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IZA DP No. 13558

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ISSN: 2365-9793

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ABSTRACT

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

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 Classification: I21, I28, I38, H53, H75, D91

Keywords: homelessness, education, K-12, neighborhoods, families, housing, poverty alleviation, welfare policy, program evaluation, causal inference

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* This research has been made possible through an abundance of kindness and support. Jennifer Hunt has been an extraordinary mentor and close reader. Roger Klein, Carolyn Moehling, Brendan O'Flaherty, and Rosanne Altshuler have offered many useful comments. The Center for Innovation through Data Intelligence (CIDI), a research unit within the Office of the Mayor of the City of New York, has graciously furnished data access and policy expertise. Particular thanks goes to Maryanne Schretzman, Eileen Johns, Andy Martens, and Jacob Berman. Generous funding has been provided by the National Academy of Education and the National Academy of Education/Spencer Dissertation Fellowship Program. Hilary Sigman, Douglas Blair, Jacob Bastian, Ira Gang, Anne Piehl, Eugene White, Norman Swanson, BingxiaoWu, Robert Collinson, Emily Oster, Philip Oreopoulos, Ingrid Gould Ellen, Howard Bloom, Richard Murnane, and Jeremy Fiel, as well as participants in Rutgers University's Applied Microeconomics Seminar, have also volunteered helpful feedback.

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

[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.

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 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-

[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.

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 education 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.

[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 (2020).

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 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.

[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).

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], macroeconomic 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,

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

[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).

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 (2020), 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 opportunity 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

[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).

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 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

[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.

[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 (2020).

[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.

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 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

[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).

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 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

[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 (2020).

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 (2020), 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") 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

[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.

(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 F.2 and G.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

[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.

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.

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

[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.

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 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

[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 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.

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 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 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

[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.

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 (2020). 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 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

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

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].

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

[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*).

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. Nevertheless, as a robustness check, I also use average days to eligibility as an alternative instrument[49].

[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.

[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.

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. About a tenth 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.

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

[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.

their educational outcomes. Most notably, homeless students struggle in school. They are chronically absent, acutely non-proficient, and unstably schooled[51].

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,

[51] Appendix Table A.4 summarizes how key treatment and outcome measures vary by year of shelter 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 F.2 and G.1. Additional tables exhaustively describing homeless students are shown in Appendix F.1.

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-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; Tables A.20A and A.20B include 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

[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.

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, 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

[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.

[56] See Cassidy (2020) for a longer discussion of this point.

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.

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 Tables A.20A and A.20B, 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

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

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.

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

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

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 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,

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

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 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

[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.

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, 3.8k students (8.7 percent) 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].

[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

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].

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,

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

the analysis in this section focuses on K–8 students.

One important causal channel is length of stay in shelter (LOS). In Cassidy (2020), 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 $t+1$), 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

[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.

[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.

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 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 (2020), 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

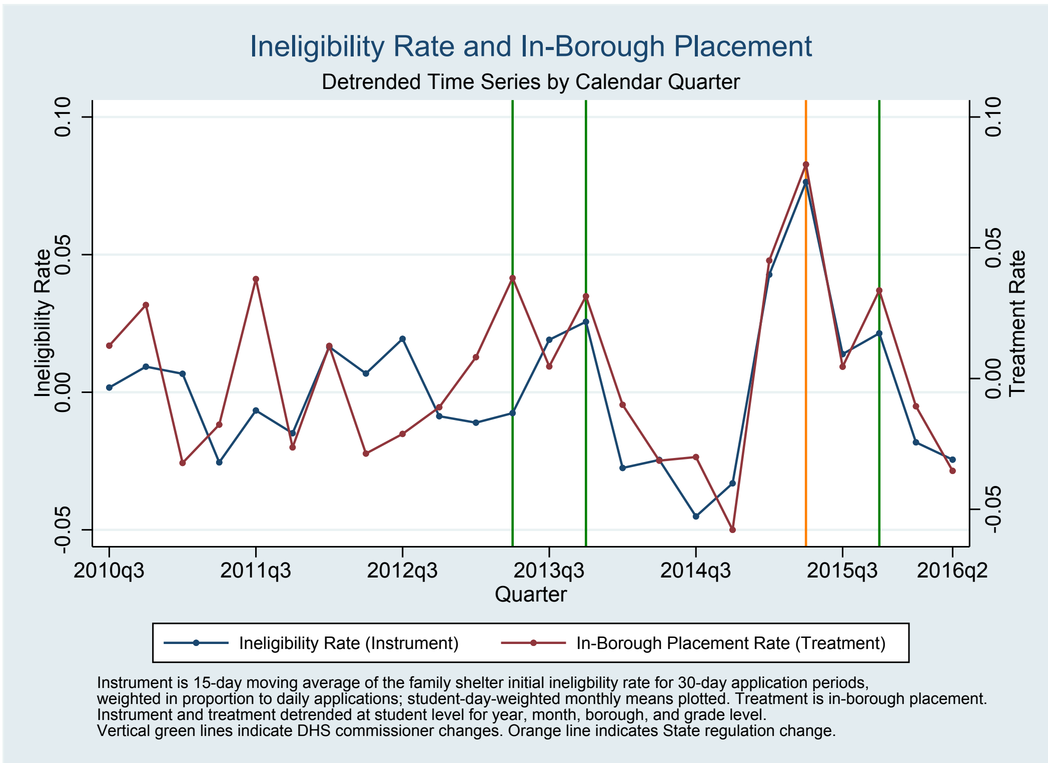


Figure 2: Family Shelter Ineligibility Rate

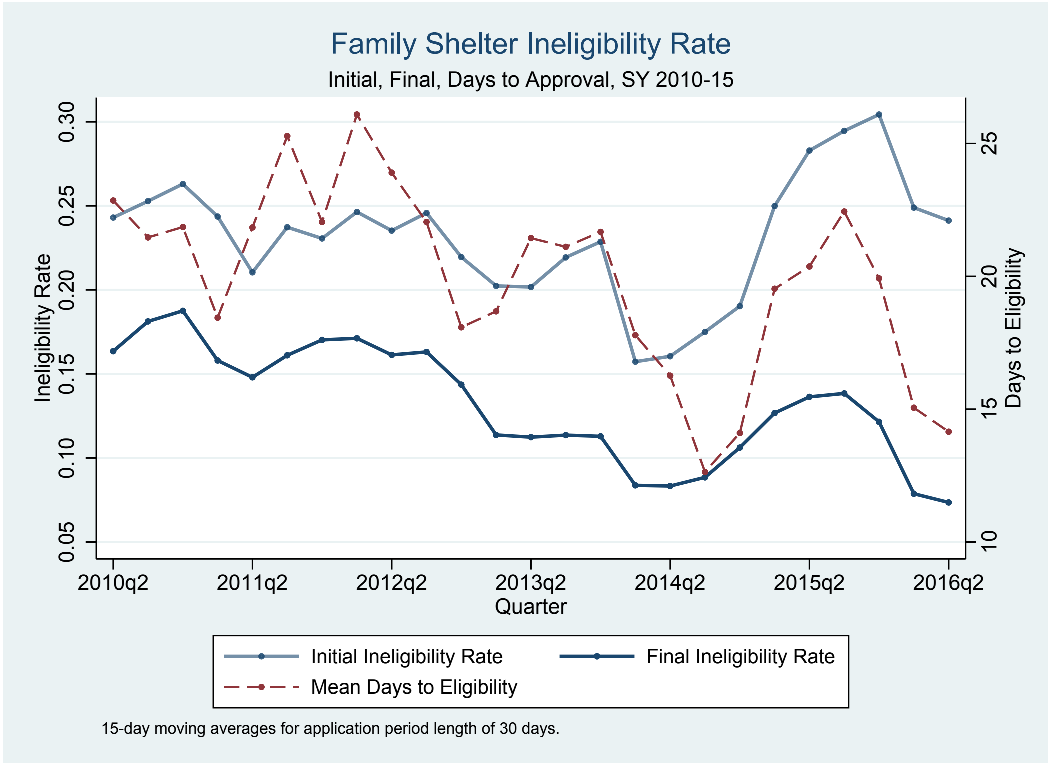
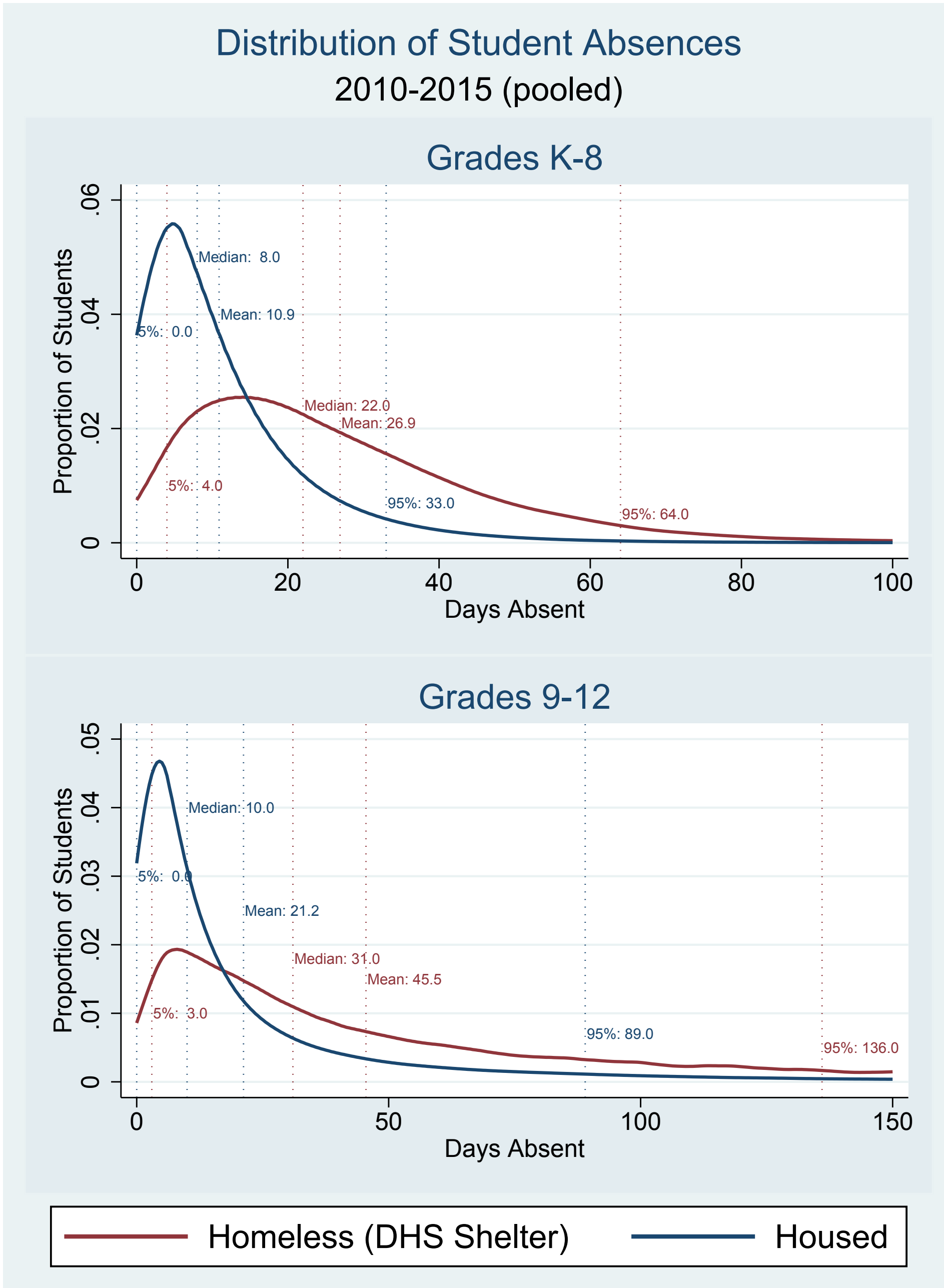
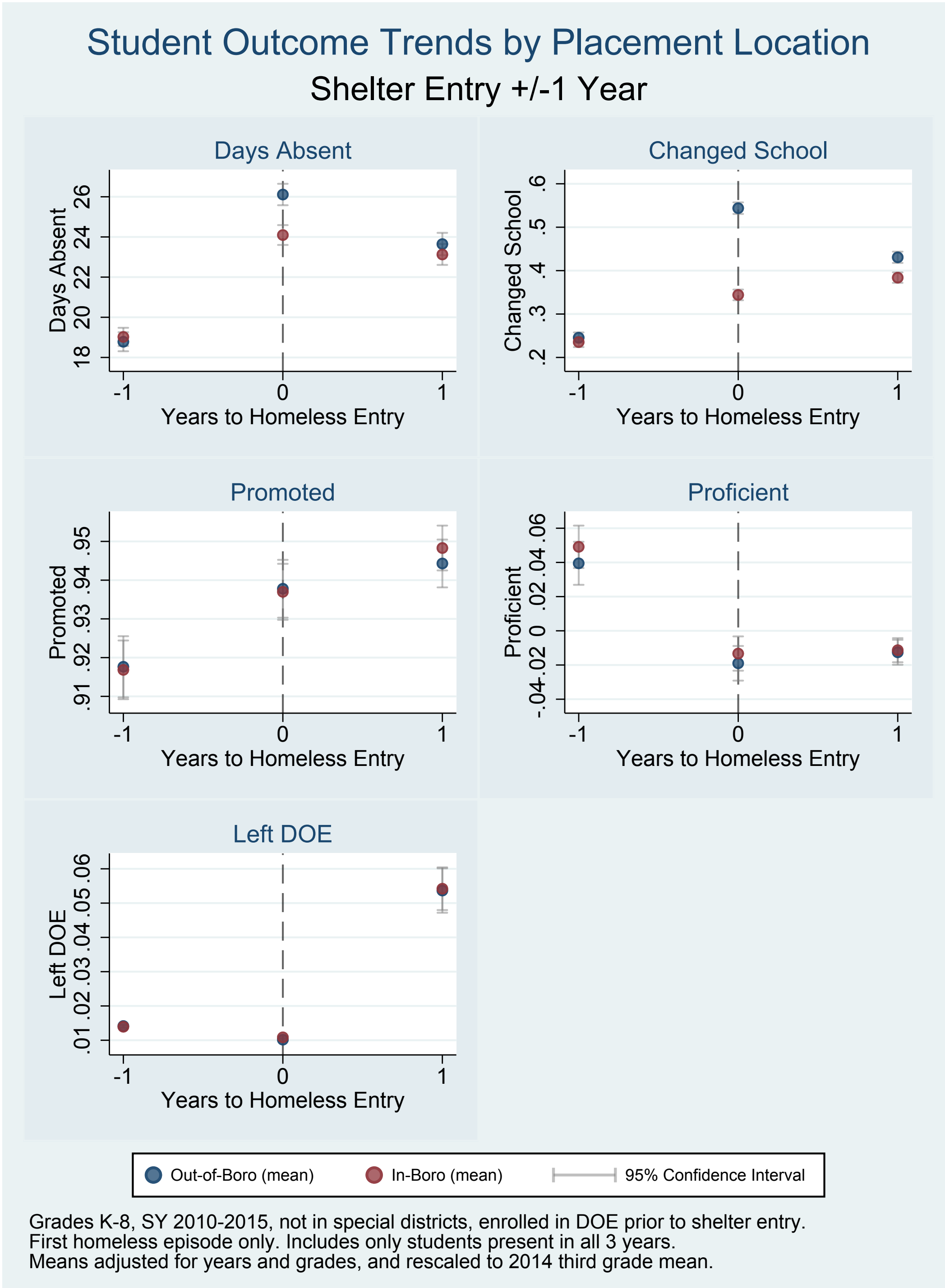


