School Proximity, Attendance, Stability, and Achievement among Homeless Students

Mike Cassidy
Princeton University

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Abstract

More than one million students in the United States experience homelessness annually. Among their challenges is getting to school. This paper uses novel administrative data and a natural experiment in shelter scarcity to assess the effects of school proximity. For the average homeless K–8 student, a 10-mile longer commute leads to 6–13 percent more absences, a quarter higher probability of changing schools, and a decline in math test scores of 0.03–0.11 standard deviations. A complementary difference-in-differences design reinforces the importance of distance. The prevalence of housing instability in public schools suggests broad policy relevance. (*JEL I21, R28, I28, I38, H53, H75*)

Data Availability Statement: The data used in this paper are confidential administrative records maintained by New York City (NYC) and New York State. Upon publication, I will make public as much code as I am permitted. Others who wish to access this data may contact the NYC Center for Innovation through Data Intelligence (`https://www.nyc.gov/site/cidi/contact/contact.page`).

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1 Introduction

1.3 million public school children in the United States—about three percent—are homeless during each academic year.[1] The share of students experiencing irregular housing at any point during their educational careers is even larger.[2] These students are among the most vulnerable to educational failure. Figure 1, for example, shows that, in New York City (NYC), the typical primary school student in a homeless shelter is absent double-to-triple the amount of their housed peers and misses school at a frequency 25 percent in excess of students who are deemed chronically absent.[3] These students face many disruptions in their lives, including the possibility of abrupt moves to shelters or other temporary residences that may be far from their existing schools.[4]

This paper examines the educational effects of distance between homeless students' shelters and schools using unique administrative data from New York City. The data combine records from the City's Department of Homeless Services (DHS) with public school records for school years 2010-11 to 2015-16. With one million students, NYC's public school system is the nation's largest school district by a factor of two and the place of learning for one in twelve homeless students nationwide (National Center for Education Statistics, 2019, 2021*b*). More than a tenth of NYC public school students endure some form of homelessness each year. In aggregate, these homeless students, by themselves, rank among the top 25 largest school districts nationwide (NYSTEACHS, 2019; National Center for Education Statistics, 2021*b*). Hence, NYC is a particularly important setting for studying student homelessness.

The City maintains a policy of placing families in the shelters nearest their youngest children's schools. However, during the time period of this study, NYC experienced a surge in homelessness, owing, in part, to the ending of a popular rental assistance program in the context of a unique municipal right-to-shelter mandate. The surge made shelter scarce and DHS was able make placements that conformed with the policy only about half the time. According to City officials—and as confirmed by empirical balance tests—placement location is largely a function of what's available at the time of shelter entry. I use this natural experiment in first-come, first-serve shelter assignment to estimate the causal effects of school proximity.

[1] U.S. Department of Education (2023); National Center for Education Statistics (2022, 2019).

[2] For example, the New York City homeless K–8 student data used in this paper (and described in Section 3) spans seven years of shelter entries and includes 71k unique students (see Table A.1), considerably more than the approximately 17k in shelter during each school year.

[3] The NYC Department of Education defines "chronically absent" as missing school 10 percent or more of the time, equal to about 18 days. The median sheltered student is absent 23 days per year.

[4] For additional comparisons between homeless and housed students in NYC, see Figures A.1 (English state test scores), A.2 (math state test scores) A.3 (overall proficiency), A.4 (school change), and A.5 (promotion).

In contrast to earlier work examining the mostly small effects of proximity to school on older and better-off children (see Angrist et al. (2022) and Cordes, Rick and Schwartz (2022)),[5] I find that proximity has large impacts on homeless students' attendance, achievement, and school stability. Primary school students (grades K–8) assigned 10-mile longer commutes[6] are 22.5–24.7 percent more likely to change schools (about 11 percentage points), have 6.4–13.0 percent more absences (about 2–4 days), and score 0.03–0.11 standard deviations (σ) worse in math. These effects on test scores are about a sixth to a third of: the value added by charter schools (Angrist, Pathak and Walters, 2013; Abdulkadiroğlu et al., 2016); small class sizes (Krueger, 1999); replacing the worst schools with average schools (Angrist et al., 2017); or (early) participation in Head Start (Kline and Walters, 2016). They are also similar to the effect of one-standard-deviation increases in either teacher quality (Chetty, Friedman and Rockoff, 2014) or specific charter school pedagogical strategies (e.g., increased instructional time) (Dobbie and Fryer Jr, 2013).

I also find meaningful heterogeneity. The students who are most sensitive to distance are those who live closest to their schools prior to shelter entry, with students in the nearest quartile of pre-shelter distance seeing attendance gains triple of that of those in the furthest quartile. Also more responsive than average are students whose schools origin are worse (e.g., students attending schools with above-median absenteeism have an attendance effect 1.6 times that of students in below-median schools), as well as those students whose shelter placements engender transfers to schools with more absenteeism than their origin schools (e.g., students in the worst quartile of increased school absenteeism have an attendance effect 2.7 times that of the quartile of students making the most attendance-favorable transfers). In addition, I find that residing very close to one's school is considerably more valuable than residing a little further away (e.g., students gain 0.01σ/mile in math for the first few miles closest to school, but the benefit of the marginal mile fades to zero by about 12 miles). Unsurprisingly, the effects of proximity are strongest during the year of shelter entry. However, the increased probability of changing schools that comes with greater distance persists in subsequent years.

These findings are based on three complementary identification strategies and are robust across a series of checks, which include alternative treatment measures, sample restrictions, outcome measures, and covariate specifications. The first, and main, identification strategy

[5]Using randomness in school assignment algorithms in New York City and Boston public schools, Angrist et al. (2022) find that attending a school further from home enhances integration but does not affect test scores or college attendance among sixth and ninth graders. Exploiting quasi-randomness in NYC school bus routes, Cordes, Rick and Schwartz (2022) find similarly modest results among (relatively advantaged) yellow bus riders—a less than one percent impact on attendance of commutes longer than an hour, and, again, no impact on test scores.

[6]The average commute distance at shelter entry is 8.2 miles.

exploits quasi-random shelter assignment and the richness of the administrative data. The second strategy takes advantage of the fact that some students experience multiple spells of homelessness and uses student fixed effects to control for unobserved family characteristics constant across spells. The third strategy, a difference-in-differences design capitalizing on the panel structure of the data, additionally confirms the attendance and school stability results among a subset of students who are observed both before and after becoming homeless, but is imprecise for math.

This paper is among the first in economics to study homeless students.[7] Most work thus far has been descriptive,[8] and confirms what has been long-observed by other social scientists: Unstably housed students struggle in school.[9] Thus, a key contribution of this paper is to introduce a causal inference perspective to student homelessness, and, in so doing, gain insight into one mechanism for addressing a challenge common to many housing-unstable or otherwise vulnerable students. My findings show that shorter trips to school can bring immediate, meaningful educational gains to this very disadvantaged population. Unlike single adult homelessness, family homelessness is mostly not pathological; it is pecuniary—the product of scarcity and happenstance (O'Flaherty, 2010; O'Flaherty, 2019). Because homeless families are like other poor families, it follows that homeless student responses to proximity may hold lessons that are generalizable—and which should be kept in mind when designing policies addressing school choice and residential mobility among poor families.

2 Background and Context

2.1 Related Literature

This paper contributes to several literatures in economics and social science. Economists have long been interested in the idea that proximity shapes educational opportunities and outcomes (Card, 1995). School quality is a key determinant of residential choice (Black, 1999). Conversely, proximity is an important determinant of school choice. Parents (and

[7] O'Flaherty (2019) and Evans, Phillips and Ruffini (2021) provide helpful summaries of the recent and growing literature on homelessness in economics; notably, research on education is not mentioned.

[8] De Gregorio et al. (2022) find that homelessness is associated with worse test scores and increased absenteeism in LA. In addition, homeless students attend schools (and live in neighborhoods of) concentrated disadvantage—though school mobility is associated with quality upgrades (Dhaliwal et al., 2021). Nationwide, at the school district level, access to cash assistance (TANF) is associated with a reduction in family homelessness (Parolin, 2021). Several works in economics also helpfully investigate the antecedents and attributes of family homelessness (e.g., O'Flaherty (2004, 2010)), as well as its (adverse) implications for educational attainment and employment in adulthood (Cobb-Clark and Zhu, 2017).

[9] For helpful summaries of the social science literature on homeless students outside of economics, see Buckner (2008); Miller (2011); Samuels, Shinn and Buckner (2010).

students) are willing to trade longer commutes for school quality (Burgess et al., 2015; Abdulkadiroğlu, Agarwal and Pathak, 2017; Barrow, 2002; Owusu-Edusei, Espey and Lin, 2007; He and Giuliano, 2018).

This potential trade-off between proximity and quality has become increasingly salient as school choice has expanded in recent decades. Indeed, it appears to be empirically operative: The rich literature on school choice is decidedly mixed, with effects that can be salubrious (Hoxby, 2003; Deming et al., 2014), modest (Cullen, Jacob and Levitt, 2006; Epple, Romano and Urquiola, 2017), or disadvantageous (Abdulkadiroğlu, Pathak and Walters, 2018), depending on context. Yet when proximity has been studied specifically, it seems to have small effects (Angrist et al., 2022; Cordes, Rick and Schwartz, 2022), raising questions about the extent to which it is responsible for the mixed results on school choice.[10] In this paper, I offer some resolution to this puzzle, demonstrating that proximity matters among a particularly disadvantaged population of students in a well-identified setting.

Closely related, given the link between long commutes and changing schools (Blagg, Rosenboom and Chingos, 2018), is the literature on school mobility. By and large, moves tend to be educationally detrimental for movers, especially in the short-run and when moves are intra-district. What's more, transfers impose considerable negative externalities on incumbents (Hanushek, Kain and Rivkin, 2004; US Government Accountability Office, 2010; Gibbons and Telhaj, 2011). However, there is some evidence for net gains to movers if moves are articulated (i.e., to start a school at its lowest grade), enduring, or permit access to substantially better schools (Welsh, 2017; Schwartz, Stiefel and Cordes, 2017). As is the case with proximity proper, a recent comprehensive review of the mobility literature concludes by cautioning that "strong causal claims are elusive" (Welsh, 2017, p. 475).

This stands in contrast to the literature on the benefits of better neighborhoods (Chyn and Katz, 2021). Although the early literature on "moves to opportunity" found little impact on low-income children's educational performance (Fryer Jr. and Katz, 2013; Ludwig et al., 2013; Jacob, Kapustin and Ludwig, 2015), the preponderance recent evidence suggests that the benefits are large and may simply take time to accumulate (Chetty, Hendren and Katz, 2016; Chetty and Hendren, 2018*a*,*b*; Chyn, 2018). These results are in keeping with the literature on the enduring legacies of early life experiences (Almond and Currie, 2011; Almond, Currie and Duque, 2018).[11] In the present study, I harmonize these themes, showing

[10]In the broader social science literature, a recent systematic review of transportation and academic outcomes found that, in urban areas, longer commutes are associated with more absences, but with no (or favorable) impacts on tests scores, likely owing to improved school quality. However, the authors caution that the evidence is mostly descriptive (Hopson et al., 2022).

