A Closer Look: Proximity Boosts Homeless Student Performance in New York City

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Abstract

Proximity augments homeless students' educational outcomes. Homeless K–8 graders whose families are placed in shelters near their schools have 8 percent (2.4 days) better attendance, are a third (18 percentage points) less likely to change schools, and exhibit higher rates of proficiency and retention. Homeless high schoolers have 5 percent (2.5 days) better attendance, 29 percent (10 pp) lower mobility, and 8 percent (1.6 pp) greater retention when placed locally. These results proceed from novel administrative data on homeless families observed in the context of a scarcity-induced natural experiment in New York City. A complementary instrumental variable strategy exploiting homeless eligibility policy reveals a subset of proximity-elastic students benefit considerably more. Panel evidence demonstrates homelessness does not cause educational impairment as much as reflect large preexisting deficits. (*JEL I21, I28, I38, H53, H75, D91*)

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1 Introduction

Some 16,100 public primary schoolers reside in homeless shelter in New York City each year. These homeless K–8 graders average 26 absences annually. 45 percent transfer schools. Just 5 percent are proficient in both English and Math, yet 94 percent are promoted. The city's 4,200 homeless high schoolers fare no better, missing an average of 45 school days per year. A quarter change schools, two in five pass a state test in any subject, and 17 percent attrit by the following year[1]. The City spends upwards of $1.2 billion annually to shelter these students and their families[2].

It is well-known that unstably-housed students struggle in school, but evidence on policies to improve performance is scant. One such intervention is school-based shelter placement. Since at least 1998, the City has maintained the explicit goal of placing homeless families in shelters in the boroughs of their youngest children's schools[3]. The theory is that minimizing educational disruption will improve academic outcomes; in addition, policymakers believe neighborhood continuity is generally beneficial for families, keeping them connected to economic and social supports[4].

I exploit this policy to study the effects of neighborhoods—specifically, school proximity—on short-term educational outcomes. I find that proximity matters. On average, homeless K–8 students placed in-borough have 8 percent (2.4 days) better attendance than their more distantly-placed peers. They are a third (18.0 percentage points) less likely to change schools, and 16 percent (1.4 pp) less likely to prematurely withdraw[5] from the City's public school system. They also have a 14 percent (1.0 pp) higher probability of being proficient. Homeless high schoolers placed in shelters near their schools experience 5 percent (2.5 days) better attendance, 29 percent (10.1 pp) lower mobility, and 8 percent (1.6 pp) greater retention.

These findings have broad policy implications. A unique municipal legal right to shelter has made family homelessness a particularly common manifestation of acute poverty in NYC. It is not unusual for resource-constrained, rent-burdened New York families to spend episodic interludes without permanent residences. Beyond the 20,000 students in shelter each year, some 80,000 more experience other sorts of temporary housing (e.g., living doubled-up with relatives) (NYSTEACHS, 2019). In other words, family homelessness is not pathological, it is pecuniary—the product of scarcity and happenstance (O'Flaherty, 2010). Accordingly, homeless family responses to policy incentives hold lessons generalizable to other social policy

[1]2014 and 2015 school year averages, excluding students in charter schools, alternative schools, and those in special programs for students with disabilities, derived from the homeless student panel described in Section 3.

[2]New York City Office of Management and Budget (2019).

[3]My primary definition of "neighborhood" in this paper is borough. NYC consists of five boroughs, or counties: Manhattan, the Bronx, Brooklyn, Queens, and Staten Island.

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

[5]Many of these students move outside NYC.

settings.

The key behavioral insight is this: shelter conditions influence short-term consumption choices. "Mere" proximity delivers meaningful improvements in homeless students' educational outcomes. That this should be the case is not a priori obvious. Proximity makes school more accessible and neighborhood networks augment resources. But proximity changes other prices, too (e.g., friends can also be a distraction), so its net impact on the relative opportunity cost of school is ambiguous[6]. My results suggest the main effect is to encourage educational consumption, at least on average.

My analysis proceeds from a novel administrative panel consisting of a near-census of primary and secondary school students whose families entered shelter in NYC during the 2010 to 2015 school years[7]. I construct it by linking administrative records maintained by the City's Department of Homeless Services (DHS) and Department of Education (DOE). For these students, I observe entire educational histories spanning 2005–2016, as well all shelter experiences occurring during calendar years 2010–2016. To this, I append additional information about family background characteristics and public benefit use from the City's Human Resources Administration (HRA), and data on employment and earnings from the New York State Department of Labor (DOL).

The challenge for causal inference is that students placed in shelters near their schools may be systematically different from those placed distantly. My identification strategy proceeds in three stages. The first stage is a natural experiment. Despite the City's emphasis on placing families in-borough, shelter capacity became scarce as the homeless family census grew rapidly from 8,165 in 2010 to 12,089 in 2015. While 83.3 percent of students were placed in-borough in 2010, just 51.8 percent were in 2015[8].

According to City officials, which families are placed locally is largely a matter of luck: what's available at the time of shelter application. I confirm this scarcity-induced random assignment characterization is empirically apt: treated (in-borough) and untreated (out-of-borough) students look remarkably similar in my data. So long as unobservables follow suit, OLS linear regression appropriately conditioned on placement criteria (e.g., family size and health limitations) consistently estimates treatment effects.

Nevertheless, it is also of interest to relax this assumption. There are two concerns: endogeneity and heterogeneity. Students whose families unobservably care more about edu-

[6]I develop a formal model of homeless family educational consumption in Appendix B and summarize it in Section 2.

[7]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).

[8]New York City Mayor's Office of Operations (2012, 2018). These numbers reflect fiscal years 2010–2011 and 2015–2016. 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.

cation may (partially) self-select into treatment. Even if they do not, students may respond differently to local placement—a non-trivial issue given treatment scarcity.

To address these concerns, the second stage of my analysis is an instrumental variable strategy based on exogenous policy shocks. My instrument is the shelter ineligibility rate, which governs the pace of shelter entry and therefore competition for shelter. Rare among jurisdictions in the United States, NYC has a legal right to shelter; however, families must demonstrate genuine need through a rigorous application process[9]. The more entrants per unit time, the worse are school-shelter matches. While the ineligibility rate is, in part, influenced by the applicant mix, my data, which spans the Bloomberg and de Blasio mayoralties, suggests policy considerations loom large. The most pronounced swings in the ineligibility rate are coincident with changes of administration or other well-documented policy shifts; on the other hand, as I show, the characteristics of shelter entrants remain consistent across policy environments.

I argue this IV approach complements, rather than supplants, OLS: operational realities combined with detailed administrative records make a persuasive case for quasi-random assignment. Instead, I interpret my IV results through the lens of heterogeneity: as is well known, under these conditions, IV identifies a local average treatment effect (LATE) among "compliers" whose treatment status is affected by the instrument.

I find that ineligibility rate compliers—those placed in-borough during strict eligibility periods, but not otherwise—tend to be students from large, health-impaired families attending school in the Bronx. Size and functional limitations restrict the inventory of suitable shelter apartments and magnify the challenges of long commutes. Bronx residence facilitates access to the City's homeless intake center, which is located in the borough; the Bronx is also home to a plurality of the City's homeless shelters and the second most geographically isolated borough, raising the stakes of treatment. When eligibility policy becomes tight, these sorts of families are positioned to benefit: competition reduction is disproportionately important for those with complex needs, while a lengthy, iterative application process is to the competitive advantage of those with ease of access.

Compliers also reap outsized rewards from local placement: in nearly all cases, my IV estimates indicate treatment effects substantially stronger in magnitude than the average treatment effects (ATE's) estimated by OLS. Primary school compliers experience attendance improvements on the order of a full month; compliant high schoolers see impressive gains in academic performance. Point estimates for other outcomes are similarly large, though imprecisely measured. In the absence of endogeneity—my preferred interpretation—these gaps between ATE's and LATE's illustrate the potential welfare gains of targeting

[9]The product of a series of lawsuits emanating from the 1980's, this mandate, in large measure, explains the rapid expansion of the City's family homeless population at the core of my scarcity-based natural experiment. For a discussion, see Cassidy (2019).

interventions to the most receptive recipients, as well as the role of IV in identifying who they are. Eligibility policy tweaks have distributional consequences, both intended and not.

The alternative, though empirically less likely, case is that treatment is confounded by selection effects. IV results greater in absolute value than covariate-adjusted mean comparisons could suggest OLS is biased toward zero by systematic over-treatment of low-responders: those whose unobservable make them resistant to treatment effects. Proximity inelasticity, in turn, may derive from lack of ability (too much to improve) or its abundance (too little). In any event, OLS is, by this interpretation, a lower bound on true treatment effects.

The third stage of my analysis exploits the longitudinal nature of my data, which allows me to observe most students before, during, and after shelter stays. This is valuable descriptively, situating homeless spells in the broader contexts of their educational careers, and allows me to address a central question extant in the social policy literature: that of whether homelessness itself impacts educational outcomes, above and beyond the disadvantages poor families perpetually face[10].

My answer is a definitive "no." Homelessness per se explains little of homeless students' educational malaise. While it is true that homeless students do slightly worse during the years in which they enter shelter—missing about three more days, with mildly lower rates of proficiency—these differences are minor in the context of chronically unsatisfactory baseline performance. What's more, the shelter-entry blip is transitory, with outcomes reverting to pre-shelter levels in subsequent years, even among students remaining in shelter.

Instead, students who become homeless are those who were *already* struggling in school. Homelessness isn't a cause of educational impairment as much as it is a manifestation of conditions inhospitable to human capital development. An implication is that policies that improve homeless students' educational performances also hold insights for the broader population of poor and highly-mobile children and youth. As with policy effects, an important corollary is that variation is vast; means obscure ample diversity in student experiences.

The panel setup also lends itself to a student fixed effects identification strategy. Many students experience multiple spells of homelessness during my study period; those whose treatment statuses also vary across spells can serve as counterfactuals for themselves. The results of this model confirm my OLS findings, underscoring the theme of random assignment and suggesting multi-spell homeless students are little different than single-spell ones.

No study in economics has addressed the specific plight of homeless students. The few economics studies of homelessness have typically focused on single adults[11], macroe-

[10] Since the 1980's, families with children have garnered increasing attention from the interdisciplinary consortium of social scientists studying homelessness. For helpful summaries of this literature, see, e.g., Buckner (2008); Miller (2011); Samuels, Shinn and Buckner (2010).

[11] Allgood, Moore and Warren (1997); Allgood and Warren (2003).

conomic issues[12], prevention[13], or theory[14], though several works—e.g., O'Flaherty (2004) and O'Flaherty (2010)— helpfully investigate the antecedents and attributes of family homelessness. O'Flaherty (2019) provides a summary of the recent literature; notably, education is not mentioned. The work perhaps most similar to my own is Cobb-Clark and Zhu (2017), who find that childhood homelessness in Australia is associated with lower educational attainment and less employment in adulthood.

Three recent reviews—Buckner (2008), Samuels, Shinn and Buckner (2010), and Miller (2011)—ably summarize work on education and homelessness in disciplines outside of economics. This broader social policy literature increasingly asks whether poor attendance, behavior, performance, stability, and retention are the causal result of homelessness. The most rigorous studies have tended to say not, finding the gap between homeless and otherwise-poor students to be small and transitory[15].

My work confirms this impression, while also informing two related literatures in economics[16]. The first is that on neighborhood effects, which typically finds that, while children who grow up in high-poverty environments fare systematically worse[17], moving to better neighborhoods has little impact on low-income children's short-term educational performance[18], though it may inculcate longer-term attainment gains when moves come at early ages[19]. The literature on the economics of education explains why: while residential communities shape social and schooling opportunities, it is peers, school quality, and, especially, family that are the pivotal determinants of educational success[20]. Mobility is neither necessary nor sufficient; indeed, moves can hinder, rather than help[21].

Most pertinently, my results complement those in Cassidy (2019), where I find that families placed in shelters in their neighborhoods of origin remain in shelter 13 percent longer (about 50 days) and access more public benefits. Taken together, these two papers suggest proximity impacts homeless families' consumption choices. Local placements are preferred (in a reveal preference sense), so families consume more shelter when there. At the same time, local placements expand budget sets—through resource augmentation, decreased opportu-

[12] Cragg and O'Flaherty (1999); Gould and Williams (2010); O'Flaherty and Wu (2006).

[13] Goodman, Messeri and O'Flaherty (2014); Goodman, Messeri and O'Flaherty (2016); Evans, Sullivan and Wallskog (2016).

[14] Glomm and John (2002); O'Flaherty (1995); O'Flaherty (2004, 2009).

[15] Buckner (2012); Rafferty, Shinn and Weitzman (2004); Cutuli et al. (2013); Herbers et al. (2012); Brumley et al. (2015); Obradović et al. (2009); Masten (2012); Masten et al. (2014).

[16] Appendix A.2 includes a much more comprehensive review of the literature.

[17] Currie (2009); Currie and Rossin-Slater (2015); Cunha and Heckman (2007, 2009); Almond and Currie (2011).

[18] Solon, Page and Duncan (2000); Fryer Jr and Katz (2013); Jacob (2004); Jacob, Kapustin and Ludwig (2015); Ludwig et al. (2013); Sanbonmatsu et al. (2006).

[19] Chetty and Hendren (2018); Chetty, Hendren and Katz (2016); Chyn (2018).

[20] Carrell, Hoekstra and Kuka (2018); Lavy and Schlosser (2011); Sacerdote (2011); Fryer Jr and Katz (2013); Altonji and Mansfield (2018); Björklund and Salvanes (2011); Solon, Page and Duncan (2000).

[21] Hanushek, Kain and Rivkin (2004); Cordes, Schwartz and Stiefel (2017); Schwartz, Stiefel and Cordes (2017).

nity costs, or both—encouraging schooling consumption and leading to better attendance, fewer transfers, and improved academic performance.

There are three policy implications. The first is that shelter quality, often neglected, is an important policy parameter. Homelessness has been a priority for every recent mayor, but policy discussions typically focus on minimizing shelter stays, often through rental subsidies, or on avoiding them entirely, using prevention services. My results demonstrate that the quality of shelter stays—of which proximity is one facet—can augment or impede objectives in economically meaningful ways. Whether other shelter attributes, such as orderliness, amenities, or services, have similar impacts is of interest.

The second implication is one of perspective. An appreciation that in-shelter experiences mediate outcomes—along with the insight that shelter entry is not primarily responsible for homeless students' struggles—recasts shelter as an opportunity rather than an obstacle. Time in shelter is time with enhanced access to (on-site) support services. These services should be strategically designed to address students' preexisting educational challenges, inculcating habits and furnishing resources to transform educational trajectories.

The third lesson is budgetary trade-offs. Interventions like proximate placements are not cheap. 50-day longer stays at the City's average cost of $200 a night means the direct cost of associated educational and labor market gains is about $10,000 per family. One question for policymakers is whether this the right price. But another, more immediate one, is how policy can be tweaked to minimize these trade-offs. The key is targeting. I show that homeless students respond heterogeneously to proximity; under conditions of scarcity, resources—here, local placements—ought be allocated to those students most likely to benefit. Policy efficiency, in turn, should yield savings that can be used to compensate distantly-placed families in other ways.

In other words, the natural experiment at the core of my identification strategy should be replaced with evidence-based placements tailored to families' unique constraints and strengths. Detailed data collected at intake makes sophisticated targeting feasible. But even in its absence, the finding that high-constraint families disproportionately benefit from proximate placements is itself instructive: difficult-to-place locally means the City probably should.

2 Theory

In Appendix B, I use the framework of consumer theory to exposit a formal model characterizing the effects of school-based shelter placements on homeless students' educational outcomes. Here I summarize the key intuitions.

In choosing the quantity and quality of their children's educations, homeless families balance the rewards of schooling with the alternative satisfactions they could receive from competing uses of their time and energy. Local placements affect resources and relative prices. Being placed in a shelter in one's neighborhood of origin augments resources by preserving connections to existing social supports as well as neighborhood-specific human capital. But the price effect is ambiguous. Local placements reduce the absolute cost of school, through shorter commutes and fewer transfers. However, they affect other prices, too—for example, enhancing the appeal of socializing by decreasing the cost of seeing neighboring family and friends. Thus, the relative price of school could decrease or increase with in-borough placement. Without more information, it is difficult to predict which pattern will hold; it depends whether school or other consumption (including leisure) is more sensitive to distance effects. In former case, price and resource effects are reinforcing, bolstering educational outcomes; in the latter case, the net effect depends on whether resource augmentation outweighs increased (relative) opportunity costs. At the same time, resources govern policy elasticity. Families which greater distance-independent resources (which may take the form of fewer constraints) will be less sensitive to placement locations.

3 Policy Background and Data

3.1 Policy Background

Homeless families are perhaps 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.

The residential fluctuations of family homelessness make it somewhat delicate to define. In this paper, I adopt the standard DHS uses when reporting the City's family homeless census: those residing in DHS shelter system. This definition excludes those who are living doubled-up or in other temporary arrangements, and whom are classified as homeless by DOE under federal education law[22]. I adopt the stricter standard since the policy I study is shelter-based[23].

Typically consisting of a high-school-educated, racial minority single mom with several young children previously living in overcrowded conditions, homeless families look like other poor families because they *are* like other poor families—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

[22]This also excludes (comparatively) small numbers of families living in specialized shelters for domestic violence and HIV/AIDS, separately managed by HRA. Due to the City's right to shelter, virtually no families go unsheltered.

[23]Further, families in shelter have been verified by DHS staff as officially homeless, while DOE's indicator, frequently used in other studies, is self-reported and unevenly collected.

to return. Family homelessness is a phase, not a trait[24].

The consequences of poverty-induced residential instability are particularly pronounced in New York City. A constellation of forces—a hospitable legal environment and notoriously expensive real estate market, in combination with a tradition of progressive politics, an enviable fiscal affluence, and an exceptionally mature municipal social service apparatus[25]—have made NYC home to one in four sheltered homeless families nationally (The U.S. Department of Housing and Urban Development, 2018). And while family homelessness has declined nationwide by a third since 2009, DHS' census of homeless families grew from 8,081 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*a*). 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[26].

Families presenting themselves as homeless must submit to an eligibility determination process. At minimum, they must have at least one member under 21 or pregnant and demonstrate that they have no suitable place to live[27]. 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. Those found eligible may remain in their initial shelter placements as long as necessary, while ineligible families may appeal their decisions through a fair hearing process or reapply, as many times as desired. Most ineligibilities occur due to failure to comply with the eligibility process or because other housing is found to be available. Families may also "make their own arrangements" and voluntarily withdraw (or fail to complete) their applications. Eligible families may request transfers to more suitable shelter units as they become available.