Figure 3: Distribution of Public Student Absences



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.

Supplemental Appendices to "A Closer Look: Proximity Boosts Homeless Student Performance in New York City"

A Policy, Literature, and Data Appendix

This section contains an expanded discussion of Section 3 in the main text. Portions are repeated for convenience.

A.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.

Although family homelessness remains curiously unpopular as a topic of economic inquiry, a handful of economists and many more social scientists have, since the 1980s, developed a strong body of research explaining its antecedents and attributes. Family homelessness is the product of individual circumstances and structural conditions (Byrne et al., 2013; O'Flaherty, 2010, 2004; Gould and Williams, 2010; Tobin and Murphy, 2013). Typically consisting of a high-school-educated, urban-dwelling, racial minority single mom with several young children living in doubled-up or 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 (Culhane et al., 2007; Fertig and Reingold, 2008; Grant et al., 2013; Tobin and Murphy, 2013; Shinn et al., 1998). Health crisis. Job loss. Domestic dispute. These are the sorts of unpredictable shocks—vagaries better-resourced families habitually withstand—that transform merely poor families into unhoused ones (Curtis et al., 2013; O'Flaherty, 2010, 2004; New York City Independent Budget Office, 2014). Predicting who among poor families will become homeless is notoriously difficult (Greer et al., 2016; Shinn et al., 1998).

To slightly oversimplify, family homelessness proceeds from a fundamental asymmetry in the household balance sheets of the poor: rents are rigid, but incomes are not. When incomes in question are also low, saving is difficult; when relatives and friends are similarly situated, borrowing is limited. As a consequence, poor families must weather life's whims effectively uninsured. When things go wrong, (housing) consumption, far from being smoothed, stops (Curtis et al., 2013; O'Flaherty, 2010; Fertig and Reingold, 2008). Most recover quickly enough, and are sheltered for brief periods and never to return. Even those who experience extended stays or repeat episodes tend to stabilize within a year or two (Culhane et al., 2007; O'Flaherty, 2010). Family homelessness is a phase, not an trait.

As it happens, the transience of family homelessness make defining it a matter of some debate. Until recently, HUD and ED did not use the same definition, a situation that was partially remedied by HEARTH Act of 2009, under which HUD adopted the more expansive ED definition (Tobin and Murphy, 2013; Homeless Emergency Assistance and Rapid Transition to Housing Act of 2009 , HEARTH Act; Perl, 2017). Under this standard, homelessness is defined as lacking "a fixed, regular, and adequate night-time residence," which encompasses living temporarily doubled up with others and residing in places not intended for permanent habitation (e.g., cars or hotels), as well as more the obvious forms of street and sheltered homelessness (McKinney-Vento Homeless Assistance Act, 2015; United States Interagency Council on Homelessness, 2018). NYC DOE's own "students in temporary housing" (STH) definition—the measure most commonly used in the agency's homeless reporting—is also based on this broader concept (New York City Department of Education, 2019).

In this paper, I adopt the stricter standard and define homeless families as those explicitly residing in DHS shelter system. I do this for several reasons. Most prosaically, the policy I study is shelter-based. But shelter is also the most natural definition for family homelessness in NYC, where the legal right to shelter means there are virtually no unsheltered families. It is also a more rigorous standard. Families in shelter have had their lack of housing verified by DHS staff, which adds precision (specific time periods are tracked) and reliability (DOE's STH indicator is self-reported and unevenly collected). That's not to suggest doubled-up or transient families don't face housing difficulties, only that those qualifying for shelter—the most acutely disadvantaged—are of special interest.

The hazards of poverty-induced residential instability are particularly pronounced in New York City. This is not because New York is bad at managing homelessness, but, in fact, quite the opposite. A constellation of forces—a hospitable legal environment and a notoriously competitive real estate market, in tandem with a tradition of progressive politics, an enviable fiscal affluence, and a vast administrative infrastructure—have made New York not only the most common, but also very probably the most comfortable, place to be homeless in the U.S (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).

In 2018, according to point-in-time estimates from the U.S. Department of Housing and

Urban Development, 45,285 people in families with children were homeless in New York City—a quarter of the 180,413 total for the U.S. as a whole. What's more, all of NYC's homeless families were sheltered, which represents fully 80 percent of the America's family shelter population (The U.S. Department of Housing and Urban Development, 2018).

And while family homelessness has declined nationwide by a third since 2009, NYC's census is on the rise. Between March 2009 and March 2019, the city's population of homeless families grew from 8,081 to 12,427, a 54 percent increase, though down somewhat from its November 2018 peak of 13,164 (New York City Department of Homeless Services, 2019*a*). In the fiscal year ending 1999, the family census was just 4,802, meaning the city's homeless family population has grown 250 percent in two decades (New York City Mayor's Office of Operations, 2003).

A large part of the explanation is a simple legal reality: NYC is one of just two jurisdictions in the U.S.—the state of Massachusetts is the other—where families have a legal right to shelter (New York City Independent Budget Office, 2014; University of Michigan Law School, 2017). The product of a series of lawsuits initiated in the 1980s, NYC is under constitutional and court mandate to provide housing to any family who can demonstrate a genuine deficit of it[1]. This, together with staggering income inequality, soaring rents, and fierce competition for scant affordable housing—all of which are complemented by an exceptionally mature municipal social service apparatus—make the sustained growth of the NYC's family homeless population none too remarkable (O'Flaherty and Wu, 2006; The City of New York, Mayor's Office, 2017; NYU Furman Center, 2016; Grant et al., 2013). With deep reserves of near-homeless families from which to draw, macroeconomic contractions and political winds—such as the State's decision in 2011 to abruptly withdraw funding for a popular rental assistance program—there is a near-constant threat of a stubborn homeless census becoming explosive (The City of New York, Mayor's Office, 2017; Ellen and O'Flaherty, 2010). However, carefully crafted policies, including prevention services, housing subsidies, rent regulations, zoning laws, and affordable housing construction have been successful at speeding shelter exits and precluding some entries entirely (Ellen and O'Flaherty, 2010; Evans, Sullivan and Wallskog, 2016; O'Flaherty, 2010).

Families presenting themselves as homeless must apply for shelter at DHS' Prevention Assistance and Temporary Housing (PATH) intake center in the Bronx[2] To qualify, families must submit to an eligibility determination process that has been in place, in some form, since 1996. At minimum, families must have at least one member under 21 or pregnant and

[1]For details, see Cassidy (2020).

[2]PATH opened in 2011. Prior to 2006, families applied at Emergency Assistance Units (EAUs) located in all boroughs but Staten Island. Between 2006 and 2011, families applied at an interim PATH in the Bronx.

demonstrate that they have no suitable place to live[3].

At intake, families are first screened for domestic violence and, if affirmative, are referred to HRA's No Violence Again (NoVA) unit, which operates a separate shelter system for the most serious cases. Next, families are screened for prevention services, including rent arrears payments, out-of-city relocation assistance, anti-eviction legal services, and housing subsidies.

Families unable to be diverted are interviewed by DHS case workers about their prior living situations. They must provide documentation demonstrating their identities, family relationships, and housing histories. They are then granted conditional shelter stays for up to 10 days while dedicated investigation staff assess their claims, which may involve conversations with landlords and visits to prior addresses. 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 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 proportionately vast. Administered by the Department of Homeless Services under the auspices of the Department of Social Services[4], 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. Fully 82 percent of DHS' budget ($1.06 billion in FY19) is allocated to some 282 contracts for homeless family services (New York City Office of Management and Budget, 2018).

About three-fifths of families reside in one of the City's 169 traditional "Tier II" homeless shelters, which offer on-site social services and security but otherwise resemble the sorts apartment buildings typically found in low-income communities; indeed, landlords often convert private market buildings to shelters to cater to these more lucrative tenants[5]. The

[3]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), as well as conversations with City officials.

[4]DHS was originally a part of DSS/HRA, but was spun off as an independent agency in 1993. In 2016, the two agencies were again consolidated under a single commissioner, but it remains conventional to refer to the departments as distinct.

[5]Facility data presented in this paragraph is from The City of New York, Mayor's Office (2017) and is as of November 2016.

next most common form of temporary housing, comprising 276 sites and about a quarter of the population, are cluster, or scatter, units, so named because they are localized groups of shelter apartments spread throughout otherwise private buildings in a given area and serviced by a single provider. The remaining 13 percent of families are placed in commercial hotels, which offer fewer services but a flexible way for the city to expand capacity to meet needs.