[11]In NYC, specifically, long intracity residential moves have seem to have negative effects on test scores, likely because they are associated with school changes. On the other hand, short-distance moves can be beneficial, perhaps owing to improved housing quality paired with school stability (Cordes, Schwartz and

that, for homeless students, residential location can have immediate impacts on educational outcomes, among the most important of which is school stability.

2.2 The Setting: Student Homelessness in New York City

A unique municipal legal right to shelter—in combination with a scarcity of affordable housing, a tradition of progressive politics, and an extensive municipal social service apparatus—has made sheltered family homelessness[12] a particularly common manifestation of acute poverty in NYC.[13] The city is home to one in five sheltered homeless families nationally (de Sousa et al., 2022; U.S. Department of Housing and Urban Development, 2023). And while family homelessness has declined nationwide by a third since 2009, DHS' census grew from 8,081 families in March 2009 to 12,427 in March 2019—though down from its November 2018 peak of 13,164 (New York City Department of Homeless Services, 2019*b*).

A large part of the explanation is that NYC is one of just two jurisdictions in the U.S. where families have a legal right to shelter, emanating from a series of consent decrees negotiated in the courts during 1980's.[14] Against this backdrop, the City's main rental assistance program for homeless families ended unceremoniously as a result of a dispute with the State in 2011 (Iosso and Rein, 2022).[15]

Some 16,800 of the city's public primary schoolers reside in homeless shelter each year. These homeless K–8 graders in NYC average 27 absences annually. 45 percent transfer schools. Just 5 percent are proficient in both English and mathematics. (Yet 94 percent are promoted.)[16] The City spends upwards of $1 billion annually to shelter these students

Stiefel, 2019).

[12]In this paper, I define "homeless" as "in DHS shelter." This is the operative concept since the policy in question is shelter-based. This is also the standard NYC DHS uses when reporting the City's family homeless census. However, it excludes some 69,000 students who are living doubled-up or in other temporary arrangements (NYSTEACHS, 2022), and whom are classified as homeless under federal education law (Homeless Emergency Assistance and Rapid Transition to Housing Act of 2009, 2009). Due to the City's right to shelter, virtually no families go unsheltered.

[13]See, e.g., O'Flaherty and Wu (2006); Ellen and O'Flaherty (2010); NYU Furman Center (2016); New York City Mayor's Office (2017); Collinson et al. (2022).

[14]The state of Massachusetts is the other. See New York City Independent Budget Office (2014) and University of Michigan Law School (2017) for details.

[15]More recently (post the period covered by this study), the City's shelter system has faced a series of unprecedented challenges. The COVID-19 pandemic—along its disruptive aftermath and unprecedented fiscal stimuli—saw the family shelter population drop to a low of 8,292 families in July 2021, followed by an even steeper rise in 2022 amid an influx of asylum seekers that saw the system reach an all-time high of 16,453 families in July 2023 (New York City Department of Homeless Services, 2023; Office of the New York City Comptroller Brad Lander, 2023*a*). For a helpful and detailed history of family homelessness in NYC, see Iosso and Rein (2022).

[16]2014 and 2015 school year averages, excluding students in charter schools, derived from the homeless student data described in Section 3. Appendix Tables A.2A–A.2B provide mean comparisons between homeless students and their housed peers. For comprehensive descriptive data on student homelessness in

and their families—equivalent to about $190 *per family per day* (New York City Office of Management and Budget, 2023; New York City Mayor's Office of Operations, 2023).[17]

Their families are among the most invisible of society's most obviously afflicted populations. Unlike the single adult street homeless who dominate the popular consciousness, homeless families are not distinguished by substance abuse or mental illness but instead by a particularly pernicious form of poverty: the lack of regular places to call home. Typically consisting of a high-school-educated, racial minority single mom with one or more young children previously living in unfavorable conditions, homeless families look like other poor families because they *are* like other poor families (see Appendix Tables A.3A–A.3B for descriptive statistics)—albeit momentarily on the losing end of chance encounters with poverty's vicissitudes, such as health crises, job losses, or domestic disputes. Most recover quickly enough, and are sheltered for brief periods, never to return. Family homelessness is a phase, not a trait.[18]

The government agency charged with addressing homelessness in NYC is the Department of Homeless (DHS), a division of the larger Department of Social Services (DSS).[19] The DHS-administered shelter system is vast, consisting of over 500 distinct sites operated largely by contracted non-profit social service organizations.[20] During the period I study, a modest majority of families are placed in traditional "Tier II" homeless shelters, which offer on-site social services and security but otherwise resemble the sorts apartment buildings typically found in low-income communities. In addition, to expand capacity on demand (under the threat of lawsuit), the City also utilizes "cluster" apartments scattered among otherwise private buildings in a given area, as well as commercial hotels (New York City Independent Budget Office, 2014; New York City Mayor's Office, 2017).[21]

While shelter is guaranteed to all who need it, families presenting themselves as homeless must submit to an eligibility determination process.[22] All families apply in-person at DHS'

NYC, see Institute for Children, Poverty & Homelessness (2022).

[17]Even this an understatement, as it excludes administrative costs, prevention programs, and permanent housing subsidies, as well as public benefits and services administered by other agencies.

[18]For more about the characteristics of family homelessness, see, e.g, O'Flaherty (2004); Culhane et al. (2007); Ellen and O'Flaherty (2010); O'Flaherty (2010).

[19]Established as a subdivision of DSS, DHS was spun off as an independent agency in 1993. In 2016, the former was once again subsumed by the latter. However, DHS is typically referred to as distinct. See New York City Department of Homeless Services (2019*a*) for more detail. DSS is also known as the Human Resources Administration (HRA).

[20]88 percent of DHS' expenditures during fiscal year 2022 were for human services contracts (New York City Mayor's Office of Operations, 2023). This is typical of social services provision in NYC.

[21]In recent years, the use of cluster sites has largely ended (NYC Department of Homeless Services, 2021).

[22]Unless otherwise noted, information on NYC's homeless eligibility and intake process in this section derives from New York City Department of Homeless Services (2019*c*), New York City Independent Budget Office (2014), New York City Independent Budget Office (2016), NYC Department of Homeless Services (2023), Office of the New York City Comptroller Brad Lander (2023*b*), and conversations with City officials.

Prevention Assistance and Temporary Housing (PATH) intake center in the Bronx. Families are first screened for domestic violence and homeless prevention services (e.g., rent arrears payments); those unable to be diverted are interviewed by DHS caseworkers about their housing situations and granted conditional shelter stays for up to 10 days while investigation staff assess their claims. At minimum, applicant families must have one member under 21 or pregnant and demonstrate that they have no suitable place to live, through documentation and review of their housing histories. Those found eligible may remain in their initial shelter placements as long as necessary. Ineligible families may appeal their decisions through a fair hearing process or reapply. Most ineligibilities occur due to failure to comply with the eligibility process (including voluntary withdrawals) or because other housing is deemed available. Eligible families may request transfers to more suitable units.[23]

To help address the challenges homeless students face, the City has, since at least 1998, maintained the explicit goal of placing homeless families the in shelters nearest their youngest children's schools.[24] In part, this neighborhood-based shelter placement policy facilitates compliance with the federal McKinney-Vento Homeless Assistance Act (42 U.S.C. 11431 et seq.), which requires local education agencies to provide the services necessary for homeless students to remain in their schools of origin, if desired. The theory is that minimizing educational disruption will improve academic outcomes. Increasingly, the policy has also come to reflect the conviction that keeping homeless families connected to their communities of origin—close not only to schools, but also to family, friends, jobs, places of worship, and other sources of support—is a means of expediting the return to more stable housing (New York City Mayor's Office, 2017).

Officially, the placement target is the shelter nearest a family's youngest child's school; in practice, DHS counts as successful any placement occurring in the youngest child's school borough (New York City Mayor's Office of Operations, 2023).[25] With the rapid expansion of the City's family homeless population during the last decade, achieving this objective has become a not inconsiderable challenge. During the period this paper studies, shelter vacancy rates consistently hover below 1 percent (New York City Mayor's Office, 2017). Whereas 82.9 percent of homeless families were successfully placed in-borough in 2008, just 49.8 percent

[23]Because in-shelter transfers may be endogenous, I define treatment based on families' initial shelter assignment. I also confirm that the main results are robust to subsamples meeting more stringent criteria (e.g., no within-shelter moves; stays of minimum length).

[24]New York City Mayor's Office (2017); New York City Mayor's Office of Operations (2002); New York City Department of Education (2019).

[25]NYC consists of five counties, or boroughs: Manhattan, the Bronx, Brooklyn, Queens, and Staten Island. The City's public school system, while unitary, is divided into 32 geographic subdivisions, called districts, for administrative and admissions purposes. Districts are further subdivided into school zones. Students may apply to attend schools outside their residential districts.

were by 2018. Subsequently, there has been some improvement, with DHS averaging an initial in-borough placement rate of 58 percent from City fiscal years 2020–2022 (New York City Mayor's Office of Operations, 2010, 2018, 2023).[26]

Aside from children's schools, DHS caseworkers also take into consideration safety (e.g., domestic violence (DV) victims are placed away from their abusers), family size (e.g., larger families legally require more bedrooms), and health limitations (e.g., walk-ups are not suitable for mobility-impaired families) when assigning shelter placements. According to City officials, conditional upon these criteria, which families end up with preferential placements near their children's schools depend entirely on what units are available at the time families apply.[27] This scarcity-induced quasi-randomness is the natural experiment at the core of my lead identification strategy.

Figure 2 depicts the geographical distribution of NYC's homeless K–8 graders across boroughs and school districts. The largest numbers of homeless students hail from the Bronx and East Brooklyn, which is not surprising given that these are among the poorest neighborhoods in NYC (Figure A.6).[28]

3 Data

3.1 Data, Definitions, and Sample

The analysis proceeds from a novel administrative panel consisting of a near-census of public school students whose families entered shelter in NYC during the 2010 to 2015 school years.[29] I construct it by linking administrative records maintained by the City's Department of Homeless Services (DHS) and Department of Education (DOE).[30] For these students, I observe educational histories spanning 2005–2016, as well all shelter experiences occurring during calendar years 2010–2016. To this, I append additional information about family

[26] City fiscal years run from July to June, and are named for the year in which they end, so they are coincident with school years, as I've defined them in this paper, though the latter are named for their starting years.

