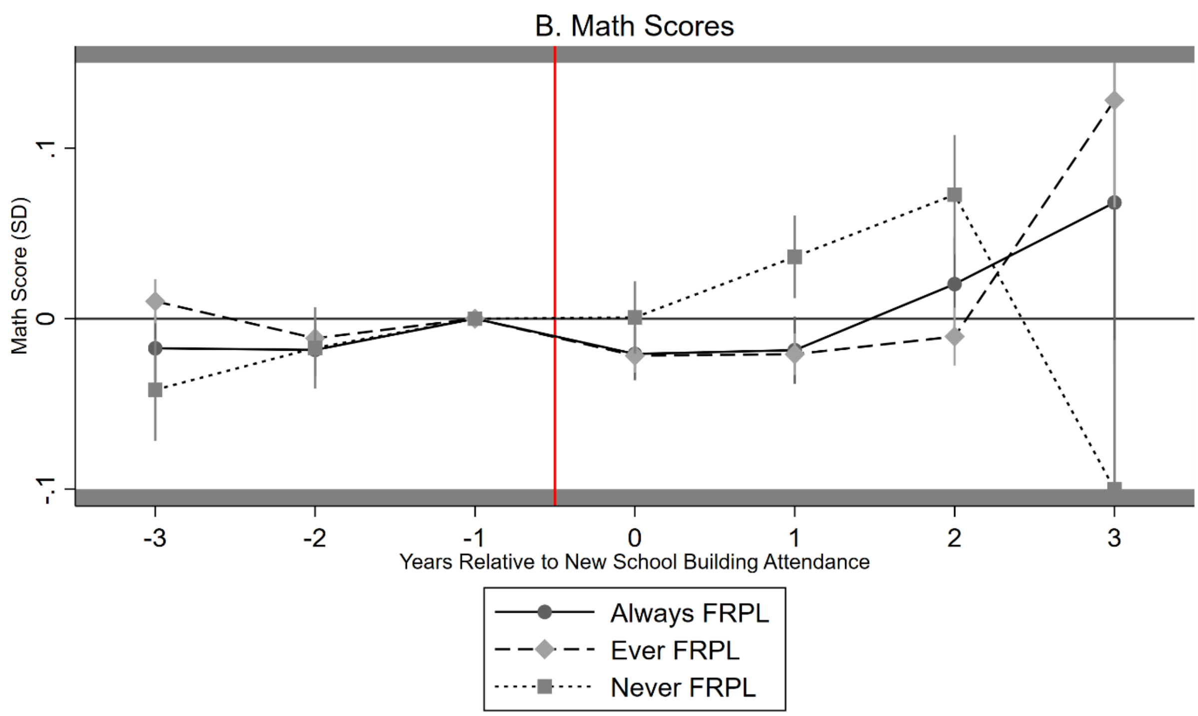


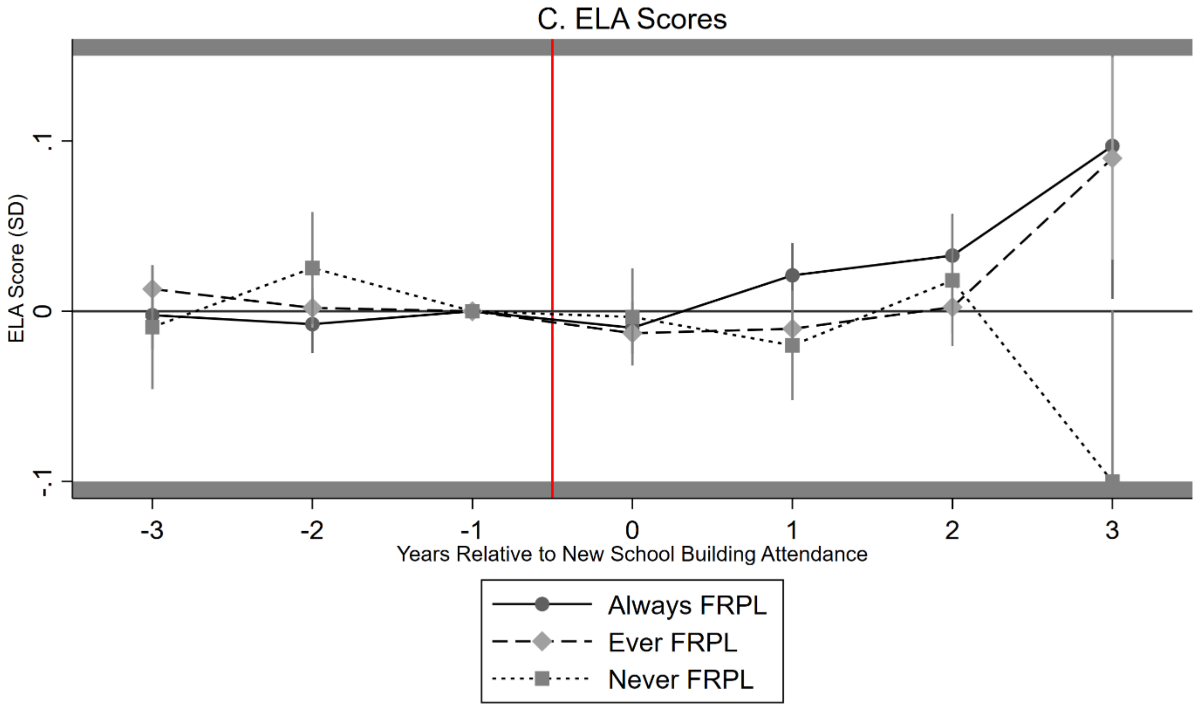


Notes: Figures show estimated coefficients from event study regressions following equation (1), which includes fixed effects for student, grade, and year, and coefficients from the Callaway & Sant-Anna estimator described in Methods. The sample includes all never- and ever-treated students in Grades 3-8, as described in Data (see Table 2). Dependent variables are attendance rate (Panel A), math test scores (Panel B), and English-language arts (ELA) test scores (Panel C). Test scores are standardized to have a mean of 0 and standard deviation of 1 using citywide data for each year-grade-subject exam. Vertical lines reflect 95% confidence intervals for the estimated coefficients. Standard errors are clustered by student. Estimates in event time $k = 3$ (after four years attending a new school building) are based on relatively few observations (see Appendix D, which shows the number of observations by event time).

Figure 7. Results: Effect of Attending at New School Building on Student Outcomes by FRPL-eligibility







Notes: Figures show estimated coefficients from the Callaway & Sant'Anna estimator described in the text. The sample includes all never- and ever-treated students in Grades 3-8, as described in Data (see Table 2), split by whether students were always eligible for free or reduced-price lunch (FRPL), ever (but not always) eligible, or never eligible for FRPL. Dependent variables are attendance rate (Panel A), math test scores (Panel B), and English-language arts (ELA) test scores (Panel C). Test scores are standardized to have a mean of 0 and standard deviation of 1 using citywide data for each year-grade-subject exam. Vertical lines reflect 95% confidence intervals for the estimated coefficients. Standard errors are clustered by student. Estimates in event time $k = 3$ (after four years attending a new school building) are based on relatively few observations (see Appendix D, which shows the number of observations by event time for the entire sample; the number of observations in the subsamples split by FRPL-eligibility is lower).