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 day* in shelter was $192 (New York City Office of Management and Budget, 2019; New York City Mayor's Office of Operations, 2018). And this and understatement, as it excludes administrative costs, prevention programs, and permanent housing subsidies, as well as services and benefits administered by other agencies.

The educational associations of homelessness are equally distressing. Descriptively—though, as I discuss, perhaps not causally—homeless students are chronically absent, change schools often, struggle to achieve proficiency, and are at increased risk of behavioral problems. These correlations—more rigorously assessed in the academic literature—are readily apparent in DOE's descriptive data, which are regularly parsed and publicized by policy analysts and advocates. Emblematic is a 2016 report by NYC's Independent Budget Office (IBO), which found two-thirds of sheltered students missed at least 10 percent of the school year, compared with a third of doubled-up students and a quarter of those permanently housed (New York City Independent Budget Office, 2016). Similarly, according to Institute for Children, Poverty & Homelessness (2017), 53.5 percent of homeless students in NYC missed at least 20 days of school in 2015–2016. They also change schools at four to six times the rate of housed students, as also documented by The Research Alliance for New York City Schools (2019); just 15.5 percent of third to eighth graders were proficient in English, and 11.7 percent proficient in Math (Institute for Children, Poverty & Homelessness, 2017). The City's official data bears this bleak portrait: in 2018, the average attendance rate for homeless students was 82.3 percent (New York City Mayor's Office of Operations, 2018).

To help address the challenges homeless students face, the City has maintained the explicit goal of placing homeless families in shelters near their youngest child's school since at least 1998 (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). 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[6]. But increasingly it 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, which are rarely situated in the neighborhoods where homelessness originates (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 suitably far from their abusers), family size (e.g., larger families legally require more bedrooms), and health limitations (e.g., multi-level walk-ups are not suitable for mobility-impaired families). when assigning shelter placements. According to City officials, conditional upon these other criteria, which families end up with preferential placements near their children's schools depends 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.

For more background on family homelessness in NYC and the City's neighborhood based shelter placement policy, see Cassidy (2020).

A.2 Previous Literature

In the main text, I highlight the works most relevant to my research. Here, I provide a more detailed discussion.

Economists notwithstanding, education has, since the 1980s, become a focal point among

[6]Originally passed in 1987 and amended several times since, most recently in the Every Student Succeeds Act of 2015, the McKinney-Vento Homeless Assistance Act governs U.S. policy concerning the education of homeless students. The 1990 amendment first established the right to remain in one's school of origin; by the same token, local education districts are required to allow homeless students to change schools to their local school once in shelter if it is in the student's best interest (Every Student Succeeds Act, 2015; McKinney-Vento Homeless Assistance Act, 1987, 2015; Panhandle Area Educational Consortium, 2019; National Center for Homeless Education, 2017; Stewart B. McKinney Homeless Assistance Amendments Act of 1990, 1990).

homelessness scholars, an often cross-disciplinary collaborative spanning the social policy, housing policy, psychology, and education domains. Three recent reviews—Buckner (2008); Miller (2011); Samuels, Shinn and Buckner (2010)—ably summarize this body of work. While there is no question homeless students struggle in school—in terms of attendance, mobility, performance, behavior, and retention—the literature has become increasingly preoccupied by the question of whether they are worse off than similarly low-income, but housed, peers[7]. In other words, is homelessness *causally* disadvantageous in the educational context?

While the first generation of studies tended to answer affirmatively (Buckner, 2008; Rubin et al., 1996), with some notable exceptions (Buckner, Bassuk and Weinreb, 2001), more rigorous recent work has generally found the gap between homeless and otherwise-poor students to be smaller and transitory (Samuels, Shinn and Buckner, 2010; Buckner, 2012; Rafferty, Shinn and Weitzman, 2004). In spite of sometimes mixed findings, there is an emerging consensus that "homeless and highly mobile" students lie downstream on a "continuum of risk," faring worse, on average, than other poor students, but not qualitatively so[8]. However, there is considerable variation, with some homeless students exhibiting "resilience" and succeeding despite their hardships (Masten, 2012; Masten et al., 2014). Beyond education, homelessness is associated with myriad adverse outcomes for children (Grant et al., 2013; Tobin and Murphy, 2013). Nevertheless, the debate is not settled, and, what's more, much of the evidence to-date fails to satisfy economists' conventional standards for asserting causality, relying on small (sometimes convenience) samples[9] or econometrically suspect methods[10].

Although economists have not been apt to study homeless students, my work informs two related literatures in economics. The first is neighborhood effects, and in particular, the burgeoning subset of studies concerned with how geography and environment promote—or preclude— social and economic opportunity, mobility, and overall well-being among disadvantaged children and their families.

It is well-known that children who grow up in poor neighborhoods fare systemically worse than those raised in affluence (Currie, 2009; Currie and Rossin-Slater, 2015). But residence is not random. Disentangling its ramifications from family unobservables, on the one hand, or structural disparities, on the other, has proven challenging (Manski, 1993; Topa, Zenou et al., 2015; Fryer Jr and Katz, 2013). To sidestep these confounders, most of the best studies have relied upon lotteries for oversubscribed housing subsidies—the most prominent being the

[7]Miller (2011); Buckner (2008); Zima, Wells and Freeman (1994); Fantuzzo et al. (2013); Rouse, Fantuzzo and LeBoeuf (2011).

[8]Cutuli et al. (2013); Herbers et al. (2012); Brumley et al. (2015); Obradović et al. (2009); Miller (2011).

[9]Buckner, Bassuk and Weinreb (2001); Rafferty, Shinn and Weitzman (2004); Rubin et al. (1996); Zima, Wells and Freeman (1994).

[10]Cutuli et al. (2013); Fantuzzo et al. (2012); Herbers et al. (2012).

Moving to Opportunity (MTO) experiment—comparing outcomes among families assisted into more auspicious surroundings with those remaining relegated to concentrated poverty (Katz, Kling and Liebman, 2001; Kling, Liebman and Katz, 2007; Ludwig et al., 2013, 2012, 2008; Sanbonmatsu et al., 2006, 2011; Galiani, Murphy and Pantano, 2015). Others have exploited quasi-experimental variation in local housing conditions—such as public housing demolitions—to make similarly credible inferences (Chyn, 2018; Jacob, 2004; Jacob, Kapustin and Ludwig, 2015; Jacob and Ludwig, 2012; Oreopoulos, 2003).

By and large, the results remain mixed, if not (normatively) disappointing. There is little evidence of contemporaneous educational gains among the children of publicly-subsidized movers (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). Indeed, despite notable neighborhood upgrades and diminished poverty, few studies find meaningful differences of any type between movers and non-movers, despite assessing a wide range of social and economic outcomes across diverse populations and time frames (Sanbonmatsu et al., 2011; Katz, Kling and Liebman, 2001; Kling, Liebman and Katz, 2007; Oreopoulos, 2003). Indeed, vouchers are found to reduce labor supply (Mills et al., 2006; Jacob and Ludwig, 2012).

One exception is health. Both adults and children who move to better neighborhoods experience improvements in physical and mental health, as well as subjective well-being (Kling, Liebman and Katz, 2007; Ludwig et al., 2013, 2012, 2008; Sanbonmatsu et al., 2011). In addition, it may be the case that neighborhood effects take time to percolate. Promising recent work finds low-income children whose families avail themselves of mobility subsidies experience longer-term gains in educational attainment, reduced incarceration, employment, and earnings, especially when they move at younger ages (Andersson et al., 2016; Chetty and Hendren, 2018, 2016; Chetty, Hendren and Katz, 2016; Chyn, 2018).

These results are in keeping with the much broader literature on the enduring legacies of early life experiences. Even seemingly small differences in childhood—and in utero—health, nutrition, cognitive enrichment, and social cultivation can have lasting impacts on many facets of adult well-being (Cunha and Heckman, 2007, 2009; Almond and Currie, 2011). Exposure to excess pollution, toxic stress, sickness, inadequate nutrition, or chronic instability can undermine children's opportunities and perpetuate inequality (Currie, 2009; Case, Fertig and Paxson, 2005; Campbell et al., 2014; Currie, 2011; Currie and Rossin-Slater, 2015; Almond, Currie and Duque, 2018), while access to well-designed safety net programs, including income supports, nutrition assistance, child care, parenting resources, and quality early childhood education programs can be remarkably effective at enhancing mobility (Ludwig and Miller, 2007; Kline and Walters, 2016; Heckman, Pinto and Savelyev, 2013; Campbell et al., 2014; Dahl and Lochner, 2012; Hoynes, Schanzenbach and Almond, 2016).

In other words, early life experiences profoundly shape children's futures, but neighborhoods—the very environments in which they grow up—seem to matter less than might be expected, especially on the short-term educational inputs to long-term achievement. The literature on education and economic well-being—the second area to which my work contributes—clarifies this paradox[11].

One reason neighborhoods matter surprisingly little is that peers and schools matter quite a lot. Exposure to propitious peers—particularly those whose academic abilities resonate with one's own—encourage long-term gains, while disruptive or incompatible ones impede progress (Carrell, Hoekstra and Kuka, 2018; Lavy and Schlosser, 2011; Sacerdote, 2011). Access to better quality schools has similarly salubrious consequences (Fryer Jr and Katz, 2013; Altonji and Mansfield, 2018). Often, these effects are not acute, but cumulative, showing up in educational attainment and earnings rather than in short-term metrics like test scores. While residential communities shape social and schooling opportunities, it is these more micro habitats that regulate educational results.

Powerful as they are, however, peers and schools pale in comparison to what is, by a wide margin, the dominant influence on human capital formation: family. Sibling comparisons demonstrate as much as half of educational attainment is attributable to family forces (Björklund and Salvanes, 2011). Once parental preferences, resources, and constraints are accounted for, there is relatively little variation left to explain (Solon, Page and Duncan, 2000).

Knowing that families and schools matter for educational attainment and economic success among disadvantaged students begs the question of what can be done to move the needle in an outcome-augmenting direction. Unfortunately, the evidence on this question is less decisive. Well-regarded research has identified teacher quality (Chetty et al., 2011; Araujo et al., 2016), class size (Dynarski, Hyman and Schanzenbach, 2013), family income (Akee et al., 2010), and school funding (Lafortune, Rothstein and Schanzenbach, 2018; Hyman, 2017; Jackson, Johnson and Persico, 2015) as particularly important inputs into the human capital production function. However, given the diversity of school and family settings, there is no silver bullet: heterogeneity predominates (Hanushek, 2002, 1979).

The evidence on mobility is even more nuanced. Changing schools tends to impede performance of movers and incumbents alike (Hanushek, Kain and Rivkin, 2004), especially in the short-run and when moves are intra-district; on the flip side, there is some evidence that

[11]Broadly, this literature concerns itself with the role of education with regard to social and economic mobility, inequality, health, and overall well-being.

benefits accrue if the moves are permanent or permit access to qualitatively better schools. In particular, it is important to distinguish between school and residential moves: while the former is almost always found to be negatively associated with educational achievement (Schwartz, Stiefel and Cordes, 2017; Ashby, 2010), some residential moves, particularly those which maintain school stability while upgrading housing, can be beneficial (Cordes, Schwartz and Stiefel, 2017). Of note, Cordes, Schwartz and Stiefel (2017) and Schwartz, Stiefel and Cordes (2017) study student mobility specifically in NYC and provide evidence suggesting that policies than enhance school stability, like school-targeted shelter placements, should be helpful for most students.

A.3 Data and Sample

A.3.1 DHS Data

One major contribution of this paper, along with its companion piece, Cassidy (2020), is the construction of an original dataset, comprehensively describing contemporary family homelessness in New York City. Given NYC's outsized importance in the realm of family homelessness, along with the extensive detail of linked longitudinal administrative data, this represents perhaps the richest portrait of family homelessness in the U.S. to date. In this section, I summarize key data management steps, with an emphasis on DOE data; for greater detail about the DHS data, see Cassidy (2020).