[27] Based on conversations with City officials. This claim has also been reported publicly; see, e.g., Shapiro (2020).

[28] Correspondingly, Figure A.7 characterizes the (largely overlapping) geographic spread of homeless students according to the school district of their (initial) shelter assignments. Perhaps not surprisingly, poor and/or far-flung school districts are also where homeless students have the most absences in the year prior to shelter entry (Figure A.8).

[29] Unless otherwise noted, all years referenced in this paper refer to school years, beginning in July and ending in June, and named for the starting year (e.g., the 2015 school year runs from July 1, 2015 to June 30, 2016).

[30] Specifically, these students' families applied and were deemed eligible for homeless shelter between 1/1/2010 and 12/31/2016. For an additional details about the construction and content of the dataset, see Appendix A.

characteristics and public benefit use from the City's Department of Social Services (DSS), and data on employment and earnings from the New York State Department of Labor (DOL).

The unit of observation is the student-school-year. As shown in Table A.1, the overall homeless student panel, consisting of all school years observed for any K–8 student whose family entered shelter during this period, contains of 348,578 observations across 70,631 unique students. Students are observed for 1–12 school years, with the average student appearing five times during grades K–8.

Because the analysis is focused on the specific question of how proximity to school affects educational outcomes, the analytical sample consists of a curated subset of observations. Table A.5 describes the path from the full data to the analytical sample. Specifically, I restrict the analysis to students in grades K–8 (primary schoolers are the focus of the proximate placement policy), during school years 2010–2015 (these are the school years with complete coverage in the DHS data), who are not in charter schools and who are not missing attendance data,[31] and who are enrolled in DOE more than 180 days prior to the date of shelter entry (to avoid spurious placements among migratory homeless families). The remaining 62,160 student-school-year observations are a mix of school years prior to, during, and post shelter spells, with new spells defined as entries occurring more than 30 days after the conclusion of a prior stay. Spells may begin at any time during the school year. Some spells span multiple school years. Some students have multiple spells.

For the main analysis, I reduce the panel to a pooled cross section, restricting the sample to students' school years of shelter entry, which sharpens the focus around the onset of homeless spells and treatment assignment. This leaves 31,886 observations. Finally, I exclude school-shelter commute distance outliers whose estimated commutes equal or exceed the 95th percentile in the sample (22.9 miles), arriving at a final sample of 29,353 student-school-years. I refer to this as the "main" sample. Students appear multiple times if they have multiple qualifying homeless spells.

The DHS portion of the data contains extensive detail about families' identities, compositions, and shelter stays. The raw data, which I use to match homeless students to their educational histories, consists of individual-level records for all family members; in defining covariates, I rework the data such the unit of observation is the family-homeless-spell. To this core DHS data, I append information about homeless families' public benefit use maintained by DSS: Cash Assistance (CA), also known as "public assistance" or "welfare," and Food Stamps (formally, the Supplemental Nutrition Assistance Program (SNAP)), using probabilistic record linkage. Finally, I add data on quarterly employment and earnings from

[31] Outcome data are inconsistently reported for students in charter schools. Students whose school districts are unknown are also excluded.

NYS DOL, using a deterministic match.

Correspondingly, the DOE data contain one record for each student during each school year and span biographical information (including demographics), school enrollment, attendance, standardized test scores, and admissions and discharges. I match DHS school-age family shelter residents with DOE records following standard City protocols for linking human service and education data. The match is probabilistic and based on first name, last name, date of birth, and sex. Overall, as described in Table A.6, approximately 87 percent of children age 5–18 in the DHS data are successfully linked to NYC public school records (not all school-aged children attend conventional public schools during their shelter stays).

I also consider two important subsamples in the main analysis. The first is the student fixed effects (SFE) sample, which consists of the 5,150 school years among students who appear more than once in the main sample and whose commute distance is different during these homeless spells. The second is the difference-in-differences (DID) subsample. This sample is comprised of the subset of main sample students who are appropriately observed in the school year prior to shelter entry, where "appropriate" consists of having: (1) not been homeless in the prior school year, (2) a geocodable last address prior to shelter entry, so as to estimate prior year school distance, and (3) been enrolled in grades K–8 during the prior school year. For students with multiple qualifying DID observations, I keep only the first such qualifying episode. Two-thirds of the main sample meet these criteria—19,895 student-school years.

Tables A.3A–A.3B provide sample means of outcomes, treatments, and covariates for the main sample and these subsamples. As might be expected given the respective subsample qualifying criteria, the SFE subsample is disadvantaged by most measures relative to the main sample, while the DID subsample is slightly better off, on average.[32]

Finally, setting aside the main analysis while translating relevant sample restrictions, I create a "complete" sample of four million student-school-years that also includes housed children. Described in Appendix A and summarized in Table A.7, this sample facilitates comparisons between homeless and housed students, as in the mean comparisons given in Tables A.2A–A.2B.

3.2 Treatment

Taking care[33] to identify homeless students' schools of origin (i.e., pre-shelter schools), I focus on three treatment measures. The leading definition is school-shelter commute distance, D^C, in miles. Estimated using Google Maps Platform's Distance Matrix API, it is defined

[32]For robustness, I consider several alternative sample restrictions in Table A.11.

[33]For details, see Appendix A.6.

according to the following rules. For student i enrolled in school e at the start of school year t and assigned to shelter s upon homeless intake:

$$D^C_{it}(s_{it}, e_{it}) = \begin{cases} WalkDistance(s_{it}, e_{it}) \text{ if } WalkDistance \leq 1 \\ TransitDistance(s_{it}, e_{it}) \text{ if } WalkDistance > 1 \text{ and public transit feasible} \\ DriveDistance(s_{it}, e_{it}) \text{ if } WalkDistance > 1 \text{ and public transit not feasible} \end{cases}$$

Although the one-mile threshold is arbitrary, this definition is meant to capture the notion that urban students walk when school is close and take public transit otherwise. In practice, 93.7 percent of students in the main sample are assigned transit distance by this set of rules; the remaining 6.3 percent are assigned walk distance.[34]

The second definition is linear (Euclidean) distance, D^L, between school and shelter, in miles (converted from feet):

$$D^L_{it}(s_{it}, e_{it}) = \sqrt{(X_s - X_e)^2 + (Y_s - Y_e)^2}/5280$$

where X and Y refer to the Cartesian coordinates of shelter (s) and school (e), respectively.

The third definition is out-of-borough placement, D^B, an indicator equal to one if a student's shelter borough is not the same as her school borough, and zero otherwise:

$$D^B_{it}(s_{it}, e_{it}) = \mathbf{1}\{b(s_{it}) \neq b(e_{it})\}$$

where $b(\cdot)$ is a borough function mapping shelters (s) and schools (e) to their counties. In-borough placement is the official policy objective, so it is of intrinsic interest. At the same time, a binary treatment definition offers a convenient shorthand for summarizing results. Average in-borough students' shelters are 3.9 miles from their schools of origin, compared with 13.5 miles for out-of-borough students, a difference of 9.6 miles. (For additional comparisons of students placed in and out of their school boroughs, see Tables A.8A–A.8C.)

Figure 3 shows the relationship between treatment defined as commute distance and treatment defined as out-of-borough shelter placement. The overall distribution of commute distance, which has a mean of 8.2 miles and a standard deviation of 6.4 miles, is a bimodal function of two essentially separate component distributions: a rather slender and left-peaked distribution for in-borough students with a mean (standard deviation) of 3.9 (2.8) miles (in red), and a flat, right-peaked, and long-tailed distribution with a mean (σ) of 13.5 (5.5)

[34] For robustness, I consider several additional treatment measures: pure transit distance, pure walk distance, an indicator for walk distance less than 0.5 miles, and commute time, in minutes, defined analogously to commute distance.

miles for out-of-borough students (in blue). 45 percent of main sample students are placed in shelters outside their school borough of origin.

Figure A.9 summarizes the geographical distribution of treatment intensity by displaying mean commute distance for the main sample by school district of origin. Homeless students from far-flung areas of Staten Island, Queens, and South Brooklyn are among those placed in shelters at greatest distance to their schools, while the shelter-abundant Bronx offers the shortest average commutes.[35]

3.3 Outcomes

The outcomes I assess span attendance, stability, and performance.

I quantify attendance using days absent and absence rates (days absent divided by days present plus days absent). My measure of stability is school change, an indicator equal to one if a student had any nonstructural school admissions during a school year.[36]

I measure proficiency using New York State Mathematics and English Language Arts (ELA) standardized tests for students in grades 3–8. Numeric scores are scaled for grade-year difficulty and translated to four levels; students at levels 3 or 4 achieve proficiency.[37] I standardize the scale scores by subtracting the complete sample grade-year mean and dividing by the corresponding standard deviation; thus, the main proficiency results are in standard deviation units.[38]

My measure of promotion is an indicator equal to one in year t if either (a) a student's grade level in school year $t + 1$ is greater, or (b) the student graduated in year t.

[35] Figure A.10 confirms the same overall picture is true for treatment measured as linear distance. Figure A.11 provides out-of-borough shelter assignment rates by school district.

[36] To be precise, I count the number of admissions for each student in each school year, and subtract one for any student who entered a school at that school's starting grade. Most commonly, these structural changes occur in kindergarten, sixth grade, and ninth grade, which are the standard entry grades to elementary, middle, and high school, respectively. Since the sample is restricted to students enrolled in DOE prior to shelter entry, this indicator should not capture "spurious" changes associated with families migrating to NYC.

[37] The levels are: (1) below proficient, (2) partially proficient, (3) proficient, and (4) exceeds proficient. Proficiency scores dropped sharply in 2012 following the introduction of new Common Core testing standards. Because all of the specifications include year dummies, which restrict the level of comparison to within-year, this is not a major impediment to the analysis.

[38] For robustness, I assess alternative proficiency measures—in particular, binary math and English proficiency indicators equal to one if a student scores level 3 or 4 and zero if they score level 1 or level 2, as well as an overall proficiency indicator equal to one if they score level 3 or 4 on both tests. In one set of such binary indicators, I include students missing tests as not proficient; in the other, I exclude students who miss tests. I also assess non-normalized scale scores separately.

3.4 Covariates

The linked administrative data allows for a rich set of controls, which are grouped here, summarized in Tables A.3A–A.3B, and described in Tables A.4A–A.4B.

- **Student and Family Covariates**:
  - **Base covariates**: Because the sample pools students whose ex ante treatment probabilities are not equal due to factors plausibly related to outcomes, the analysis must adjust for these institutional determinants (e.g., school year fixed effects).
  - **Placement covariates**: These are factors expressly considered as shelter assignment criteria or which may systemically impact school or shelter assignment (e.g., family size). Together with base covariates, placement covariates comprise **balance test covariates**, which are the set of factors conditional upon which quasi-random assignment holds.
  - **Student covariates**: Student characteristics (e.g., race).
  - **Family covariates**: Family characteristics (e.g., head-of-household employed in the year prior to shelter entry).
  - **School-level covariates**: School-year-specific school characteristics (e.g., enrollment).
- **Prior School Year Student Covariates**: Student outcomes in the year prior to shelter entry.