Table 1. New Construction Projects in Sample: Buildings Completed 2008-2019 and Serving Students who Switch into New Building in Grades 3-8

	New Construction	Major Renovation
N projects	55	8
Average Total Cost (2019$)	$95 million	$36 million
Cost Per Square Foot (2019$)	$1,010	$534
Forecast Capacity	743	551
Cost per seat (2019$)	$129,347	$74,217
Design to Completion (months)	49	28
Construction to Completion (months)	32	14

Note: Forecast capacity is only reported for 58 of the 63 projects. Therefore, the cost per seat is based on the maximum enrollment in the building once it is open.

Table 2. Summary Statistics, Traditional NYC Public School Students who Never or Ever Attend a New School Building from 2008-2019 in Grades 3-8

	Never Treated	Ever Treated (t-1)
N students	1,061,190	23,980
Female	50%	53%
White	16%	20%
Black	27%	26%
Hispanic	41%	41%
Asian/Other Race	16%	12%
FRPL-Eligible	72%	68%
ELL	13%	11%
SWD	18%	18%
Math (SD)	0.028	0.114
ELA (SD)	0.028	0.122
Att. Rate (%)	93.7	94.8
Building Age	65.9	73.5
>30 Building Age	78%	90%
Has TCU	22%	14%
Utilization	90%	98%
>100% Utilization	26%	44%
Enroll/1,000 sq. ft.	9.1	10.0
Co-location	28%	21%
Multisite	13%	21%
Residential Move	10.0%	11.7%

Notes: For never-treated students, values reflect means across all student-year observations. For ever-treated students, values reflect means in the year before they attend a new school building. Utilization refers to building-level utilization (see Appendix B for further discussion of the utilization measure by building and school). Co-location refers to schools co-located with another public school in the same building (it does not count co-location with special education only schools, which typically use less space and often only utilize one classroom in a building). Multisite refers to schools with students across multiple buildings that have unique building identifiers.

Table 3. Mechanisms – How switching to a new school building affects building age, crowding, and teacher/peer characteristics

Years in new school building:	1 (β_0)	2 (β_1)	3 (β_2)
a. Building Age	-60.81*** (1.823)	-59.76*** (1.736)	-59.01*** (1.773)
b. Building Utilization (0-1+)	-0.030* (0.017)	0.031** (0.015)	0.063*** (0.017)
c. Enroll/1,000 sq. ft.	-2.04*** (0.287)	-1.46*** (0.261)	-1.06*** (0.264)
d. PTR	-0.683*** (0.221)	-0.683*** (0.199)	-0.672*** (0.175)
e. % of teachers out of cert. (0-100)	7.105*** (1.048)	7.066*** (1.251)	7.138*** (1.646)
f. % White students (0-100)	1.730*** (0.444)	2.001*** (0.458)	1.938*** (0.483)
g. % FRPL-eligible students (0-100)	-2.088** (1.026)	-3.487*** (0.983)	-4.018*** (1.082)

Notes: Standard errors, clustered by student and school, in parenthesis. *** p<0.01, ** p<0.05, * p<0.1 Estimates are from event study regressions following equation (1), which includes fixed effects for student, grade, and year, using the specified variable as the outcome measure. The reported coefficients, $\beta_0 – \beta_2$, are the effects after one to three years in a new school building, respectively.

Table 4. Results: Impact of attending a new school building in NYC

	(1)	(2)	(3)
A. Attendance Rate			
Estimator	Event Study w/ Time-varying controls	Event Study	C&S
Event Time (k)			
-3	-0.017	-0.021	0.109***
	(0.045)	(0.045)	(0.033)
-2	0.063*	0.060	0.105
	(0.036)	(0.036)	(0.027)
-1	.	.	.
	.	.	.
0	0.535***	0.538***	0.567***
	(0.060)	(0.061)	(0.028)
1	0.601***	0.595***	0.569***
	(0.089)	(0.090)	(0.037)
2	1.088***	1.083***	0.926***
	(0.106)	(0.106)	(0.048)
3	0.715**	0.722**	0.948***
	(0.358)	(0.357)	(0.271)
Observations	4,889,342	4,889,342	
B. Math Scores			
Estimator	Event Study w/ Time-varying controls	Event Study	C&S
Event Time (k)			
-3	-0.016	-0.014	-0.006
	(0.011)	(0.011)	(0.005)
-2	-0.017**	-0.016**	-0.013***
	(0.008)	(0.008)	(0.004)
-1	.	.	.
	.	.	.
0	-0.016	-0.016	-0.018***
	(0.017)	(0.017)	(0.004)
1	-0.025	-0.025	-0.011**
	(0.021)	(0.021)	(0.005)
2	0.027	0.026	0.008
	(0.028)	(0.028)	(0.007)
3	0.037	0.035	0.081***
	(0.056)	(0.058)	(0.025)
Observations	4,733,131	4,733,131	

Table 4. Results: Impact of attending a new school building in NYC

	(1)	(2)	(3)
C. ELA Scores			
Estimator	Event Study w/ Time-varying controls	Event Study	C&S
Event Time (k)			
-3	0.001	0.004	0.007
	(0.008)	(0.008)	(0.006)
-2	0.001	0.002	0.004
	(0.006)	(0.006)	(0.005)
-1		.	.
		.	.
0	0.002	0.002	-0.009**
	(0.012)	(0.012)	(0.004)
1	0.005	0.005	-0.002
	(0.013)	(0.013)	(0.005)
2	0.009	0.007	0.010
	(0.017)	(0.018)	(0.006)
3	0.017	0.016	0.060**
	(0.058)	(0.060)	(0.024)
Observations	4,736,117	4,736,117	

Notes: Standard errors, clustered by student and school, in parenthesis. *** p<0.01, ** p<0.05, * p<0.1 The table presents estimated coefficients from event study regressions following equation (1), which includes fixed effects for student, grade, and year, as well as coefficients from the Callaway & Sant-Anna (C&S) estimator described in Methods (note column 1 is an event study following equation (1) that additionally has time-varying controls for whether a student is FRPL-eligible, an English-language learner, in special education, retained in the current year, or retained in any prior year). Dependent variables are attendance rate (Panel A), standardized math test scores (Panel B), and standardized English-language arts (ELA) test scores (Panel C). Test scores are standardized relative to the citywide mean and standard deviation for each year-grade-subject exam. The number of observations and unique students varies based on missingness of the outcome measures (see Appendix D).

Appendix

Appendix A: Comparing Prior Studies of the Impacts of New School Facilities on Student Outcomes

This Appendix presents additional details regarding the prior studies of new school facilities/news school construction: the study of Los Angeles, CA (Lafortune & Schönholzer, 2022) and the study of New Haven, CT (Neilson & Zimmerman, 2014), to facilitate their comparison to each other and to this study of New York City.

Number of New Buildings and Grade Levels

In New Haven, of the 37 new school buildings constructed, 31 were elementary or middle schools and six were high schools. Neilson and Zimmerman's analysis focused on elementary and middle schools. In Los Angeles (LA), the 114 newly constructed campuses studied served 144 schools, of which 80 served elementary school grades (K-5) and the remaining 63 served middle and high school grades (6-12).