The shelter system into which these families are placed is vast. Administered by DHS under the auspices of the Department of Social Services, it consists of more than 500 distinct shelter sites spread across the five boroughs (New York City Independent Budget Office, 2014; The City of New York, Mayor's Office, 2017). Although DHS runs several shelters directly, most day-to-day shelter operations are managed by contracted non-profit social service providers, as is the norm with human services in NYC (New York City Office of Management and Budget, 2018). The costs are substantial. In the fiscal year ending in June 2018, DHS spent $1.2 billion to shelter homeless families; the average cost per family *per*

[24] Culhane et al. (2007); O'Flaherty (2010); Fertig and Reingold (2008); Grant et al. (2013); Tobin and Murphy (2013); Shinn et al. (1998); Curtis et al. (2013); O'Flaherty (2004); New York City Independent Budget Office (2014); Greer et al. (2016); Shinn et al. (1998); Fertig and Reingold (2008).

[25] O'Flaherty and Wu (2006); The City of New York, Mayor's Office (2017); NYU Furman Center (2016); Grant et al. (2013); Ellen and O'Flaherty (2010); Evans, Sullivan and Wallskog (2016); O'Flaherty (2010).

[26] The state of Massachusetts is the other. For details, see Cassidy (2019).

[27] 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*b*); New York City Independent Budget Office (2014); and conversations with City officials.

day in shelter was $192 (New York City Office of Management and Budget, 2019; New York City Mayor's Office of Operations, 2018)[28].

To help address the challenges homeless students face, the City has, since at least 1998, maintained the explicit goal of placing homeless families in shelters near their youngest children's schools[29]. 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. But increasingly the policy has 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 (The City of New York, Mayor's Office, 2017).

Officially, the placement target is the shelter nearest the 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, 2018). With the rapid expansion of the City's family homeless population during the last decade, achieving this objective has become a not inconsiderable challenge. In recent years, shelter vacancy rates consistently hover below 1 percent; forced by threat of lawsuit to expand capacity essentially on-demand, the City has had to increasingly resort to booking rooms for families in commercial hotels (The City of New York, Mayor's Office, 2017). Whereas 82.9 percent of homeless families were successfully placed in-borough in 2008, just 49.8 percent were by 2018 (New York City Mayor's Office of Operations, 2010, 2018).

Aside from children's schools, DHS caseworkers also take into consideration safety (e.g., 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. This scarcity-induced quasi-randomness is the natural experiment at the core of my identification strategy.

3.2 Data and Sample

My data consists of an unbalanced panel covering the 2005–2016 school years among students whose families entered homeless shelter during calendar years 2010 to 2016, derived from

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

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

linking administrative records maintained by DOE and DHS[30].

The unit of observation is the student-school-year. The full homeless student panel, consisting of all school years observed for any student whose family entered shelter during this period, contains of 479,914 observations across 73,518 unique students. Students are observed for 1–12 school years, with the average student appearing 6.5 times.

Table 1 describes the path from the full data to my analytical sample. I restrict the analysis to students in grades K–12 (pre-K is voluntary), during school years 2010–2015 (the school years with complete coverage in the DHS data), not enrolled in special school districts (charter schools, students with disabilities, alternative schools, or unknown), and who are enrolled in DOE prior to the date of shelter entry (to avoid spurious treatment among non-NYC residents). These remaining 216,177 student-school-year observations are a mix of school years prior to, during, and post shelter spells. Spells may begin at any time during the school year. Some spells span multiple school years. Some students have multiple spells.

For my main analysis, I further restrict the sample to the school year of shelter entry. The information lost by treating a panel as a pooled cross-section is more than compensated making treatment comparable across students, at least conditional on month and year of shelter entry—since students enter shelter at different points during, and across, school years. In addition, one would expect the impact of temporary shelter placement would be largest contemporaneous to when it occurs. This leaves me with 43,449 observations, 34,582 of which correspond to students in grades K–8 and 8,867 of which refer to high schoolers. Henceforth I refer to this as my "Main" sample[31]. Students can appear multiple times if they have multiple homeless spelss. Usually I analyze primary and high schoolers separately. Occasionally I focus exclusively on K–8 students, as younger children are the main policy focus.

In terms of content, the DHS portion of my data, adapted from Cassidy (2019), contains extensive detail about families' identities, compositions, and shelter stays. The raw data consists of individual-level records for all family members; it is these records that I use match homeless students to their educational histories. I rework these data such the unit of observation is the family-homeless-spell, defined as beginning with a shelter entry more than 30 days subsequent to the end of a previous stay, which is natural in this setting[32]. To this core DHS data, I append information about homeless families' public benefit use maintained by HRA (Cash Assistance (CA); also known as "public assistance" or "welfare")

[30]Specifically, these students' families applied and were deemed eligible for homeless shelter between 1/1/2010 and 12/31/2016. For an extended discussion about the construction and content of the dataset, see Appendix A.3; for extensive detail on the DHS data specifically, see Cassidy (2019).

[31]Due do a minor coding issue that does not affect results, 16 students in this sample potentially had their applications entered or approved outside the calendar year 2010–2016 period.

[32]While arbitrary, 30 days is the conventional standard DHS uses to mark separate shelter stays; for administrative purposes, families returning within 30-days are considered not to have left.

and Food Stamps (formally, the Supplemental Nutrition Assistance Program (SNAP)), using probabilistic record linkage, as well as data on quarterly employment and earnings from the New York State Department of Labor (DOL), using a deterministic data linkage. For simplicity, I refer to the HRA and DOL data under the umbrella of "DHS" since the linkage is performed with the DHS data.

All DHS-derived covariates are defined at the time of shelter entry (or as near as possible). Individual-specific variables, such as age, are defined at the individual level. Attributes shared by all family members, such as eligibility reason or shelter type, are defined at the family level. The exceptions are variables derived from HRA and DOL: CA, SNAP, employment, earnings, and self-reported educational attainment, which are defined by head of household and treated as "family-level" variables common to all members. Families that are not matched to HRA or DOL are assumed genuinely not receiving benefits or not employed, respectively. I take the extra step of creating an "unknown" education category for families that do not match HRA in order to include head educational attainment as a covariate without restricting the sample; families missing educational attainment data are those not receiving public benefits.

Correspondingly, DOE's data contains records for each student during each school year (the unit of observation is the student-school-year), with separate annual "topical" files for June biographical information (demographics, student characteristics, and enrollment details, including school ID and attendance; so named because records are reconciled at the end of the school year, in June), test scores (3rd–8th grade state standardized tests and Regents exams for high schoolers), and graduation (for high schoolers). In addition to the topical files, there is also a separate transactions file detailing all admissions and discharges (including scheduled school level promotions to middle and high school, as well as non-normative transfers), and associated dates, over all school years in the sample. All variables are student-specific.

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.1, approximately 87 percent of children age 5–18 in the DHS data are successfully linked to NYC public school records—which is about as high a rate as could be hoped, given not all children attend public schools during their shelter stays.

As detailed in Appendix A.3, which describes all data management tasks in greater detail, I also create a second broader "Complete" sample, summarized in Table 1, that encompasses housed students, in order to contextualize homeless student outcomes. These comparisons are presented in Appendices E.2 and F.1.

3.3 Treatment and Outcomes

3.3.1 Treatment

My leading treatment definition is in-borough placement, an indicator equal to one if shelter borough is the same as school borough, and zero otherwise. While conceptually straightforward, it requires navigating two delicate issues. The first is data coarseness. Shelter entry dates are exact in the DHS data, but DOE's standard school identifier (June biographical data) reflects students' end-of-year enrollment. As such, students who change into schools near their shelters during the school year will be erroneously marked as treated in this data[33]. To address this concern, I implement an algorithm, described in Appendix A.3, that parses the DOE transactions data to identify each student's original school for each school year.

Second, I define treatment at the level of the individual student, rather than for the family as a whole. Although the official policy considers an entire family treated if it is placed in the borough of its youngest child's school, siblings do not necessarily attend schools in the same boroughs. Untreated students in "treated" families will dilute the effects of proximity, so I focus on the personal measure. In practice, it is rare for siblings to have different treatment statuses: the treatment concepts have a correlation of 0.91 among primary schoolers and 0.84 among high schoolers.

As the official policy objective, boroughs are a sensible way to conceptualize "neighborhoods" in NYC. Nevertheless, they implicate somewhat arbitrary boundaries and the usual loss of information embedded in binary treatments. A student placed 0.5 miles from school, but out-of-borough, is considered untreated, while one placed 5 miles away in-borough is. Thus, as a robustness check, I also consider a continuous treatment measure: the Euclidean (straight-line) distance between school and shelter, in miles. It is defined as:

$$N_i^C = \frac{1}{5280}\sqrt{(x_i^e - x_i^s)^2 + (y_i^e - y_i^s)^2}$$

where x_i^e and x_i^s are the x-coordinates for student i's school and shelter, respectively, measured in feet from an (arbitrary) origin, and analogously for the y's. As an additional check, I also consider the City's 32 geographical school districts as the unit of neighborhood.

53 percent of K–8 students in my Main sample are placed in their school boroughs, in shelters that are an average distance of 5.9 miles from their schools. For high schoolers, the borough treatment probability is 48 percent, and students are placed an average of 6.2 miles from their schools. School district placement rates are 11 percent and 8 percent, respectively.

[33] In addition, about 10 percent of K–12 homeless students in non-special districts originate from outside NYC during the 2010–2015 school years. I exclude these non-NYC students from my analysis.

3.3.2 Outcomes

The outcomes I assess span attendance, stability, retention, and performance. I pay particular heed to attendance and stability, which prior research identifies as homeless students' most acute educational impediments and theory suggests will have the greatest elasticity with respect to proximity.

I primarily quantify attendance using days absent. For robustness, given some students are not enrolled for full years, I also calculate results using absence rates, defined as days absent divided by days present plus days absent. My measure of stability is school changes, an indicator equal to one if a student had any non-normative school admissions during a school year[34]. For retention, I create an indicator "left DOE," equal to one if a student is not enrolled in DOE in the subsequent school year and did not graduate. As such, it captures non-normative exits from the public school system at any grade.

I consider one academic performance measure common to all students: a promotion 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].

My other aptitude measures differ between my primary- and high-school samples. Students in grades 3–8 take NYS Math and English Language Arts (ELA) standardized tests[36]. Numeric scores are scaled for grade-year difficulty and translated to four levels; students at levels 3 or 4 achieve proficiency[37]. I construct binary Math and English proficiency indicators consistent with this definition, modified such that students who miss a test (true of many homeless students) are classified as non-proficient. I also create an an overall proficiency indicator equal to one if a student scores 3 or higher on both tests.

For high schoolers, I consider two specific performance measures: binary indicators for any Regents exam taken and any Regents exam passed. To graduate high school in New York, students must pass five such tests, which are typically taken in the year of course completion, but can be retaken[38]. Given heterogeneity in high school trajectories, these

[34] 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 normative level changes occur in kindergarten, sixth grade, and ninth grade, which are the standard entry grades to elementary, middle, and high school, respectively. Since my 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.

[35] Because I generally focus on the placement effects in the year of shelter entry, a year-to-year promotion indicator is preferable to cumulative outcome measures, like graduation or drop out, which are observed only for a subset of my sample, and with varying propinquity to the timing of shelter entry.

[36] There are no standardized performance indicators for students in grades K-2.

[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 my specifications include year dummies, which restrict the level of comparison to within-year, this is not a major impediment to the analysis.

[38] Regents are named for the board that oversees the NYS Education Department (NYSED). At least one of the five exams must be in each of the core subject areas: English Language Arts, Math (Algebra, Geometry, Trigonometry), Science (Living Environment, Chemistry, Earth Science, Physics), and Social Studies (Global

generic indicators permit the widest comparability between students.

4 Empirical Approach

The central econometric challenge is to discern the causal effects of school-based shelter placements in the presence of potentially confounding selection effects. I use three approaches to identification: OLS, IV, and fixed effects.

I proceed from the potential outcomes framework, which is a natural way to organize observational policy evaluation. Letting Y_{Nip} denote an educational outcome Y (say, days absent) for student i during spell p under treatment N, I have, in the binary treatment case, two counterfactual states of the world

$$Y_{Nip} = \begin{cases} Y_{0ip} = \alpha_i & \text{if } N_{ip} = 0 \text{ (out-of-borough)} \\ Y_{1ip} = \alpha_i + \tau_i & \text{if } N_{ip} = 1 \text{ (in-borough)} \end{cases}$$

where $N_{ip} = \mathbf{1}\{borough_{ip,school} = borough_{ip,shelter}\}$ is an indicator for in-borough placement, τ_i is the treatment effect, and α_i are individual characteristics.

The challenge for causal inference is that no student is simultaneously observed in both treatment states.

4.1 Conditional Independence and OLS

As shown in Section 5, the data suggests shelter scarcity—quasi-random assignment—does, as DHS suggests, play a leading role in determining which families end up where, conditional upon the shelter entry environment and factors expressly considered as placement criteria. Under this conditional independence assumption, OLS is a consistent estimator of treatment effects. Accordingly, I model outcomes as depending on treatment and covariates (both observed and unobserved) in a linear, separable fashion, while allowing for the possibility of heterogeneous treatment effects. My general estimating equation is:

$$Y_{ip} = \mathbf{X}_{ip}\boldsymbol{\beta} + \tau^{OLS} N_{ip} + \varepsilon_{ip} \tag{1}$$

Educational outcome Y for student i during spell p is a function of myriad individual and institutional characteristics, to be described below, including unobservables ε_{ip}. The parameter of interest is τ^{OLS}, the coefficient on the in-borough placement indicator, which gives

History, U.S. History). NYSED may accept approved alternative subjects, such as a language exam, to fulfill one of the five tests. To graduate, students must also satisfy certain course credit requirements. Exams are given three times per year, in January, June, and August. They are graded on a scale of 0-100; passing is defined as 65 or higher. Students who pass nine exams receive an Advanced Regents diploma.

the average effect being placed in a shelter in one's school borough, controlling for the covariates and fixed effects ($\mathbf{X}_{ip}$) included in the model, which will be discussed shortly. The estimand of interest is the average treatment effect (ATE) of local placement, which is the population mean difference in outcomes between in-borough and out-of-borough placements. Under conditional independence,

$$\tau^{OLS} = E[\tau_i|\mathbf{X}_{ip}] = E[Y_{1ip} - Y_{0ip}|\mathbf{X}_{ip}] = ATE$$

Because my sample pools students whose ex ante treatment probabilities are not equal due to factors plausibly related to outcomes, my analysis must, at minimum, adjust for these institutional determinants. In my **"Base" specification**, I control for secular patterns in treatment probabilities and educational outcomes, by including fixed effects for school year, month, school borough, and grade[39]. These controls demean treatment and outcomes for time trends and education policy (years), seasonality (months), educational trajectories (grade levels), and the geography of homelessness (boroughs), so as to put all students on approximately equal footing. My **"Main" specification** augments the analysis to account for *student characteristics*[40], and *family characteristics*[41].

To add an additional layer of scrutiny, I also consider a **"Lag"** variant of my Main specification which includes days absent in the year prior to shelter entry. The idea is to proxy educational unobservables, and this is the outcome most consistently reported for all students. However, it is not my preferred specification for two reasons. First, for some students, prior year attendance is unobserved or unrepresentative, which reduces my sample size considerably[42]. Second, lagged absences eat up much of the variation in the data. While this is an important observation—past student tendencies explain future patterns—the effects of other factors become imprecisely estimated[43]. I view omitting the lag as an

[39] Specifically, I include: dummies for 2011—2015, with 2010 the omitted category; dummies for February–December with January omitted; dummies for Bronx, Brooklyn, Queens, and Staten Island with Manhattan omitted; and dummies for grades 1–8 with K omitted (primary school) and grades 10–12 with 9 omitted (for high school).

[40] Indicators for sex, English learner status, disability status, non-English speaking homes, NYC nativity, foreign birthplace, and seven categories of race (dummies for Hispanic, White, Asian, Native American, Multi-Racial, and unknown, with Black omitted).

[41] Indicators for head sex, head employed in the year prior to shelter entry, head SNAP receipt at the time of shelter entry, head partner presence, family health issue, pregnant family member; counts of student and non-student family members; five categories of head age (dummies for 18–20, 21–24, 25–34, and 45+, with 35–44 omitted); four categories of head education (dummies for high school graduate, some college or more, and unknown, with less than high school omitted); six categories of shelter eligibility (dummies for overcrowding, housing conditions, domestic violence, other, and unknown, with eviction omitted); and four categories of shelter type (dummies for cluster unit, commercial hotel, and other, with traditional Tier II shelters omitted).

[42] Eighth grade attendance is an example of unrepresentative control, as high school attendance is qualitatively different than middle school.

[43] For this reason, I do not include both lagged attendance and school and shelter fixed effects in the same model.

acceptable omission, as in-borough and out-of-borough students are virtually identical in pre-shelter outcomes.

Finally, my **"Refined" specification** adds school of origin and shelter fixed effects, refining the comparison to students within each of the 1,640 schools and 245 shelters in my sample. This model rules out bias from unobservable school and shelter characteristics invariant across students, the leading cases of which are systematic differential quality of teachers or shelter staff. This refinement puts a considerable burden of proof on detecting treatment effects: students placed locally must outperform their class- and shelter-mates—after accounting for all the other administrative controls. In addition, I add several time-varying *school characteristics*[44] to account for factors that may be idiosyncratic to a particular school year.

Throughout the main analysis, I estimate Equation 1 separately for primary school (grades K–8) and high school (grades 9–12) students. The reason is that the educational dynamics of high school, where students have greater independence, are categorically different than that of elementary and middle school, where parental volition exerts greater influence. As described in Section 3, I also restrict the analysis to the year of shelter entry for each student-spell.

To account for arbitrary covariances of unobservables 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, which I identify through a novel linking algorithm in Cassidy (2019). In most cases, family groups are consistent with the DHS (and standard) definition of family; 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.

4.2 Instrumental Variables and Heterogeneity

Operational administrative realities combined with detailed records make a strong case for conditional random assignment, but do not guarantee it. If treatment is endogenous and students placed in their boroughs of origin are systemically different from those placed out-of-borough in respects not captured by the data, OLS will be biased and inconsistent.

To guard against this possibility, I pursue an instrumental variables strategy based on the share of applicants found ineligible for shelter at the time of a family's shelter entry. Under the assumption of constant treatment effects, a second layer of quasi-randomness induced by a suitability exogenous instrument can recover a consistent ATE estimate in this setting.