For simplicity, I sometimes refer to this collection of controls as "**main**" covariates. Unless otherwise noted, these covariates are assumed to be included in the empirical specifications.

4 Empirical Methods

4.1 Quasi-Random Assignment and Linear Regression

In trying to discern the causal effects of school proximity, the basic econometric challenge is that students who live near their schools may be different than those who do not, for reasons (e.g., parental motivation or resources) that may also impact educational outcomes. I use three identification strategies.

The primary identification strategy is the quasi-random assignment of students to shelters. Although the City endeavors to place families in the shelters nearest their children's schools, actual assignments are determined by the inventory available at the time of shelter application—a constraint made especially stark as the homeless family census in NYC grew from 8,165 families in 2010 to 12,089 in 2015.[39]

In Section 5.1, I confirm this scarcity-induced random assignment characterization is empirically apt. Students placed relatively near to and far from their schools look remarkably similar. Accordingly, ordinary least squares (OLS) linear regression conditioned on detailed administrative data should deliver consistent and precise estimates of (variance-weighted) average treatment effects (ATE's) (Angrist and Pischke, 2008). Accordingly, the main estimating equation is:

$$Y_{it} = \mathbf{X}_{it}\boldsymbol{\beta} + \tau^{OLS} D^C_{it}(s_{it}, e_{it}) + \big[\gamma_s + \delta_e\big] + \varepsilon_{it} \tag{1}$$

Educational outcome Y for student i during school year t is a function of observables ($\mathbf{X}_{it}$), unobservables (ε_{it}), and commuting distance (D^C_{it}), which is written to emphasize it is a function of the locations of a student's shelter s and school e. $\mathbf{X}_{ip}$ consists of the rich set of base, placement, student, family, school, and prior year (collectively, "main") covariates described in Section 3.4 and summarized in Tables A.3A–A.3B.

The broad observability facilitated by detailed administrative data strengthens the case for the natural experiment. Especially noteworthy are the controls for pre-shelter educational outcomes and distance from school, which should capture an appreciable share of students' (their families') abilities, resources, preferences, and constraints. In augmented specifications, I additionally control for shelter (γ_s) and school (δ_e) fixed effects, which neutralize, respectively, any unobservables shared by students attending the same schools and students residing in the same shelters. The main sample contains 1,148 unique schools and 226 shelters.

To account for arbitrary correlations among siblings, as well as for the presence of students with multiple spells, I cluster standard errors at the "family group" level. Family groups are clusters of families linked by at least one overlapping member. In most cases, family groups are consistent with a conventional definition of family (people living together); however, because homeless households are subject to compositional volatility (e.g., children may temporarily reside with relatives), this broader measure results in more conservative standard errors.

Under conditional independence, the parameter of interest, $\widehat{\tau}^{OLS}$, equals the estimand of interest, the average treatment effect of commute distance on educational outcomes:

$$\tau^{OLS} = E[\tau_i|\mathbf{X}_{ip}] = E\left[\frac{\partial Y_{ip}}{\partial D^C_{it}}\middle|\mathbf{X}_{ip}\right]$$

[39]New York City Mayor's Office of Operations (2012, 2018).

Since all students are, in some sense, "treated" by commute distance, this equivalence also assumes linearity reasonably summarizes the relationship; τ_i is written with the i subscript to emphasize that true treatment effects may vary by individual. I investigate heterogeneity in Section 6 by repeatedly splitting the sample by covariates of particular in interest and estimating the model within each subgroup. In alternative specifications, I substitute the alternative treatment definitions, D^L and D^B, the latter of which provides a convenient summary of the magnitudes of distance effects at "typical" move distances—i.e., the difference between being assigned shelter within and outside school boroughs (9.6 miles).

The panel nature of the data lends itself to a natural extension of the linear regression framework to a student fixed effects model. Nearly a fifth of the main sample consists of students experiencing multiple spells of homelessness. When treatment intensity varies across these shelter stays, these students serve as counterfactuals for themselves. I implement the student fixed effects estimator by modifying Equation 1 to include individual student dummies, α_i:

$$Y_{it} = \alpha_i + \mathbf{X}_{it}\boldsymbol{\beta} + \tau^{OLS} D^C_{it}(s_{it}, e_{it}) + \left[\gamma_s + \delta_e\right] + \varepsilon_{it} \tag{2}$$

I continue to cluster standard errors at the family group level to allow for arbitrary correlations of unobservables among siblings and across spells.

4.2 Difference-in-Differences

Although the evidence suggests the conditional quasi-random shelter assignment assumption is reasonable, plausibility does not equal proof, and linear regression remains vulnerable to unobservables jointly associated with shelter assignment and student outcomes. For example, parental preferences are not measured directly, and it may be the case that generally similar families may vary in how much importance they place on education.

To address these concerns, I take a subset of main sample observations and exploit the longitudinal nature of the underlying (full) data. Specifically, I keep those students for whom I observe a non-homeless K–8 school year immediately prior to a shelter-entry school year during 2010–2015 (who have a geocodable prior address). For students with repeat shelter spells, I keep only the first such qualifying episode. For each student, the DID panel then consists of a pair of observations in relative-to-event time (R): the retained main sample student-school year ($r = 1$) and the immediately previous non-homeless school year ($r = 0$). In the simple binary case, treatment, D^B_{ir} consists of being assigned an out-of-borough placement in $r = 1$. No student is treated in period $r = 0$ because pre-shelter residential decisions are not random; treatment consists of *shelter* assignment. In summary, the panel consists of two observations for each student, one in which they are housed and a second in which they are placed in shelter, which may be in or out of borough. I estimate regressions of the form:

$$Y_{ir} = \alpha_i + \phi_r + (\mathbf{1}\{t = 1\} \times \mathbf{1}\{D^B_{i1} = 1\})\tau^{DID} + \nu_{ir} \tag{3}$$

In words, outcome Y for student i in year r is a linear function of individual fixed effects (α_i), period fixed effects (ϕ_r; $r \in \{0, 1\}$), and the DID term interacting an indicator for the second period, $r = 1$, with an indicator for membership in the group of students placed out-of-borough during the second period, $D^B_{i1} = 1$. Standard errors, ν_{ir}, continue to be clustered by family group.

The cornerstone of consistency for the TWFE estimator is a parallel trends assumption that, in the absence of treatment, outcomes of the treatment and control groups would have evolved similarly.[40] This assumption is partially testable, and I provide evidence that these tests are satisfied in Section 5.3, following best practices proffered by Rambachan and Roth (2023) and Freyaldenhoven et al. (2021). I perform these tests among the subsample of DID students who I observe for three years prior to shelter entry ($r \in [-2, 1]$), which provides a wider context in which to assess potential pre-trends.

Under parallel trends, the coefficient of interest, τ^{DID} has the interpretation of the average treatment effect on the treated (ATT), which, while conceptually different than the ATE, should be close if treatment and control groups are comparable (as they are here) and/or treatment effects are relatively homogeneous.

Due to the plausibility of parallel trends, the main analysis does not account for time-varying covariates—which also has the virtue of avoiding thorny issues that can arise with covariates in the TWFE setting (Huntington-Klein, 2023; Caetano and Callaway, 2023). Nevertheless, in the appendix, I repeat the main analysis while controlling for a select subset of covariates—fixed effects for school borough, school year, and grade level—and find that the estimates are little changed.

Of course, there is a rapidly expanding literature finding fault with traditional TWFE regression—namely, that parallel trends does not suffice in the (empirically likely) case that treatment effects are heterogeneous.[41] However, the design here—a classic 2×2 balanced panel with two groups and two periods where treatment occurs once, at period two—does not feature the complications (e.g., multiple groups or periods; staggered treatment timing) arousing TWFE criticisms. Hence, the newer, more robust DID estimators produce identical

[40] A less discussed but also important assumption is that there are no anticipation effects of treatment, which seems reasonable in the case of shelter placements.

[41] See Roth et al. (2023) and De Chaisemartin and d'Haultfoeuille (2022) for helpful overviews.

point estimates (not reported).

Finally, while DID is easiest to interpret in the binary treatment case, I provide estimates for continuous (linear) distance.[42] Here, DID distance variable is equal to linear school-shelter distance in $t = 1$ and and linear pre-shelter address to school distance in $t = 0$ (in miles). Treatment is then equal to the change in distance from $r = 0$, $\Delta(D_{i1}^B)$ and the estimating equation is

$$Y_{ir} = \alpha_i + \phi_r + \Delta(D_{i1}^B)\tau^{DID} + \nu_{ir} \tag{4}$$

which is, by construction, equal to zero for all students in period $r = 0$, but may be positive or negative in period $r = 1$.

5 Main Results

5.1 Balance Test

Assessing the plausibility of random assignment is the first empirical task. Figures 4 and A.12 portray a comprehensive assessment of balance between students placed in shelters at varying distances from their schools of origin. Each figure is divided into panels by characteristic. Black dots and spikes give covariate-adjusted means and pointwise 95 percent confidence intervals for the main sample divided into 12 commute distance groups, according to commute distance rounded to the nearest two-mile.[43] These means and CI's are obtained from separate linear regressions of each characteristic of interest on two-mile commute dyad fixed effects (omitting the 0-mile group as the baseline) and balance test covariates. Red and blue lines, respectively, give linear and quadratic fits (and shaded 95 percent CI's) obtained from separate OLS regressions of each characteristic on continuous commute distance, controlling for balance test covariates. The dashed vertical gray lines give the in-borough (leftmost) and out-of-borough (rightmost) commute distance means. All standard errors are clustered by family group. Mean differences in characteristic means at the in- and out-of-borough commute distance means are reported under each panel, with standard errors and p-values presented in parentheses, in that order.

Overall, the evidence for quasi-random shelter assignment appears strong. Particularly notable is that there are no statistically significant differences in either pre-shelter student outcomes (Figure 4) or pre-shelter household head public benefit use (CA and SNAP),

[42] I do not measure nonlinear commute distances between pre-shelter addresses and schools.

[43] All students with commute distances 22 miles or greater are grouped together. This accounts for 2.5 percent of the main sample.

employment, and earnings (Figure A.12). On the other hand, Blacks are slightly underrepresented (and Hispanics slightly overrepresented) at medium commute distances, and household heads may be slight older and better educated at very long commute distances. However, the associated magnitudes are small, the confidence intervals are generally inclusive of zero, and the imbalances suggest, if anything, that students placed further from their schools have characteristics typically associated with educational advantage.