Timing of Observed Effects

The difference in timing of positive effects in Neilson & Zimmerman's estimates compared to Lafortune and Schönholzer's—six years and four years, respectively—may reflect a difference in their definition of treatment, the dynamics of their treatment estimate, and their sample.[23] Neilson and Zimmerman assign treatment based on whether the student is in the neighborhood where a new school was constructed in the current year and "includes students who are present in schools only after or before building occupancy"—that is, their post-treatment estimates include students who were not observed pre-building occupancy (p. 22; this refers to

[23] In addition, though Neilson and Zimmerman (2014) only report positive and statistically significant effects in reading after six years of building occupancy, the point estimates for effects on reading scores in New Haven are positive in all years after building occupancy (as well as statistically significant at the 10% level)—so positive effects may materialize earlier.

their preferred student fixed effect estimator). They define the first year of treatment as the first year of building occupancy and bin effects at six or more years. While Lafortune and Schönholzer similarly include “always treated” students (i.e., students who are only observed after they attend a new school building) in their main sample, they define the first year of treatment as the year a student switches into a new school building (so “always treated” students do not contribute to treatment effect estimates). In addition, they note “few students attend a new school facility for more than four years in the data” (p. 266) so they bin treatment timing at four years post-treatment. Neilson and Zimmerman’s sample size is also smaller: approximately 38,000 observations compared to Lafortune and Schönholzer’s 724,000 observations, as expected given the size of the New Haven school district compared to the LA school district.

In contrast to Lafortune and Schönholzer (2022) and Nielson and Zimmerman (2014), for most of the prior literature on school infrastructure investments (e.g., studies using district-level data and an RD design around bond referenda), year one represents the first year after a capital bond is passed. Therefore, Jackson and Mackevicius (2024), in their meta-analysis, consider the first two years of capital spending a “construction/adjustment period” and measure outcomes six years after increased capital spending. Their meta-analysis finds “by about 5 or 6 years after a capital spending increase, one observes improved outcomes in most cases” and “one rejects that the effect of capital spending is zero at the 5-percent level by year five” (p. 63).

Financing of New School Construction

LA’s school construction program was financed primarily through local voter-approved bonds. The New Haven school construction program was largely financed by the federal and state government, and local funding came from overall city revenues rather than a voter-approved property tax increase specifically associated with school infrastructure.

Assignment to New School Buildings

In New Haven most students attend zoned elementary and middle schools and Neilson and Zimmerman (2014) assign students to treatment based on whether their address is in a school zone with new construction. In Los Angeles, students are similarly assigned to new school buildings based on residential catchment areas and most students (75%) attend their residentially assigned school.

Appendix B: Operationalizing Overcrowding

The New York City Department of Education produces an annual "Enrollment, Capacity, and Utilization" report on public schools and buildings. "Utilization" is the City's measure of crowding in schools: enrollment divided by capacity. The formula for calculating a school building's capacity is "based on a set of assumptions uniformly applied"—all instructional rooms equal to or greater than 240 square feet are assigned a capacity, which is then adjusted based on the programmatic use of the rooms (for example, class sizes for lower grades are assumed to be smaller, and science labs are programmed differently than traditional classrooms). This information is provided at the school-building level, as well as totals at both the school and building level. When one school wholly occupies a building, the information will be the same at the school-building, school, and building level. However, New York City has both co-located schools: multiple schools located in the same building, and multi-site schools: schools that serve students across multiple physical locations. The capacity of a co-located school is therefore based on the portion of the school building the school uses (in this case, the school-building capacity and the school capacity will be the same but will differ from the building capacity). Similarly, for a multi-site school, capacity is totaled across all of the school's physical locations (in this case, assuming no co-location, each school-building capacity would be the same as the respective building capacity, but the school capacity would differ). While co-locations are common, most schools—89% of school-year observations—are contained entirely in one building (i.e., not multi-site).

I only observe the school a student attends in student-level data from the New York City Department of Education, not the building. However, I use building-level rather than school-level utilization throughout this paper, because my other measure of crowding, enrollment per square foot, is only available at the building-level. For multi-site schools I match the school to the building with the greatest portion of its enrollment to determine which building's crowding measures to use. Among the small portion of multi-site schools, two-thirds of the enrollment is in the building with the greatest portion of the school's enrollment, on average.

The City historically produced two measures of capacity (and, therefore, two measures of utilization): historical and target. The historical capacity and utilization allowed for the comparison of these measures over time, while the target capacity and utilization "reflects aspirational goals for school buildings," which may change. In particular, the maximum classroom capacity assumed by the formulas used for calculating target capacity has decreased over time. This means the target capacity and utilization are typically more conservative measures—target capacity is usually lower than the historical capacity, and target utilization is usually higher than the historical utilization.

In 2016, the NYC DOE stopped producing the historical capacity and utilization calculations. In order to construct a measure of utilization that is comparable over time, I use target capacity to predict historical capacity for the years 2016-2019, and then use this predicted value to calculate historical utilization. Target capacity and historical capacity are highly correlated; for 2008-2015, their correlation is 0.99. As expected, target capacity is more conservative—for 86% of building-year observations from 2008-2015, historical capacity is higher than target capacity. When regressing building-level historical capacity on target capacity, target capacity squared, and a set of year indicators, the coefficient of determination is 0.99. This is how the imputed capacity measure is generated, and the imputed historical utilization (hereafter, utilization) is enrollment divided by the imputed historical capacity.

One additional correction is made to the raw Blue Book data to ensure comparable utilization over time. Prior to 2018, the Blue Book listed enrollment for temporary classroom units (TCUs), and prior to 2014, the Blue Book listed capacity for TCUs. However, it has long been a goal of NYC DOE to remove all TCUs, and as such, they are now considered to have zero capacity (to reflect that additional permanent capacity is needed so the TCUs can be removed). To have consistent enrollment and capacity calculations across years, all TCUs are assigned a capacity of zero. For the years in which TCU enrollment was listed separately and not included with the main school or building enrollment, each TCU's enrollment is added to the enrollment of the school with which the TCU is associated, as well as to the enrollment of the main building of that school. TCUs are identified through the name of the building in the Blue Book data (all have "Transportable" or "Trans" in the name). Given this adjustment, I calculate utilization rates myself, rather than using those reported in the Blue Book data, even for the years in which historical utilization is available. In addition, the raw Blue Book utilization data are rounded to the nearest percentage point, so calculating the utilization myself allows for greater precision.

A building's capacity, as calculated in the Blue Book, can change significantly over time, for reasons unrelated to changes in the capacity formula. Looking at building historical capacity for 2008-2015 (which is not imputed), 15% of buildings have changes in capacity of more than 20% and almost half of buildings have changes in capacity of more than 10%. In some cases, this reflects an addition to the building (when the addition is not a stand-alone building it is not given its own building ID; instead, it is considered part of the main building). However, in many cases, it may reflect changes to the internal configuration of space, such as combining or separating classrooms, or creating specialized instructional spaces, which have different capacity calculations. It may also reflect a change in the grade levels served by the building, since class sizes are assumed to be smaller for the primary school grades than the secondary school grades. Finally, the enrollment and capacity data are provided for grades K-12, so the introduction of additional pre-K classrooms can result in lower K-12 capacity (and higher utilization). In 2015, after the introduction of universal pre-K in NYC, students in grades K-5 saw an increase in their utilization. Even though there was no physical change to school buildings, classrooms set aside to serve pre-K students were no longer available for K-12 students. Therefore, in buildings newly serving pre-K students (or serving more pre-K students), K-12 capacity decreased. If non-pre-K capacity decreased without a commiserate reduction in non-pre-K enrollment in these buildings, utilization increased.

The target capacity and utilization measures are the ones used in the creation of the New York City Department of Education's Five-Year Capital Plans, and inform how much new school construction is funded and where this funding is targeted. Target utilization is also the metric the NYC DOE uses to report information on overcrowding in the Mayor's Management Report. Because I do not use target capacity and utilization (because they are not comparable over time), my measures do not match these publicly reported data.