My data comes from two foundational sources: DHS and DOE. The DHS portion constitutes my core sample: all eligible families with children entering shelter from January 1, 2010 to December 31, 2016. These records, which contain details on families' compositions, demographics, and conditions of shelter entry, as well as basic identifying information, are extracted from DHS' Client Assistance and Rehousing Enterprise System (CARES), which is the City's management information system for homeless services. Note that this sample is essentially a census, excluding only those (rare) individuals with missing data on critical identifying variables.

CARES contains individual level records for each family member. In Cassidy (2020), I rework this data so that the unit of observation becomes the family-spell. That is, there is one observation per family per shelter stay, with new spells defined as those occurring more than 30 days subsequent to the end of a previous stay[12]. This is the natural level of analysis for assessing outcomes applicable to the family as a whole (which is the focus in Cassidy (2020)); 30-day gaps are considered as discrete encounters with the homeless services system.

The DHS data contains rich information about families and their shelter stays, most of

[12]DHS considers returns to shelter within 30 days of leaving to be part of the same spell.

which comes from the Temporary Housing Assistance (THA) applications families fill out to apply for shelter. Variables include basic identifying information (name, date of birth), family relationships, the presence of health issues, official shelter eligibility reason, and housing history (most recent address). Shelter stay attributes, including facility type, address, and dates of stay, come from Lodge History extracts, another CARES subcomponent. A third CARES facilities query is used to extract information about shelter locations and characteristics.

The data is collected primarily for management rather than analysis, and so requires extensive processing to be econometrically coherent. As is often the case with administrative data, neither variables nor observations are analytically appropriate "off-the-shelf." Key data management steps including defining and discretizing shelter episodes (including length of stay calculations), geocoding addresses, and defining analytical variables, including those derived from existing fields (e.g., creating a summary categorical variable for main eligibility reasons) and those assembled across observations (e.g., a count of family members). These steps are detailed in Cassidy (2020).

I augment this core DHS data by linking it to administrative records maintained by other agencies. I obtain information on public benefit use—Cash Assistance (CA) (i.e., public assistance or "welfare," consisting of federal Temporary Assistance for Needy Families (TANF) and NYS Safety Net Assistance (SNA)) and the Supplemental Nutrition Assistance Program (i.e., SNAP or "Food Stamps")—from HRA, using probabilistic matching techniques based on Social Security Number (SSN), first name, last name, and date of birth[13]. The HRA data also includes information on race and self-reported education. In a similar fashion, the New York State Department of Labor (DOL) provides data on quarterly employment and earnings, through a deterministic match on SSN[14].

To ease computational burden, which is not insubstantial in fuzzy big data matches, my public benefit and labor matches are restricted to head of family. Because (a) most homeless families consist of a single adult and several children and (b) heads of case are most likely to appear in the benefit and labor data, this restriction should not meaningfully change the results relative to an exhaustive match of all family members.

For purposes of assessing family outcomes, as in Cassidy (2020), the natural unit of observation is the family-episode. In the present study, the underlying individual level records come to the fore. From the CY2010-2016 CARES census, I cull the records of all individuals aged 4 to 21 during any point in their shelter stays. I choose these cutpoints because

[13]For brevity,I refer to Cash Assistance as CA and Food Stamps as SNAP.

[14]For simplicity, I refer to the HRA and DOL under the umbrella of "DHS" since the linkage is performed with the DHS data.

they represent the minimum (children can begin pre-K at age 4) and maximum (children can attend school through the school year in which they turn 21) ages individuals can be enrolled in DOE[15]. In total, there are 89,337 unique such children.

Using CARES' individual and family identifiers, I then relink these individuals to the family-shelter episodes of which they are a part. In this manner, the unit of observation becomes the individual-homeless-episode. Several comments are in order regarding the definition of DHS-derivative analytical variables. All covariates are defined at the time of shelter entry (or as near as is possible). Person-specific variables, such as age, are, as would be expected, defined at the individual level. Correspondingly, 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 level of education, which are defined by head of household but 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 (though, due to the fuzzy nature of the match, there may be some false negatives).

I take the extra step of creating an "unknown" education category for families that do not match HRA in order to include education as a covariate without restricting the sample; because families missing education data are those not receiving public benefits, it is reasonable to assume they are either have higher educational attainment or are immigrants. For a similar reason—avoiding unduly excluding families from the sample—I also create an "unknown" category for homeless eligibility reason, which is a DHS CARES variable missing for a handful of families.

In sum, the DHS data consists of unique observations for each school-age child during each homeless episode experienced by their families, complete with all covariates, both individual and family-level, associated with each episode.

A.3.2 DOE Data

I then match these candidate homeless students to a database of school records maintained by DOE, spanning school years 2005-06 to 2016-17 (my second foundational data source). DOE's database contains records for each student during each school year, with separate topical tables for June biographical information (demographics, student characteristics, and enrollment details, including school ID and attendance), test scores (3–8 grade state standardized tests and Regents for high schoolers), and graduation (for high schoolers). The biographical table is given the "June" designation because it is reconciled at the end of each school year, in June, and reflects each student's most up-to-date information as of then.

[15] 21-years-of-age is also the DHS definition of child.

For data size reasons—each table includes all public school students, not only homeless ones—there is a separate topical table for each school year.

In addition to the topical tables, there is also a separate Transactions table detailing all admissions and discharges (including normative promotions as well as non-normative school changes) over all school years in the sample. Of note, the topical tables are reconciled in June of each school year, providing the end-of-year status of each student; the Transactions table, by contrast, records the precise date and reason for each school change. Each student has a unique ID, which permits linking fields across topics and years. In practice, a fair amount of data processing must take place to shape the records into a form suitable for analysis. Key tasks include harmonizing variables across years (as available fields and definitions change over time) and linking a student's records across topics (each topical table entails distinct processing steps) and over time.[16]

Key DOE variables used in the analysis are described in Section 4. As with the DHS data, some of these variables are not native to the administrative data, but rather are constructed from the underlying fields. For example, my promotion indicator is constructed by comparing students' grade levels in year n to that in $n+1$; students for whom $grade_{n+1} > grade_n$ are defined as promoted. The data management tasks involved in translating administrative records to an econometrically suitable data structure is not inconsiderable. Stata code exhaustively detailing this process is available upon request. In addition, as might be expected, more variables are available than are used; alternative specifications and robustness checks are available upon request.

Of particular note, schools are identified by unique "DBNs," comprised of school district (D), school borough (B), and school number (N) codes. In this sense, school borough, which central to the analysis, is derivative of DBNs. To measure school-shelter distances, I link these DBNs to publicly available school geocode files, which, in addition to school names and address, contain X-Y coordinates[17].

The final DOE data step is to aggregate the disparate tables into a single observation for each student in each school year.

A.3.3 Data Match

The matching procedure to link DHS' candidate homeless students with DOE records, performed with the assistance of CIDI and DOE staff, follows standard City protocols for linking human service and education data. I use The Link King version 9.0 (Campbell, 2018), a SAS

[16] Stata code detailing all data management tasks is available from the author upon request.

[17] Note that at the time of this writing, I lack geographic data on a subset of schools that had closed at the time the school geocode data was published.

application, with default settings and match records based on first name, last name, date of birth, and sex. The Link King uses a variety of sophisticated algorithms to deterministically and probablistically match records across datasets. For details, see Kevin Campbell (2018). I accept match certainty levels 1 (highest possible) to 6 (low-moderate) as true matches, while level 7 (probabilistic maybe), along with unmatched records, are defined as non-matches. Close cases, including those with several match candidates, are reviewed manually. Once the match is complete, data is deidentified by stripping names and official identifiers and replacing them with scrambled student ID.

Given 12 years of education records and 7 years of homeless data, my match is over-inclusive. There are three types of matched students: (1) children who are in school during their shelter stays, (2) adult family members (typically heads of household) who attended DOE schools at some time in the recent past, (3) children too young to be in school during their time in shelter but who enrolled in DOE subsequently. Because I am interested in the contemporaneous and short-term effects of shelter policy, my interest is in the first group.

Even restricting the match sample to age-relevant individuals, the panel nature of the data guarantees a number of irrelevant matches. A non-trivial share of household heads age 18–21 (group 2 above) are, in fact, heads of household who previously completed their DOE careers (given that DOE records extend back to 2005-06). Thus, I trim the match sample by eliminating all matches involving heads of household. Note that, by design, this also excludes all in-school heads of household, on grounds that my primary interest is in outcomes among minor students; adult students with dependents can reasonably be expected to be subject to different, potentially confounding, dynamics. In a similar way, I drop all matches where a homeless child in question is too young to be in school during a homeless episode (group 3 above); these children match due to enrollment in DOE during a subsequent post-shelter year. (For example, a child may be in shelter from 2011–13, when she is age 1-3, and then enroll in DOE in 2015 at age 5. Such a student is not relevant for my analysis.)

Table A.1 details my match results by birth year, focusing specifically on children aged 5–18 during a shelter stay. Overall, 64,728 of 74,058 unique candidate students (87 percent), accounting for 78,465 of 88,582 student-homeless-episodes present in the DHS data (89 percent), have successful DOE matches[18]. For students in the "core" birth (calendar) years of 1995–2008, the match rate is 90 percent or greater; these children are in the prime schooling years during the 2010–16 period that comprises my homelessness window. As expected,

[18] In terms of my full match universe of students age 4–21 while in shelter, the match rate is, as expected, somewhat less. As shown in Table A.2, 82 percent of unique students aged 4–21 (corresponding with 84 percent of student-episodes) are matched. This understates the true match rate, however, again due to over-inclusivity. Four- and five-year-olds are not required to be in school; at the other end of the spectrum, many 19–21-year-olds have completed their academic careers, due either to graduation or dropout.

match rates are lower for older and younger children[19].

A.3.4 Analytical Sample

Matched records in hand, I construct an (unbalanced) panel consisting of all available school years (2005–2016) for all matched students from my homeless student cohort (i.e., those whose families entered homeless shelter during calendar years 2010 to 2016). The unit of observation is the student-school-year. As shown in Table A.3, there are 479,914 observations (Col 1) across 73,518 unique students (Col 4).

Students are observed for 1–12 school years. The average student is observed 6.5 times. Note that the counts in column (1) are nested, while in column (4) they are mutually exclusive. The way to read the table is as follows. There are 73,518 year-one observations for students; while 1,657 students are observed only once. Similarly, there are 43,541 year-sixes; 8,884 students are observed exactly 6 times. 5,373 students are observed the maximum 12 years. Most common are students with 4–7 observations, with in excess of 8,000 students in each of these categories.

However, I do not use the full set of data for my main analysis, for reasons which I'll now describe. In brief, the objective is to trim extraneous noise from the data to sharpen the policy analysis. These sample refinements are summarized in Table 1.

As a preliminary step, I exclude Pre-K students, whose school enrollment and attendance is voluntary. My first major sample restriction is to limit the sample to school years 2010-2015. I choose this period because these are the only years in which I have complete education and homelessness data. (My DHS data also covers the second half of the 2009 school year and the first half of the 2016 school year.) This reduces the number of observations to 262,446.

Next, I restrict the sample to students who are enrolled in DOE prior to the date of shelter entry. This is meant to eliminate spurious treatments where school mechanically corresponds to shelter borough, because the latter precedes the former. Although proximity effects can still operate in this context, my interest is in specifically isolating the effect of being placed in shelter near one's "home" borough, with school location a proxy for place-based affinity. This is the effect the policy is intended to produce. By and large, the shelter-precedes-school population consists of shelter entrants from outside NYC, whose circumstances might be quite different from city residents. The population of migratory homeless is not trivial, accounting for about 10 percent family shelter entrants[20]. This reduces my student-school-years to 247,498.

[19]There are several legitimate reasons a school-age homeless child may not show up in DOE records, including moves into and out of NYC contemporaneous with homeless episodes and enrollment in parochial or private school. I assume that any matching false-negatives are at random.