In contrast, Figure A.13, which provides a complementary analysis of the outcomes to be explored in detail below, shows obvious increases in absenteeism and school changes as distance from school increases, as well as a decrease in math scores.

As a complement to this graphical summary of balance, Appendix Tables A.8A–A.8C provide precise comparisons of both raw and regression-adjusted differences for a slightly expanded set of characteristics. The takeaway is the same: students placed in shelter near and far from their schools are remarkably similar.

5.2 Quasi-Random Assignment and Student Fixed Effects

Table 1 presents the main results, estimated using Equations 1 (Columns 2, 3, 6, and 7) and 2 (Columns 4, 5, 8, and 9). Outcomes are listed in rows. Covariate specifications are indexed by column. Column 1 gives outcome means (standard deviations in parentheses). Each cell in Columns 2–5 reports the coefficient on continuous school-shelter commute distance (in miles) from separate OLS regressions of the row-indexed outcome on commute distance and the covariates described at the bottom of the table. In complement, each cell in Columns 6–9 reports analogous results for treatment defined as out-of-school-borough shelter placement. Standard errors clustered by family group are given in parentheses, followed by sample sizes (which differ because certain outcomes are undefined for some students and for some specifications) and percent changes from outcome means (in brackets). For commute distance, percent changes are calculated at the difference in mean distance between in- and out-of-borough placements (9.6 miles).

The benefits of proximity are obvious. Residing nearer to one's school leads to better attendance, math scores, and school stability. Focusing on the main covariate specifications (Columns 2 and 6), absences increase by 0.186 days (0.11 percentage points) per mile, equivalent to 2.1 days (1.3 pp) with out-of-borough placement, a difference of 6–8 percent. The effect of proximity on school stability is even more pronounced: per the main specifications, the probability of changing schools increases by 24–30 percent—about 1.15 pp per mile or 13.8 pp with out-of-borough placement. Math test scores also worsen with distance. Homeless 3–8 graders score about 0.003σ/mile worse on state tests—equivalent to 0.04σ at the level of borough displacement, an effect size of about 4–6 percent. There appears to be little effect of distance on English tests scores and promotion, perhaps in part because proficiency and grade retention are both (somewhat incongruously) rare for this population.

Adding school and shelter fixed effects (Columns 2 and 7) little change these estimates. However, including student fixed effects (Columns 4–5 and 8–9) leads to a near doubling (in absolute value) of attendance and math effect estimates. According to the "full" covariate model (Column 9), placement in shelter outside one's school borough leads to 4.1 additional absences (a 14.7 percent increase) and a 0.13σ reduction in math scores. Promotion rates also decrease by an estimated 3.8 pp at the borough level. The probability of changing schools is 31 percent higher (14.6 pp) ,

On one hand, this pattern of larger (in absolute value) student fixed effects results derived from comparing multi-spell student to themselves suggests that simpler OLS models may be downward biased, which would be the case, for example, if lower-performing students were systematically more likely to be placed in shelters closer to their schools. On the other hand, the student fixed effects sample is itself (perhaps unsurprisingly) negatively selected. As shown in Tables A.3A–A.3B, students in the SFE subsample have 17 percent more absences and are 34 percent more likely to change schools in the year prior to shelter entry than the main sample as a whole.

Appendix Tables A.9–A.12 subject these results to a series of robustness checks for treatment definitions, outcome measures, covariate specifications, and sample inclusion criteria. The analysis is summarized in Appendix B.2. The main results are confirmed.

5.3 Difference-in-Differences

In Appendix B.1, I show that there are no statistically significant outcome pre-trends, and, what's more, that the post-treatment estimates are large enough to be tolerant of parallel trends violations double that of the largest violations observed pre-treatment.

Table 2 gives the main DID results. As before, rows index outcomes and cells give treatment effect estimates (in this case ATT's) from separate regressions. Columns 1–3 use binary out-of-borough treatment, while Columns 4–6 use the change in linear school-shelter distance.

For comparison, Columns 1 and 4 replicate the main sample OLS specification (Equation 1) for the DID subsample of the main sample. Column 2 provides standard TWFE DID estimates (Equation 3) for full DID subsample. Column 3 gives TWFE DID estimates for the subset of DID students used in the pre-trends analysis—i.e., those observed for three years prior to treatment (though, to be clear, the estimation includes only two years for each student: the year of shelter entry, and the prior year). For attendance and school change, the TWFE estimates are very similar to the main OLS results. Students placed out-of-borough miss an estimated 2.2–2.8 days more of school and are 13.2–15.7 pp more likely to change schools. However, math effects (like those of English and promotion) are less apparent in the DID subsample. The point estimate for math in the pre-trends sub-subsample (-0.032) is similar to OLS (-0.043), but it is imprecisely estimated, as is the estimate for the full DID subsample (-0.008), which, additionally, is only a fifth the size of OLS. Part of the explanation may be that the DID subsample is somewhat positively selected relative to the main sample (Tables A.3A–A.3B); also, third grade test outcomes are excluded from the analysis by construction.

The results for changes in linear distance (Columns 4–6) are analogous, mirroring OLS for attendance and stability, but imprecise for math. Appendix Table A.13 repeats the DID analysis with select covariates—borough, year, and grade fixed effects—and finds the main results little changed.[44]

6 Extensions

In this section, I augment the main results by summarizing the evidence on heterogeneity and mechanism. Additional discussion can be found in Appendices B.3 and B.4.

6.1 Heterogeneity

Table 3 highlights the three characteristics exhibiting the most interesting patterns of heterogeneity: pre-shelter school distance, out-of-school-borough treatment, and summer shelter entry. Students who live nearest their schools prior to shelter generally benefit the most from being assigned school-proximate shelters (Panel A). School-to-shelter commute distance increases absences 0.278 days/mile for students in the first quartile of pre-shelter (linear) distance to school, dropping to just 0.095 days/mile for students in the fourth quartile (Column 1). Similarly, pre-shelter school distance Q1 students are 1.47 pp more like to change schools for every mile further they are placed from their school of origin, decreasing to 1.12 pp for Q4 distance students. In contrast, the most adverse math score effects occur among the students initially living the furthest from their schools.

Equally (or more) important is in-shelter distance to school. Panel B shows that the preponderance of treatment effects are concentrated in students placed within their school

[44] Most main specification covariates are not observed in the year prior to shelter entry (e.g., household employment) or not relevant in the DID context (e.g., race).

boroughs of origin. For in-borough students, every additional mile spent commuting increases absences by 0.307 days, increases the probability of school change by 1.1 pp, and decreases math test scores by 0.006σ, compared with 0.078 absences/mile, 0.73 school-change-pp/mile, and no math effect among out-of-borough students.

These patterns suggest that potentially important nonlinearities associated with treatment intensity may be obscured by the simplifying assumptions of the linear model. Figure 5 confirms this conjecture. It plots the mile-by-mile average marginal effects of commute distance obtained by modifying the main OLS estimating equation (Table 1, Column 2) to allow for a quadratic in commute distance.[45]

Treatment intensity matters. Being very close to school is more important than being kind of close. Students placed within a mile of their schools gain nearly a half day of attendance per mile closer to school (equivalently, about 0.2 pp of attendance rate per mile), an effect which becomes statistically indistinguishable from zero at about 15 miles. The effects of distance on the probability of school changes persist at longer distances, but drop from about 0.18 pp/mile adjacent to school to zero at about 22 miles. Math scores worsen at a rate of 0.01σ/mile near school, but incremental distances become unimportant around 10 miles.

Finally, returning to the broader heterogeneity analysis in Table 3, Panel C shows that attendance and proficiency effects are concentrated among students entering shelter while the school year is in session; hence, the summer may be an especially important time to address housing instability.

Tables A.14A–A.14D, discussed in Appendix B.3, show there is not much meaningful heterogeneity among student and family characteristics. In contrast, there is school-level heterogeneity (Table A.14E). Students attending high-absenteeism schools pre-shelter and students switching into worse-attendance schools subsequent to shelter entry are more responsive to placement distance.

6.2 Mechanisms

Appendix B.4 (Tables A.16–A.18) explores several means through which the effects of proximity may be transmitted. In brief, less proximate placements shorten homelessness spells, but school mobility effects persist into future school years. School changes are, in general, associated with worse outcomes, but nevertheless may be preferable to yielding to long commutes.

[45] I also tested more flexible functional forms (results unreported), but higher-order terms did not better fit the data.

7 Conclusion

While families have long sorted themselves residentially in order to gain access to better schools (Black, 1999),[46] the expansion of school choice in recent decades (Abdulkadiroğlu and Sönmez, 2003) has increasingly brought to the fore the question of how distance to school affects educational outcomes: Is it worth it for my children to travel further to attend better schools?

In this paper, I find that proximity boosts educational outcomes among homeless students. The average homeless student is sheltered 8.2 miles from their school. Were those students instead residing immediately adjacent to their schools, my results suggest they would: have 5–10 percent better attendance (1.5–3 days); be 20 percent less likely to change schools (9 pp); and score about 0.05 standard deviations better in math.

As would be expected under quasi-random shelter assignment, these findings are robust across treatment definitions, outcome measures, included covariates, and alternative samples. The estimated effects of proximity are, if anything, stronger using a student fixed effects strategy, and the attendance and school stability results are also confirmed by a difference-in-differences analysis.

Some students in some situations may benefit more than others. Especially distance-elastic are students who live very near their schools prior to shelter entry. Other high responders to proximity are students enrolled in poor-attendance schools prior to shelter, as well as those students who switch into schools with worse attendance after shelter entry. Finally, treatment intensity matters. Results from nonlinear models show that being placed very close to school is more valuable than marginal changes at greater distances.

A back-of-the envelope way to quantify benefits is in terms of the effect of proximity on math test scores. A move closer to school worth 0.05 standard deviations in math is equivalent to about 2 percentiles around the median in a standard normal distribution. Chetty et al. (2011) find that a one percentile increase in elementary test scores is associated with a \$133 increase in annual earnings in adulthood (in 2023 dollars), controlling for demographic characteristics. Assuming 45-year working careers beginning 10 years hence (the average K–8 student in the Main sample is 9.8 years old), and a discount rate of 3 percent, this translates to $\sum_{t=10}^{54} \frac{(2\times\$133)}{(1+0.03)^t} = \$4,999$ in present value.