Appendix C: Assignment of Students to New School Buildings

The NYC Panel for Education Policy, with members appointed by the NYC Mayor and Borough Presidents, as well as one parent member, is the governing body of the NYCDOE. In addition to the PEP, there is a CEC for every community school district (CSD), consisting of parents and political appointees, with limited powers and duties specified in New York State law. Per NYC Chancellor's Regulation A-190, significant changes in school utilization—including the re-siting of existing schools in newly constructed facilities—must be approved by the PEP (NYC DOE, 2019). These proposals are made by the Chancellor and prepared by the Division of School Planning and Development. When, for example, an existing school is re-sited in a newly constructed school building, this is a significant change in school utilization, so a detailed proposal is posted publicly and approved by the PEP. While significant changes in school utilization does not include new schools opening in newly constructed school buildings, per Chancellor's Regulation A-185, zoning lines for elementary and middle schools—including any changes to zoning lines resulting the opening of a new school—must be approved by CECs. Similar to proposals regarding significant changes in school utilization, proposals for new or changed zoning lines are created by the NYC DOE Office of Student Enrollment Planning and Operations in consultation with CSD Superintendents and the School Construction Authority (SCA).

Below are excerpts from proposals brought to the PEP regarding significant changes in school utilization related to two of the new school buildings considered in this paper. They illustrate that highly local, idiosyncratic conditions govern how NYC DOE, in consultation with the SCA, determine how to use new school buildings.

From the November 2, 2009 Environmental Impact Statement (EIS) regarding building K807:

> Beginning with the 2010-2011 school year, P.S. 163 Bath Beach (20K163, "P.S. 163") an existing school serving grades PK-5, will implement a gradual grade reconfiguration plan to expand its current grade configuration to PreK-8. Plans are already in place for P.S. 163 to re-located from its current school building, K163 (hereinafter referred to as "K163"), located at 1664 Benson Avenue, Brooklyn in Community School District 20 ("District 20"), to K807 (hereinafter referred to as "K807"), a newly constructed school facility addition to K163, also located at 1664 Benson Avenue, Brooklyn. A District 75 program will be co-located with P.S. 163 in K807. K807 has a capacity of 665. K807 has sufficient space for P.S. 163 and the D75 program to operate at full organization capacity with the grade expansion of P.S. 163.
>
> This grade expansion will create approximately 130-180 new middle school seats in District 20. This proposal will create more middle school seats to help relieve overcrowding in District 20. Specifically, it is anticipated that the middle school seats created by the expansion of P.S. 163 will relieve overcrowding in J.H.S 201 The Dyker Heights (20K201, "J.H.S. 201") as students will have another option for middle school in addition to their zoned option.

From the January 7, 2010 Environmental Impact Statement (EIS) regarding building X338:

> In the 2010-2011 school year, 09X204 (hereinafter referred to as "P.S. 204"), an existing zoned elementary school serving grades K-5, will be housing in school building X338 (hereinafter referred to as "X338") located at 1740 Macombs Road, Bronx, in Community School District 9 ("District 9"). A newly constructed school facility, X338 has a capacity of 642. X338 is located within P.S. 204's current zone, and there will not be a zoning change related to the relocation.
>
> 09X204 is currently split-sited in X824 at 108 West 174th Street, Bronx, NY and X980 at 1732 Davidson Avenue, Bronx, and serves grades K-5. In 2010-2011, it will move to X338 and will continue to serve grades K-5 in X338 with an enrollment of 315-400 students. The relocation of P.S. 204 will provide the school with sufficient space to accommodate all zoned students. This proposal will help to improve elementary school options for students in the northern area of District 9.

Appendix D: Event Time Data

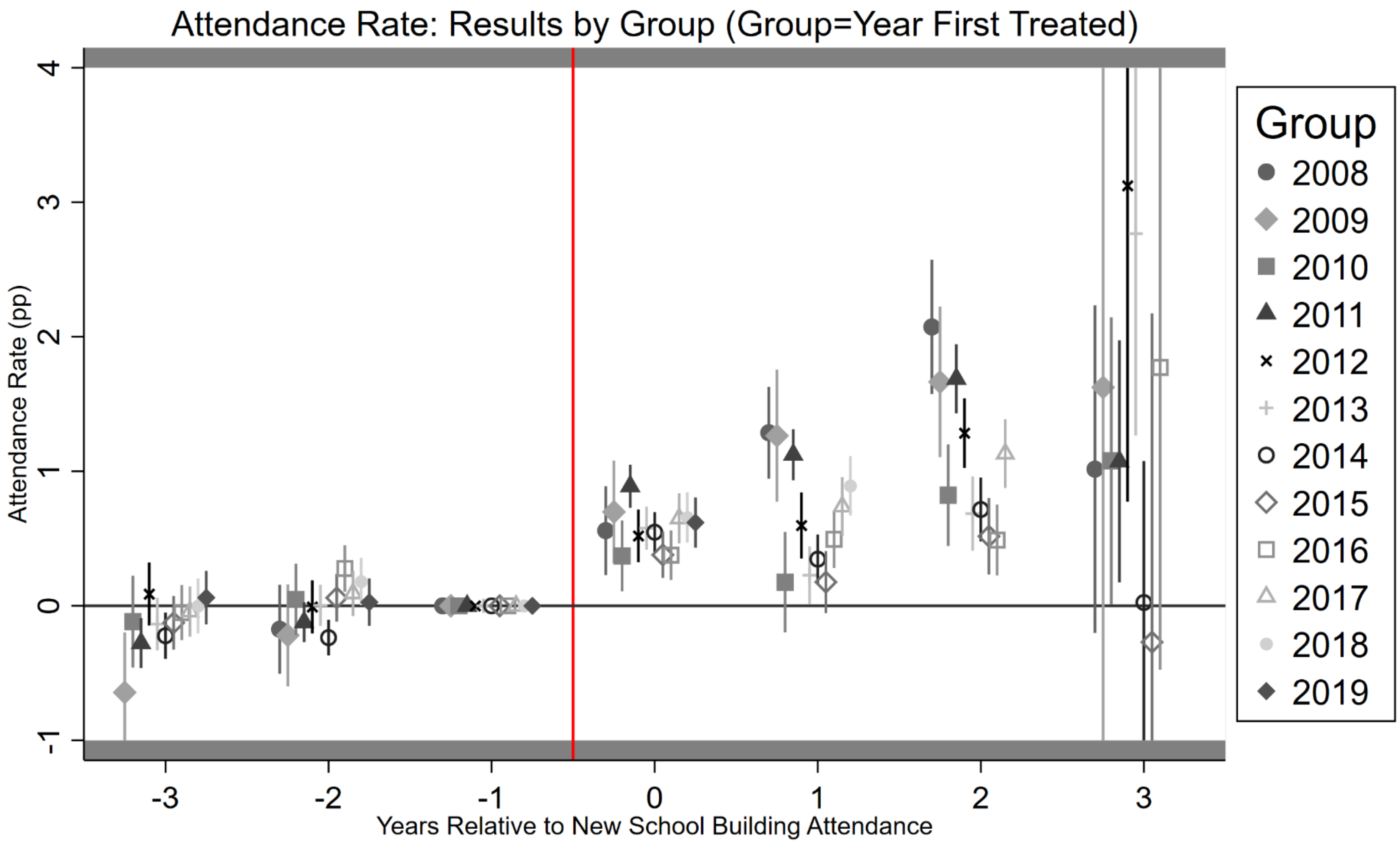
Table D1. Observations by Event Time

Event Time	Attendance Rate	Math Score	ELA Score
-4 or earlier	3,838	3,729	3,627
-3	19,322	18,951	18,693
-2	21,682	21,279	21,015
-1	23,980	23,749	23,776
0	23,979	23,606	23,641
1	18,459	17,807	18,027
2	13,639	10,916	13,166
3 or later	1,466	1,173	1,371
Total, Ever-treated Observations	126,365	121,210	123,316
Never-treated Observations	4,762,977	4,611,921	4,612,801
Total Observations	4,889,342	4,733,131	4,736,117
Unique Ever-treated Students	23,980	23,749	23,776
Unique Never-treated Students	1,061,190	1,061,190	1,061,190
Unique Students Total	1,085,170	1,084,939	1,084,966