On the other hand, if, as the evidence presented in Section 5 suggests, treatment assignment is truly random, but responses to it are diverse, IV does something more: it identifies

[44]Annual school enrollment, homeless student share, English language learner share, learning disability share, poverty share, and NYC native share.

the LATE among students whose treatment status is affected by the instrument. If this compliant subpopulation is also policy relevant, IV estimates can uncover policy insights the ATE obscures—even in the absence of endogeneity. Given local placements are scarce, understanding heterogeneous responses can help allocate slots in an aggregate welfare maximizing manner.

My instrument is the 15-day moving average of the initial ineligibility rate for rolling 30-day application periods. The City is legally required to provide shelter, but families are required to prove their need for housing. State rules and legal precedent regulate eligibility determinations, but City officials retain considerable discretion[45]. As described in Appendix C, which details the construction of my instrument, families may apply for shelter as many times as desired. These applications may be accepted, rejected, or voluntarily withdrawn (usually through non-completion). The 30-day periods reflect the agency view that repeat applications within a month reflect the same housing issue. A new period begins following a gap of more than 30 days from the date of a family's previous application; these periods are "rolling" in the sense that the 30-day clock resets with each application. "Initial" ineligibility refers to the disposition of a family's first application within each period. The 15-day moving averages smooth out noise in the data; they include each family's date of shelter entry and the 14 days prior, and are weighted in proportion to daily applications.

Strict eligibility policies restrict the pace of shelter entry, thereby reducing competition for scarce shelter units and raising the probability of in-borough placement for those deemed eligible. Whether the instrument is also exogenous depends upon whether changes in the ineligibility rate are independent of the types of families who are admitted to shelter. Because my Main sample consists of *eligible* family shelter entrants, my instrument plays a direct role in its composition. If strict eligibility policy changes the characteristics of shelter entrants, my results will be biased; the instrument will be picking up changes in the types of students who tend to enter shelter when eligibility policy is tight rather than treatment effects.

Fortunately, there is strong evidence that this sort of sample selection is not present. Simple time series trends demonstrate that the most pronounced swings in the ineligibility rate are coincident with policy changes. As shown in Figure 1, there is a striking discontinuity in eligibility in January 2014, when the Bloomberg administration was replaced by de Blasio mayoralty. In keeping with the latter's more generous stance towards the poor, ineligibility plummeted, only to rebound as the shelter census expanded during the following year. Similar spikes and troughs are evident around the times DHS commissioner changes, as well as during other well-documented policy changes[46].

[45]For example, see the discussions in New York City Independent Budget Office (2014); Routhier (2017*a*); Harris (2016).

[46]O'Flaherty (2019) describes several of these policy changes. See also the references in the prior footnote as well as Fermino (2016*a*); Eide (2018); New York Daily News Editorial (2014); Fermino (2016*b*); Katz (2015); Routhier (2017*b*).

Even more convincingly, the average characteristics of students and their families do not appear to be influenced by the ineligibility rate. As shown in Table 2, students who enter shelter during periods of unusually high and low eligibility are similar in most observable respects. The table, which pools grades K–12, reports contrasts between students who enter shelter when the normalized ineligibility rate is one standard deviation (or more) below the mean those those entering when it is one standard deviation (or more) above the mean (students entering during more unremarkable times are omitted). Results are the average differences in characteristics between students entering in high versus low eligibility periods, after adjusting for Base covariates[47].

Few differences are statistically significant. A notable exception is that students entering shelter during strict policy environments (periods of high ineligibility) come from smaller families. It is tricker for large families to apply for shelter. Each member is typically required to be present at some point during the application process and documentation requirements expand commensurately, so there are more opportunities for things to go wrong. In addition, students entering during strict periods are less likely to have been promoted (by 4 percentage points) and to have passed a Regents in the prior school year (by 13 pp). Although this could be interpreted as mild evidence of negative selection, other key educational metrics, including prior year absences and proficiency, are not statistically different; nor are family employment and benefit use.

A key reason for this compositional uniformity is that most families eventually become eligible for shelter. Eligibility policy is mostly about the flow of shelter entrants, not the stock. Strict eligibility delays shelter entry rather than preventing it. A sizable share of families apply multiple times within a given time period before being found eligible. Using 30-day application periods, Figure 2 plots the quarterly mean of the 15-day moving average of the initial and final ineligibility rates[48]. The final ineligibility rate is lower and less volatile than the initial one. During my sample period, the initial ineligibility rate ranges from 11.6 percent to 34.7 percent, with a mean of 23.1 percent, while the final rate varies from 5.2 to 22.4 percent, with a mean of 13.1 percent. In part due to repeat applications, strict ineligibility lengthens the time it takes to become eligible, as shown on the right axis. During lenient times, the average family becomes eligible within 5 days of applying; during strict times, it can take more than 10 days. The slowing of shelter entry raises the chances of local placement, but without sample selection.

I discuss additional arguments in favor of instrument validity in Appendix C. Neverthe-

[47]The ineligibility rate continues to be the 15-day moving average. This Base covariate adjustment is necessary to account for the same time, seasonal, borough, and grade trends that affect my main results. Furthermore, I never use the instrument without at least Base covariates, so what matters is not the raw instrument values, but the net-of-covariate residuals.

[48]The underlying quantities averaged are 15-day moving averages because that is what I use as my instrument. The picture looks the same using daily ineligibility rates.

less, as a robustness check, I also use average days to eligibility as an alternative instrument[49]. The typical lag between initial application and eventual approval is, of course, related to the ineligibility rate. However, because approval lags don't directly "select" the sample in the same way as the ineligibility rate (days to eligibility are a characteristic of the eligible), it captures the part of eligibility policy plausibly least related to applicant unobservables.

With Z_{ip} the instrument and N_{Zip} indexing potential treatment states, I estimate the ineligibility rate LATE, $\tau^{IV} = E[Y_{1ip} - Y_{0ip} | N_{1ip} > N_{0pi}, \mathbf{X}_{ip}]$, via two-stage least squares, with Equation 1 the second stage and the first stage given by:

$$N_{ip} = \tau^1 Z_{ip} + \mathbf{X}_{ip}\boldsymbol{\beta}^1 + \boldsymbol{\varepsilon}^1_{ip} \tag{2}$$

where the superscripts denote first-stage parameters, and first-stage predictions, $\widehat{N}_{ip}$, replace actual treatment status in the second stage.

4.3 Student Fixed Effects

The panel nature of my data also allows me to pursue a third identification strategy: student fixed effects. A fifth of my Main sample consists of students experiencing multiple spells of homelessness. When treatment status varies across these shelter stays, I can use these students as counterfactuals for themselves.

I implement my student fixed effects estimator by modifying Equation 1 to include individual student dummies, α_i. That is, for student i during shelter spell p,

$$Y_{ip} = \alpha_i + \tau^{FE} N_{ip} + \mathbf{X}_{ip}\boldsymbol{\beta} + +\varepsilon_{ip} \tag{3}$$

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

My student fixed effects estimator is consistent, at least for multi-spell students, if student unobservables relevant to treatment and outcomes remain constant across spells. Results consonant with OLS lend additional credence to the OLS validity argument; on the other hand, divergent findings may indicate that students with multiple stays are different than those with single stays. Given the "bad luck" underpinnings of family homelessness, a priori one would expect the former situation to hold.

[49]Specifically, using the same rolling 30-day application period as for the ineligibility rate, I take the 15-day moving average of the mean days elapsed between families' initial application dates and eventual eligibility dates.

5 Results

5.1 Descriptives and Randomization Check

Assessing the plausibility of the random assignment assumption is my first empirical task. If students placed in-borough and out-of-borough are observably comparable, it increases the likelihood their unobservables also align.

Tables 3A and 3B compare mean characteristics of students placed in-borough (Local) and out-of-borough (Distant), separately for the primary and high school Main samples. The contrasts are obtained from bivariate regressions of each variable on an indicator for in-borough treatment, with standard errors clustered at the family group level[50]. Locally- and distantly-placed students are quite similar; even without adjusting for year, borough, or grade, the random assignment assumption is plausible. Due to the large sample size, contrasts are frequently statistically significant, but the associated magnitudes are small, generally not greater than a percentage point or two.

There are several exceptions. Locally-placed primary school students come from families with 0.35 fewer persons and and whom are 12 percentage points (pp) less likely to have domestic violence as their eligibility reasons. The same is true of in-borough high schoolers, by margins of 0.28 persons and 9 pp, respectively.

There are also statistically significant, but quantitatively modest, differences in other characteristics. Locally-placed primary schoolers are 3 pp less likely to have an Individualized Education Program (IEP; an indicator of disability). In the year prior to shelter entry, they are 2 pp less likely to have changed schools and have 8 percent greater family earnings. In-borough high schoolers miss 1.9 fewer school days in the year prior to shelter entry. As a whole, in-borough students are also more likely to be Hispanic and less likely to be placed in commercial hotels (by about 3 pp in each case), but these differences are likely attributable to borough and year of shelter entry.

At the same time, the results emphasize why controlling for year, month, and borough is essential. Students entering shelter earlier (2010 or 2011), during the non-summer months (September–June), or from the Bronx or Brooklyn are systematically more likely to be placed in-borough. Competition for local shelter slots is weaker for these students. In addition, students in younger grades are generally more likely to enter shelter.

Besides confirming the comparability of treated and untreated students, the remainder of Tables 3A and 3B provides rich detail about the characteristics of homeless students and their educational outcomes. Most notably, homeless students struggle in school. They are chronically absent, acutely non-proficient, and unstably schooled[51].

[50] To conserve space, several less-interesting covariates are omitted or collapsed; a full enumeration of randomization checks are shown in Appendix Tables A.16–A.18. Appendix Figure A.17 presents these results graphically, with coefficients scaled in standard deviation units.

[51] Appendix Table A.4 summarizes how key treatment and outcome measures vary by year of shelter

Figure 3 makes clear how these students compare with their housed peers. The figure presents kernel density plots of days absent, pooling across school years 2010–2015 in my Complete sample (which includes non-homeless students), separately for K–8 and high school. The average homeless primary school student misses 26.9 days per year, or 1.5 times the DOE standard of chronic absence (which is 10 percent, or approximately 18 days). Were this pattern to hold throughout grades K–8, such a student would miss well in excess of a full school year by high school. By comparison, the averaged housed student misses 10.9 days per year. Matters are even more extreme for homeless high school students, who are absent an average of 45.5 days per year, compared with 21.2 days among housed students—and here, estimates are biased downward as drop-outs are selected out of the sample.

But means don't tell the whole story. The variance is vast and the right tails are very thick. While the median K–8 homeless student is absent 22 times per year, those at the 95th percentile miss 64 days of school annually. The contrast with housed K–8 students, who have a median of 8 days absent and a 95th percentile of 33 days, is striking. Once again, matters are starker for high schoolers. Homeless 9–12 graders at the 95th percentile miss 136 days per year. While it is true that some homeless students have good attendance, the takeaway is that averages, if anything, understate the scope of the challenges homeless students face[52].

5.2 Primary School Main Results

Table 4 presents my main results for primary schoolers. Outcomes are listed in rows and specifications in columns; each cell corresponds to a separate regression. The first four columns give OLS treatment effects estimates, while the last four give IV. Standard errors clustered at the family group level are given in parentheses below each coefficient. The OLS cells also contain sample sizes in braces (analogous IV sample sizes are the same); IV cells present first-stage F-stats in brackets. Overall, the results show clearly that local shelter placement benefits homeless students educationally.

There is a major attendance impact. According to my Base specification (Column 1), which controls for year, month of shelter entry, borough, and grade, homeless students placed in shelters in their school boroughs miss 2.8 fewer school days in the year of shelter entry, compared with those placed out-of-borough. As expected given covariate balance, additionally controlling for student and family characteristics in my Main specification (Col 2) hardly changes the coefficient, which drops to 2.4 days, but remains highly significant. Including lagged prior-year absences (Col 3) or school and shelter fixed effects, along with time-varying school characteristics, (Col 4) have no further effect. Compared with out-of-

entry. Tables A.5–A.12 present informative cross-tabulation-style summaries of sample shares, treatment, and selected outcomes by grade, borough, and year.

[52] Additional tables and figures comparing homeless and housed students are shown in Appendices E.2 and F.1. Additional tables exhaustively describing homeless students are shown in Appendix E.1.

borough students, this is an absence reduction of 8.3 percent. Using the absence rate as the dependent variable yields the same conclusion. According to my preferred Main specification, which strikes a balance between extensive controls and overly-refining the unit of comparison, the absence rate improves by 1.5 pp (8.8 percent) with local placement.

These are powerful effects. However, my IV results, presented in Cols 5–8, suggest these ATE's may, if anything, understate matters. Given the compelling evidence for random assignment in Tables 3A and 3B, my preferred IV interpretation is as indicative of treatment effect heterogeneity—the contrast between ATE's and LATE's. Nevertheless, skeptical readers may also regard my IV results as an endogeneity check.

The first IV observation is that the ineligibility rate instrument is strong, with first-stage F-statistics always above 13 and usually greater than 20. According to the first stage, whose coefficient is a highly statistically significant 0.67 in my Main specification (Col 6), for every percentage point increase in the ineligibility rate, homeless primary schoolers are 0.67 pp more likely to be placed in-borough.

The LATE effect on absences is about 23 fewer missed days per year according to my Main specification (Col 6). The IV treatment effect remains at 16 fewer absences even controlling for prior attendance (Col 7). And it *rises* to 26 days in my Refined specification (Col 8). This is a massive effect—a nearly 100 percent improvement relative to mean absences (29 days) among untreated students. But it is not implausibly large. Recall homeless students at the 95th percentile of the absence distribution miss 64 days per year, so the room for improvement is not inconsiderable. Using the absence rate as the dependent variable yields an identical conclusion. Students who end up placed in-borough by virtue of tight eligibility policy see their absence rates drop by an average of about 14 pp (control mean is 18 percent).

Stability gains are equally impressive, on average. According to OLS, in-borough placement dramatically reduces the probability of transfer for the average student. During the year of shelter entry, treated students are 17–20 pp less likely to experience a non-normative school change (Cols 1–4). This is a reduction of nearly a third in comparison to the 59 percent of out-of-borough students who change schools. By contrast, ineligibility rate compliers do not experience stability gains: IV point estimates for school changes are close to zero (Base and Main) or positive (Lag and Refined), and quite imprecise. Indeed, changing schools is the lone exception to an otherwise consistent IV-greater-than-OLS pattern in my results.

There is also evidence that in-borough placement improves academic performance. Per OLS, locally placed 3rd–8th graders are a statistically significant 0.9–1.3 pp more likely to be proficient in both Math and English. While small in absolute terms, 1 pp represents a 14.2 percent increase in the probability of proficiency, compared to the out-of-borough baseline of 7 percent. Most of this improvement is attributable to better Math performance. According to my preferred Main specification (Col 2), Math proficiency rates increase by 1.2

pp (an 8 percent increase relative to a baseline of 15 percent), while the differential in English performance is a statistically insignificant 0.8 pp. However, in the Base specification (Col 1), both coefficients are significant and of similar magnitude (0.014 for English and 0.016 for Math), so there is some evidence of across-the-board gains. The IV point estimates follow a similar pattern. Focusing on the Main specifications (Col 6), the probability of overall proficiency increases by 12.1 percentage points, with Math improving by 17.5 pp and ELA by 10.5 pp. However, the IV confidence intervals are wide and not exclusive of zero.

In-borough placement also improves retention. In-borough students are 1.4 pp less likely to leave DOE by the subsequent school year (Col 2)—a 16 percent reduction from the 9 percent of out-of-borough students who go elsewhere by the following year. For in-borough ineligibility rate compliers, this rises to a (not statistically significant) 15.4 pp reduction (Col 6). Although the destinations DOE leavers is unclear, one interpretation is that the students who stay (and their families) are more satisfied by the educations they are receiving from DOE.

In contrast, promotion rates appear relatively unaffected by placement. OLS estimates are near zero and insignificant, though the Base specification suggests a modest 0.6 pp boost. The LATE point estimate for compliers, at 8.5 pp (Col 6), is again larger, but still insignificant. This null promotion result is likely for two reasons. First, the overwhelming majority of homeless students are promoted; second, even though the academic performance of in-borough students is better, it is still low in an absolute sense.

To recap, OLS ATE estimates (Cols 1–4) indicate substantial gains in attendance, stability, proficiency, and retention for the typical homeless student. In Appendix B, I contextualize and explain these results with a microeconomic model of homeless family educational behavior. With the exception of stability, 2SLS LATE coefficients (Cols 5–8) for ineligibility rate compliers are similarly signed as OLS, much larger in magnitude, and additionally suggestive of promotion gains. However, except for attendance, these LATE's are imprecisely estimated and cannot rule out zero effects.

The potentially large gap between ATE's and LATE's makes it of considerable interest to understand who these compliers are; given limited local slots, prioritizing in-borough placement for students poised to benefit the most is a sensible policy rule.

Identifying and characterizing compliers is straightforward in the textbook binary instrument case (Angrist and Pischke, 2008). Calculating complier shares and characteristics is more complicated when, as here, the instrument is continuous. To do so, I adopt the approach to discretizing the continuous instrument outlined in Dahl, Kostøl and Mogstad (2014) and Dobbie, Goldin and Yang (2018). Full methodological details are described in Appendix C; here I focus on results.

I estimate that compliers comprise 13 percent of my primary school sample (Table A.19). Table 5 describes their characteristics, as well as how they contrast with non-compliers (always- and never-takers). Standard errors (in parentheses) and t-statistics testing the difference in means (in brackets below group differences) are calculated using 200 bootstrap replications, clustering for family groups. For brevity, only the most interesting attributes are shown; Table A.20 includes additional characteristics.

Compliers and non-compliers are similar in many respects. But the differences are telling. Compliant students come from larger—or, more accurately, medium-sized—families. 82 percent have at least one other sibling in school[53], compared with 69 percent among non-compliers. 67 percent come from families with four or five members; among non-compliers, just 40 percent do.

Compliers are also more likely to have disabilities or learning impairments: 34 percent have an Individualized Education Program (IEP), versus 22 percent among non-compliers. This pattern extends to their families as a whole. 42 percent of compliant families report a health issue (physical, mental, and/or substance abuse), compared with 32 percent of non-compliant ones, though this contrast narrowly misses significance at the 10 percent level.