On the other hand, the policy is not without its costs. In Cassidy (2020), locally placed families remain in shelter longer, by about 13 percent, or roughly 50 days. At an average nightly cost of about \$190, this means educational gains cost the City about \$9,500 per family, or, since the average family has two children in school, \$4,750 per student. At the same time,

[46] See also Billings, Brunner and Ross (2018) and Bibler and Billings (2020).

short moves produce non-educational benefits for families as well. Heads of families placed in shelters in their home boroughs earn about 10 percent more while homeless, equal to about \$750 during a typical shelter stay. Thus, to a first approximation, each school-based shelter placement creates a social surplus of about \$1,000.[47]

While homeless students often face larger challenges than their housed classmates, student homelessness is neither rare (1.3 million US students are homeless any given year) nor permanent (the average length of stay in my sample is 1.25 years). Moreover, predicting who will become homeless among poor families is notoriously difficult (Rolston et al., 2013; NYC CIDI and NYU Furman, 2017; O'Flaherty, Scutella and Tseng, 2018)—and a majority (52.1 percent) of U.S. public school students are poor, by the standard of qualifying for free or reduced-price school lunch (National Center for Education Statistics, 2021*a*; USDA, 2023). Consequently, these insights about the value of proximity are potentially relevant for the many primary schoolers at risk for housing instability. Specifically, my results suggest that reducing commutes is a policy parameter that school assignment mechanisms ought to consider.

An open challenge is to better identify distance-elastic students—those who, due to transportation, mobility, or other constraints, may benefit more than average from proximate schools. At the core of the present study is a natural experiment in shelter assignment; one question for future research is whether spots in school ought be deterministically prioritized for certain students, homeless or otherwise. Another is to identify additional environmental variables beyond proximity that can lead to favorable changes for students at reasonable costs.

[47] A full accounting of benefits would include attendance and school stability gains, as well as consider the disruptive impact of transfers on incumbent students (Hanushek, Kain and Rivkin, 2004); at the same time, further prioritizing school proximity would likely bring additional costs in the form of the shelter system maintaining excess capacity to better tailor placements. On balance, the large number of non-homeless students affected by homeless children changing schools, along with the size of this effect—Hanushek, Kain and Rivkin (2004) estimate the effect on incumbent students to be on the order of a 0.2σ decrease in test scores for for each 1σ ($\approx$ 11 percent) increase in transfer students—suggests a more comprehensive accounting would increase the size of the estimated surplus created by school proximity.

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9 Tables

Table 1: Main Results

		Commute Distance				Out-of-Borough			
	Mean	Main	S+S FE	Stud. FE	Full	Main	S+S FE	Stud. FE	Full
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Days Absent	28.126	0.186	0.180	0.261	0.367	2.126	2.165	2.922	4.129
	(20.853)	(0.021)	(0.021)	(0.063)	(0.076)	(0.260)	(0.268)	(0.792)	(0.959)
	29,353	29,353	29,263	5,174	4,852	29,353	29,263	5,174	4,852
		[6.4]	[6.1]	[8.9]	[12.5]	[7.6]	[7.7]	[10.4]	[14.7]
Absence Rate	0.1657	0.0011	0.0011	0.0016	0.0021	0.0127	0.0132	0.0189	0.0270
	(0.1225)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0015)	(0.0016)	(0.0047)	(0.0057)
	29,353	29,353	29,263	5,174	4,852	29,353	29,263	5,174	4,852
		[6.3]	[6.1]	[9.1]	[12.3]	[7.7]	[7.9]	[11.4]	[16.3]
Changed School	0.4665	0.0115	0.0113	0.0114	0.0105	0.1381	0.1374	0.1393	0.1457
	(0.4989)	(0.0006)	(0.0006)	(0.0016)	(0.0019)	(0.0071)	(0.0073)	(0.0217)	(0.0258)
	29,353	29,353	29,263	5,174	4,852	29,353	29,263	5,174	4,852
		[23.7]	[23.2]	[23.4]	[21.7]	[29.6]	[29.5]	[29.9]	[31.2]
ELA Standardized Score (SD units)	-0.5649	0.0008	0.0015	-0.0006	0.0025	0.0093	0.0054	0.0242	0.0125
	(0.9540)	(0.0011)	(0.0012)	(0.0026)	(0.0052)	(0.0139)	(0.0149)	(0.0344)	(0.0678)
	16,840	16,840	16,732	2,169	1,781	16,840	16,732	2,169	1,781
		[-1.3]	[-2.5]	[1.0]	[-4.2]	[-1.6]	[-1.0]	[-4.3]	[-2.2]
Math Standardized Score (SD units)	-0.6608	-0.0026	-0.0022	-0.0058	-0.0105	-0.0377	-0.0363	-0.0714	-0.1310
	(0.9033)	(0.0011)	(0.0011)	(0.0030)	(0.0052)	(0.0139)	(0.0149)	(0.0405)	(0.0781)
	16,840	16,840	16,735	2,158	1,755	16,840	16,735	2,158	1,755
		[3.7]	[3.2]	[8.5]	[15.3]	[5.7]	[5.5]	[10.8]	[19.8]
Promoted	0.9264	-0.0000	0.0000	-0.0008	-0.0013	-0.0032	-0.0035	-0.0165	-0.0377
	(0.2611)	(0.0003)	(0.0003)	(0.0009)	(0.0011)	(0.0036)	(0.0037)	(0.0113)	(0.0141)
	27,312	27,312	27,223	4,633	4,303	27,312	27,223	4,633	4,303
		[-0.0]	[0.0]	[-0.9]	[-1.3]	[-0.3]	[-0.4]	[-1.8]	[-4.1]
Sample	Main	Main	Main	Stud FE	Stud FE	Main	Main	Stud FE	Stud FE
Student and Family Covariates	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prior School Year Covs.	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School and Shelter FE	No	No	Yes	No	Yes	No	Yes	No	Yes
Student FE	No	No	No	Yes	Yes	No	No	Yes	Yes

Outcomes are listed in rows. Analytical specifications are indexed by column. Unit of observation is a student school year. Column 1 gives outcome means (standard deviations in parentheses). Each cell in Columns 2–5 reports the coefficient on continuous school-shelter commute distance (in miles) from a separate OLS linear regression of the row-indexed outcome on commute distance (i.e., treatment) and the covariates described at the bottom of the table. Each cell in Columns 6–9 reports analogous results for treatment defined as an indicator for out-of-school-borough shelter placement. Standard errors clustered by family group are given in parentheses. Sample sizes are given below standard errors. Percent changes from outcome means are given in brackets; for commute distance, percent change is calculated at the difference in mean distance between in- and out-of-borough placements (9.6 miles). All results are for the main sample, though only a subset of observations contribute to identification in the specifications with school, shelter, and, especially, student fixed effects.

Table 2: Difference-in-Differences Results

	Out-of-Borough Treatment			Linear Distance Change		
	OLS (1)	TWFE (2)	TWFE-Pre (3)	OLS (4)	TWFE (5)	TWFE-Pre (6)
Days Absent	2.286	2.240	2.846	0.273	0.292	0.318
	(0.321)	(0.319)	(0.514)	(0.038)	(0.034)	(0.056)
	16,198	32,054	11,198	16,198	32,054	11,198
Absence Rate	0.0146	0.0127	0.0157	0.0017	0.0017	0.0019
	(0.0018)	(0.0019)	(0.0029)	(0.0002)	(0.0002)	(0.0003)
	16,198	32,052	11,196	16,198	32,052	11,196
Changed School	0.1424	0.1569	0.1316	0.0165	0.0132	0.0106
	(0.0096)	(0.0120)	(0.0185)	(0.0011)	(0.0013)	(0.0020)
	16,198	32,396	11,200	16,198	32,396	11,200
ELA Standardized Score (SD units)	0.0182	0.0004	-0.0072	0.0017	0.0024	0.0028
	(0.0171)	(0.0157)	(0.0216)	(0.0020)	(0.0017)	(0.0023)
	9,961	15,414	7,624	9,961	15,414	7,624
Math Standardized Score (SD units)	-0.0430	-0.0082	-0.0315	-0.0030	0.0019	0.0020
	(0.0173)	(0.0177)	(0.0230)	(0.0020)	(0.0019)	(0.0025)
	9,909	15,502	7,512	9,909	15,502	7,512
Promoted	0.0019	0.0015	-0.0075	0.0001	0.0008	-0.0000
	(0.0046)	(0.0062)	(0.0082)	(0.0005)	(0.0006)	(0.0009)
	15,254	30,506	10,620	15,254	30,506	10,620
Sample	DID-Main	DID	DID-Pre	DID-Main	DID	DID-Pre
Covariates	Main	No	No	Main	No	No

Outcomes are given in rows. Estimation methods are indexed by column. Unit of observation is a student school year. Each cell reports the average treatment effect on the treated (ATT) of the supercolumn-indexed treatment on the row-indexed outcome from a separate estimation using the column-indexed method. The sample and covariates for each method are summarized at the bottom of the table. Columns 1 and 4 repeat the main OLS specification from Table 1 for the main sample subsample of the DID sample (i.e., shelter entry years only), corresponding to Table 1, Columns 6 and 2, respectively. Columns 2 and 4 give the standard two-way fixed effects DID estimates, controlling for student and relative time fixed effects. Columns 3 and 6 limit the DID sample to the students observed continuously for relative-time school years,$-2 \leq R \leq 1$, where $R = 1$ in the treated school year and estimate the TWFE model for this sub-subsample; these are the students who are included in the pre-trends analysis. Each cell in Columns 1–3 reports results for treatment defined as an indicator for out-of-school-borough shelter placement. Each cell in Columns 4–6 reports the coefficient on continuous linear school-shelter commute distance (in miles). Standard errors clustered by family group are given in parentheses. Sample sizes are given below standard errors.