Notes: This table shows observations by event time for the sample presented in Table 2 and used to estimate the results in Table 4 and Figure 6. Event time 0 is the first year a student attends a new school building. For all outcomes, 96% of ever-treated observations fall between event time -3 (three years before attending a new school building) and 2 (the third year a student attends a new school building). The same sample of students are used for all three outcomes, however, the number of observations varies due to different missingness in the outcome measures, and the number of unique ever-treated students varies because the outcome must be observed in event time -1 for the student to be included in the estimation for that outcome (see Methods).

Appendix E: Robustness and Sensitivity Analyses

Figure E1: Attendance Rate Results by Group



Note: These event study estimates for each group estimate form the basis of the event time estimates presented in Figure 6A and Table 4 (Callaway & Sant'Anna estimates). Vertical lines reflect 95% confidence intervals, trimmed to fit the graph as needed.

Table E1. Robustness of Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Main Results	K-12 Sample	Balanced In Event Time	No Residential Movers	Not-Yet Treated Comparison	Switch First Year Building Opens
A. Attendance Rate						
Event Time (k)						
-3	-0.109***	-0.025	-0.145***	-0.144***	-0.069	-0.514***
	(0.033)	(0.032)	(0.042)	(0.034)	(0.126)	(0.074)
-2	0.010	-0.024	-0.082**	-0.024	-0.028	-0.236***
	(0.027)	(0.026)	(0.037)	(0.029)	(0.106)	(0.054)
-1	.	.	.	.	.	.
	.	.	.	.	.	.
0	0.567***	0.365***	0.646***	0.512***	0.717***	0.593***
	(0.028)	(0.034)	(0.039)	(0.030)	(0.155)	(0.052)
1	0.569***	0.900***	0.559***	0.512***	1.766***	0.507***
	(0.037)	(0.042)	(0.046)	(0.041)	(0.569)	(0.074)
2	0.926***	1.193***	1.048***	0.880***	0.139	0.742***
	(0.048)	(0.056)	(0.055)	(0.053)	(1.676)	(0.106)
3	0.948***	1.060***		1.241***	0.009	0.521*
	(0.271)	(0.087)		(0.293)	(0.569)	(0.288)

	(1)	(2)	(3)	(4)	(5)	(6)
B. Math Scores						
Event Time (k)						
-3	-0.006		0.006	-0.008	0.100***	-0.059***
	(0.005)		(0.008)	(0.006)	(0.019)	(0.011)
-2	-0.013***		-0.002	-0.010**	0.043***	-0.052***
	(0.004)		(0.006)	(0.005)	(0.014)	(0.009)
-1	.		.	.	.	.
	.		.	.	.	.
0	-0.018***		0.002	-0.016***	-0.062***	-0.008
	(0.004)		(0.007)	(0.004)	(0.016)	(0.008)

Table E1. Robustness of Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Main Results	K-12 Sample	Balanced In Event Time	No Residential Movers	Not-Yet Treated Comparison	Switch First Year Building Opens
1	-0.011**		0.008	-0.006	0.050	0.006
	(0.005)		(0.007)	(0.005)	(0.034)	(0.010)
2	0.008		-0.003	0.014*	0.003	0.077***
	(0.007)		(0.008)	(0.008)	(0.064)	(0.014)
3	0.081***			0.048	0.114**	0.067**
	(0.024)			(0.029)	(0.052)	(0.032)

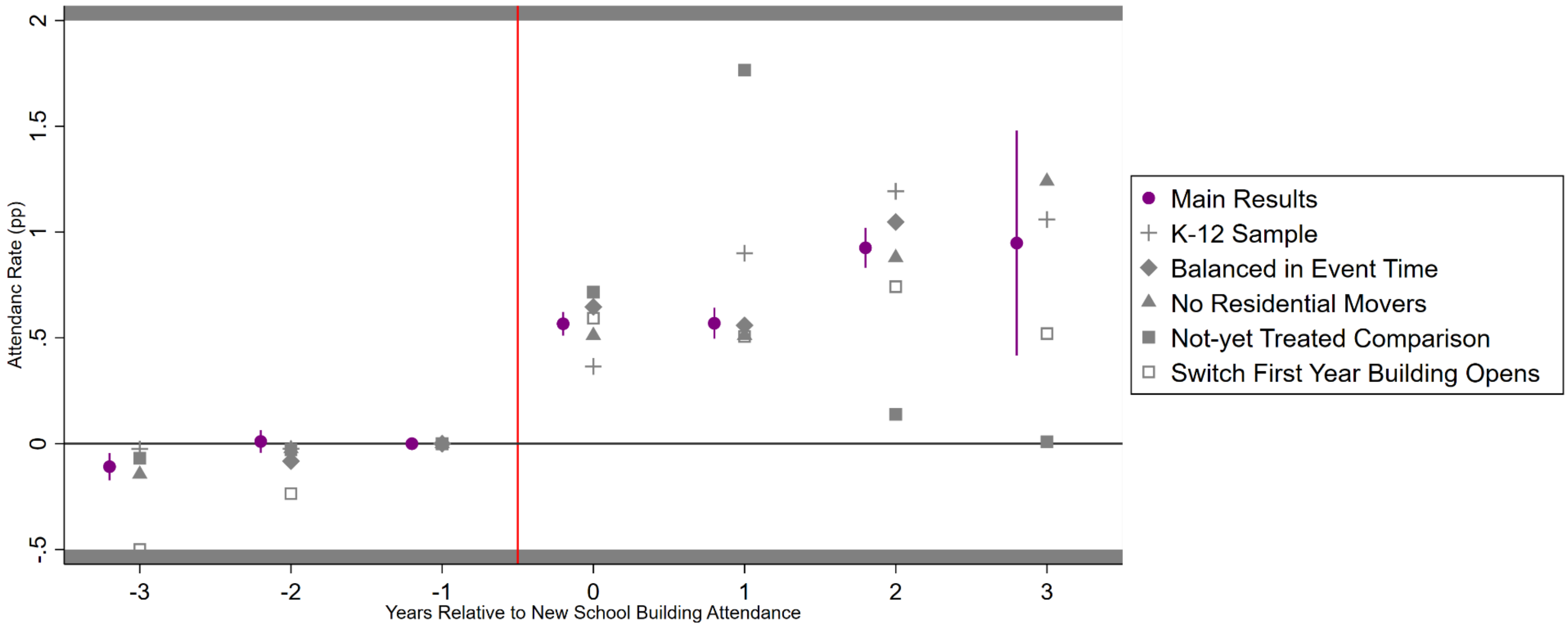
C. ELA Scores

	(1)	(2)	(3)	(4)	(5)	(6)
Event Time (k)						
-3	0.007		0.018**	0.006	0.117***	0.005
	(0.006)		(0.008)	(0.006)	(0.019)	(0.013)
-2	0.004		0.028***	0.005	0.073***	0.008
	(0.005)		(0.007)	(0.005)	(0.015)	(0.012)
-1	.		.	.	.	.
	.		.	.	.	.
0	-0.009**		0.003	-0.012**	-0.028*	0.042***
	(0.004)		(0.007)	(0.005)	(0.015)	(0.009)
1	-0.002		0.008	-0.002	0.036	0.007
	(0.005)		(0.007)	(0.006)	(0.034)	(0.011)
2	0.010		0.023***	0.011	0.075	0.067***
	(0.006)		(0.008)	(0.007)	(0.098)	(0.015)
3	0.059**			0.046	0.110**	0.025
	(0.023)			(0.028)	(0.051)	(0.031)