These differences are crucial, as family size and health issues are two factors expressly considered as placement criteria. Larger families and those with disabilities are harder to place, as there are fewer suitable apartments[54]. Consequently, these families and their student members disproportionately benefit from tight ineligibility policy: when the rate of shelter entry slows, the chances of finding a unit that meets their more complex needs increases.

Geography is also pivotal. The majority of compliers (52 percent) are from the Bronx, versus a third of non-compliers. While this contrast narrowly misses statistical significance, related, more-precise results for other boroughs confirm this impression: just 6 percent of compliers, but 32 percent of non-compliers, come from Manhattan, Queens, and Staten Island. This is in keeping with the scarcity story. When policy gets tighter, those best positioned to benefit are families from the Bronx, which is home to a plurality of the City's shelter units as well as the City's PATH intake center. Easy access facilitates multiple application rounds, yielding a competitive advantage vis-a-vis out-of-borough competition[55].

Compliers and non-compliers are otherwise observably similar; while point estimates do differ, the standard errors (particularly for the smaller group of compliers) are large enough that the nulls of characteristic equality cannot be ruled out. At the same time, it is important to bear in mind that unobservables and interactions between characteristics are surely at work as well; after all, absolute majorities of students with "complier" traits are, in fact,

[53] I use the term "sibling" loosely, to mean another family member who is a child.

[54] A reason compliers tend to have medium-sized families rather than strictly the largest ones may be that families with 6+ persons—the hardest to place–are more likely to be never-takers; symmetrically, 1–3-person families may mostly be always-takers.

[55] Compliant students are also less likely to be female (40 percent vs. 52 percent among non-compliers). Why this is the case is not clear, but it is possible that homeless boys tend to come from larger families, or from ones with more health issues.

non-compliers.

Why are compliers' treatment effects estimated to be so much greater in magnitude than that of the average homeless student? The qualities that make their families more difficult to place—largeness and functional limitations—may reflect exactly those educational constraints most receptive to the influence of proximity. Nearness is theoretically more decisive for families juggling the sometimes contradictory needs of multiple children—and even more so in the presence of mobility limitations or other disabilities. At the extreme, a student's absences are a maximum function of his own and his siblings: everyone misses school when anyone does. Along similar lines, the Bronx is poorest and second most geographically-isolated borough, which makes local placement particularly valuable[56]. But most informative of all may be the null effect for school changes: if compliers, perhaps due to their constraints, are unlikely to change schools regardless of placement, it would indeed make sense that their attendance and performance would be highly sensitive to shelter assignment.

5.3 High School Main Results

Table 6 gives analogous results for high schoolers. OLS ATE's (Cols 1–4) are quite similar to those for K–8 students. High schoolers placed in-borough have better attendance (+2.5 days in the Main specification (Col 2), a 5.4 percent increase), are less likely to change schools (−10.1 pp, a 29 percent decrease), and are more likely to remain in DOE (−1.6 pp, a 8.4 percent decrease). These findings hold across all specifications. There is also some evidence of proficiency gains, with the probabilities of taking a Regents (+2.4 pp) and passing one (+2.0 pp) increasing according to the Base model (Col 1). That coefficients for all outcomes shrink as more controls are added suggest that selection effects may be a larger issue in high school than in earlier grades. One econometric concern is dropout, which occurs for about 27 percent of the homeless students in my data[57]. High schoolers in older grades (who have not dropped out) may be different from those in younger grades (who have not yet had the option).

As with K–8 students, IV coefficients are generally much larger in absolute value than OLS; however, given the high school sample size is only about a quarter that of the primary school sample, the instrument is correspondingly weaker and thus results are generally quite imprecise. First stage F-stats are generally around 10–14, while first stage coefficients are 0.58–0.67. In-borough high school compliers are 44.3 pp less likely to change schools, significant at the 10 percent level in the Main specification (Col 6). Taking other (Main) coefficients at their face values, in-borough compliers miss 12.5 fewer days per year and are 20.2 pp less likely to leave DOE—although they are also 16.4 pp less likely to be promoted, which, since the alternative may be dropping out, is a partially favorable outcome here.

[56] See Cassidy (2019) for a longer discussion of this point.

[57] Main high school sample 2010–2012 cohorts through 2016.

One striking departure from OLS, however, is academic performance. IV results imply a massive and statistically significant performance boost for compliers. According to my Main specification, compliers are 76 pp more likely to take a Regents and 72 pp more likely to pass one when placed locally. These results are suggestive of significant gains, even if the linearity assumptions embedded in 2SLS are too strong to be interpreted literally in this case.

To better understand these IV results—as well as their differences from the primary school pattern—it is helpful to return to Table 5 to look at the characteristics of high school compliers. While the small sample sizes preclude detecting many statistically significant differences, taking the point estimates at face value provides suggestive explanations.

Like primary school compliers, high school compliers come from larger families (59 percent have 4–5 members, compared with 38 percent among non-compliers) that are more likely to have health limitations (49 percent vs. 36 percent), and the students themselves are more likely to have disabilities (32 percent vs. 21 percent). Unlike K–8 compliers, high school compliers may be somewhat positively selected: their families are less likely to be on SNAP (48 percent vs. 70 percent) and more likely to be employed (68 percent vs. 37 percent). As shown in Table A.20, they are also more like to have taken (63 percent vs. 52 percent) or passed (39 percent vs. 33 percent) a Regents in the prior year. Overall, these characteristics suggest that high school compliers face similar barriers to local placement as do K–8 ones, but also have somewhat greater familial resources than other homeless high schoolers, which may account for the performance impact.

Overall, local placement helps high schoolers somewhat less than primary schoolers. As with grades K–8, the largest effect is a reduced probability of changing schools; unlike primary school, academic performance is impacted more than than attendance. One reason this may be so is that distance means less for attendance in high school than it does at younger grades. Indeed, many housed NYC high school students proactively choose schools that are out-of-borough or distantly located. In addition, educational decision-making shifts from parents to students in high school, which also implies proximity effects may differ.

5.4 Primary School Robustness

The results thus far represent profound policy effects, but econometric evidence is only as credible as its embedded assumptions. Beyond endogeneity, there are three other potential concerns: treatment definition, instrument propriety, and treatment timing.

Table 7 provides robustness checks to address these three issues, with alternative treatments in supercolumns, identification assumptions (estimation methods) in columns, and time periods in panels. As before, each row considers a distinct outcome (the most important of those discussed earlier). Each cell is a separate regression, all of which consist of my preferred Main specification. I consider two alternative treatment definitions (school district and distance), one alternative instrument (days to eligibility), and one alternative treatment effect time period (the year post-shelter-entry[58]).

Panel A continues to assess outcomes during the year of shelter entry. The first three columns retain my main borough-based treatment definition. Columns 1 and 2 are repeated from Table 4 for completeness. Column 3 presents results for my alternative days to eligibility instrument and confirms my main IV results. Days to eligibility compliers have a statistically significant 15-day attendance improvement. Results are imprecise for proficiency and promotion but suggestive of small effects. Compliers are also a statistically significant 19 pp less likely to leave DOE, which is a stronger finding that that using the ineligibility rate. The days IV point estimate for school changes similarly indicates a larger benefit to compliers than does the ineligibility rate[59].

The second set of columns considers an alternative treatment definition: placement within one's school district. Since the City is comprised of 32 school districts, this narrower unit of geography provides a more stringent treatment standard. Along with the change to the treatment indicator, Equation 1 is modified to include school district rather than school borough dummies.

The main OLS findings (Col 4) are confirmed. Students placed in their school districts have 2.6 fewer absences, are less likely to change schools (16 pp), and are more likely to be proficient (1.3 pp). Promotion and retention appear unaffected by school district placement. In general, these magnitudes are on par with their borough counterparts, which suggests that school district isn't qualitatively more important than school borough. The IV results (Cols 5 and 6) follow the same pattern as borough treatment: larger than OLS in absolute value (except for school changes), but imprecise. In the district case, the instrument is very weak, with first-stage F-stats always smaller than 3. Only 11 percent of students are placed in their school districts; the small treated sample clouds precision. Consequently, point estimates, while suggestive of large benefits to compliers, should not be interpreted literally.

The third set of columns presents an even more exacting check of proximity effects: treatment defined as distance in miles between school and shelter. My main results are confirmed. According to OLS (Col 7), homeless students are absent 0.27 fewer days for each mile their shelters are closer to their schools. A one standard deviation reduction (4.9 miles) in school-shelter distance thus improves attendance by 1.3 days; two SD's replicate the OLS ATE estimate. Similarly, a mile decrease in school-shelter distance reduces the probability of changing schools by 2.1 pp and increases the probability of retention by 0.14 pp. Proficiency effects continue to be modest, with a mile reduction in distance increasing the probability of proficiency by 0.09 pp. Promotion is unaffected by distance. Of course, it is unlikely for

[58] That is, the year $(t+1)$ following the year (t) of shelter entry.

[59] As described in Appendix Tables A.26 and A.27, days to eligibility compliers do, in fact, resemble ineligibility rate compliers: in particular, they come from medium-large families.

the effects of distance to be uniform at every distance. In Appendix Figures A.20 and A.21, I show there are diminishing marginal effects of distance on attendance and school changes when I allow for a quadratic specification.‘

As with my main results, ineligibility rate IV effects are much larger in magnitude than OLS. Compliers see their attendance improve by an average of 3.3 days for every mile they are placed closer to school. A one SD decrease in distance is worth 16 days of attendance. Days to eligibility IV confirms this pattern, with attendance improving a statistically significant 1.9 days per mile. Ineligibility IV results for other outcomes are similar to borough treatment—indicative of educational gains but imprecisely estimated. A one SD decrease in distance increases proficiency by 7 pp and promotion by 6 pp for compliers; however the likelihood of school change does not appear to be influenced much. For retention, however, the results are more precise: compliers are 11–12 pp more likely to remain in DOE when placed one SD more proximately, with statistical significance achieved for the days instrument.

To this point, I've focused entirely on policy effects in the school year of shelter entry. To assess whether these effects persist, Panel B shows results for the year following shelter entry (if shelter entry is defined as year t, this is year $t+1$). As expected, effects attenuate in comparison to the year of shelter entry, but some are still present[60]. According to OLS in the borough treatment case (Col 1), students placed in-borough miss an average of 0.6 fewer days in the year post shelter entry. They are 4.9 pp less likely to change schools, and 0.7 pp less likely to leave DOE. IV results generally follow a similar pattern as in the year of shelter entry: imprecisely estimated larger benefits to compliers. Ineligibility rate IV suggests an attendance improvement of 10 days and a reduced probability of changing schools of 13 pp, as well as a 7.8 pp greater likelihood of promotion and a 4.4 pp greater likelihood of retention. Days to eligibility IV suggests smaller benefits on these fronts, but finds compliers to have a statistically significant 15 pp greater likelihood of proficiency.

School district and distance treatment results are consistent with the main findings. There are generally small impacts in the year post-shelter entry, and IV estimates are imprecise. However, there is evidence that local placement reduces the probability of changing schools: per OLS, students placed in-district are 3.1 pp more likely to remain in their schools of origin; using distance as treatment definition, the school stability boost is 0.6 pp per mile.

Overall, my main results are robust to alternative treatments and identification strategies, and display explicable time dynamics. That the distance treatment measure confirms the official borough-based treatment definition is comforting: it demonstrates there is an underlying proximity effect, and not simply quirks of county[61]. Compliers in the days to eligibility IV substantially overlap ineligibility rate compliers; the former also helps guard

[60] I do not account for shelter exits or reentry, as these dynamics are endogenous.

[61] In Appendix Table A.25, I consider one additional treatment definition: residential borough. Treated students are those placed in shelters in the boroughs of their most recent home addresses, regardless of school location. Reassuringly, the main findings are confirmed.

against sample selection issues. Finally, treatment effects, while still present in the year after shelter entry, appear to attenuate quite rapidly, with the biggest enduring boon being school stability. To summarize, local placement benefits homeless primary school students, on average; some benefit tremendously.

5.5 High School Robustness

Table 8 assesses the robustness of these results to the same alternative treatments, identification strategies, and time periods as considered for primary schoolers.

The OLS findings in the year of shelter entry are confirmed (Panel A). Per school district treatment (Col 4), local placement reduces absences by an average of 2.7 days and the probability of changing schools by 10 pp, both on par with their borough treatment counterparts. Similarly, distance treatment (Col 7) demonstrates these effects are continuous. Absences decrease by 0.27 days for each mile shelter is closer to school, while the probability of changing schools is reduced by 1.1 pp per mile. As with borough, other outcomes appear unaffected. One exception is that the reduced probability of leaving DOE in the borough case is not replicated with the other treatment definitions.

Ineligibility rate IV results for school district (Col 5) and distance (Col 7) also affirm the borough findings (Col 2). Point estimates are almost all in the direction of OLS and larger in magnitude. For distance, the point estimates imply similar effects among compliers as in the borough case, while school district magnitudes are much larger—too large to be taken literally. Likely this is due to low instrument power in the district case. The most striking finding—in terms both of magnitude and statistical significance—remains the elevated probabilities of taking and passing Regents exams among treated compliers (by 9.7 pp and 8.4 pp per mile, respectively, in the distance case).

The days to eligibility IV reaffirms the ineligibility IV results, with the usual pattern of similarly-signed point estimates larger than OLS paired with large standard errors. Once again, the academic performance results are precise and strong, with the days compliers' probabilities of taking and passing a Regents increasing by 58 pp and 59 pp, respectively, for borough treatment (Col 3). The Regents-taking result also holds up for days to eligibility IV in the distance treatment case, increasing 8.4 pp per mile. In both the borough and distance cases, days IV effects are generally smaller than ineligibility IV, while the opposite holds for school district, though here instruments are far too weak to be credible.

As with primary school students, treatment effects attenuate in the year following shelter entry (Panel B). Also similar is that the greatest impact is a reduced probability of changing schools, which decreases by 3.2 pp with in-borough placement, according to OLS. The small sample size makes detecting other effects difficult, but the coefficients are generally of the expected signs, with IV results continuing to be substantially larger than OLS in absolute value.

5.6 Panel Results: Student Fixed Effects and Event Study

Reducing my homeless student panel to a student-spell cross section sharpens the policy analysis, at the cost of ignoring potentially useful information. Restoring its panel dimension serves two functions.

First, a student fixed effects model permits a qualitatively different robustness check relying upon wholly alternative identification assumptions. Table 9 presents my student fixed effects results, which dispense with unobserved spell-invariant student heterogeneity, yielding a quite exacting comparison of same-student outcomes when placed locally or distantly. In the pooled K–12 Main sample, 7,904 students experience multiple homeless spells during the 2010–2015 period; 59.2 percent of these experience different treatment assignments (i.e., both in- and out-of-borough) during these stays. I consider five outcomes, all defined as before except proficiency, which, given the pooling of primary and high school homeless spells, is now the union of (a) joint English and Math proficiency for grades 3–8 and (b) passing any Regents for grades 8–12 (eighth graders are eligible to take Regents). As before, all outcomes correspond to the year of shelter entry. The first three columns present borough treatment and the following three assess distance.

The results conform quite closely to OLS. In-borough students miss 2.7–3.1 fewer days of school, or 0.29–0.41 days for every mile they are placed closer to school. The probability of changing schools drops considerably with local placement—by about 15 pp for in-borough placement and 1.7 pp for every mile closer to school. Both attendance and stability outcomes are significant at the five percent level across all specifications. Proficiency and promotion point estimates are also in line with OLS, though with standard errors than cannot rule out null effects. These estimates suggest in-borough students are about 1.5 pp more likely to be proficient and about 1 pp more likely to be promoted; in the Refined specification (Col 3), the promotion gain is 2.3 pp, significant at the 10 percent level.

While the estimated benefits are far smaller than those suggested by IV, they are not necessarily incompatible. Ineligibility rate compliers come from families with specific placement constraints and opportunities. By contrast, students in the fixed effects sample experience multiple spells of homelessness, which potentially marks them as among the most deeply disadvantaged of all homeless students. This chronic instability (or its antecedents) may make them somewhat less responsive to treatment. Then again, if homelessness is viewed as bad luck, these students may be *more* representative of the general population of homeless students than are instrument compliers.

Beyond delivering a student fixed effects ATE estimator, the longitudinal nature of my data also allows me to follow homeless students over the courses of their educational careers and thus provide a clear answer to the central causality question currently debated by homelessness researchers. While it is undeniably true that homeless students fare worse educationally than their housed peers (see Figure 3), it is not homelessness, nor entering the shelter system per se, that causes these unfortunate outcomes. Instead, homeless students' struggles begin *prior* to shelter.

To see this, Figure 4 returns to my Main K–8 sample but expanded to include a one-year window around shelter entry, summarizing treatment effect dynamics for five key outcomes—absences, school changes, promotion, proficiency, and leaving DOE. Because the data aggregates across years and grades, outcomes are first detrended and scaled to the 2014 third grade mean. Years are measured relative to first shelter entry. Only students whose educational records are observed in all three years are included, and only for their first observed homeless spell, in order to guard against selection bias[62].

There are three key takeaways. First, pre-shelter outcomes are similar among students eventually placed in-borough and out-of-borough, reinforcing the propriety of the conditional random assignment assumption. Second, while outcomes are typically dismal, they don't get much worse in the year of shelter entry. What's more, attendance and stability begin reverting to pre-shelter levels quickly. Third, treatment effects are visualized. The increases in days absent and school changes are less pronounced for students placed locally; meanwhile, other outcomes remain similar, in part due to the minimal variation in proficiency, promotion, and retention among homeless students.

Table 10 formalizes this event-study analysis. Each column presents predicted average outcomes in the year of shelter entry, as well as in the years preceding and succeeding it, separately for treated and untreated students[63]. Confirming the by-now familiar patterns, treated and untreated students are similar before and after their shelter experiences. However, during the year of shelter entry, absences for in-borough students are 2 days less and their probability of changing schools is 19 pp lower; both gaps reflect smaller increases relative to pre-shelter rather than absolute reductions. The relative reduced probability of school changes persists in the following year as well (by 5 pp). Other contrasts are imprecise, though there is suggestive evidence that proficiency slightly increases among treated students during the year of shelter entry[64].

[62]All students are present in the data in their year of shelter entry, but not all are observed before and after—for example, those who enter shelter in first or eighth grade. As a complement, Figure A.19, which features a two-year window, includes any student observed in any year, so as to maximize sample coverage. It also separates students remaining in shelter from those who exited in post-shelter entry years.