[20]The right to shelter applies regardless of whether prior residence was in NYC.

Finally, I exclude students who begin or end the school year with "special" school district designations: 75 (students with disabilities), 79 (alternative schools), 84 (charter schools), and 88 (missing data). This leaves me with 216,177 observations.

These remaining 216,177 student-school-year observations are a mix of school years prior to, during, and post shelter episodes. Episodes may begin at any time during the school year. Some episodes span multiple school years. Some students have multiple episodes. These irregularly-initiated, unevenly-lengthy, potentially-reoccurring episodes make treatment itself heterogeneous: students do not experience homelessness in a uniform manner. Controlling for shelter outcomes, like length of stay or episodes within a given period, could make matters worse, as outcomes are endogenous[21].

Consequently, to create a consistent treatment concept, I restrict my sample to the school year of shelter entry for my main analysis. This restriction is also desirable from the standpoint of isolating treatment effects: one would expect the impact of temporary shelter placement would be largest contemporaneous to when it occurs. The information lost by treating a panel as a pooled cross-section (students can appear multiple times if they have multiple episodes) is more than compensated by having a coherent treatment concept, comparable across students, at least conditional on month and year of shelter entry. 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. However, I also consider outcomes in the year following the school year of shelter entry to broaden the scope of the analysis.

The upshot of this considerable data processing effort is an unprecedented chronicle of student homelessness, detailing students' educational histories in the context of their families' homelessness experiences, as well as their characteristics, composition, labor market experiences, and public benefit use. I describe the key variables implicated in my analysis in Section 4.

A.3.5 Complete Sample

Beyond my core dataset of homeless students, I also create a second broader sample includes all students in all available school years. I refer to this as the "Complete" sample. As shown in Table 1, it spans school years 2010–2015 and contains 6,798,801 student-school-year observations, of which 2 percent (121,496 observations) coincide with spells of student homelessness.

The purpose of the Complete sample is to compare homeless students with their housed peers, which provides a frame for interpreting results. Because my homelessness data spans

[21] See Angrist and Pischke (2008).

CY2010–2016 shelter entries, students who entered shelter prior to CY2010, and remained in shelter in subsequent school years, are not identified as homeless. This will cause some degree of attenuation bias in housed-homeless contrasts, particularly in the early years of my data. However, because most family shelter stays are less than a year-and-a-half, comparisons from 2011 on should be mostly unaffected.

I also use the Complete sample to construct school-level covariates for my main analysis. Appendix F.2 provides additional statistics describing this sample.

A.3.6 Additional Data

As described in Section 4, several data elements—most prominently, my treatment and instrumental variables—are, in part, constructed from auxiliary administrative records, encompassing facility geography (school and shelter addresses) and shelter applications (my core query consists only of *eligible* families).

My instruments data set consists of a query of all family with children homeless shelter applications from calendar year 2009 though 2016. Fields include family ID, individual ID, case number, application date, application outcome, and detailed eligibility and ineligibility reason. I collapse the raw data to the family-case level, and then define discrete application periods, which begin with initial application and end either with eligibility or a gap of more than 30 days before a reapplication (in the case of prior rejection), whichever comes first. Note that unlike my core DHS sample, this data includes all families who apply for shelter, not only those eventually deemed eligible. Further details about my instruments are provided in Section 4.

A.3.7 School Borough of Origin

While the DHS data contains exact dates of shelter stay, DOE's preferred source of school enrollment, the June Biographical data, reports only students' end-of-year status. Thus, using this data will erroneously mark students who change schools during the year in response to shelter placements as treated.

To address this concern, I turn to the DOE Transactions data and employ the following algorithm to identify each student's original school borough for each school year. If a student's first school year is present in the data, they are assigned the school borough of their first-ever DOE admission from the transactions data for this school year. Students who entered DOE prior to 2005 are assigned their June 2005-06 school borough. Next, students with "normative" school changes—that is, scheduled promotion into middle school (usually grade 6) or high school (usually grade 9)—are assigned the school of first transaction for that school year. For all remaining school years (those which are neither a student's first in DOE nor entail normative changes), students are assigned the school borough of the prior June (on the assumption that the school in which a student ended the prior school year is the school in which, homelessness aside, they should begin the next one). If prior year school is missing, they are assigned the school of first admission in the current school year; if transactions records are also missing, they are assigned the end-of-year June school. By assigning each student the earliest possible school with which they are associated in each school year, the risk of mechanical treatment is minimized.

A second issue is that, while the school-shelter nexus is the most policy relevant treatment definition—the explicit goal, after all, is to keep children in their "home" schools—it is not the only sensible way to define treatment. For each student, there are three relevant locations: home (pre-shelter residence), school, and shelter. Even among non-homeless students, home and school borough many differ. Any of the three pairwise links identifies a coherent treatment concept, as does requiring all three to coincide.

I choose to focus of the school-shelter link for two reasons. First, as proxies for genuine "home" boroughs, school identities are likely to be more stable and less error prone than address of prior residence, as the latter is both self-reported and more transient, given frequent moves among families at-risk of homelessness. Second, my interest in this paper is on the effect of shelter proximity on educational outcomes, so the relevant distance is that between shelter and school, regardless of prior residence[22]. In practice, there is substantial overlap between the treatment concepts[23].

B Theory Appendix

A useful way to think about homeless family responses to school-based shelter placements is as a generalized consumer optimization problem. Homeless families value their children's educations, but they care about other things, too. Given resource scarcity, a family will choose the quantity and quality of children's schooling[24] that maximizes family utility, taking into account its preferences, endowments, and (opportunity) costs. Optimal schooling

[22]By contrast, in Cassidy (2020), I use the home-shelter treatment concept. As with the present study, the choice is guided by the outcomes under consideration. For whole family outcomes, residential geography holds greater import than the location of children's schools. Further, as a practical matter, DOE confidentiality standards restrict my ability to observe children's schools in my whole-family dataset.

[23]As shown in Tables A.22 and A.23, the correlation between school-shelter treatment and home-shelter treatment is 0.67 for primary schoolers and 0.58 for high schoolers. But because the home-shelter treatment standard includes students who attend school out-of-borough, one would expect the effects of proximity to be diminished. Overall treatment group sizes and shares are shown in Table A.21.

[24]For tractability, one can think of schooling consumption as some combination of attendance and performance.

consumption balances the rewards of education with satisfactions derived from competing uses of a family's time and effort, such as work and leisure. Since most homeless spells are relatively brief, this is a static, one-period model.

Family i has preferences[25] over schooling S_i and all other (time) consumption C_i. This latter composite good is to be construed broadly as including not only goods and services, but also other uses of time, such as housing search, working (or seeking work), seeing friends, and recreation; as the numeraire, its price is normalized to one. For simplicity, assume all families have identical preferences. Consumption bundles are valued through a standard concave, twice continuously differentiable utility function $U_i(S_i, C_i)$, increasing in both arguments.

The City's neighborhood shelter assignment policy enters the problem in two places: it affects the price of school and it affects family resources. The (relative) price of schooling, $P(d)$, is a function of the distance d between school and shelter[26]. The central tension in the model is that the sign of the distance derivative[27], P_d, is unknown. If $P_d > 0$, the relative price of school—i.e., its opportunity cost—increases with distance (and therefore decreases with in-borough placement). Causes of price increases include longer, more complicated, commutes and school changes (which impose transaction costs). If $P_d < 0$, distance reduces schooling costs, perhaps through neighborhood unfamiliarity making other forms of consumption less attractive. It is not a priori obvious which case will hold: with local placement, school becomes more accessible, but so too are the consumption patterns that gave rise to homelessness in the first place.

The second policy effect is on resources, which is also where heterogeneity enters the model, $R_i(d, e_i)$. Resources are a function of school-shelter distance and a family's endowment of distance-independent assets (e.g., earnings, savings, public benefits, human capital stock, a car), e_i, which may take the form of fewer constraints (e.g., smaller family or no health limitations) and varies among families. I make the important, but plausible, assumption that $R_{di} < 0$. Due to social supports and preexisting neighborhood-specific human capital, familial resources are greater when placed in neighborhoods of origin. However, as indicated by the i subscripts, the magnitude of this response varies based on a family's non-distance endowment, e_i. Specifically, I assume $R_{di}(e_i)$ is decreasing in endowments. Intuitively, distance matters less for families with more resources or fewer constraints. This seems uncontroversial. (To simplify notation in what follows, I will drop the i subscripts.)

[25] Assume a unitary decision maker for all educational decisions for all students in a family. Typically, this will be the family head, or negotiated through intra-familial bargaining. The parental authority assumption may break down for high schoolers, which is a main reason why I treat high schoolers separately in my results.

[26] I present the model in terms of continuous distance; the translation to the binary borough-based treatment definition is obvious and requires replacing derivatives with corresponding discrete differences.

[27] With the exception of i indexing individual families, subscripts in this section indicate partial derivatives.

The family's consumption problem is written:

$$\max_{S,C\geq 0} \; U(S,C) \quad \text{subject to} \quad P(d)S + C \leq R(d,e)$$

The Lagrangian for this problem is:

$$\mathscr{L} = U(S,C) - \lambda(P(d)S + C - R(d,e))$$

with first-order conditions[28]

$$\frac{\partial \mathscr{L}(\cdot)}{\partial S} = 0 \implies U_S = \lambda P(d)$$
$$\frac{\partial \mathscr{L}(\cdot)}{\partial C} = 0 \implies U_C = \lambda$$

where subscripts denote partial derivatives. Dividing the FOC's, I arrive at the function implicitly characterizing the family's optimal consumption bundle (C^*, S^*):

$$\frac{U_C^*}{U_S^*} = \frac{1}{p(d)} \implies U_S(S^*, R(d) - P(d)S^*) - P(d)U_C(S^*, R(d) - P(d)S^*) = 0 \tag{F}$$

where the stars emphasize this equation holds that the optimum[29] and $C^* = R(d) - P(d)S^*$. As usual, marginal benefits are proportional to marginal costs. When the price of school is relatively cheaper, or the returns are relatively higher, families will consume more of it.

My main interest is in the impact of proximity on schooling consumption (where more consumption is taken to be equivalent to better educational outcomes). This is the policy effect, $\tau_i = \frac{\partial S^*}{\partial d}$. Characterizing this effect is a standard comparative statics exercise. Applying the implicit function theorem to Equation F,

$$\tau_i = \frac{\partial S^*}{\partial d} = \frac{\frac{\partial F}{\partial d}}{-\frac{\partial F}{\partial S^*}} = \frac{\overbrace{\overset{(-)}{(R_d} - \overset{(?)}{P_d S^*})(\overset{(+)}{U_{SC}} - \overset{(+)}{P}\,\overset{(-)}{U_{CC}})}^{(1)} - \overbrace{\overset{(?)}{P_d}\overset{(+)}{U_C}}^{(2)}}{-(\underset{(-)}{U_{SS}} - 2\,\underset{(+)}{P}\,\underset{(+)}{U_{SC}} + \underset{(+)}{P^2}\underset{(-)}{U_{CC}})} = \frac{?}{+}$$

Assuming complementarity, $U_{SC} = U_{CS} > 0$, the denominator of this expression is positive. There are three cases for the numerator.

[28] To satisfy complementary slackness, I make the standard assumption that the budget constraint binds with equality. No resources are wasted.

[29] I assume an interior solution. While it is possible for families to choose zero or perfect attendance, their are legal constraints on the lower bound for education, and, in addition, schooling can be construed broadly to have a quality component, such that all perfect attendances are not equal—some impart greater learning.

1. If $P_d > 0$, the numerator is negative and $\frac{\partial S^*}{\partial d} < 0$. In words, the school-shelter distance increases the cost of school and decreases family resources, leading to a decline in schooling consumption.