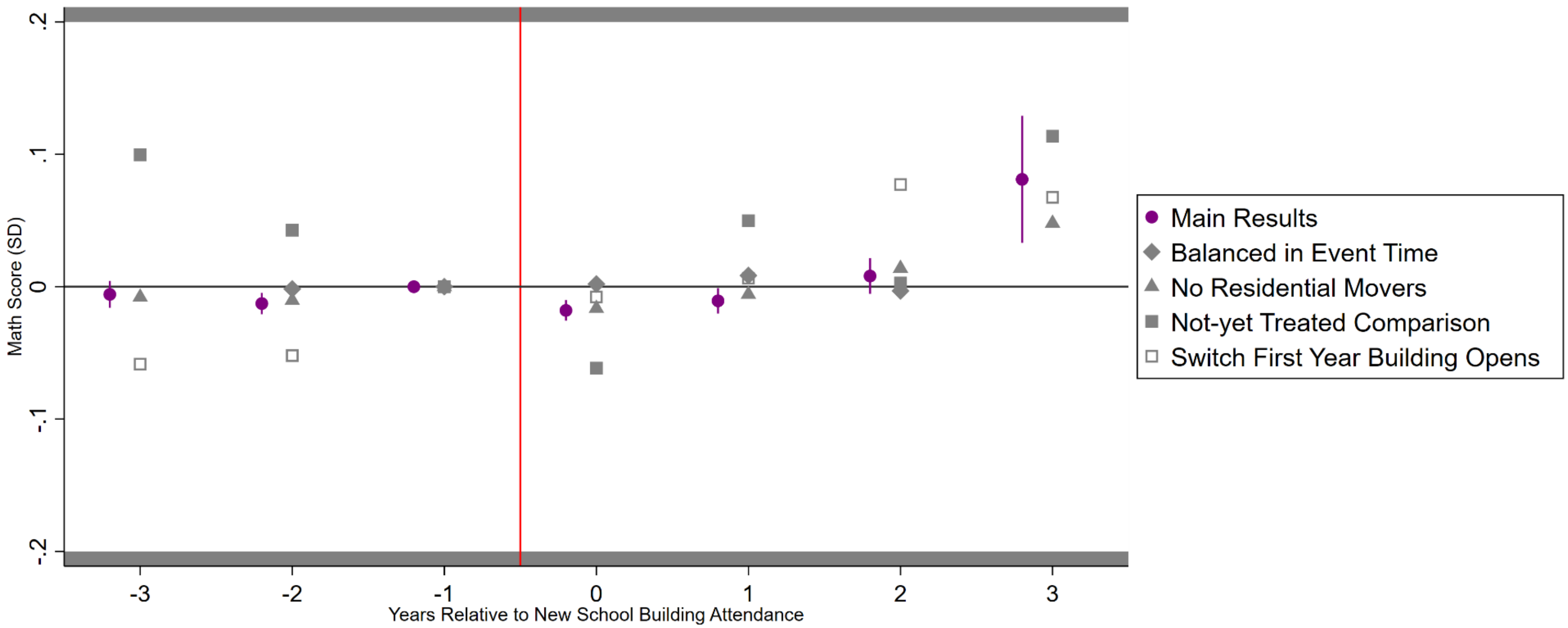
Notes: The table shows estimates from the Callaway & Sant-Anna estimator described in Methods. "Main Results" results replicate the results in Table 4. "K-12 Sample" uses a sample of all students in grades K-12 who otherwise meet the inclusion criteria described in the text (this only applies to Attendance Rate results). "Treated Students Balanced in Event Time" limits the sample of treated students to students whose outcomes are observed in each event time from k=[-2, 2] (but still uses all never treated students as the comparison group). "No Residential Move Sample" excludes students who make a residential move the year before or the same year they switch into a new school building. "Not Yet Treated as Comparison" uses students not-yet treated as the comparison. "Students Treated First Year New Building Opens" limits the sample of treated students to students who switch into a new school building in the year it opens (but still uses all never-treated students as the comparison group).