[63]Specifically, I regress each column-enumerated dependent variable on Main covariates and in-borough treatment interacted with the school years prior to, during, and following a student's first shelter entry after 2010. The sample consists of the subset of Main K–8 sample students who are observed in all three years (pre-, during-, and post-shelter entry) during the time period encompassing school years 2010–2015. Predictions assume mean values of all other covariates. T-statistics for tests for equality of treated (in-borough) and untreated (out-of-borough) outcomes in each period are given at the bottom of the table.

[64]Discrepancies from the main analysis are due to the more rigid sample restriction that students be

To summarize, regardless of where they are placed, homeless students miss a lot of school and rarely attain proficiency—*but mostly not because they are homeless*. Instead it is the factors—familial, institutional, or otherwise—that give rise to homelessness that likely also explain these fundamental deficits—deficits that are not reflected in their rates of promotion.

5.7 Extensions: Mechanisms

It is clear that neighborhood-based shelter placements improve educational outcomes. But also of interest to understand why and how. While controlling for intermediate outcomes raises well-known endogeneity issues, analyses featuring the interaction between treatment and selected outcomes can provide suggestive evidence as to causal mechanisms. For brevity, the analysis in this section focuses on K–8 students.

One important causal channel is length of stay in shelter (LOS). In Cassidy (2019), I demonstrate that families placed in their borough of prior residence stay in shelter considerably longer than those placed distantly. Table 11 confirms this finding, though here school defines borough of origin. The setup is the same as Table 4. I consider several length of stay measures.

Focusing on my Main OLS specification (Col 2), students whose families are placed in-borough stay in shelter an average of 3.9 days longer during the school year of shelter entry (row one), or approximately 5.6 percent as measured by changes in logs (row two)—and fully 22.1 days longer in total (row three), a difference of 10.4 log points (row four)[65]. The probabilities of ever being homeless in the two years following shelter entry are unchanged (rows five and six). The average homeless family prefers, in the revealed preference sense, to be placed locally; when they are, they stay.

The ineligibility rate IV results are generally estimated imprecisely, but the point estimates suggest a quite different pattern: locally-placed compliers have shorter stays, to the tune of 91 fewer days in shelter. Further, they are a statistically significant 31 pp less likely to be in shelter during the school year following shelter entry. Why this is the case is not certain. One possibility is that, for compliers, the policy is working as intended: when larger, health-constrained families are kept connected to their communities, they are able to return to permanent housing more quickly. Another, less charitable, explanation is that shelter is especially unpleasant for these families; local shelter options may sacrifice comfort for location, and in so doing, compel them to move out sooner.

Table 12 suggests length of stay does contribute to observed treatment effects. As in Table 7, this table assesses outcomes in the year *following* shelter entry (that is, year $t+1$),

observed in all three years, which, for example, eliminates younger and older students.

[65]To be specific, length of stay is measured at the family level; it is possible some family members enter and leave during the course of the family's stay. I observe family shelter spells through CY2017; the small share of families not exiting by then have censored LOS's.

but allows treatment effects to vary between students remaining in shelter (stayers) and those who've exited (leavers) by including an indicator for "still homeless" during this school year along with its interaction with in-borough placement[66]. The reason for considering outcomes in the year post-shelter-entry is that students are homeless for differing lengths of time during the year of shelter entry; continued homelessness in the following school year is thus a fairer proxy for length of stay, as all families have a least a full summer to navigate housing options. All results feature the Main K–8 sample and control for Main covariates.

Focusing on the OLS results in Panel A, continued homelessness, as expected, slightly negatively impacts educational outcomes, but treatment (in-borough placement) attenuates these effects. The most notable effects are with attendance. Students still homeless a year after shelter entry miss an additional 3.5 days of school, compared with re-housed students (Col 1). Those who were placed in-borough, however, miss one day less. By contrast, there is no enduring attendance effect among students who've exited. Students remaining in shelter are also at an elevated risk for changing schools, by 4.2 pp; having been placed in-borough reduces this risk by 6.6 pp. There is no effect on school stability among leavers.

Similar patterns hold for promotion (Col 6) and retention (Col 7). Being in shelter is not good for future year advancement prospects. Out-of-borough homeless students remaining in shelter are 1.2 pp less likely to be promoted in the year following shelter entry relative to out-of-borough leavers. However, for treated students, this gap is reversed, with those remaining in shelter experiencing a 0.6 pp gain in the likelihood of promotion relative to treated leavers. Put differently, the difference in treatment effects between stayers and leavers is 1.8 pp; as with attendance, there is no continued treatment effect among leavers. In a similar way, untreated students remaining homeless an additional year are 1.4 pp more likely to leave DOE by the conclusion of that year, but having been placed in-borough eliminates this propensity to withdraw. In sum, treatment effects for attendance and academic progress in the year post-entry are strongest for those remaining in shelter.

An opposite pattern holds for Math proficiency (Col 3). In-borough students who exit shelter by the next school year see a 1.9 pp gain in Math proficiency; treated still-homeless students see a near null impact. Proficiency is a more difficult needle to move than attendance or promotion; perhaps it is the case that the academic benefits of local placement are offset by the familial disadvantages of long-stayers. There appear few effects on English proficiency or dual proficiency.

To summarize, long shelter stays are associated with worse educational outcomes, though inferring causality is clouded by unobserved differences between short- and long-staying families. Nevertheless, the benefits of local placement, in terms of attendance, stability, and academic progress, persist for these longer stayers while phasing out for those who exit. But

[66] Included among those counted as still homeless are students who exit shelter but begin a new spell during this school year. In this case, treatment is still defined as treatment status as of the prior spell.

proficiency gains are hampered by long stays.

The IV results in Panel B, which use the ineligibility rate instrument, are all insignificant, due to the loss in power having to instrument for the main treatment effect and its interaction with homelessness. For absence, the point estimates are as expected: in the direction of OLS, but larger in magnitude. However, for stability, proficiency, promotion, and retention, compliant leavers display larger salubrious point estimates than stayers. In other words, for compliers, the benefits of local placement are larger after leaving shelter for all outcomes except attendance. This may have to do with the above finding that compliers are likely to leave shelter more quickly.

A second causal mechanism is school changes. Excess mobility has been established as an educational impediment in prior research. Table 13 confirms this is true in my data as well. Similar in setup to Table 12 but returning to outcomes in the year of shelter entry, Table 13 interacts treatment with the indicator for school changes (to this point considered as an outcome), thereby allowing placement effects to differ among students who transfer and those who stay put. OLS (Panel A) gives three key results. First, mobility is associated with impaired performance. Absences increase; proficiency, promotion, and retention decrease. While I can't be sure movers are similar to non-movers on unobservables, it is exceedingly likely, on the basis of the well-developed student mobility literature, that this relationship is causal.

Second, the benefits of local placement are reduced for school changers, though some effects are imprecise. Treated students who remain in their schools of origin miss 2.1 fewer days than untreated students (Col 1); this effect is halved, to 1.1 days, when in-borough students change schools. This pattern holds, at least in terms of point estimates, for all other outcomes as well.

Third, school changes are worse for treated students. Out-of-borough school changers miss four more days of school than out-of-borough students who do not change schools; in-borough school changers miss five more days of school than in-borough non-changers. This suggests school changes are more deleterious for those students who are forced, or choose, to change schools despite in-borough placements. This may be because out-of-borough school changes offset disruption by offering access to better schools. But it could also be attributable to differences in unobserved characteristics among students who decide to change schools even when placed conveniently. Again, this pattern holds for proficiency, promotion, and retention.

The IV results (Panel B) are all imprecisely estimated, but the point estimates are suggestive. With the exception of promotion, the coefficients on treatment and the interaction term are the same signs as OLS but larger in magnitude; however, the signs on school change are reversed. Taken literally, this suggests school changes are beneficial for never-takers, who largely consist of families with domestic violence issues or other constraints on in-borough placement. Since these students are never placed in-borough, school changes yield shorter commutes, and, perhaps, environments more conducive to academic growth. As with OLS, treatment leads to better outcomes (e.g., 29.2 fewer days absent (Col 1)), but these benefits are reduced with school changes (e.g., to 17.2 fewer absences). As with the main results, compliers benefit more from treatment than the average student. On the other hand, promotion presents a quirky case: never-taker school changers are less likely to be promoted, as are compliant non-changers, while treatment effects are greatest for school-changing compliers. Why this pattern obtains is unclear.

Overall, these results indicate that not only is stability an important effect of school-based shelter placements, but it is also an important channel through which other impacts are conveyed.

6 Conclusion

Proximity boosts educational outcomes among homeless students. Those placed in shelters near their schools have considerably better attendance, stability, performance, and retention. The average homeless student experiences gains of 5–10 percent with respect to each of these outcomes when placed locally. The most conspicuous benefits are attendance and stability, which, not incidentally, are homeless students' most distinctive deficiencies. The finding that they miss about two-and-and-half fewer days of school when placed in shelters near their schools is robust across a wide spectrum of treatment definitions, identification strategies, and included covariates. Perhaps even more striking is school stability: locally placed students are a third less likely to change schools, and this greater permanence extends beyond the school year of shelter entry. Improved attendance and greater stability are the logical antecedents to gains in academic performance.

My complementary IV strategy demonstrates some students benefit quite a bit more than average. I argue that I do not require IV to circumvent endogeneity. Treatment-control balance in student characteristics and predetermined outcomes confirms the administrative impression that shelter is quasi-randomly assigned. Instead, by identifying the local average treatment effect among compliers, IV based on the family shelter ineligibility rate sheds light on heterogeneous responses among a policy-relevant subgroup: students whose families face particularly salient placement constraints or opportunities. These students tend to come from larger-than-average families with health or educational impairments residing in the Bronx. When placed locally, they experience larger than average benefits. Primary school compliers gain upwards of a month of attendance, while their high school counterparts become exceedingly more likely to remain in their schools of origin and to make progress toward graduation. There is suggestive evidence that other outcomes improve commensurately.

At the same time, homelessness does not impair educational performance so much as reflect it. While outcomes are slightly worse following shelter entry, the main point is that they are generally awful at baseline. Homeless students are like other disadvantaged students (including themselves when not homeless); accordingly, interventions that bolster their prospects can be generalized to other students in difficult circumstances.

School-based shelter placements have other effects as well. As I show in Cassidy (2019), families placed in shelters in their home boroughs remain in shelter longer, by about 13 percent, or roughly 50 days. At an average nightly cost of $200, this means students' educational gains cost the City about $10,000 per family, or, since the average family has two children in school, $5,000 per student. At the same time, families earn about 10 percent more when placed locally and also access more public benefits. For policymakers, one challenge is to determine the proper trade-off between these benefits and costs. More fundamentally, it is also necessary to understand whether longer shelter stays are themselves intrinsically valuable. Neighborhood-based placements are clearly expensive, but if, in addition to their educational merits, they enhance household and housing stability post-shelter, the additional upfront costs may be a wise investment.

These insights have important implications for policy. That homelessness is a symptom of fundamental family struggles rather than the primary cause of educational hardship means shelter is an opportunity as much as it is a challenge—a chance for professional educators and social workers to intervene in the lives of children facing long odds. In addition, heterogeneous responses to treatment suggest broad welfare gains are possible by targeting resources to the students and families most poised to benefit. While proximate placements implicate budgetary trade-offs, a necessary first step toward policy efficiency is evidence-based shelter assignments tailored to families' circumstances. The natural experiment that informs these recommendations should be replaced with systematically customized shelter services, with special priority given to families facing the most complex challenges.

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8 Tables

Table 1: Data and Sample Overview

	Main Sample		Complete Sample	
Refinement	Obs	Homeless Share	Obs	Homeless Share
All Data	479,914	0.37	6,798,801	0.02
In-School (Grades K–12)	419,405	0.33	6,416,995	0.02
School Years 2010–2015	262,446	0.44	6,416,995	0.02
Excluding Special School Districts	229,412	0.44	5,749,322	0.02
Enrolled in DOE Prior to Shelter	216,177	0.40	–	–
First School Year of Shelter Entry	43,449	1.00	–	–
Grades K-8	34,582	1.00	3,941,760	0.02
Grades 9-12	8,867	1.00	1,807,562	0.01

Sample refinements are cumulative: each row imposes an additional restriction on the row above it. Data from matched NYC DHS (calendar years 2010–2016) and DOE (school years 2005–2016) administrative records, as described in text. Dash indicates restriction doesn't apply.

Table 2: Ineligibility Instrument Shelter Entrants Comparison

	Low	High	Diff.	SE(Diff.)	T-Stat.	Obs.
Days Absent Prior Year	27.23	26.34	0.89	2.08	0.43	10,905
Absence Rate Prior Year	0.16	0.16	0.00	0.01	0.07	10,904
Admission Prior Year	0.30	0.30	0.00	0.04	0.01	11,377
Promoted Prior Year	0.91	0.87	0.04*	0.02	1.67	11,264
Proficient Prior Year	0.06	0.06	0.01	0.03	0.21	5,064
Took Regents Prior Year	0.55	0.51	0.04	0.06	0.62	2,458
Passed Regents Prior Year	0.41	0.28	0.13*	0.07	1.73	2,458
Student Age	10.89	10.86	0.03	0.06	0.57	13,755
Female	0.51	0.50	0.01	0.03	0.29	13,755
Black	0.55	0.53	0.02	0.05	0.44	13,755
Hispanic	0.40	0.43	-0.03	0.05	-0.63	13,755
White	0.03	0.02	0.00	0.01	0.17	13,755
IEP	0.27	0.24	0.02	0.03	0.78	13,755
ELL	0.10	0.08	0.02	0.02	0.83	13,755
Non-English	0.18	0.17	0.00	0.03	0.09	13,755
Foreign-Born	0.06	0.07	-0.01	0.02	-0.43	13,755
NYC-Born	0.78	0.77	0.01	0.04	0.27	13,755
Family Size	4.57	4.21	0.36*	0.22	1.68	13,755
Students in Family	2.46	2.22	0.24	0.16	1.47	13,755
Non-students in Family	2.11	1.99	0.12	0.11	1.12	13,755
Head Age	35.81	35.25	0.56	0.66	0.85	13,755
Female Head	0.89	0.94	-0.04	0.03	-1.62	13,755
On CA	0.37	0.30	0.07	0.05	1.40	13,755
On SNAP	0.73	0.67	0.06	0.05	1.42	13,755
Employed	0.39	0.41	-0.02	0.05	-0.48	13,755
Log Avg. Quarterly Earnings, Year Pre	2.78	2.97	-0.19	0.36	-0.52	13,755
Health Issue	0.40	0.42	-0.02	0.05	-0.52	13,755
Head Education: Less Than High School	0.62	0.55	0.07	0.05	1.45	13,755
Head Education: High School Grad	0.28	0.32	-0.04	0.05	-0.86	13,755
Head Education: Some College	0.04	0.06	-0.02	0.02	-1.19	13,755
Head Education: Unknown	0.05	0.06	-0.01	0.03	-0.31	13,755
Partner Present	0.24	0.28	-0.03	0.05	-0.70	13,755
Pregnant	0.04	0.05	-0.01	0.02	-0.40	13,755
Eligibility: Eviction	0.39	0.45	-0.05	0.05	-1.07	13,755
Eligibility: Overcrowding	0.21	0.18	0.02	0.04	0.62	13,755
Eligibility: Conditions	0.07	0.06	0.01	0.03	0.41	13,755
Eligibility: DV	0.24	0.25	-0.01	0.04	-0.29	13,755
Shelter Type: Tier II	0.54	0.57	-0.04	0.05	-0.75	13,755
Shelter Type: Commerical Hotel	0.17	0.17	0.00	0.04	0.07	13,755
Shelter Type: Family Cluster	0.29	0.24	0.05	0.05	1.11	13,755

Ineligibility rate normalized to mean 0, standard deviation 1. Low refers to periods/ where ineligibility rate was 1+ SD's below the mean; high refers to periods where it was 1+ SD's above the mean. Observations within 1 SD of mean are excluded. Group contrasts obtained from separate regressions of each characteristic on indicator for high ineligibility, controlling for Base covariates. Group means assume average Base covariate values. Differences are are coefficients on high ineligibility indicator. Data consists of Main sample, pooling grades K–12. Standard errors clustered at family group level. Number of observations differ for some characteristics due to inapplicability or missing data for some students.
* $p < 0.10$, ** $p < 0.05$

Table 3A: Descriptives and Random Assignment

	Primary School (K-8)					High School (9-12)				
	Overall		Randomization Check			Overall		Randomization Check		
	Mean	SD	Distant	Local	Diff.	Mean	SD	Distant	Local	Diff.
School Year (in 20xx form)	12.50	1.72	12.71	12.33	-0.38**	12.49	1.73	12.70	12.28	-0.42**
Calendar Month of Shelter Entry	6.73	3.41	6.85	6.63	-0.22**	6.77	3.37	6.87	6.66	-0.20**
Grade	3.53	2.54	3.51	3.54	0.03	10.04	1.07	10.07	10.00	-0.06**
School Borough: Manhattan	0.12	0.32	0.18	0.06	-0.12**	0.19	0.39	0.29	0.08	-0.21**
School Borough: Bronx	0.39	0.49	0.25	0.52	0.28**	0.34	0.47	0.20	0.49	0.29**
School Borough: Brooklyn	0.33	0.47	0.31	0.34	0.03**	0.31	0.46	0.27	0.35	0.07**
School Borough: Queens	0.13	0.34	0.20	0.07	-0.13**	0.13	0.34	0.18	0.08	-0.11**
School Borough: Staten Island	0.03	0.17	0.06	0.01	-0.05**	0.03	0.16	0.05	0.00	-0.04**
Student Age	9.46	2.78	9.45	9.47	0.02	16.57	1.48	16.62	16.50	-0.12**
Female	0.50	0.50	0.50	0.50	0.00	0.54	0.50	0.55	0.52	-0.03**
Black	0.53	0.50	0.53	0.52	-0.01	0.57	0.50	0.58	0.56	-0.02
Hispanic	0.43	0.49	0.41	0.44	0.03**	0.39	0.49	0.38	0.41	0.03**
ELL	0.10	0.30	0.10	0.10	0.01	0.09	0.29	0.09	0.10	0.01
Foreign-Born	0.05	0.22	0.05	0.05	-0.00	0.10	0.30	0.10	0.10	-0.00
IEP	0.24	0.43	0.25	0.23	-0.03**	0.22	0.42	0.23	0.22	-0.02
Head Age	34.43	7.39	34.41	34.45	0.04	40.43	7.89	40.23	40.65	0.43**
Female Head	0.92	0.27	0.93	0.92	-0.00	0.90	0.29	0.91	0.90	-0.01
Students in Family	2.33	1.26	2.46	2.22	-0.23**	2.40	1.32	2.48	2.31	-0.17**
Non-students in Family	2.11	1.16	2.17	2.05	-0.12**	1.88	1.07	1.93	1.83	-0.11**
Head Education: Less Than High School	0.59	0.49	0.58	0.59	0.01*	0.58	0.49	0.57	0.59	0.02
Head Education: High School Grad	0.30	0.46	0.30	0.30	0.01	0.31	0.46	0.32	0.31	-0.01
Head Education: Some College	0.05	0.22	0.05	0.05	-0.01**	0.06	0.23	0.06	0.06	0.00
Head Education: Unknown	0.06	0.24	0.07	0.06	-0.01**	0.05	0.22	0.05	0.05	-0.01
Health Issue	0.33	0.47	0.34	0.32	-0.01**	0.38	0.48	0.39	0.37	-0.02*
Partner Present	0.27	0.45	0.29	0.26	-0.02**	0.21	0.41	0.23	0.20	-0.04**
Pregnant	0.05	0.21	0.05	0.04	-0.01	0.02	0.15	0.03	0.02	-0.00
On CA	0.36	0.48	0.36	0.36	-0.00	0.31	0.46	0.31	0.32	0.01
On SNAP	0.71	0.45	0.71	0.72	0.01	0.68	0.47	0.67	0.68	0.01
Employed	0.38	0.48	0.37	0.38	0.01	0.41	0.49	0.41	0.41	-0.00
Log Avg. Quarterly Earnings, Year Pre	2.66	3.56	2.62	2.70	0.09*	3.03	3.78	3.03	3.02	-0.00
Eligibility: Eviction	0.44	0.50	0.40	0.49	0.09**	0.53	0.50	0.51	0.55	0.05**
Eligibility: Overcrowding	0.17	0.37	0.16	0.17	0.01**	0.16	0.37	0.15	0.17	0.02*
Eligibility: Conditions	0.07	0.25	0.06	0.07	0.01**	0.07	0.26	0.07	0.07	0.01
Eligibility: DV	0.24	0.43	0.30	0.19	-0.12**	0.17	0.38	0.21	0.13	-0.08**
Shelter Type: Tier II	0.54	0.50	0.54	0.55	0.00	0.53	0.50	0.53	0.54	0.01
Shelter Type: Commercial Hotel	0.18	0.38	0.19	0.16	-0.03**	0.18	0.39	0.19	0.17	-0.02**
Shelter Type: Family Cluster	0.27	0.44	0.26	0.29	0.03**	0.27	0.44	0.27	0.28	0.01