2. If $P_d < 0$, the (opportunity) cost of school decreases with distance. There are three possibilities.

   (a) Numerator term (2) is positive. If $R_d - P_d S^* > 0$, i.e., $\underbrace{-P_d S^*}_{\text{savings}} > \underbrace{-R_d}_{\text{resource loss}}$, $\frac{\partial S^*}{\partial d} > 0$. In this case, the lower cost of school more than offsets the resource loss, so schooling increases.

   (b) If $R_d - P_d S^* < 0$, the sign of the numerator depends upon the relative magnitudes of term (1) and term (2):

   $$\underbrace{-(R_d - P_d S^*)(U_{SC} - PU_{CC})}_{\text{marginal savings}} < \underbrace{-P_d U_C}_{\text{marginal cost}}$$

   With $R_d < 0$ and $P_d < 0$, consumption (C^*) unambiguously decreases. Since the resource loss exceeds the savings, the question is whether schooling also decreases or whether consumption decreases enough such that schooling increases. The above inequality, which shows the gains and losses associated with the marginal unit of consumption, expresses this trade-off, as valued in terms of the price of schooling. If the inequality holds (i.e., the cost of an additional unit of consumption exceeds its benefit), the numerator will be positive and $\frac{\partial S^*}{\partial d} > 0$. Schooling consumption increases with distance. The opposite case obtains if the inequality does not hold—an additional unit of consumption is worth the cost—and schooling decreases.

   (c) If $R_d - P_d S^* = 0$, savings and resources offset and the sign of the numerator depends only on term (2), which is assumed to have a positive sign, $- -P_d U_C > 0$. Hence, $\frac{\partial S^*}{\partial d} > 0$ and schooling increases.

3. If $P_d = 0$, the schooling impact depends only on proximity's effect on resources, assumed to be negative. $\frac{\partial S^*}{\partial d} < 0$. Schooling decreases.

Also of interest is the policy elasticity, or how $\frac{\partial S^*}{\partial d}$ changes with respect to resources. Given my assumption that resource effects are decreasing in non-distance endowments, $\frac{\partial}{\partial e_i}(R_{di}(e_i)) < 0$. It follows that the treatment effect, $\frac{\partial S^*}{\partial d}$ is decreasing in endowed resources: R_d enters the τ_i expression only in the numerator, so a decrease in its absolute value represents a muting of the policy effect.

To summarize, school-predicated shelter placements affect the relative price of schooling that homeless families face. When school is closer, it becomes more attractive, but so do competing priorities, like seeing family or friends, or enjoying consumption goods in their presence. Consequently, the net price effect of neighborhood-based shelter assignments is theoretically ambiguous. What seems more clear—though it remains an assumption—is that distance reduces families' resources, by diminishing access to preexisting social supports and depreciating the value of neighborhood-specific human capital[30]. If distance increases the relative cost of schooling, price and resource effects operate in tandem to reduce schooling consumption. But if distance makes school relatively more attractive, the overall policy impact will depend upon the relative magnitudes of resource losses and cost savings. At the same time, the larger is a family's distance-independent resource endowment (or, equivalently, the fewer are its constraints), the smaller will be the policy effect.

C Empirical Appendix

This section contains additional details about my empirical methods, described in Section 4 in the main text.

C.1 Ineligibility Rate Instrument and Identification Strategy

The rigor of the family shelter application process provides ample opportunity for administrative discretion: stringent scrutiny can limit, or at least slow, the flow of shelter entrants, while leniency has the opposite effect. As discussed in the main text, I pursue an instrumental variables strategy based on shelter eligibility—or, more accurately, ineligibility, which makes coefficients easier to interpret, as treatment (local placement) becomes more likely the higher is the ineligibility rate.

My instrument is the 15-day moving average of the initial ineligibility rate for 30-day application periods. Each of these components requires some comment. Many families apply for shelter multiple times during my sample period. Because applications are necessarily not independent events, the question is which should be grouped together. Some applications come in quick succession; given the complexity of the application process, oftentimes a

[30]A more general model could allow the resource effect to be ambiguous as well (i.e., for some families, moves to more affluent neighborhoods may yield better job opportunities or access to better schools), but this would complicate the presentation without providing much additional insight. The basic point of distinguishing between resource and price effects is as a heuristic device, accounting (separately) for the possibilities that the school-based shelter placement policy has: (1) ambiguous effects on families' consumption choices (the price part), and (2) heterogeneous responses (the resource part). These two components can be interpreted generically, if doing so makes the assumptions more palatable.

rejection is soon followed by an acceptance. For this reason, treating each application as a unique event is misleading. I thus define "application periods" as lasting 30 days, in order to get an the idea of discrete bouts of homelessness. My assumption is that applications that fall within this month-long window reflect the same underlying issue, whereas gaps of more than 30 days reflect a new condition[31]. While this choice period length is somewhat arbitrary, it is consistent with the 30-day standard DHS uses when measuring families lengths of stay, where returns to shelter within 30-days are considered to be part of a continuous shelter episode.

With application periods set, it is possible to distinguish between "initial" and "final" ineligibility. Initial applies to the verdict of the family's first application within an application period; if the family is ruled eligible, this is also the final outcome, but not otherwise. If a family initially ruled ineligible applies again (potentially multiple times) within the application period, the final outcome is their last observed application. I focus on the latter because it is arguably more exogenous than that expressed through subsequent application rounds, which depend on family effort.

Note that eligible and ineligible are not the only possible outcomes; families may also "make own arrangements" (MOA), which means they voluntarily withdraw their applications, or they may be "diverted," in which case specialized intake staff help them find a remedy (such as a one-time rent arrears payment) that avoids shelter entry[32]. The initial eligibility rate for a given time period is then the count of ineligible applications in that time period divided by the total number of applications in that period (ineligible, eligible, MOA, diverted). In making this calculation, I include all family shelter applicants, not simply those in my sample (i.e., the calculation includes families with no students), as it is all applicants, and not only families with students, that impact shelter availability.

To best estimate ineligibility policy at the time of a family's application for shelter, I take an (weighted) 15-day moving average of the initial ineligibility rate, ending on the family's shelter start date and including the 14 days preceding it[33]. The moving average is a more accurate reflection of true eligibility policy than simply a daily rate, as it smooths out noise in the data, which may reflect, among other things, the composition of applicants on a given

[31] Note that I use a rolling 30-day window. That is, the period is extended whenever an application comes within 30 days of the preceding application; it is not constrained to the 30 days following the first application in a period. For example, if a family filed 3 unsuccessful applications, each separated by 30 days, the full application period would be 88 days (because of two overlaps of periods ending and beginning). The exception to this 30-day rule is a successful application. Once a family is deemed eligible, the application period resets.

[32] As with ineligible applications, MOAs and diversions are frequently followed near-term reapplications.

[33] A family's shelter start date is defined retroactively to the date of their application, though it may take up to 10 days to determine eligibility.

day.

Formally, for student i in family f entering shelter on day $D = d$, my instrument $Z_{if,d}$ is defined as follows:

$$Z_{if,d} = \frac{\frac{1}{15}\sum_{D=(d-14)}^{d}\sum_{f\in D}\mathbf{1}\{O_f = inel\}}{\frac{1}{15}\sum_{D=(d-14)}^{d}\sum_{f\in D}1} \tag{A.1}$$

with $\mathbf{1}\{\cdot\}$ the indicator function and $O_f \in \{eligible, ineligible, MOA, diversion\}$ a random variable denoting family $f's$ application outcome[34]. The numerator calculates the average daily number of ineligible applications during the 15 days culminating in family f's shelter entry, while the denominator is the average number of daily applications during this period (thus the inner summation is just a count of all families f applying on day D). Because I take the moving averages of ineligibles and applications separately, this formulation is weighted average, with the weights proportional to the number of applications on each day within the 15-day period[35].

My IV model consists of the following two-equation system via two-stage least squares:

$$\begin{aligned} N_{ip} &= \tau^1 Z_{ip} + \mathbf{X}_{ip}\boldsymbol{\beta}^1 + \boldsymbol{\varepsilon}_{ip}^1 \qquad \text{(first stage)} \\ Y_{ip} &= \tau^{IV}\widehat{N}_{ip} + \mathbf{X}_{ip}\boldsymbol{\beta} + \boldsymbol{\varepsilon}_{ip} \qquad \text{(second stage)} \end{aligned} \tag{A.2}$$

where the "1" superscripts denote first-stage parameters.

C.2 Instrument Validity

To consistently identify heterogeneous treatment effects—the LATE for compliers—my instrument must satisfy the following three well-known conditions in order to be valid:

1. Independence: $\{Y_{0i}, Y_{1i}, N_{0i}, N_{1i}\} \perp Z_i$
   - Note that writing Y_{Ni} indexed by N_i and not (N_i, Z_i) implies exclusion: $Y(N, Z = 0) = Y(N, Z = 1) = Y_{Ni}$
2. First-stage: $E[N_{1i} - N_{0i}] \neq 0$
3. Monotonicity: $N_{1i} \geq N_{0i} \ \ \forall i$

[34] Note that the instrument varies at the family f, rather than individual i, level, and so, like all family characteristics, apply to all students in the family.

[35] Pedantically, a leave-one-out estimator will be preferable, but given the numbers involved are large—during my sample, there are an average of 1,016 applications and 236 ineligibles during each 15-day period—it does not make a meaningful difference.

There is no question about instrument relevance. As shown in Figures A.10 (raw) and 1 (detrended), which give the quarterly time series for the ineligibility rate and treatment, the first-stage relationship between the ineligibility rate and local placement is quite strong. Of particular note is how the relationship strengthens after detrending the instrument and treatment for base covariates, which is the econometrically relevant case. The picture is even clearer in the month-level scatterplots presented in Figures A.11 (raw) and A.12 (detrended): the linear first-stage relationship is much stronger once the year, seasonal, borough, and grade influences are removed. The probability of in-borough placement is considerably higher when the ineligibility rate is high (after adjusting for base trends).

As usual, the validity verdict comes down to exclusion: whether the only manner in which the ineligibility rate influences student outcomes is through shelter placement locations. As discussed in the main text, the biggest threat to instrument exogeneity is a nuanced variant of sample selection. Because my sample consists of *eligible* family shelter entrants, my instrument very directly plays a role in selection: I only see the students who come from eligible families. If strict eligibility policy changes the characteristics of shelter entrants, my results will be biased; the instrument will be picking up changes in student unobservables rather than policy effects. That is, the instrument might change the distribution of potential outcomes.

Fortunately, as I argue in the main text, there is strong evidence that this sort of sample selection is not present, with Table 2 demonstrating that students who enter shelter during periods of unusually high and low eligibility are similar in most observable respects. Instead, the ineligibility rate is largely an exogenous policy variable determined by administrative and political considerations.

In this section, I provide additional evidence for the validity of the ineligibility rate instrument.

Families are deemed ineligible for two broad reasons: non-cooperation and other housing. Non-cooperation stems from the complexity of the application process, which can take as long as 10 days and entails extensive documentation, including detailed housing histories and multiple appointments with case workers. Missed appointments or incomplete documents frequently result in rejections. Other housing refers to cases in which DHS investigations uncover the availability of satisfactory shelter alternatives—for example, returning to an apartment shared with other family members that, while crowded, meets City standards.

It's also important to note that eligible and ineligible are not the only two possible outcomes. Families make also "make own arrangements," which means a voluntarily application withdrawal, or be "diverted," to non-shelter housing through the efforts of specialized City staff. Figure A.13 shows this broader context[36]. The final eligibility rate generally trends upwards, while the final ineligibility rate trends downward, though with small amplitude. At the same time, diversions increase in 2013 and decline after 2014, while own arrangements basically hold steady.

The auxiliary outcomes of MOA and diversions are incorporated in my instrument denominator. While they also preclude shelter entry, I do not count them as "ineligible," for two reasons. First, each heightens endogeneity concerns. MOA, which is at applicant discretion, is clearly endogenous. The concern for diversion is more subtle. Unlike eligibility determination, which are guided by state rules, diversion is a purely discretionary City endeavor to reduce shelter entry. Consequently, families offered diversion services may be quite different than those not offered services; periods of high and low diversion may thus imply greater sample selection[37]. The second reason is empirical: including only official "ineligibles" has the strongest first-stage relationship with treatment probability.