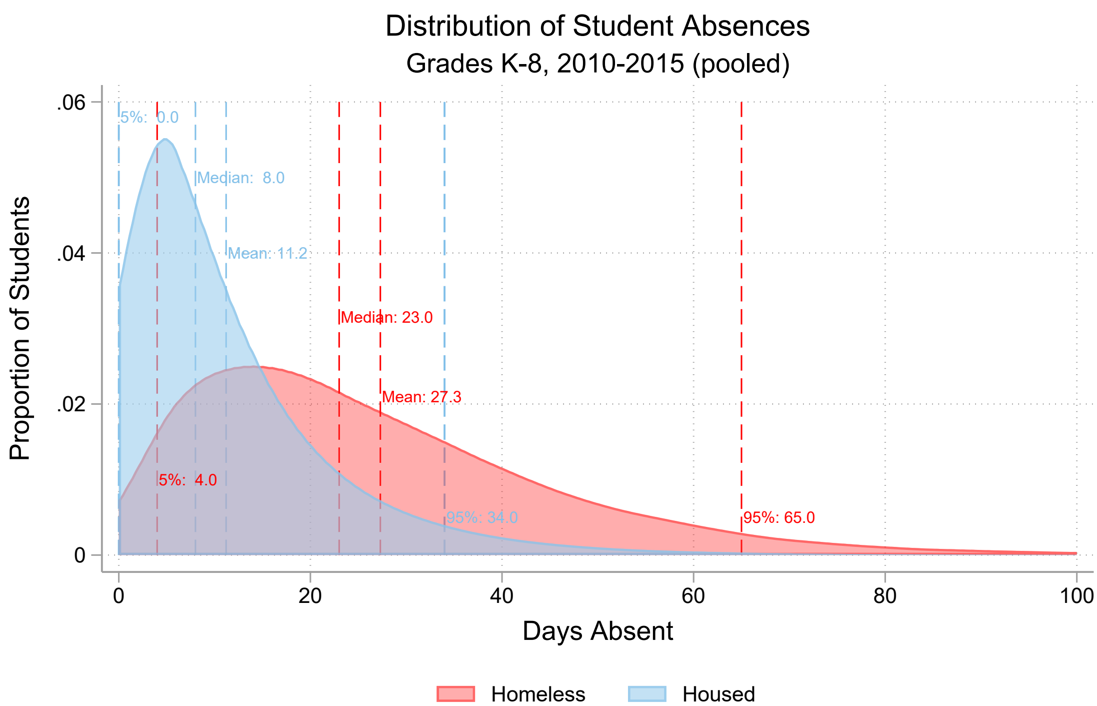
Table 3: Heterogeneity Analysis

	Days Absent (1)	Absence Rate (2)	Changed School (3)	ELA Standardized (4)	Math Standardized (5)	Promoted (6)
A. Pre-Shelter School Distance Quartile						
Q1	0.278	0.0016	0.0147	0.0017	0.0016	0.0001
	(0.040)	(0.0002)	(0.0012)	(0.0023)	(0.0024)	(0.0006)
	6,411	6,411	6,411	3,189	3,204	6,020
Q2	0.190	0.0012	0.0120	0.0035	-0.0030	0.0009
	(0.042)	(0.0002)	(0.0012)	(0.0024)	(0.0024)	(0.0006)
	6,409	6,409	6,409	3,680	3,683	6,030
Q3	0.213	0.0012	0.0075	-0.0026	-0.0035	-0.0004
	(0.043)	(0.0002)	(0.0012)	(0.0022)	(0.0021)	(0.0006)
	6,410	6,410	6,410	4,191	4,173	6,019
Q4	0.095	0.0006	0.0112	0.0008	-0.0038	0.0004
	(0.043)	(0.0003)	(0.0011)	(0.0024)	(0.0023)	(0.0006)
	6,410	6,410	6,410	3,642	3,628	5,949
Unknown	0.108	0.0003	0.0102	0.0018	-0.0044	-0.0012
	(0.058)	(0.0004)	(0.0013)	(0.0030)	(0.0032)	(0.0007)
	3,713	3,713	3,713	2,138	2,152	3,294
Difference in Means	-0.183	-0.0010	-0.0035	-0.0009	-0.0054	0.0004
	(0.059)	(0.0003)	(0.0017)	(0.0033)	(0.0033)	(0.0009)
	0.0018	0.0039	0.0327	0.7930	0.1082	0.6612
B. Out-of-School-Borough Treatment						
Yes	0.078	0.0005	0.0073	0.0002	0.0005	0.0005
	(0.038)	(0.0002)	(0.0010)	(0.0021)	(0.0021)	(0.0005)
	13,203	13,203	13,203	7,442	7,446	12,209
No	0.307	0.0016	0.0110	0.0017	-0.0055	-0.0007
	(0.056)	(0.0003)	(0.0016)	(0.0031)	(0.0034)	(0.0009)
	16,150	16,150	16,150	9,398	9,394	15,103
Difference in Means	-0.229	-0.0012	-0.0037	-0.0015	0.0060	0.0012
	(0.068)	(0.0004)	(0.0019)	(0.0038)	(0.0040)	(0.0010)
	0.0008	0.0030	0.0549	0.6987	0.1312	0.2460
C. Summer Shelter Entry						
Yes	0.103	0.0006	0.0115	0.0012	0.0006	0.0002
	(0.037)	(0.0002)	(0.0010)	(0.0021)	(0.0020)	(0.0005)
	8,188	8,188	8,188	4,699	4,667	7,603
No	0.220	0.0013	0.0113	0.0006	-0.0038	-0.0001
	(0.025)	(0.0001)	(0.0007)	(0.0013)	(0.0013)	(0.0003)
	21,165	21,165	21,165	12,141	12,173	19,709
Difference in Means	-0.117	-0.0007	0.0003	0.0006	0.0044	0.0003
	(0.045)	(0.0003)	(0.0012)	(0.0024)	(0.0024)	(0.0006)
	0.0083	0.0061	0.8141	0.8164	0.0687	0.6346

This table conducts a heterogeneity analysis by repeating the main analysis from Table 1 for subgroups. Unit of observation is a student school year. Outcomes are listed in columns. Rows index the characteristics and levels defining the subsamples among which the heterogeneity analysis is conducted. Each cell in a characteristic-level row reports the coefficient on continuous commute distance (in miles) from a separate regression of the column-enumerated outcome on commute distance and the main covariate specification from Table 1 for the subsample defined by the characteristic-level row. Standard errors clustered by family group are given in parentheses. Sample sizes are given in the third row of each cell. Difference in Means row for each characteristic gives the difference in coefficients, standard errors of the differences (in parentheses), and p-values (in the third row). For binary characteristics, the comparison is between the two levels; for ordered categorical variables, the comparison is between the highest and lowest levels.

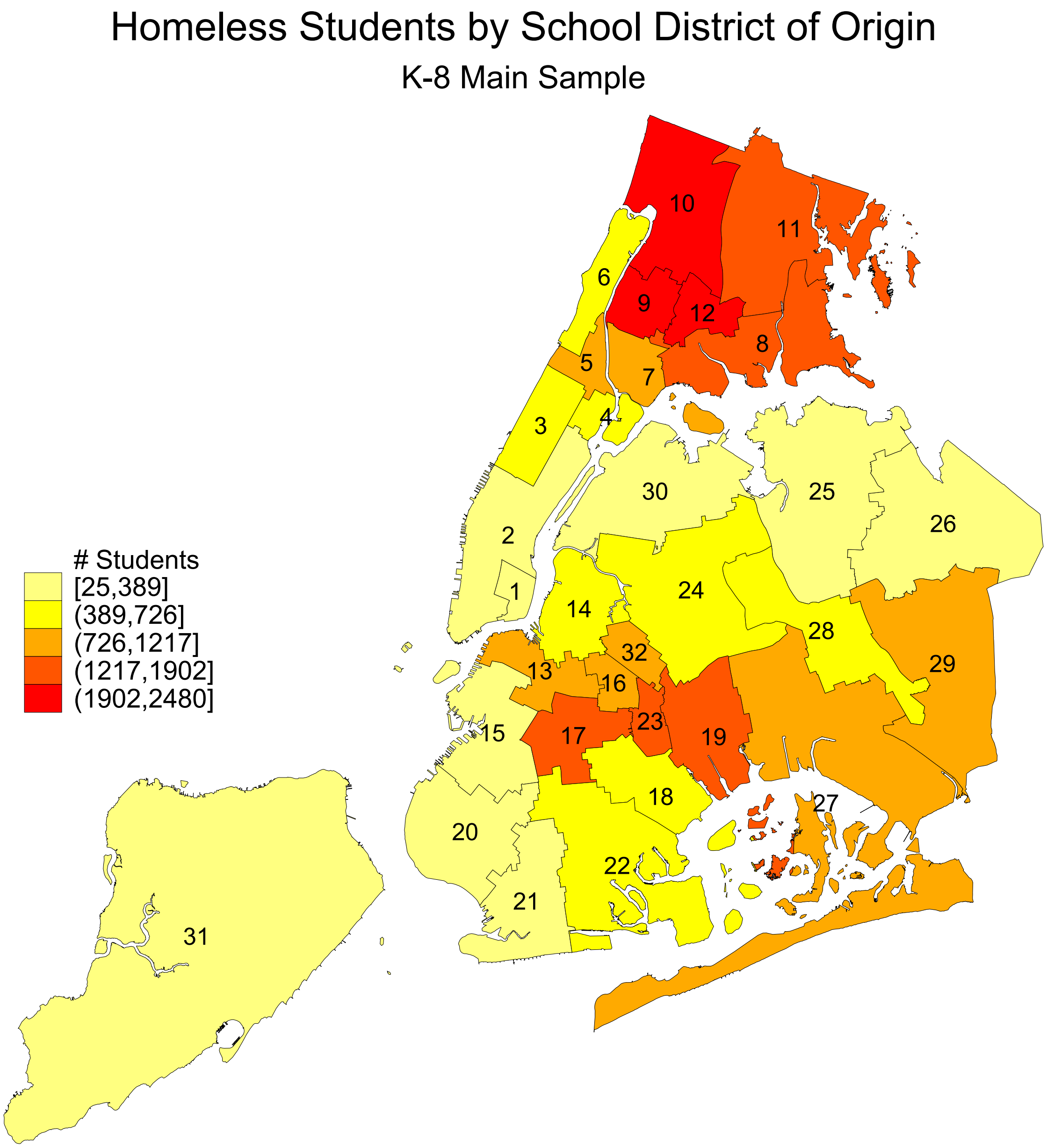
10 Figures

Figure 1



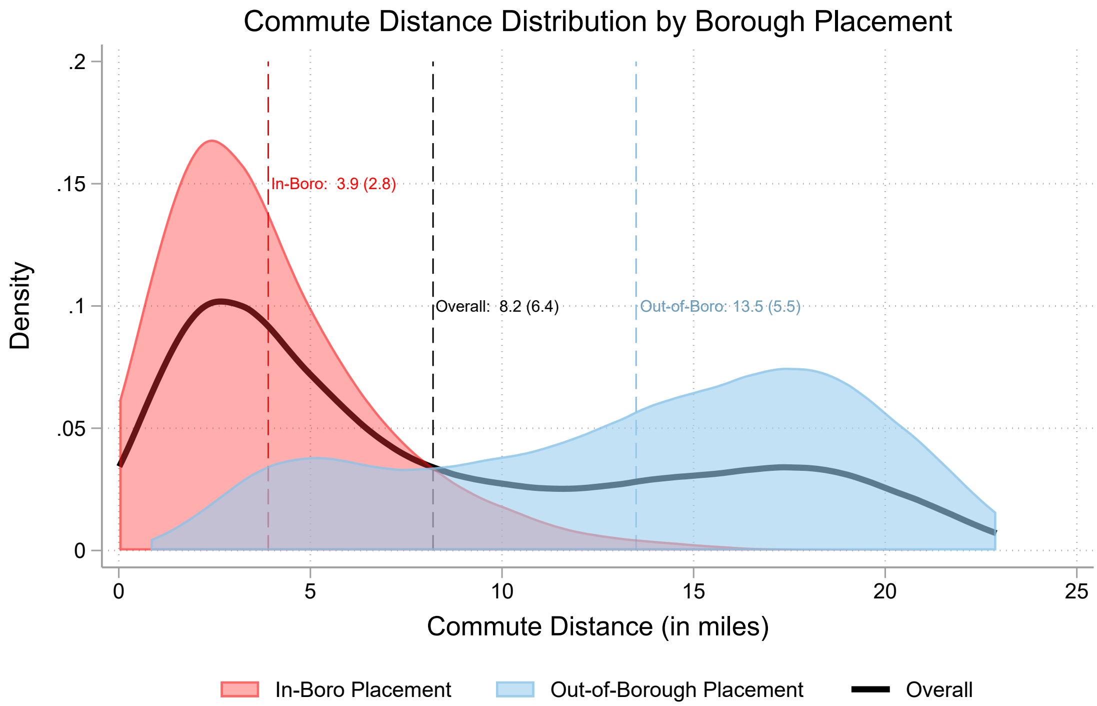
Kernel density plots of days absent using a bandwidth of 3 days for the complete sample of primary schoolers, pooling school years 2010–2015. Homeless is defined as residing in DHS shelter; housed is defined as all other students. Plots are truncated at 100 days.