Data consists of Main primary school (grades K–8) and high school (9–12) samples, assessed separately. As described in the text, the Main samples are limited to school years of shelter entry among students enrolled in DOE prior to shelter entry and not in special school districts 75, 79, 84, and 88. Treatment defined as placed in-borough. Group contrasts obtained from separate bivariate OLS regressions of each characteristic of interest on treatment indicator. Differences between in-borough and out-of-borough means are coefficients on treatment indicator. Standard errors clustered at the family group level.

* $p < 0.10$, ** $p < 0.05$

Table 3B: Descriptives and Random Assignment

	Primary School (K-8)					High School (9-12)				
	Overall		Randomization Check			Overall		Randomization Check		
	Mean	SD	Distant	Local	Diff.	Mean	SD	Distant	Local	Diff.
Days Absent Prior Year	24.49	18.77	24.61	24.39	-0.22	36.57	35.07	37.48	35.61	-1.87**
Absence Rate Prior Year	0.15	0.11	0.15	0.14	-0.00**	0.23	0.22	0.23	0.22	-0.01**
Changed School Prior Year	0.31	0.46	0.32	0.30	-0.02**	0.24	0.43	0.24	0.24	-0.00
Promoted Prior Year	0.92	0.28	0.92	0.91	-0.00	0.76	0.43	0.76	0.76	-0.00
Proficient Prior Year	0.11	0.31	0.10	0.11	0.01**	0.07	0.25	0.08	0.06	-0.02*
Took Regents Prior Year	0.03	0.16	0.02	0.03	0.01	0.54	0.50	0.54	0.53	-0.01
Passed Regents Prior Year	0.02	0.13	0.02	0.01	-0.01	0.34	0.47	0.34	0.34	0.00
Days Absent	27.81	20.51	29.00	26.77	-2.23**	44.65	40.68	45.92	43.31	-2.61**
Absence Rate	0.17	0.12	0.18	0.16	-0.02**	0.30	0.27	0.31	0.28	-0.02**
Changed School	0.49	0.50	0.59	0.39	-0.20**	0.30	0.46	0.34	0.25	-0.10**
Promoted	0.92	0.27	0.92	0.92	-0.00	0.70	0.46	0.70	0.70	0.00
Behind Grade	0.33	0.47	0.33	0.33	-0.00	0.59	0.49	0.59	0.58	-0.01
Left DOE	0.08	0.28	0.09	0.08	-0.01**	0.18	0.38	0.19	0.16	-0.03**
Math Proficient	0.16	0.37	0.15	0.17	0.03**	.	.	.	.	.
ELA Proficient	0.14	0.35	0.13	0.15	0.01**	.	.	.	.	.
Proficient	0.08	0.28	0.07	0.09	0.02**	.	.	.	.	.
Regents Taken	0.08	0.26	0.07	0.09	0.02*	0.65	0.48	0.65	0.65	0.00
Regents Passed	0.06	0.23	0.04	0.07	0.02**	0.40	0.49	0.40	0.40	-0.00
Placed in School District	0.11	0.32	0.00	0.21	0.21**	0.08	0.28	0.00	0.17	0.17**
School-Shelter Distance	5.89	4.86	9.71	2.54	-7.16**	6.22	4.51	9.21	2.95	-6.26**
Placed in School Boro	0.53	0.50	0.00	1.00	1.00	0.48	0.50	0.00	1.00	1.00

Data consists of Main primary school (grades K–8) and high school (9–12) samples, assessed separately. As described in the text, the Main samples are limited to school years of shelter entry among students enrolled in DOE prior to shelter entry and not in special school districts 75, 79, 84, and 88. Treatment defined as placed in-borough. Group contrasts obtained from separate bivariate OLS regressions of each characteristic of interest on treatment indicator. Differences between in-borough and out-of-borough means are coefficients on treatment indicator. Standard errors clustered at the family group level.

* $p < 0.10$, ** $p < 0.05$

Table 4: Primary School (K-8) Main Results

	OLS				IV			
	(1) Base	(2) Main	(3) Lag	(4) Refined	(5) Base	(6) Main	(7) Lag	(8) Refined
Days Absent	-2.77**	-2.39**	-2.36**	-2.41**	-23.96**	-22.67**	-16.15**	-26.05**
	(0.30)	(0.29)	(0.27)	(0.30)	(7.62)	(7.15)	(6.87)	(9.46)
	{33,866}	{33,866}	{26,475}	{33,782}	[35.5]	[38.3]	[26.6]	[24.6]
Absence Rate	-0.018**	-0.015**	-0.016**	-0.015**	-0.140**	-0.136**	-0.087**	-0.152**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.046)	(0.044)	(0.039)	(0.057)
	{33,866}	{33,866}	{26,475}	{33,782}	[35.5]	[38.3]	[26.6]	[24.6]
Changed School	-0.196**	-0.180**	-0.170**	-0.176**	-0.010	-0.007	0.075	0.083
	(0.007)	(0.007)	(0.008)	(0.007)	(0.168)	(0.161)	(0.198)	(0.207)
	{34,429}	{34,429}	{26,651}	{34,343}	[36.7]	[39.3]	[26.5]	[24.6]
Math Proficient	0.016**	0.012**	0.012*	0.011*	0.160	0.175	0.100	0.176
	(0.006)	(0.006)	(0.006)	(0.006)	(0.135)	(0.130)	(0.149)	(0.167)
	{20,235}	{20,235}	{17,102}	{20,115}	[19.5]	[21.1]	[16.0]	[13.7]
ELA Proficient	0.014**	0.008	0.009	0.008	0.086	0.105	0.038	0.080
	(0.005)	(0.005)	(0.006)	(0.006)	(0.127)	(0.121)	(0.140)	(0.156)
	{20,235}	{20,235}	{17,102}	{20,115}	[19.5]	[21.1]	[16.0]	[13.7]
Proficient	0.013**	0.010**	0.010**	0.009**	0.120	0.121	0.047	0.139
	(0.004)	(0.004)	(0.005)	(0.004)	(0.097)	(0.093)	(0.106)	(0.122)
	{20,235}	{20,235}	{17,102}	{20,115}	[19.5]	[21.1]	[16.0]	[13.7]
Promoted	0.006*	0.004	0.004	0.004	0.080	0.085	0.059	0.130
	(0.003)	(0.003)	(0.004)	(0.004)	(0.076)	(0.075)	(0.085)	(0.105)
	{31,525}	{31,525}	{24,973}	{31,435}	[34.3]	[36.2]	[24.9]	[21.4]
Left DOE	-0.013**	-0.014**	-0.011**	-0.013**	-0.137	-0.154	-0.128	-0.203
	(0.004)	(0.004)	(0.004)	(0.004)	(0.097)	(0.094)	(0.099)	(0.125)
	{34,429}	{34,429}	{26,651}	{34,343}	[36.7]	[39.3]	[26.5]	[24.6]
First Stage								
In-Borough Placement					0.659**	0.669**	0.617**	0.526**
					(0.109)	(0.107)	(0.120)	(0.106)
Base Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Main Covariates	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Lagged Absences	No	No	Yes	No	No	No	Yes	No
School Covariates	No	No	No	Yes	No	No	No	Yes
School & Shelter FE	No	No	No	Yes	No	No	No	Yes

Each cell reports the coefficient on in-borough shelter placement from a regression of the row-delineated outcome on the treatment indicator, controlling for the column-enumerated covariates, using the super-column-indicated method. Base covariates are indicators for school year, month of shelter entry, school year beginning borough, and grade. Main covariates augment the Base specification with student characteristics (indicators for sex, race, English language learner, foreign-speaking family, foreign birthplace, non-NYC birthplace, and disability); family characteristics (indicators for head sex, age category, partner present, education level, employment, SNAP receipt, and family health issue, as well as counts of students and non-students in the family); and shelter placement characteristics (indicators for eligibility reason and shelter type). Lag specification adds prior year days absent to Main covariates. Refined specification adds school and shelter fixed effects to Main specification, as well as year-varying school characteristics (enrollment, homeless share, ELL share, disability share, poverty share, and non-NYC share). The instrument for 2SLS is the family shelter ineligibility rate at the time of shelter entry. The unit of observation is a student-year; only school years of shelter entry are included. Standard errors clustered at family group level in parentheses. Number of observations given in braces; corresponding OLS and IV covariate models have equal N's. First-stage F-stats in brackets. * $p < 0.10$, ** $p < 0.05$

Table 5: Complier Characteristics, Ineligibility Rate Instrument

	Primary School (K-8)			High School (9-12)		
	Compliers	Non-Compliers	Diff.	Compliers	Non-Compliers	Diff.
Household Size: 1-3	0.18	0.36	-0.17	0.37	0.39	-0.01
	(0.004)	(0.000)	[-2.64]	(0.029)	(0.000)	[-0.07]
Household Size: 4-5	0.67	0.40	0.27	0.59	0.38	0.21
	(0.008)	(0.000)	[3.08]	(0.039)	(0.001)	[1.05]
Household Size: 6+	0.16	0.24	-0.09	0.17	0.22	-0.05
	(0.007)	(0.000)	[-0.99]	(0.022)	(0.000)	[-0.35]
1 Student in Family	0.18	0.31	-0.13	0.28	0.29	-0.01
	(0.004)	(0.000)	[-2.01]	(0.020)	(0.000)	[-0.06]
$>$ 1 Students in Family	0.82	0.69	0.13	0.73	0.71	0.02
	(0.005)	(0.000)	[1.82]	(0.021)	(0.000)	[0.15]
On SNAP	0.74	0.71	0.03	0.48	0.70	-0.22
	(0.007)	(0.000)	[0.30]	(0.041)	(0.001)	[-1.08]
Employed	0.33	0.38	-0.06	0.68	0.37	0.32
	(0.007)	(0.000)	[-0.67]	(0.044)	(0.000)	[1.49]
Health Issue	0.42	0.32	0.11	0.49	0.36	0.13
	(0.005)	(0.000)	[1.55]	(0.030)	(0.000)	[0.73]
IEP	0.34	0.22	0.12	0.32	0.21	0.12
	(0.003)	(0.000)	[2.17]	(0.020)	(0.000)	[0.82]
ELL	0.12	0.10	0.02	-0.01	0.11	-0.12
	(0.003)	(0.000)	[0.37]	(0.010)	(0.000)	[-1.15]
Female	0.40	0.52	-0.12	0.31	0.57	-0.26
	(0.005)	(0.000)	[-1.74]	(0.029)	(0.000)	[-1.52]
Black	0.43	0.54	-0.11	0.64	0.56	0.08
	(0.008)	(0.000)	[-1.22]	(44.703)	(0.001)	[0.01]
Hispanic	0.49	0.42	0.08	0.28	0.41	-0.12
	(0.007)	(0.000)	[0.91]	(28.487)	(0.001)	[-0.02]
School Borough: Manhattan	0.01	0.14	-0.12	0.09	0.20	-0.11
	(0.002)	(0.000)	[-2.41]	(0.013)	(0.000)	[-0.97]
School Borough: Bronx	0.52	0.37	0.15	0.52	0.31	0.20
	(0.008)	(0.000)	[1.62]	(0.033)	(0.000)	[1.12]
School Borough: Brooklyn	0.35	0.32	0.03	0.33	0.31	0.03
	(0.007)	(0.000)	[0.29]	(0.028)	(0.000)	[0.15]
School Borough: Queens	0.04	0.15	-0.11	-0.09	0.17	-0.26
	(0.003)	(0.000)	[-2.10]	(0.017)	(0.000)	[-1.97]
School Borough: Staten Island	0.01	0.03	-0.03	0.03	0.03	0.01
	(0.000)	(0.000)	[-1.71]	(0.001)	(0.000)	[0.18]
Days Absent Prior Year	25.61	24.32	1.29	41.90	35.80	6.10
	(8.589)	(0.208)	[0.44]	(66.293)	(1.646)	[0.74]
Changed School Prior Year	0.33	0.31	0.02	0.21	0.25	-0.04
	(0.006)	(0.000)	[0.23]	(0.032)	(0.000)	[-0.21]

Main sample. Treatment is in-borough placement. Instrument is 15-day moving average of the initial ineligibility rate for 30-day application period. Compliers are those students placed in-borough when the ineligibility rate is high, but not otherwise. Non-compliers consist of always-takers and never-takers. Compiler and non-complier characteristics, adjusted for year and month of shelter entry, are estimated from the algorithm described in Appendix C.4. Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family.

Table 6: High School (9–12) Main Results

	OLS				IV			
	(1) Base	(2) Main	(3) Lag	(4) Refined	(5) Base	(6) Main	(7) Lag	(8) Refined
Days Absent	-4.62**	-2.53**	-1.48*	-2.84**	-25.84	-12.46	5.88	4.44
	(1.02)	(0.99)	(0.80)	(1.06)	(26.87)	(22.32)	(18.86)	(26.39)
	{8,608}	{8,608}	{7,501}	{8,349}	[11.0]	[14.0]	[11.2]	[10.7]
Absence Rate	-0.035**	-0.019**	-0.010**	-0.018**	-0.279	-0.174	-0.092	-0.033
	(0.007)	(0.006)	(0.005)	(0.007)	(0.182)	(0.146)	(0.116)	(0.167)
	{8,608}	{8,608}	{7,501}	{8,349}	[11.0]	[14.0]	[11.2]	[10.7]
Changed School	-0.104**	-0.101**	-0.089**	-0.083**	-0.447	-0.443*	-0.378	-0.252
	(0.011)	(0.011)	(0.011)	(0.012)	(0.286)	(0.258)	(0.265)	(0.282)
	{8,816}	{8,816}	{7,635}	{8,555}	[11.5]	[14.4]	[11.5]	[10.4]
Regents Taken	0.024**	0.007	0.007	0.015	0.868**	0.761**	0.724**	0.591*
	(0.011)	(0.011)	(0.011)	(0.012)	(0.368)	(0.315)	(0.332)	(0.347)
	{8,816}	{8,816}	{7,635}	{8,555}	[11.5]	[14.4]	[11.5]	[10.4]
Regents Passed	0.020*	0.003	0.002	0.004	0.776**	0.719**	0.633**	0.418
	(0.011)	(0.011)	(0.011)	(0.012)	(0.355)	(0.307)	(0.322)	(0.324)
	{8,816}	{8,816}	{7,635}	{8,555}	[11.5]	[14.4]	[11.5]	[10.4]
Promoted	0.018	0.007	0.007	-0.001	-0.246	-0.164	-0.221	-0.347
	(0.012)	(0.012)	(0.012)	(0.013)	(0.345)	(0.277)	(0.293)	(0.342)
	{7,246}	{7,246}	{6,362}	{6,992}	[7.6]	[11.4]	[9.8]	[9.1]
Left DOE	-0.026**	-0.016*	-0.015	-0.016	-0.295	-0.202	-0.045	-0.074
	(0.010)	(0.009)	(0.009)	(0.010)	(0.270)	(0.231)	(0.244)	(0.321)
	{8,152}	{8,152}	{7,018}	{7,889}	[8.8]	[11.4]	[8.4]	[6.1]
First Stage								
In-Borough Placement					0.613**	0.674**	0.640**	0.579**
					(0.181)	(0.178)	(0.188)	(0.180)
Base Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Main Covariates	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Lagged Absences	No	No	Yes	No	No	No	Yes	No
School Covariates	No	No	No	Yes	No	No	No	Yes
School & Shelter FE	No	No	No	Yes	No	No	No	Yes

Each cell reports the coefficient on in-borough shelter placement from a regression of the row-delineated outcome on the treatment indicator, controlling for the column-enumerated covariates, using the super-column-indicated method. Base covariates are indicators for school year, month of shelter entry, school year beginning borough, and grade. Main covariates augment the Base specification with student characteristics (indicators for sex, race, English language learner, foreign-speaking family, foreign birthplace, non-NYC birthplace, and disability); family characteristics (indicators for head sex, age category, partner present, education level, employment, SNAP receipt, and family health issue, as well as counts of students and non-students in the family); and shelter placement characteristics (indicators for eligibility reason and shelter type). Lag specification adds prior year days absent to Main covariates. Refined specification adds school and shelter fixed effects to Main specification, as well as year-varying school characteristics (enrollment, homeless share, ELL share, disability share, poverty share, and non-NYC share). The instrument for 2SLS is the family shelter ineligibility rate at the time of shelter entry. The unit of observation is a student-year; only school years of shelter entry are included. Standard errors clustered at family group level in parentheses. Number of observations given in braces; corresponding OLS and IV covariate models have equal N's. First-stage F-stats in brackets. * $p < 0.10$, ** $p < 0.05$