Overall, during my sample period, the majority—61 percent—of families eventually become eligible for shelter. The message is hammered home by Figures A.14 and A.15, which plot the relationship between the final eligibility rate, and, respectively, initial and final ineligibility rates. Points are monthly average of the underlying 15-day moving averages. The initial ineligibility rate has little relationship with the final eligibility rate (the coefficient on the best fit line is not significantly different from zero), while the strong relationship between the final rates is obvious. Taken together, the preponderance of evidence suggests sample selection should not be much of a problem.

The lack of endogenous sampling can also be reconciled by appealing to theory. To illustrate this situation, label all family unobservables as "ability" and, for convenience, consider families of three types, low. medium, and high ability. Medium ability families are always eligible for shelter. On the other hand, either (or both) low and high ability families could be affected by strict policy. Policy strictness can take various forms. On one hand, it might limit access among better-resourced families; on the other, it could require more resources to navigate successfully.

Indeed, these categories of rejections neatly comport will official definitions. Recall that ineligibility falls into two broad categories: non-cooperation and other housing. Simplifying somewhat, the former would seem to be most associated with low ability—families rejected due to inability to muster the discipline necessary complete the application process. Meanwhile, the latter group—those with alternative housing options—would seem to fall primarily

[36]Once again, the figure shows "doubly-smoothed" plots of quarterly means of underlying 15-day moving averages.

[37]Nevertheless, rates MOA and diversion, in part, can be influenced by ineligibility policy. In certain circumstances, diversion and ineligibility can be substitutes for controlling the number of shelter entrants.

in the high-ability end of the spectrum, given their access to greater resources.

As shown in Figure A.16, both reasons have played important roles in the evolution of the ineligibility rate over time. A reduction, and subsequent increase in non-cooperation explains most of the dramatic eligibility changes between 2014 and 2016. On the other hand, other-housing rejections gradually decreased for most of the 2010–2015 period, followed by an abrupt drop in 2016. What this means is that the evidence suggests both very high and very low ability families may have had reduced access during strict eligibility periods, meaning that the average composition of the sample unobservables was not much affected.

A related concern actually strengths the case for my instrument. The composition of shelter applicants could affect the eligibility rate. However, this is innocuous, so long as the composition of entrants remains unaffected. If it is the applicant pool, rather than policy considerations, that are driving ineligibility rate changes, the principal impact will be to weaken my instrument because, insofar as treatment is concerned, what matters is the route from ineligibility to fewer entrants relative to capacity. If the ineligibility rate rises solely due more applications without fewer acceptances, the impact on local placement probability will remain unaffected.

C.3 Instrument Robustness

Taken together, there is compelling evidence that, conditional upon year, month, borough and grade, the initial shelter ineligibility rate is independent of student unobservables related to educational outcomes. Nevertheless, as a robustness check, I also consider an alternative instrument: average days to shelter eligibility. 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, it captures the part of eligibility policy least related to applicant characteristics.

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. For student i in family f entering shelter on day $D = d$, the days-to-eligibility (DTE) instrument $Z_{if,d}^{DTE}$ is:

$$Z_{if,d}^{DTE} = \frac{1}{15} \sum_{D=(d-14)}^{d} \frac{1}{N_D} \sum_{f \in D} (eligiblity\_date_f - application\_date_f)$$

where N_D is the number of families applying on date D and *application_date* is the date of initial application within a period.

C.4 Measuring and Describing Compliers

To describe compliers, I implement an algorithm following the procedure described by Angrist and Pischke (2008), Dahl, Kostøl and Mogstad (2014), and Dobbie, Goldin and Yang (2018). The first step is to calculate the portion of the sample that are compliers; the second is to identify their average characteristics. I make two contributions to this literature: (1) extending the algorithm to continuous characteristics, and (2) calculating standard errors and performing formal t-tests of mean differences.

The idea is to discretize the continuous instrument by defining compliers as those students whose treatment status (placement location) would be been different if they entered shelter during the strictest eligibility regime (highest ineligibility rate) than during the most lenient (lowest ineligibility rate). Following convention, I define "most lenient" (z^L) and "most strict" (z^H) as the 1st and 99th percentiles of the instrument distribution, though I also explore the sensitivity of this assumption by alternative using the bottom/top 1.5 and 2 percentiles. Also necessary is an estimate of the effect of the instrument on the probability of treatment, which I estimate from a simplified linear first-stage, controlling for year and month,

$$N_i = \pi_0 + \pi_1 Z_i + \boldsymbol{\delta}_t + \boldsymbol{\omega}_m + \boldsymbol{\varepsilon}_i \tag{A.3}$$

which delivers an estimate $\widehat{\pi}_1$ of the relationship between the ineligibility rate and the probability of treatment. Accordingly, the complier share is estimated as

$$CS = \widehat{\pi}_1(z^H - z^L)$$

Correspondingly, always-takers are those who are treated even in the most treatment-adverse regime (low ineligibility rate and probability of treatment), $AS = \widehat{\pi}_0 + \widehat{\pi}_1 z^L$, and never-takers are those who are placed out-of-borough even when eligibility conditions are the most favorable (high ineligibility), $NS = 1 - \widehat{\pi}_0 - \widehat{\pi}_1 z^H$.

As shown in Table A.19, I estimate that the complier share for my primary school sample is 13 percent, and is not particularly sensitive to assumptions about the cutoff percentiles for strict and lenient instrument. Always-takers comprise 56 percent of the sample, while never-takers represent 30 percent.

While it is, of course, impossible to identify individual compilers, it is possible to describe their average characteristics. For binary attributes, doing so is a straightforward application of Bayes' rule.

The first insight is that the mean of a binary characteristic X is a probability, $E(X) =$

$1 \cdot Pr(X)$. Letting C be an indicator for complier, and NC for non-complier, what I want to estimate is $E(X|C) = Pr(X = 1|C = 1)$. This expression cannot be evaluated directly, as there is no way of knowing who the individual compliers are. Fortunately, the second insight is that Bayes' Rule allows me to reformulate the problem in terms of known quantities $Pr(X = 1|C = 1) = \frac{Pr(X \cap C)}{Pr(C)} = \frac{Pr(C|X)Pr(X)}{Pr(C)}$. All of the quantities in the last expression are estimatable from known quantities in the data. $Pr(X)$ is just the mean of X in the full sample. $Pr(C) = \widehat{\pi}_1(z^H - z^L)$ is the complier share of the sample, estimated above. $Pr(C|X) = Pr(C = 1|X = 1)$ is just the complier share in the subpopulation with the characteristic of interest, estimated by multiplying the instrument rate $(z^H - z^L)$ by $\widehat{\pi}_1^X$, estimated from Equation A.3 in the subsamples with $X = 1$. As before, partialing out year and month of shelter entry are important, given that I argue the ineligibility rate instrument—and in a larger sense, treatment itself—is exogenous conditional upon time period and seasonal trends, which capture systematic variation in the population of homeless shelter applicants. That is, within year and month of shelter entry, the eligibility rate is driven primarily by policy considerations.

In turn, the noncomplier, NC, mean is $E(X = 1|C = 0) = \frac{Pr(X=1 \cap C=0)}{1-Pr(C)} = \frac{Pr(X=1)(1-Pr(C=1|X=1))}{1-Pr(C=1)} = \frac{Pr(X=1)-Pr(X=1)Pr(C=1|X=1)}{1-Pr(C=1)}$, where all the necessary quantities are calculated in the complier step.

For ordered categorical and continuous characteristics, I extend (to my knowledge) the existing literature (which has only considered discrete characteristics) by, in the former case, partitioning the covariate into levels, and, in the latter, grouping into discrete deciles, and then repeating the above algorithm for each level/decile and calculating a weighted average.

I also improve upon the existing literature in a second way: by explicitly calculating standard errors, using bootstrap re-sampling (200 repetitions, and clustering by family), and performing formal t-tests of mean differences between compliers and non-compliers[38].

C.5 Student Fixed Effects Details

I argue the cases for quasi-random treatment assignment and instrument validity are strong. Nevertheless, it is useful to consider a complementary identification strategy based on entirely different assumptions: student fixed effects. Using these multiply observed students, my fixed effects setup dispenses with unobserved spell-invariant student heterogeneity, yielding a quite exacting comparison of same-student outcomes when placed locally or distantly.

For students present in the data in both treatment states, I observe actual outcome contrasts. If treatment status and outcomes are not being driven by spell-varying unobservables,

[38] Stata estimation commands implementing the complete complier characterization procedure is available upon request.

these observed outcomes will be indicative of the potential outcomes that underly them, and thus representative of true treatment effects. Mathematically, the individual fixed effects purge the analysis of spell-invariant individual heterogeneity, delivering a "within" estimator demeaned at the student level. $\widehat{\tau}^{FE}$ is a consistent estimator of treatment effects so long as the individual-demeaned error, conditioned on covariates, is uncorrelated with shelter placements: $cov(\varepsilon_{ip} - \overline{\varepsilon}_i, N_{ip} - \overline{N}_i | \mathbf{b}_{ip} - \overline{\mathbf{b}}_i, \mathbf{X}_{ip} - \overline{\mathbf{X}}_i) = 0$. Given quasi-random assignment, this more exacting level of scrutiny is not strictly necessary. Yet, as with my IV strategy, it sheds light on the heterogeneity of treatment effects.

D Additional Results

D.1 Residential Borough

The second supplementary treatment definition is home borough. That is, students are considered treated if they are placed in shelters in the boroughs of their most recent residence, irrespective of where their schools are located. This is the leading treatment definition in Cassidy (2020), as residential borough is the more natural treatment concept where family and adult outcomes are the focus. The downside of home borough treatment is that prior residence is a lower-quality field in the DHS data; in addition to more opportunities for data entry mistakes, homeless families tend to be quite mobile in general, so "most recent" residence may not reflect the places these families truly consider "home."

Appendix Table A.25 presents the results, again following the format of Table 4. Reassuringly, the main findings are confirmed[39]. According to OLS, students placed in their home boroughs miss 2.1 fewer school days, are 10.5 pp less likely to change schools, and are 0.9 pp less likely to leave DOE (controlling for Main covariates). Once again, the IV results delivering LATEs for compliers are mostly greater in magnitude and still statistically significant. Treated compliers miss 20.5 fewer days and are 17.2 pp less likely to leave DOE, though they appear no less likely to change schools.

On the other hand, proficiency and promotion do not appear impacted by shelter's correspondence with residence, either in general or for instrument compliers; this is true of promotion in my leading school-based treatment definition, but not proficiency. It may be that being sheltered in one's school borough has more influence on test performance than does being placed in one's borough of prior residence.

[39]The sample sizes are slightly smaller due to a higher frequency of missing data for most recent address.

D.2 Non-Linear Distance Effects

It is unlikely for the effects of distance to be uniform at every distance. Figures A.20 and A.21 show there are diminishing marginal effects of distance when I allow for a quadratic specification. (I impose the linearity constraint in Table 7 to simplify interpretation.) The effects of distance are concentrated in the bottom half of the distance distribution. At distances of less than 2 miles, each mile closer to school is worth more than an extra half day of attendance. By 8 miles, the marginal mile is worth just 0.25 fewer absences; by 12 miles, the effect is indistinguishable from zero. Being really close to school is more advantageous than being pretty close. The same pattern holds for school changes. The marginal "transfer-avoidance" gain is 2 pp or more for students placed closer than 4 miles to school and declines linearly, decreasing to 1 pp by 15 miles.