Figure 2



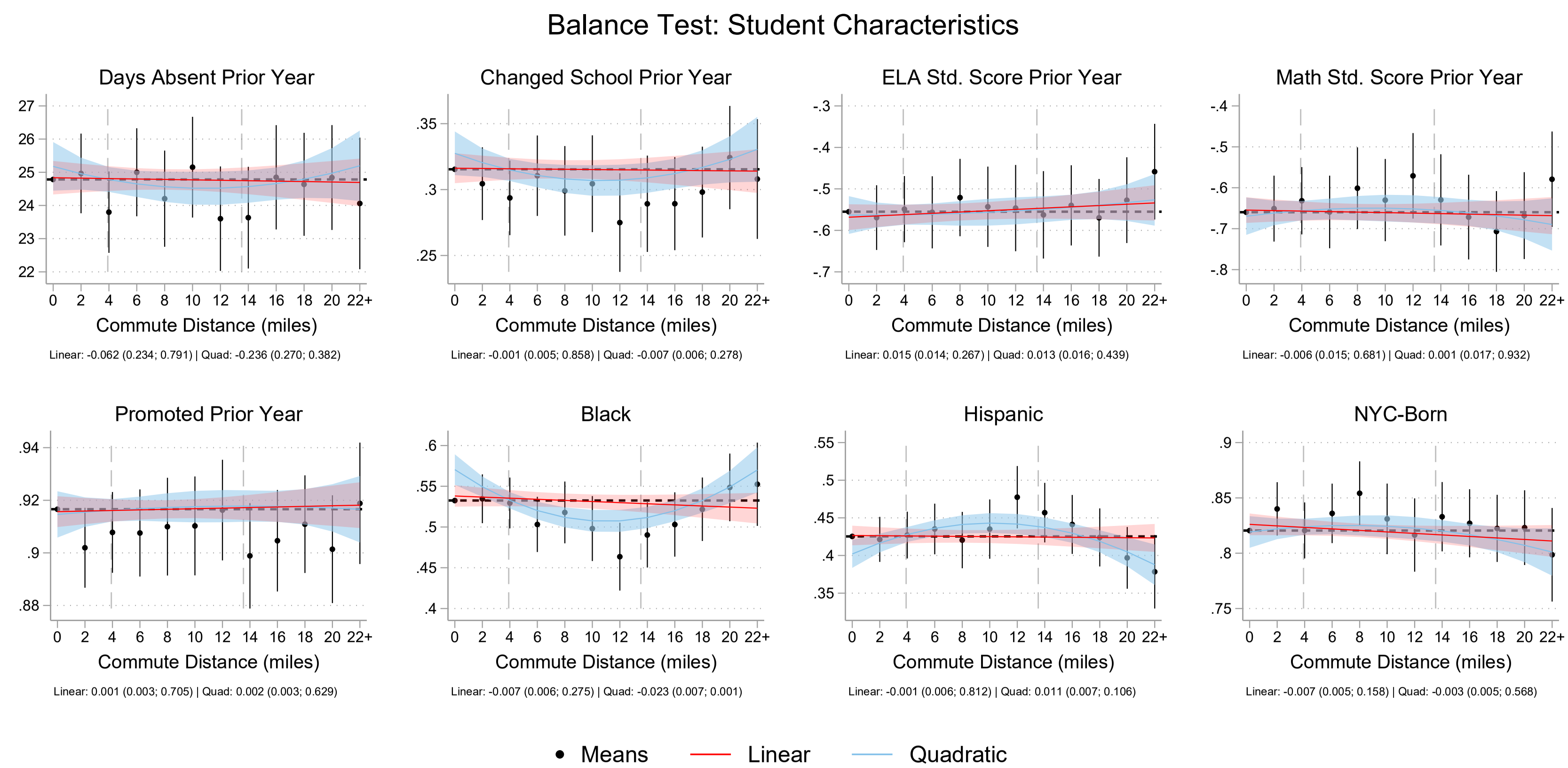
This figure depicts a heatmap of main sample homeless primary schoolers by school district of origin, pooling school years 2010–2015. Limits of choropleth shading bins are set at 0, 25, 50, 75, 90, 100 percentiles of homeless student counts.

Figure 3



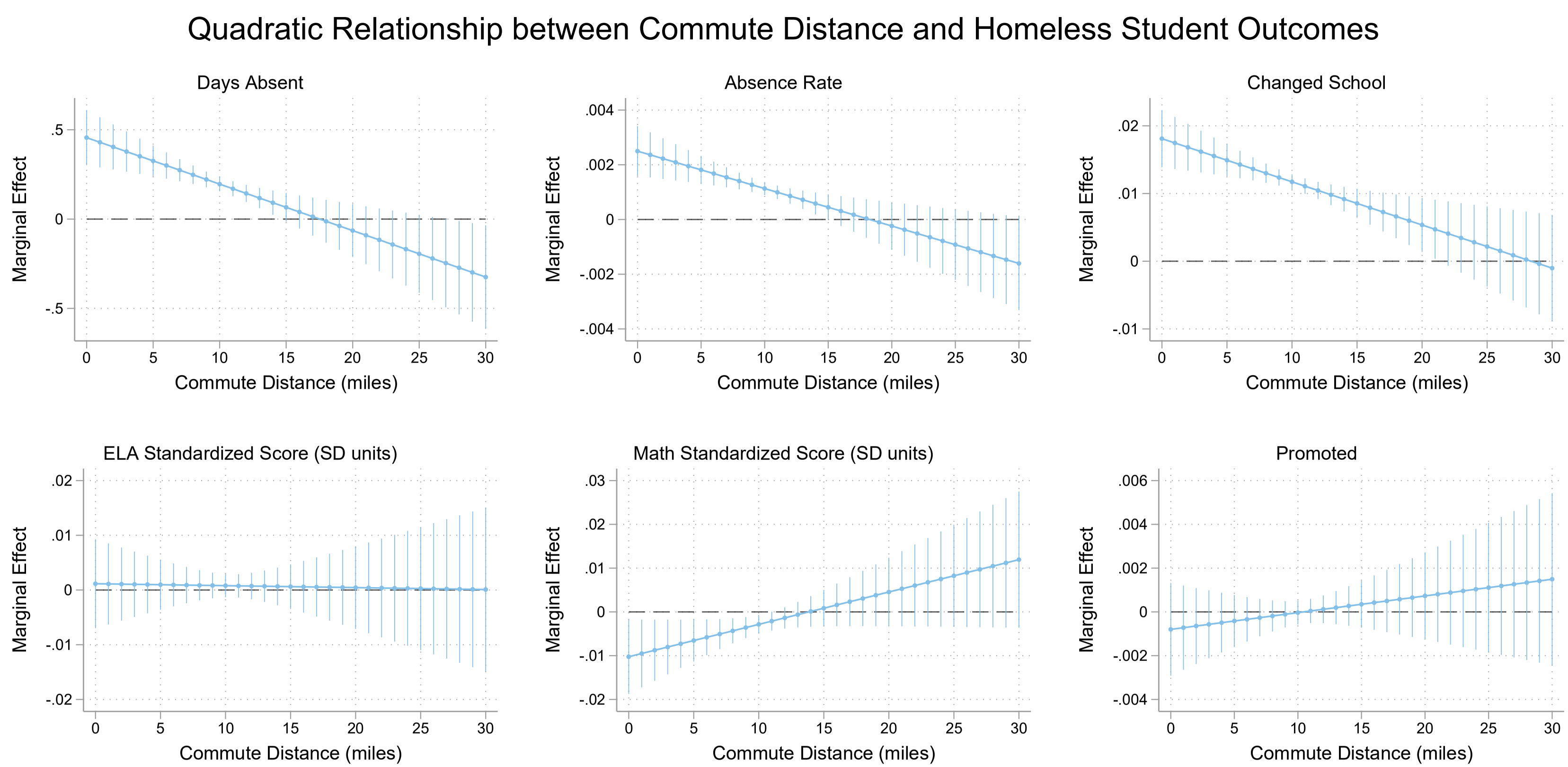
The figure graphs the distributions homeless student commute distances, splitting main sample students by whether they are placed within or outside their school boroughs of origin. Means and standard deviation (in parentheses) are reported for each graph. Kernel bandwidth of one mile.

Figure 4



The figure presents a comprehensive balance test of student characteristics for the main sample, split into one panel for each characteristic. Black dots and black vertical lines give covariate-adjusted means and 95 percent confidence intervals for the sample divided into 12 commute distance groups according to rounded-to-the-nearest-two-mile commute distances. (All students with commute distances 22 miles or greater are grouped together.) Estimates are obtained from separate linear regressions of each characteristic of interest on commute distance group fixed effects (omitting the 0-mile group as the baseline) and balance test covariates. Red and blue lines, respectively, give linear and quadratic fits (and shaded 95 percent confidence intervals) obtained from separate OLS regressions of each characteristic on continuous commute distance, controlling for balance test covariates. Dashed vertical gray lines give the in-borough (leftmost) and out-of-borough (rightmost) commute distance means. All standard errors are clustered by family group. Mean differences in characteristic means at the in- and out-of-borough commute distance means are reported under each panel, with standard errors and p-values presented in parentheses in that order.

Figure 5



This figure depicts the mile-by-mile predictions of the average marginal effect of commute distance (as well as 95 percent confidence intervals) on student outcomes, obtained from separate OLS regressions of each outcome on a quadratic in commute miles, controlling for main covariates, among the main sample of K–8 students.