Table 7: Primary School (K-8) Robustness Checks

	School Borough Treatment			School District Treatment			Distance Treatment (miles)		
	(1) OLS	(2) Inel. IV	(3) Days IV	(4) OLS	(5) Inel. IV	(6) Days IV	(7) OLS	(8) Inel. IV	(9) Days IV
	Panel A: Outcomes in School Year of Shelter Entry								
Days Absent	-2.39**	-22.67**	-14.99**	-2.63**	-150.57	-65.24*	0.27**	3.25**	1.87**
	(0.29)	(7.15)	(6.10)	(0.43)	(113.47)	(36.42)	(0.03)	(1.21)	(0.83)
	{33,866}	[38.3]	[47.9]	{33,866}	[2.0]	[5.8]	{33,082}	[16.6]	[27.2]
Changed School	-0.180**	-0.007	-0.026	-0.155**	0.014	-0.078	0.021**	-0.005	-0.001
	(0.007)	(0.161)	(0.145)	(0.010)	(1.168)	(0.649)	(0.001)	(0.025)	(0.020)
	{34,429}	[39.3]	[48.3]	{34,429}	[1.7]	[5.5]	{33,564}	[16.6]	[27.0]
Proficient	0.010**	0.121	0.015	0.013*	0.512	0.067	-0.0009**	-0.014	-0.001
	(0.004)	(0.093)	(0.081)	(0.007)	(0.491)	(0.280)	(0.0004)	(0.012)	(0.009)
	{20,235}	[21.1]	[27.9]	{20,235}	[2.8]	[5.6]	{20,075}	[12.8]	[23.2]
Promoted	0.004	0.085	0.055	-0.005	0.571	0.246	-0.0001	-0.012	-0.007
	(0.003)	(0.075)	(0.069)	(0.005)	(0.663)	(0.343)	(0.0004)	(0.012)	(0.010)
	{31,525}	[36.2]	[42.5]	{31,525}	[1.8]	[4.5]	{30,736}	[14.6]	[22.1]
Left DOE	-0.014**	-0.154	-0.190**	-0.002	-1.134	-0.857	0.0014**	0.023	0.024**
	(0.004)	(0.094)	(0.088)	(0.006)	(1.113)	(0.528)	(0.0005)	(0.015)	(0.012)
	{34,429}	[39.3]	[48.3]	{34,429}	[1.7]	[5.5]	{33,564}	[16.6]	[27.0]
	Panel B: Year Post-Shelter-Entry Outcomes								
Days Absent	-0.58*	-10.32	-4.18	-0.45	-65.87	-18.67	0.05	1.51	0.47
	(0.32)	(7.26)	(6.46)	(0.46)	(66.46)	(30.86)	(0.03)	(1.19)	(0.90)
	{31,277}	[35.0]	[42.7]	{31,277}	[1.9]	[4.7]	{30,536}	[14.3]	[22.4]
Changed School	-0.049**	-0.13	0.13	-0.031**	-0.85	0.69	0.0059**	0.017	-0.020
	(0.0073)	(0.17)	(0.16)	(0.011)	(1.31)	(0.81)	(0.00080)	(0.027)	(0.023)
	{31,612}	[35.3]	[42.0]	{31,612}	[1.8]	[4.4]	{30,818}	[13.9]	[21.4]
Proficient	0.0028	0.14	0.15*	0.0097	0.66	0.52	0.000017	-0.018	-0.020*
	(0.0040)	(0.094)	(0.083)	(0.0063)	(0.58)	(0.34)	(0.00040)	(0.014)	(0.011)
	{19,750}	[22.6]	[27.5]	{19,750}	[2.5]	[5.6]	{19,619}	[11.9]	[16.3]
Promoted	0.0039	0.078	-0.024	0.012**	0.48	-0.14	-0.00023	-0.013	0.0019
	(0.0041)	(0.087)	(0.090)	(0.0056)	(0.69)	(0.40)	(0.00042)	(0.012)	(0.011)
	{23,889}	[25.9]	[24.8]	{23,889}	[1.4]	[2.9]	{23,317}	[14.6]	[17.4]
Left DOE	-0.0074*	-0.044	-0.035	0.0012	-0.29	-0.16	0.00058	0.0062	0.0040
	(0.0039)	(0.091)	(0.083)	(0.0058)	(0.66)	(0.40)	(0.00041)	(0.014)	(0.011)
	{31,527}	[36.2]	[42.5]	{31,527}	[1.8]	[4.5]	{30,738}	[14.6]	[22.1]

Each cell reports the treatment coefficient from a regression of the row-delineated outcome controlling for Main covariates. Super-columns give treatment definitions; columns enumerate estimation methods. Inel. IV is 2SLS based on the ineligibility rate instrument. Days IV in 2SLS based on the days to eligibility instrument. Panel A presents year-of-shelter entry, while Panel B considers outcomes in the school year following the shelter entry school year. Standard errors clustered at family group level in parentheses. Number of observations given in braces; corresponding OLS and IV covariate models have equal N's. First-stage F-stats in brackets.* $p < 0.10$, ** $p < 0.05$

Table 8: High School (9–12) Robustness Checks

	School Borough Treatment			School District Treatment			Distance Treatment (miles)		
	(1) OLS	(2) Inel. IV	(3) Days IV	(4) OLS	(5) Inel. IV	(6) Days IV	(7) OLS	(8) Inel. IV	(9) Days IV
	Panel A: Outcomes in School Year of Shelter Entry								
Days Absent	-2.53**	-12.46	-6.92	-2.72*	-32.36	-54.56	0.27**	1.04	0.45
	(0.99)	(22.32)	(22.10)	(1.56)	(67.53)	(242.11)	(0.11)	(2.97)	(3.56)
	{8,608}	[14.0]	[13.9]	{8,608}	[5.2]	[0.4]	{8,454}	[9.3]	[6.3]
Changed School	-0.101**	-0.443*	-0.290	-0.101**	-1.283	-2.997	0.011**	0.057	0.038
	(0.011)	(0.258)	(0.251)	(0.017)	(0.895)	(5.204)	(0.001)	(0.036)	(0.040)
	{8,816}	[14.4]	[13.9]	{8,816}	[5.3]	[0.4]	{8,630}	[9.2]	[6.6]
Regents Taken	0.007	0.761**	0.577**	0.005	2.256*	6.095	-0.000	-0.097**	-0.084*
	(0.011)	(0.315)	(0.285)	(0.018)	(1.239)	(9.797)	(0.001)	(0.046)	(0.050)
	{8,816}	[14.4]	[13.9]	{8,816}	[5.3]	[0.4]	{8,630}	[9.2]	[6.6]
Regents Passed	0.003	0.719**	0.587**	-0.001	2.101*	6.034	0.000	-0.084*	-0.081
	(0.011)	(0.307)	(0.292)	(0.018)	(1.184)	(9.678)	(0.001)	(0.044)	(0.050)
	{8,816}	[14.4]	[13.9]	{8,816}	[5.3]	[0.4]	{8,630}	[9.2]	[6.6]
Promoted	0.007	-0.164	0.103	0.031	-0.658	0.536	-0.001	0.018	-0.013
	(0.012)	(0.277)	(0.242)	(0.019)	(1.080)	(1.497)	(0.001)	(0.029)	(0.031)
	{7,246}	[11.4]	[14.2]	{7,246}	[2.8]	[1.4]	{7,151}	[13.2]	[10.9]
Left DOE	-0.016*	-0.202	-0.179	0.006	-0.623	-2.796	0.001	0.018	0.022
	(0.009)	(0.231)	(0.213)	(0.015)	(0.770)	(7.646)	(0.001)	(0.029)	(0.034)
	{8,152}	[11.4]	[12.6]	{8,152}	[3.8]	[0.2]	{7,977}	[7.8]	[5.5]
	Panel B: Year Post-Shelter-Entry Outcomes								
Days Absent	-0.71	-22.46	-0.82	-1.04	-85.26	-0.37	0.17	2.20	-0.21
	(1.18)	(31.94)	(24.29)	(1.92)	(128.73)	(161.92)	(0.13)	(3.11)	(3.27)
	{6,630}	[7.7]	[13.0]	{6,630}	[1.9]	[1.0]	{6,555}	[9.5]	[8.6]
Changed School	-0.032**	-0.370	-0.026	-0.020	-1.377	-0.205	0.003**	0.035	0.007
	(0.012)	(0.329)	(0.246)	(0.018)	(1.465)	(1.780)	(0.001)	(0.032)	(0.033)
	{6,875}	[8.1]	[12.0]	{6,875}	[2.0]	[0.8]	{6,784}	[9.6]	[7.8]
Regents Taken	0.011	0.246	0.289	-0.010	0.771	1.785	-0.001	-0.013	-0.030
	(0.013)	(0.365)	(0.283)	(0.022)	(1.383)	(2.727)	(0.001)	(0.034)	(0.037)
	{6,723}	[7.5]	[12.1]	{6,723}	[1.9]	[0.8]	{6,637}	[9.7]	[8.6]
Regents Passed	-0.000	0.062	0.006	-0.027	0.097	-0.309	0.002	-0.000	0.004
	(0.013)	(0.357)	(0.280)	(0.022)	(1.276)	(2.005)	(0.002)	(0.034)	(0.036)
	{6,723}	[7.5]	[12.1]	{6,723}	[1.9]	[0.8]	{6,637}	[9.7]	[8.6]
Promoted	0.004	0.487	0.536	0.008	2.057	7.182	-0.002	-0.039	-0.057
	(0.015)	(0.430)	(0.397)	(0.024)	(3.006)	(22.448)	(0.002)	(0.033)	(0.041)
	{4,529}	[5.7]	[7.0]	{4,529}	[0.8]	[0.1]	{4,483}	[11.3]	[7.6]
Left DOE	-0.015	-0.187	-0.344	-0.002	-0.750	-2.656	0.000	0.016	0.046
	(0.011)	(0.298)	(0.244)	(0.019)	(1.208)	(3.973)	(0.001)	(0.035)	(0.040)
	{5,890}	[7.8]	[11.6]	{5,890}	[1.9]	[0.6]	{5,813}	[6.4]	[5.6]

Each cell reports the treatment coefficient from a regression of the row-delineated outcome controlling for Main covariates. Super-columns give treatment definitions; columns enumerate estimation methods. Inel. IV is 2SLS based on the ineligibility rate instrument. Days IV in 2SLS based on the days to eligibility instrument. Panel A presents year-of-shelter entry, while Panel B considers outcomes in the school year following the shelter entry school year. Standard errors clustered at family group level in parentheses. Number of observations given in braces; corresponding OLS and IV covariate models have equal N's. First-stage F-stats in brackets. * $p < 0.10$, ** $p < 0.05$

Table 9: Student Fixed Effects Results, Grades K–12

	Borough Treatment			Distance Treatment		
	(1) Base	(2) Main	(3) Refined	(4) Base	(5) Main	(6) Refined
Days Absent	-2.77**	-2.72**	-3.07**	0.31**	0.29**	0.41**
	(0.77)	(0.78)	(0.86)	(0.080)	(0.081)	(0.098)
	{7,915}	{7,915}	{7,462}	{7,688}	{7,688}	{7,286}
Changed School	-0.154**	-0.151**	-0.152**	0.017**	0.017**	0.016**
	(0.018)	(0.018)	(0.021)	(0.002)	(0.002)	(0.002)
	{8,087}	{8,087}	{7,649}	{7,826}	{7,826}	{7,431}
Proficient	0.014	0.015	0.020	-0.001	-0.002	-0.002
	(0.014)	(0.014)	(0.021)	(0.001)	(0.001)	(0.002)
	{4,627}	{4,627}	{4,090}	{4,567}	{4,567}	{4,068}
Promoted	0.007	0.009	0.023*	-0.000	-0.001	-0.001
	(0.011)	(0.011)	(0.013)	(0.001)	(0.001)	(0.002)
	{7,022}	{7,022}	{6,554}	{6,814}	{6,814}	{6,388}
Left DOE	0.001	0.000	0.015	0.000	0.001	-0.001
	(0.010)	(0.010)	(0.012)	(0.001)	(0.001)	(0.001)
	{7,975}	{7,975}	{7,545}	{7,718}	{7,718}	{7,327}
Base Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Main Covariates	No	Yes	Yes	No	Yes	Yes
Lagged Absences	No	No	No	No	No	No
School Covariates	No	No	Yes	No	No	Yes
School & Shelter FE	No	No	Yes	No	No	Yes

Setup follows to Table 4. Data consists of Main sample pooling grades K–12. Each cell reports the coefficient on in-borough shelter placement from a regression of the row-delineated outcome on the treatment indicator, controlling for the column-enumerated covariates, using the super-column-indicated treatment definition. All regressions include individual student fixed effects. The unit of observation is the student-school-year. Proficient is defined as having passed both ELA and Math State tests for grades 3–8, or having passed any Regents for grades 8–12. See the note for Table 4 and the text for additional detail. Standard errors clustered at family group level in parentheses. Number of observations in braces. * $p < 0.10$, ** $p < 0.05$

Table 10: Primary School (K-8) Event Study Results

	(1) Days Absent	(2) School Changes	(3) Math Proficient	(4) ELA Proficient	(5) Proficient	(6) Promoted	(7) Left DOE
Predicted Outcomes by Year, Main Covariate Specification							
Untreated × Year Pre	22.8	0.28	0.24	0.19	0.12	0.91	1.9e-09
	(0.23)	(0.0061)	(0.0078)	(0.0072)	(0.0061)	(0.0038)	(0.00014)
Treated × Year Pre	22.9	0.27	0.25	0.18	0.13	0.91	-1.6e-09
	(0.22)	(0.0059)	(0.0076)	(0.0069)	(0.0061)	(0.0036)	(0.00012)
Untreated × Year Enter	28.3	0.54	0.17	0.14	0.082	0.92	1.9e-09
	(0.26)	(0.0066)	(0.0061)	(0.0056)	(0.0045)	(0.0036)	(0.00014)
Treated × Year Enter	26.3	0.35	0.18	0.14	0.089	0.92	-1.6e-09
	(0.24)	(0.0061)	(0.0060)	(0.0054)	(0.0045)	(0.0035)	(0.00012)
Untreated × Year Post	25.0	0.38	0.096	0.10	0.044	0.95	0.061
	(0.27)	(0.0066)	(0.0046)	(0.0046)	(0.0032)	(0.0032)	(0.0033)
Treated × Year Post	24.7	0.33	0.097	0.099	0.044	0.95	0.061
	(0.26)	(0.0061)	(0.0045)	(0.0044)	(0.0031)	(0.0030)	(0.0032)
T-Values for Tests Equality of Mean Outcomes							
Year Pre	0.34	1.23	0.87	0.49	1.10	0.09	0.00
Year Enter	5.66	22.05	1.18	0.26	1.17	0.07	0.00
Year Post	0.81	5.26	0.22	0.59	0.04	0.66	0.04

Each column presents predicted outcomes from a regression of column-enumerated dependent variable on Main covariates and in-borough treatment interacted with the school years prior to, during, and following a student's first shelter entry after 2010. The sample is limited students in grades K–8 observed in all three years (pre-, during-, and post-shelter) during the time period encompassing school years 2010–2015. It excludes students in special school districts 75, 79, 84, and 88, as well as those enrolling in DOE subsequent to shelter entry. Standard errors are clustered at the individual student level in parentheses. Predictions assume mean values of all other covariates. T-statistics for t-tests for equality of treated (in-borough) and untreated (out-of-borough) outcomes are given at the bottom of the table.

Table 11: Primary School (K-8) Homelessness Outcomes

	OLS				IV			
	(1) Base	(2) Main	(3) Lag	(4) Refined	(5) Base	(6) Main	(7) Lag	(8) Refined
Length of Stay (School Year)	4.7**	3.9**	4.3**	3.9**	-27.1	-31.0	-28.8	-45.6
	(1.1)	(1.1)	(1.3)	(1.1)	(23.0)	(22.6)	(27.3)	(30.1)
	-	-	-	-	[36.6]	[38.9]	[26.1]	[24.4]
Log Length of Stay (School Year)	0.073**	0.056**	0.055**	0.053**	-0.256	-0.306	-0.282	-0.518
	(0.011)	(0.011)	(0.012)	(0.011)	(0.254)	(0.248)	(0.304)	(0.327)
	-	-	-	-	[36.6]	[38.9]	[26.1]	[24.4]
Length of Stay	19.2**	22.1**	19.0**	22.3**	-56.5	-90.6	-175.2	-78.1
	(6.4)	(6.2)	(7.1)	(6.4)	(136.3)	(132.4)	(165.4)	(171.2)
	-	-	-	-	[36.6]	[38.9]	[26.1]	[24.4]
Log Length of Stay	0.123**	0.104**	0.094**	0.097**	-0.435	-0.536	-0.787	-0.709
	(0.019)	(0.018)	(0.021)	(0.019)	(0.426)	(0.412)	(0.517)	(0.536)
	-	-	-	-	[36.6]	[38.9]	[26.1]	[24.4]
Homeless Year 1 Post-Entry	0.003	0.002	0.002	0.001	-0.299*	-0.307**	-0.190	-0.369*
	(0.007)	(0.007)	(0.008)	(0.007)	(0.154)	(0.152)	(0.180)	(0.209)
	-	-	-	-	[33.3]	[35.0]	[23.8]	[20.4]
Homeless Year 2 Post-Entry	-0.017*	-0.007	-0.008	-0.007	0.018	0.056	0.034	0.060
	(0.009)	(0.009)	(0.010)	(0.010)	(0.204)	(0.212)	(0.257)	(0.308)
	-	-	-	-	[29.2]	[27.7]	[18.9]	[13.6]
Obs.	34,429	34,409	26,640	34,323	34,405	34,386	26,623	34,299
Base Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Main Covariates	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Lagged Absences	No	No	Yes	No	No	No	Yes	No
School Covariates	No	No	No	Yes	No	No	No	Yes
School & Shelter FE	No	No	No	Yes	No	No	No	Yes

Setup is identical to Table 4, except outcomes assess student length of stay in shelter. Treatment is defined as shelter placement within one's school borough of origin. Each cell reports the coefficient on in-borough shelter placement from a regression of the row-delineated outcome on the treatment indicator, controlling for the column-enumerated covariates, using the super-column-indicated method. The unit of observation is the student-school-year. The sample is limited to shelter entry years for students in grades K–8 during school years 2010–2015. It excludes students in special school districts 75, 79, 84, and 88, as well as those enrolling in DOE subsequent to shelter entry. Observation counts are given for days absent regressions. Standard errors clustered at family group level in parentheses. First-stage F-stats in brackets. See the note for Table 4 and the text for additional detail. $p < 0.10$, ** $p < 0.05$.