Figure E2. Robustness

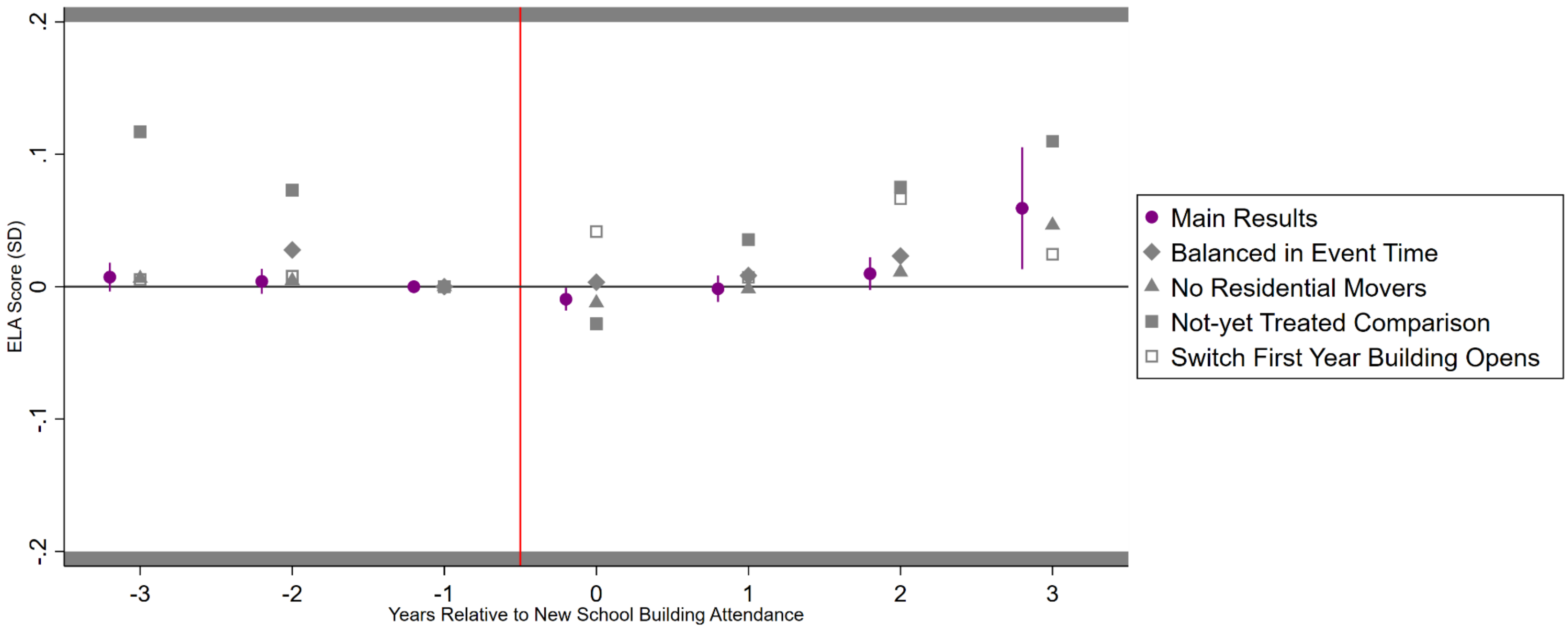
A. Attendance Rate



B. Math

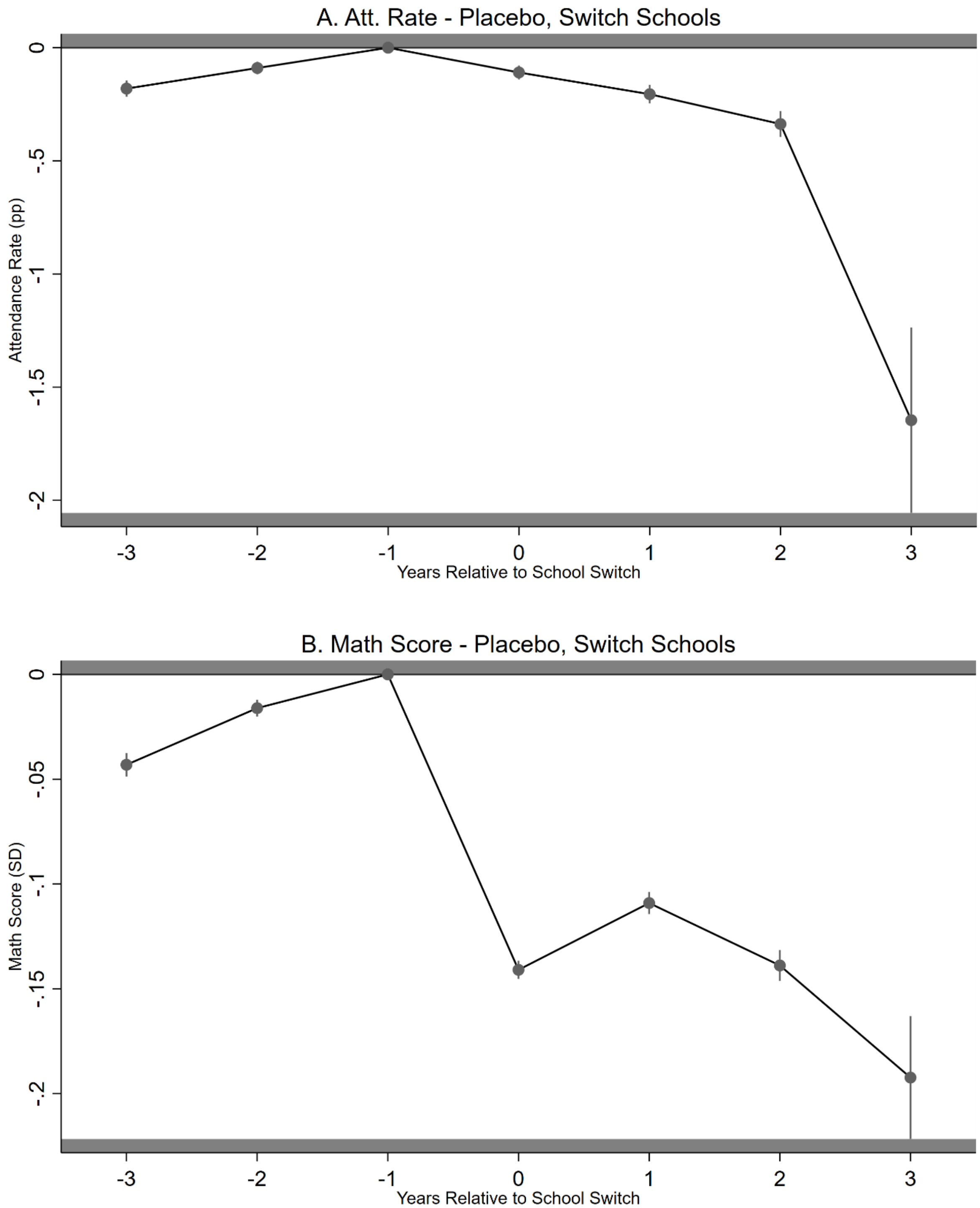

Math Score (SD)
.2
.1
0
-.1
-.2
-3
-2
-1
0
1
2
3
Years Relative to New School Building Attendance
Main Results
Balanced in Event Time
No Residential Movers
Not-yet Treated Comparison
Switch First Year Building Opens