Table 12: Primary School (K-8): Mediating Effects Remaining in Shelter on Post-Shelter-Entry Year Outcomes

	(1) Days Absent	(2) School Changes	(3) Math Proficient	(4) ELA Proficient	(5) Proficient	(6) Promoted	(7) Left DOE
			Panel A: OLS				
Treatment	0.86	0.0072	0.019*	0.0074	0.0050	-0.010	0.0036
	(0.57)	(0.014)	(0.011)	(0.010)	(0.0080)	(0.0076)	(0.0068)
Still Homeless	3.46**	0.042**	0.0012	-0.0072	-0.0024	-0.012*	0.014**
	(0.47)	(0.012)	(0.0083)	(0.0084)	(0.0063)	(0.0062)	(0.0057)
Treatment × Still Homeless	-1.89**	-0.073**	-0.015	-0.0057	-0.0029	0.018**	-0.014*
	(0.64)	(0.015)	(0.012)	(0.012)	(0.0087)	(0.0085)	(0.0078)
			Panel B: IV				
Still Homeless	5.49	-0.12	0.28	-0.071	0.10	0.031	-0.10
	(14.3)	(0.34)	(0.31)	(0.22)	(0.19)	(0.047)	(0.16)
Treatment	-4.91	-0.33	0.69	-0.061	0.30	0.12	-0.21
	(22.9)	(0.55)	(0.52)	(0.35)	(0.31)	(0.10)	(0.25)
Treatment × Still Homeless	-5.76	0.25	-0.55	0.12	-0.21	-0.063	0.21
	(27.7)	(0.65)	(0.59)	(0.43)	(0.36)	(0.087)	(0.31)
	[1.7]	[1.9]	[1.8]	[1.8]	[1.8]	[12.4]	[1.9]
Obs.	31,277	31,612	19,750	19,750	19,750	23,889	31,527
Base Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Main Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Absences	No	No	No	No	No	No	No
School Covariates	No	No	No	No	No	No	No
School & Shelter FE	No	No	No	No	No	No	No

Each column gives results for a separate regression of the column-indicated outcome in the year following shelter entry on an indicator for in-borough placement interacted with an indicator for remaining in shelter in the year following shelter entry, controlling for Main covariates. The unit of observation is the student-school-year. The sample is the Main K–8 sample. Standard errors clustered at family group level in parentheses. First-stage F-stats in brackets. * $p < 0.10$, ** $p < 0.05$

Table 13: Primary School (K-8): Mediating Effects of School Changes

	(1) Days Absent	(2) Math Proficient	(3) ELA Proficient	(4) Proficient	(5) Promoted	(6) Left DOE
			Panel A: OLS			
Treatment	-2.10**	0.0066	0.0024	0.0068	0.0018	-0.014**
	(0.38)	(0.0077)	(0.0073)	(0.0059)	(0.0042)	(0.0053)
School Change	3.96**	-0.048**	-0.034**	-0.026**	-0.026**	0.023**
	(0.40)	(0.0076)	(0.0072)	(0.0057)	(0.0046)	(0.0059)
Treatment × School Change	1.04**	-0.0086	-0.0015	-0.0039	-0.0059	0.010
	(0.52)	(0.010)	(0.0098)	(0.0077)	(0.0065)	(0.0078)
			Panel B: IV			
School Change	-5.38	0.32	0.10	0.23	-0.13	-0.075
	(17.7)	(0.54)	(0.44)	(0.39)	(0.20)	(0.23)
Treatment	-29.2	0.52	0.22	0.36	-0.040	-0.24
	(19.8)	(0.61)	(0.49)	(0.44)	(0.22)	(0.26)
Treatment × School Change	12.0	-0.66	-0.23	-0.46	0.22	0.15
	(32.4)	(1.01)	(0.82)	(0.72)	(0.36)	(0.43)
	[2.8]	[0.8]	[0.8]	[0.8]	[2.2]	[2.8]
Obs.	33,866	20,235	20,235	20,235	31,525	34,429
Base Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Main Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Absences	No	No	No	No	No	No
School Covariates	No	No	No	No	No	No
School & Shelter FE	No	No	No	No	No	No

Each column gives results for a separate regression of the column-indicated outcome on an indicator for in-borough placement interacted with an indicator for school changes, controlling for Main covariates. The unit of observation is the student-school-year. The sample is the Main K–8 sample. Standard errors clustered at family group level in parentheses. First-stage F-stats in brackets. * $p < 0.10$, ** $p < 0.05$

9 Figures

Figure 1: Instrument and Treatment Quarterly Time Series: Detrended

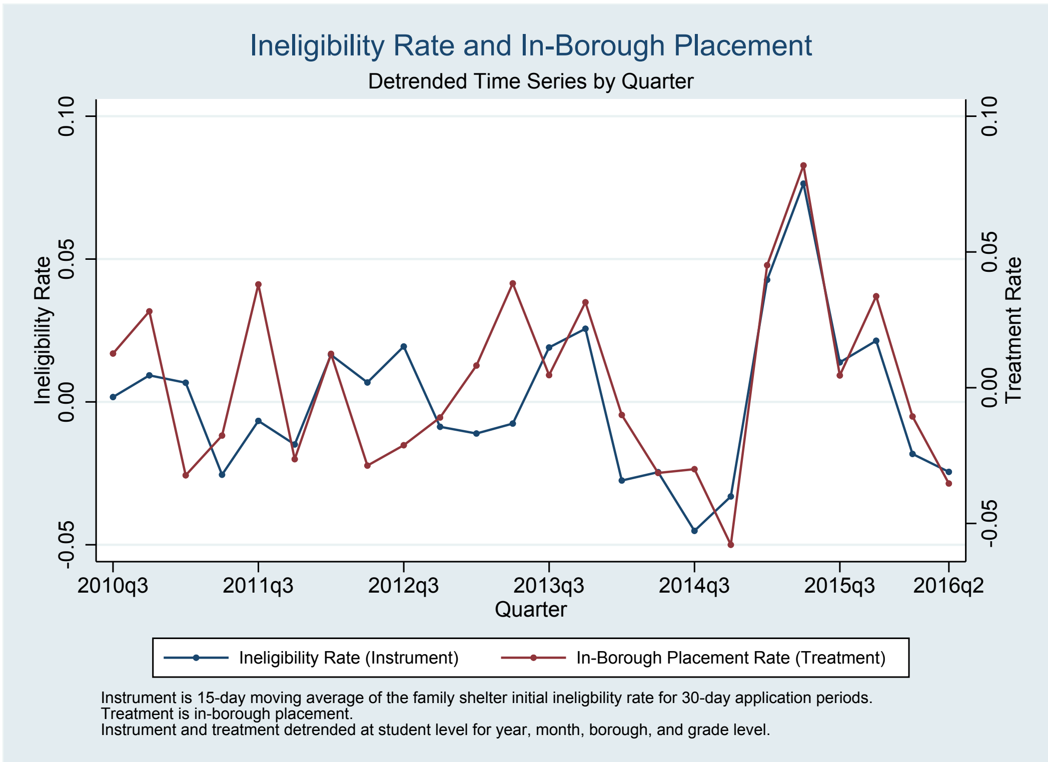


Figure 2: Family Shelter Ineligibility Rate

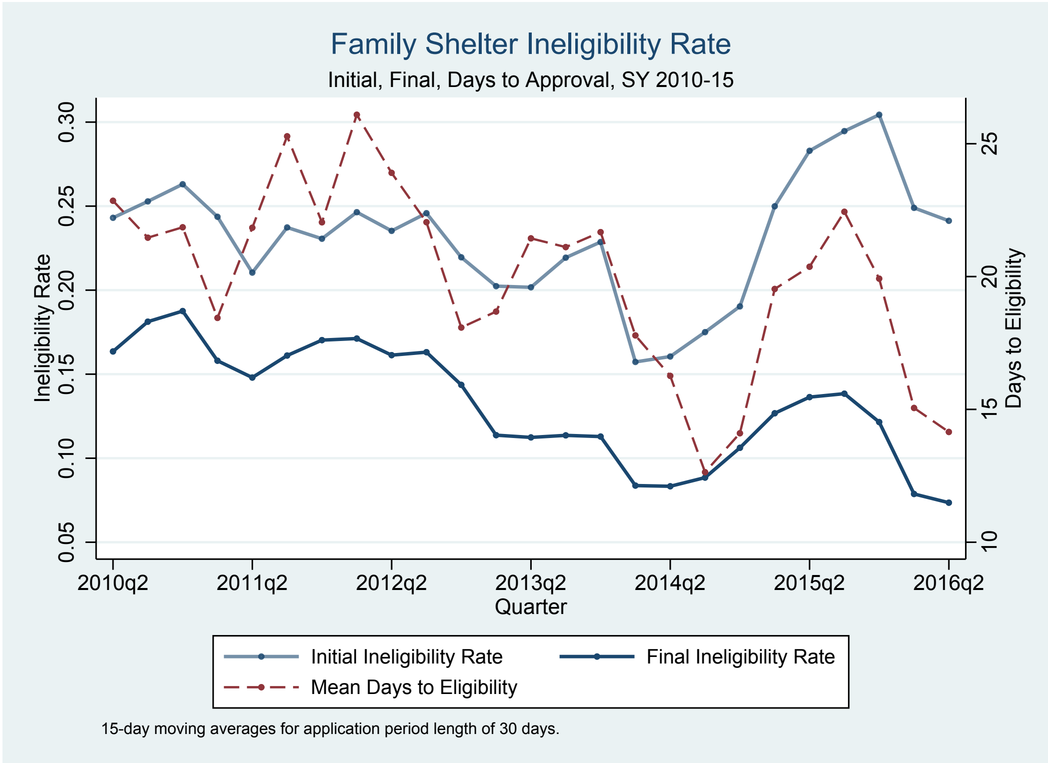
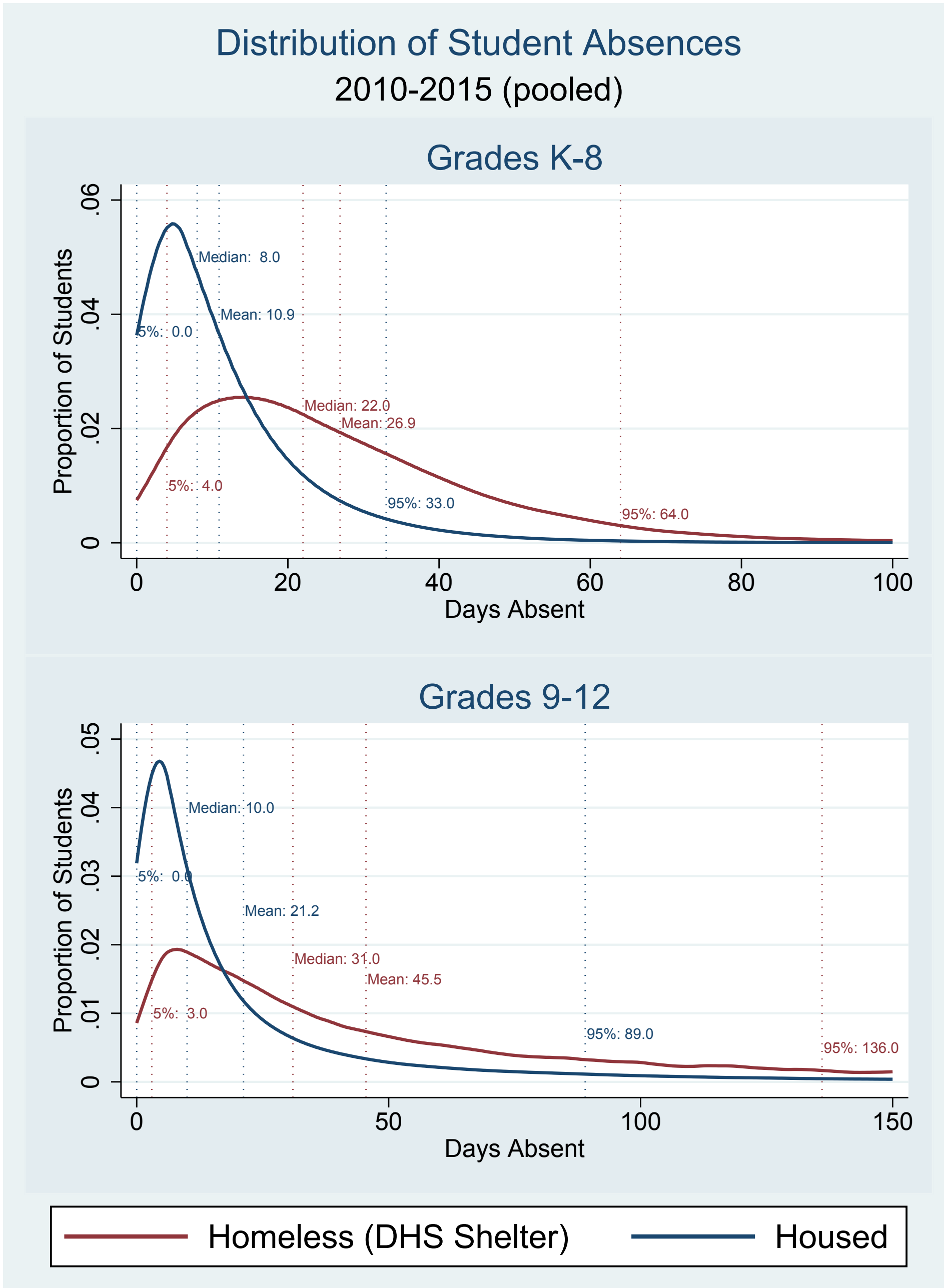
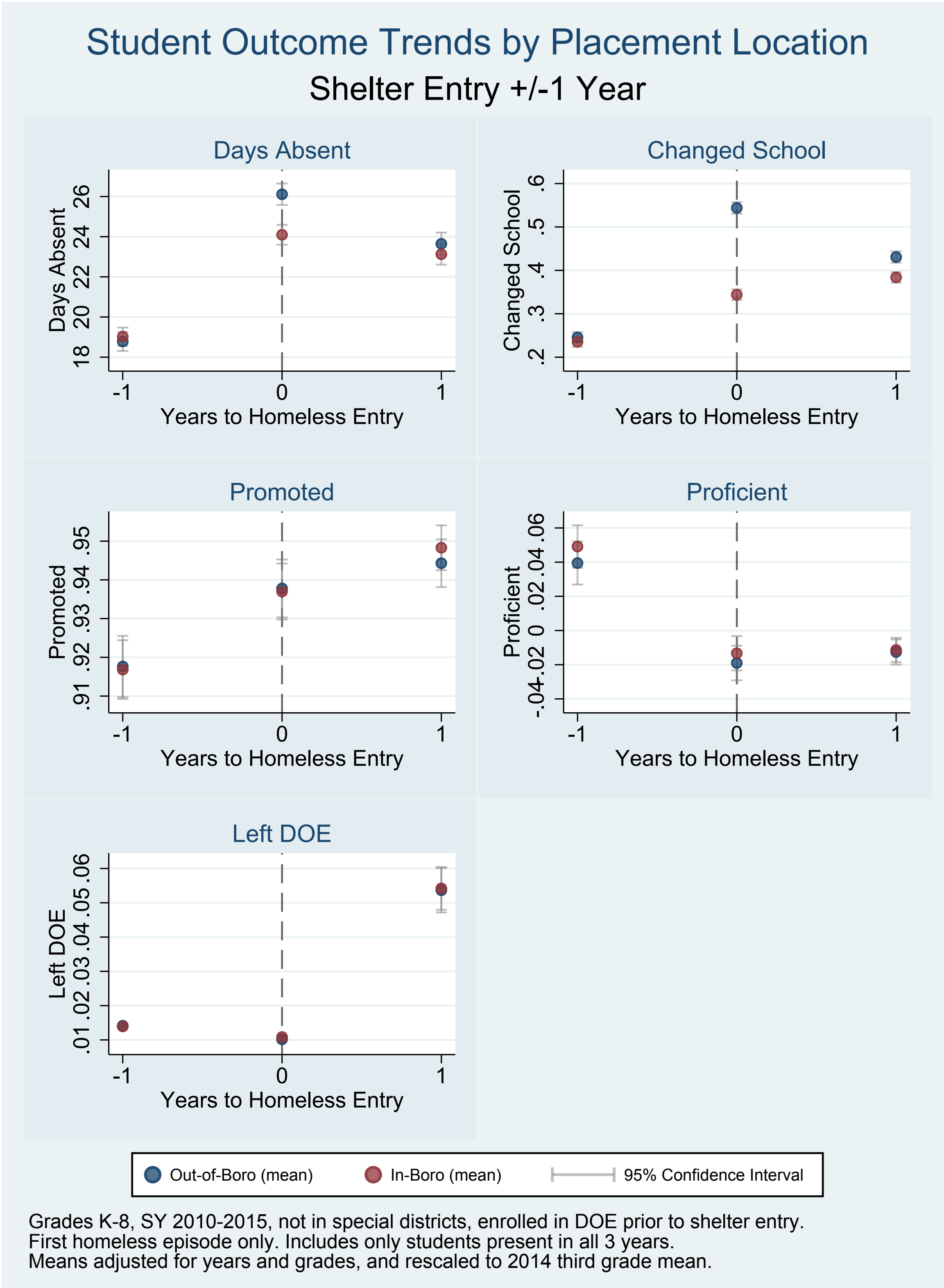


Figure 3: Distribution of Public Student Absences



Notes: Kernel density plots of days absent using a bandwidth of 3 days. Sample pools school years 2010–2015. Excludes special school districts 75, 79, 84, and 88. Homeless defined as in DHS shelter (and having entered in 2010 or later); housed defined as all other students. Plots truncated at 100 and 150 days, respectively.

Figure 4: Three-Year Student Outcome Trends by Placement



Notes: Grades K-8, SY 2010-2015, not in special districts, enrolled in DOE prior to shelter entry. First homeless episode only. Includes only students observed in all 3 years. Means adjusted for years and grades, and rescaled to 2014 third grade mean.