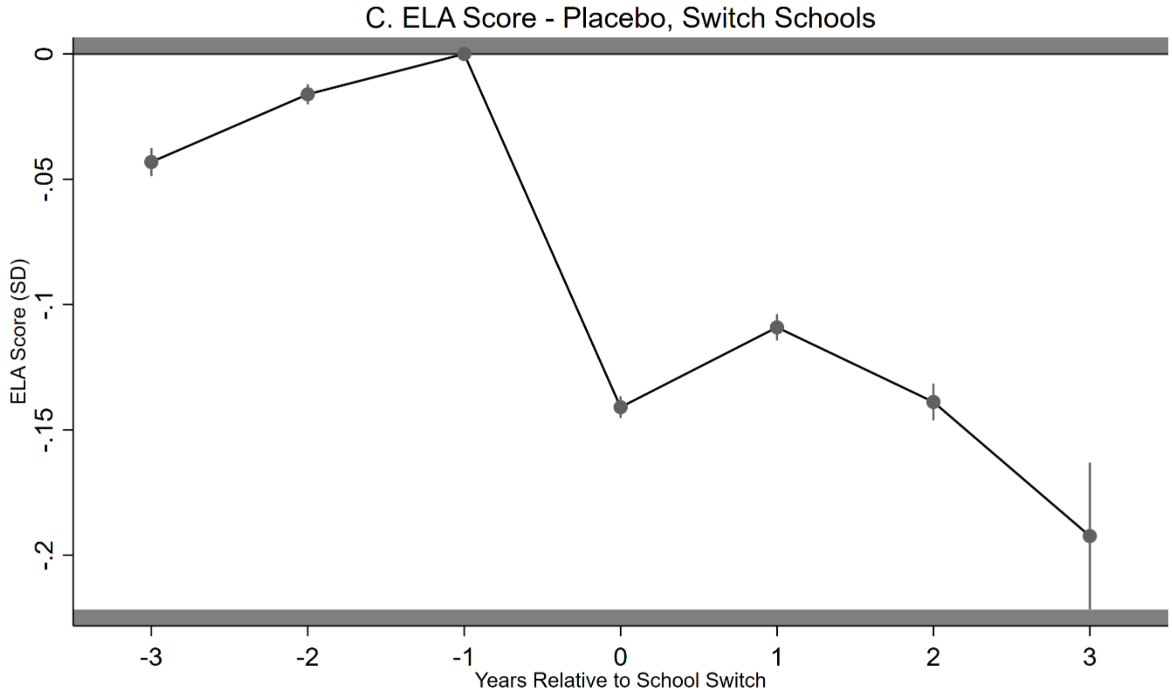
C. ELA



Notes: The figures show estimated coefficients from the Callaway & Sant-Anna estimator described in Methods and presented in Table E1. The "Main Results" results replicate the results in Figure 6. Vertical lines reflect 95% confidence intervals for these estimated coefficients; confidence intervals for the other estimates are not shown for clarity (see Appendix Table E1). "K-12 Sample" uses a larger sample of all students in grades K-12 who otherwise meet the inclusion criteria described in the text (note this only applies to Attendance Rate results). "Treated Students Balanced in Event Time" limits the sample of treated students to students whose outcomes are observed in each event time from k=[-2, 2] (but still uses all never treated students as the comparison group). "No Residential Move Sample" excludes students who make a residential move the year before or the same year they switch into a new school building. Dependent variables are attendance rate (Panel A), math test scores (Panel B), and English-language arts (ELA) test scores (Panel C). Test scores are standardized to have a mean of 0 and standard deviation of 1 using citywide data for each year-grade-subject exam. "Not Yet Treated as Comparison" uses students not-yet treated as the comparison. "Students Treated First Year New Building Opens" limits the sample of treated students to students who switch into a new school building in the year it opens (but still uses all never treated students as the comparison group).

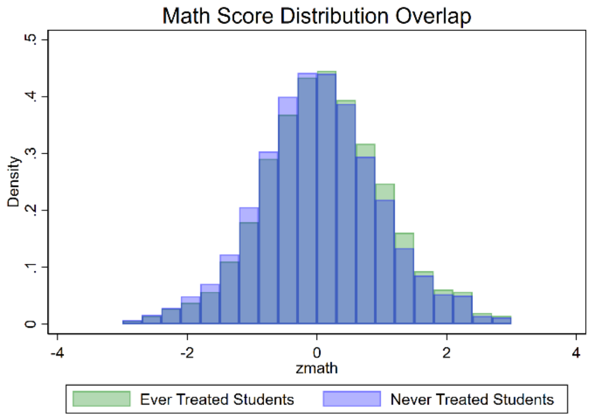
Figure E3. Placebo, Estimating the Effect of School Switches Unrelated to Attending a New School Building

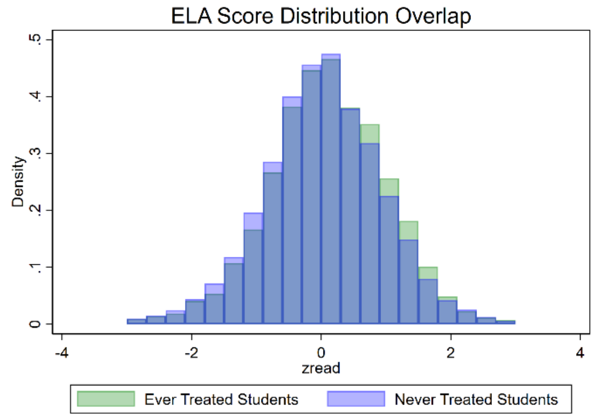




Notes: Figures show estimated coefficients from event study regressions using the Callaway & Sant'Anna estimator described in the text, however, the "treatment" is defined as switching into a new school (but not a new school building). Dependent variables are attendance rate (Panel A), math test scores (Panel B), and English-language arts (ELA) test scores (Panel C). Test scores are standardized to have a mean of 0 and standard deviation of 1 using citywide data for each year-grade-subject exam. Vertical lines reflect 95% confidence intervals for the estimated coefficients. Standard errors are clustered by student. Estimates in event time $k = 3$ (after four years attending a new school building) are based on relatively few observations.

Figure E3. Comparing Distribution of Test Scores for Ever Treated and Never Treated Students





Note. The distribution of test scores for ever treated students is based on test scores in the year before treatment (i.e., the year before they switch into a new school building). The distribution of test scores for never treated students is based on all student-year observations. For clarity, a small number of outliers (students with standardized math or reading scores greater than 3 or less than -3) are dropped. For math, this drops 55 students in the ever-treated sample (0.2%) and 25,316 student-year observations in the never treated sample (0.5%). For ELA, this drops 216 students in the ever-treated sample (0.9%) and 44,984 student-year observations in the never treated sample (0.9%).

Appendix F: Benchmarking Estimates Against Expected Test Score Effects and an Informal Break-Even Analysis

The average cost per seat for the projects in my sample is $117,814 (2019$). Since capital spending goes toward durable assets used for several years, it is inappropriate to relate outcomes in a given year to spending on capital that same year. I follow Jackson and Mackevicius (2024) in calculating an amortized flow value of the capital outlay: they assume 4.7% annual depreciation for 50 years for new school buildings. Based on this calculation, the average per-student spending over the first four years of the useful life of a school building in NYC is $5,159.

Jackson and Mackevicius (2024), in their meta-analysis, find each $1,000 in capital spending per student annually over four years is associated with a 0.027 standard deviation (sd) improvement in test scores. For the average annual spend of $5,159 in the NYC projects I considered, I would therefore expect a 5.159 x 0.027 or 0.139 sd improvement in test scores. The confidence intervals on my estimates for math effects after 3-4 years in new school building are [-0.005, 0.021] and [0.033, 0.129], respectively. The confidence intervals for my estimates for ELA effects after 3-4 years in a new school building are [-0.003, 0.022] and [0.013, 0.105], respectively. Therefore, the expected effect of 0.139 sd is outside the 95% confidence interval of my estimates (see Table 4 for point estimates and standard errors).

However, a smaller improvement in test scores may still give the project a positive net present value, depending on the value of the test score improvement. Assigning a dollar value to test score improvements is challenging, as there are few published shadow prices of gains in achievement (Levin et al., 2018). Chetty et al. (2011) provide present values of certain types of education investments based on observed impacts on wages at age 27. They estimate that the present value of a 0.1 SD increase in test scores due to improved class quality is $3,910 (2009$), using a social discount rate of 3% and discounting back to age 6, the age of the intervention they are studying. I adjust this estimate using a social discount rate of 3.5%, discount back to age 10 (the average age from Grade 3-8, the grades treated in my sample), and convert to 2019 dollars. Based on this, a 0.1 sd improvement in test scores is worth $4,526. Therefore, for the new school construction projects to "pay for themselves", they would need to generate average test score improvements of $5,159/($4,526/0.1sd) or 0.114 sd. This is close to or outside the upper limit of my 95% confidence interval of the effects of attending a new school building after four years.

There are significant limitations to this informal break-even analysis. Total costs are likely higher than the direct spending on infrastructure that I account for—for example, there are likely increased facilities maintenance and operation costs associated with these new school buildings, as well as the cost of dead weight loss from taxation used to pay for these investments. However, the more significant limitation is the enumeration of benefits and calculation of their value. School buildings are social infrastructure (Klinenberg, 2018): they provide sites for after school and summer school programming, playgrounds and recreational spaces for the community, and serve other community purposes such as voting sites and emergency shelters. Failing to account for these benefits will understate the value of new school buildings. Even if impacts on students served during the school day were the only benefit of school facilities, the monetization of test score impacts may understate their true value. Jackson and Mackevicius (2024) find that test scores may understate the benefits of school spending, because school spending impacts on educational attainment are larger than impacts on test scores. That is, using a shadow price from a different intervention may understate the actual benefit that students derive from attending a new school building.