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F Supplementary Tables

F.1 Main Analytical Sample: Match and Summary Statistics

Table A.1: Match Stats: Students Age 5–18

	Students			Episodes		
Year of Birth	Obs	Matched	Match Rate	Obs	Matched	Match Rate
1992	493	341	0.69	499	343	0.69
1993	971	819	0.84	1,004	849	0.85
1994	1,577	1,390	0.88	1,720	1,518	0.88
1995	1,901	1,720	0.90	2,116	1,922	0.91
1996	2,390	2,179	0.91	2,778	2,539	0.91
1997	2,815	2,562	0.91	3,327	3,043	0.91
1998	3,501	3,202	0.91	4,219	3,875	0.92
1999	3,713	3,451	0.93	4,584	4,288	0.94
2000	4,022	3,676	0.91	4,886	4,493	0.92
2001	4,170	3,809	0.91	5,222	4,805	0.92
2002	4,246	3,875	0.91	5,292	4,879	0.92
2003	4,470	4,124	0.92	5,539	5,147	0.93
2004	4,938	4,523	0.92	6,216	5,753	0.93
2005	5,374	4,868	0.91	6,844	6,262	0.91
2006	5,544	5,017	0.90	7,020	6,425	0.92
2007	5,332	4,815	0.90	6,593	6,006	0.91
2008	5,287	4,735	0.90	6,366	5,757	0.90
2009	4,725	4,204	0.89	5,329	4,767	0.89
2010	4,062	3,576	0.88	4,380	3,876	0.88
2011	2,870	1,801	0.63	2,983	1,876	0.63
2012	1,657	41	0.02	1,665	42	0.03
Total	74,058	64,728	0.87	88,582	78,465	0.89

Results of probabilistic linkage of DHS (calendar year 2010–2016) and DOE (school year 2005–2016) administrative data. Sample universe is all DHS family shelter entrants from 2010–2016. Children matched on first name, last name, date of birth (month and year) and sex. Includes only children ages 5–18 at some point during shelter episode.

Table A.2: Match Stats: Students Age 4–21

Year of Birth	Students Obs	Students Matched	Students Match Rate	Episodes Obs	Episodes Matched	Episodes Match Rate
1989	780	45	0.06	810	46	0.06
1990	1,149	182	0.16	1,264	198	0.16
1991	1,430	574	0.40	1,673	643	0.38
1992	1,757	1,229	0.70	2,044	1,420	0.69
1993	2,215	1,806	0.82	2,591	2,121	0.82
1994	2,730	2,317	0.85	3,289	2,810	0.85
1995	3,153	2,722	0.86	3,784	3,308	0.87
1996	3,090	2,727	0.88	3,705	3,303	0.89
1997	3,183	2,855	0.90	3,818	3,445	0.90
1998	3,501	3,202	0.91	4,219	3,875	0.92
1999	3,713	3,451	0.93	4,584	4,288	0.94
2000	4,022	3,676	0.91	4,886	4,493	0.92
2001	4,170	3,809	0.91	5,222	4,805	0.92
2002	4,246	3,875	0.91	5,292	4,879	0.92
2003	4,470	4,124	0.92	5,539	5,147	0.93
2004	4,938	4,523	0.92	6,216	5,753	0.93
2005	5,374	4,868	0.91	6,844	6,262	0.91
2006	5,987	5,352	0.89	7,750	7,028	0.91
2007	6,041	5,314	0.88	7,711	6,884	0.89
2008	5,914	5,167	0.87	7,458	6,604	0.89
2009	5,470	4,723	0.86	6,559	5,729	0.87
2010	4,899	4,177	0.85	5,580	4,797	0.86
2011	4,113	2,547	0.62	4,457	2,758	0.62
2012	2,992	66	0.02	3,120	69	0.02
Total	89,337	73,331	0.82	108,415	90,665	0.84

Results of probabilistic linkage of DHS (calendar year 2010–2016) and DOE (school year 2005–2016) administrative data. Sample universe is all DHS family shelter entrants from 2010–2016. Children matched on first name, last name, date of birth (month and year) and sex. Includes only students ages 4–21 at some point during shelter episode.

Table A.3: Panel Summary: Observations and School Years Per Student

	Observations (Student-Years)			Students		
Times Observed	(1) All	(2) Main: All	(3) Main: Sample	(4) All	(5) Main: All	(6) Main: Sample
1	73,518	39,192	39,192	1,657	746	35,290
2	71,861	38,446	3,902	4,677	2,119	3,578
3	67,184	36,327	324	6,873	3,242	297
4	60,311	33,085	27	8,104	4,147	23
5	52,207	28,938	4	8,666	4,682	4
6	43,541	24,256		8,884	5,397	
7	34,657	18,859		8,106	6,077	
8	26,551	12,782		6,597	12,782	
9	19,954			5,152		
10	14,802			4,847		
11	9,955			4,582		
12	5,373			5,373		
Total	479,914	231,885	43,449	73,518	39,192	39,192

Observations pane gives the number of student-school-years present in the data for students observed the row-delineated number of times. Students pane gives the individual number of students observed the row-delineated number of times. Note that for observations, rows are cumulative in the sense that all being observed n times implies being observed $[1, n-1]$ times as well. However, for students, rows are mutually exclusive in the sense that students in row n are observed $> n-1$ but $< n+1$ times. "All" refers to the unrestricted full dataset. "Main: All" refers to students in the main sample across the full set of school years 2009-2016 (these observations are relevant when lagging and leading years feature in the analysis.) "Main: Sample" refers only to student observations included in the main sample.

Table A.4: Summary Statistics by School Year of First Shelter Entry

	2010	2011	2012	2013	2014	2015	Total
All Students	7,534	6,958	7,405	6,927	7,067	7,558	43,449
Primary School (K-8)	5,983	5,483	5,931	5,564	5,596	6,025	34,582
High School (9-12)	1,551	1,475	1,474	1,363	1,471	1,533	8,867
School-Shelter Distance	5.0	5.8	6.1	6.0	6.3	6.7	6.0
Grade	4.9	5.0	4.8	4.8	4.8	4.7	4.9
Students in Family	2.3	2.4	2.4	2.4	2.3	2.3	2.3
Days Absent	31.8	31.3	31.6	33.6	30.9	28.4	31.2
Placed in School Boro	0.64	0.54	0.50	0.52	0.48	0.44	0.52
Changed School	0.45	0.46	0.47	0.46	0.46	0.42	0.45
Regents Taken	0.50	0.52	0.49	0.48	0.53	0.55	0.51
Regents Passed	0.33	0.33	0.30	0.29	0.32	0.34	0.32
Promoted	0.87	0.87	0.87	0.88	0.89	0.90	0.88
School: Manhattan	0.13	0.14	0.13	0.14	0.14	0.13	0.13
School: Bronx	0.38	0.38	0.38	0.38	0.37	0.39	0.38
School: Brooklyn	0.33	0.32	0.34	0.33	0.32	0.31	0.32
School: Queens	0.12	0.13	0.13	0.13	0.14	0.14	0.13
School: Staten Island	0.04	0.03	0.02	0.03	0.03	0.03	0.03
Elementary School	0.57	0.56	0.58	0.59	0.59	0.60	0.58
Middle School	0.22	0.23	0.22	0.21	0.20	0.19	0.21
High School	0.21	0.21	0.20	0.20	0.21	0.20	0.20

Data is Main sample, pooling grades K–12. That is, the sample is limited to school years of shelter entry among students enrolled in DOE prior to shelter and not in special school districts 75, 79, 84, and 88.

Table A.5: Grade and School Boro Sample Shares

	Manhattan	Bronx	Brooklyn	Queens	Staten Island	Total
K	1.46	3.90	3.17	1.43	0.26	10.23
1	1.44	4.84	3.98	1.55	0.37	12.18
2	1.18	4.14	3.40	1.38	0.30	10.40
3	1.13	3.75	3.06	1.28	0.31	9.54
4	1.01	3.33	2.75	1.17	0.25	8.51
5	0.87	2.97	2.43	0.98	0.25	7.50
6	0.79	2.90	2.58	0.99	0.27	7.52
7	0.80	2.76	2.34	0.94	0.23	7.07
8	0.77	2.63	2.23	0.84	0.18	6.65
9	1.49	2.95	2.44	1.21	0.29	8.38
10	1.13	1.95	1.81	0.77	0.14	5.81
11	0.67	1.13	1.06	0.38	0.07	3.32
12	0.60	0.89	1.00	0.36	0.04	2.90
Total	13.35	38.15	32.26	13.29	2.96	100.00

Data is Main sample, pooling grades K–12. That is, the sample is limited to school years of shelter entry among students enrolled in DOE prior to shelter and not in special school districts 75, 79, 84, and 88.

Table A.6: Year and School Boro Sample Shares

	Manhattan	Bronx	Brooklyn	Queens	Staten Island	Total
2010	2.29	6.64	5.65	2.15	0.61	17.34
2011	2.18	6.12	5.11	2.12	0.48	16.01
2012	2.23	6.46	5.72	2.22	0.41	17.04
2013	2.22	6.08	5.20	2.03	0.41	15.94
2014	2.24	6.09	5.21	2.26	0.47	16.27
2015	2.18	6.76	5.36	2.50	0.59	17.40
Total	13.35	38.15	32.26	13.29	2.96	100.00

Data is Main sample, pooling grades K–12. That is, the sample is limited to school years of shelter entry among students enrolled in DOE prior to shelter and not in special school districts 75, 79, 84, and 88.

Table A.7: Borough Treatment by Grade and Boro

	School Borough					
Grade	Manhattan	Bronx	Brooklyn	Queens	Staten Island	Total
K	0.32	0.71	0.55	0.30	0.13	0.53
1	0.27	0.69	0.55	0.29	0.09	0.53
2	0.28	0.71	0.54	0.32	0.08	0.54
3	0.27	0.70	0.54	0.29	0.07	0.52
4	0.27	0.71	0.55	0.23	0.10	0.52
5	0.27	0.70	0.53	0.30	0.10	0.52
6	0.28	0.74	0.57	0.29	0.07	0.55
7	0.30	0.70	0.59	0.26	0.07	0.54
8	0.29	0.70	0.56	0.25	0.09	0.53
9	0.19	0.71	0.56	0.26	0.10	0.49
10	0.19	0.69	0.57	0.31	0.11	0.49
11	0.21	0.67	0.48	0.25	0.00	0.45
12	0.22	0.63	0.49	0.31	0.06	0.45
Total	0.26	0.70	0.55	0.28	0.09	0.52

Treatment defined as placed in school borough.

See note to Table A.4 for sample restrictions.

Table A.8: Days Absent by Grade and Boro

	School Borough					
Grade	Manhattan	Bronx	Brooklyn	Queens	Staten Island	Total
K	32.2	32.1	32.4	34.2	35.0	32.6
1	28.5	29.6	30.7	31.8	36.0	30.3
2	24.6	26.6	27.0	27.2	32.2	26.8
3	22.9	25.6	26.1	24.8	30.6	25.5
4	22.8	24.7	24.7	23.0	33.0	24.5
5	20.7	24.4	23.7	24.2	27.9	23.8
6	20.5	26.0	25.2	27.4	31.6	25.5
7	22.9	28.0	28.8	28.2	38.8	28.1
8	27.1	32.9	31.8	32.6	36.6	31.9
9	41.4	49.5	46.7	52.2	57.6	47.9
10	38.5	42.4	44.7	43.8	54.7	42.8
11	36.7	40.6	43.9	37.9	37.3	40.5
12	43.1	42.1	45.0	45.8	30.4	43.6
Total	29.6	30.9	31.4	32.1	36.8	31.2

See note to Table A.4 for sample restrictions.

Table A.9: Borough Treatment by Year and Borough

	School Borough					
Year	Manhattan	Bronx	Brooklyn	Queens	Staten Island	Total
2010	0.38	0.79	0.73	0.31	0.11	0.64
2011	0.29	0.70	0.62	0.28	0.08	0.54
2012	0.23	0.72	0.49	0.21	0.09	0.50
2013	0.30	0.73	0.51	0.27	0.09	0.52
2014	0.17	0.66	0.49	0.33	0.06	0.48
2015	0.18	0.62	0.44	0.29	0.10	0.44
Total	0.26	0.70	0.55	0.28	0.09	0.52

Treatment defined as placed in school borough.

See note to Table A.4 for sample restrictions.

Table A.10: Days Absent by Year and Boro

	School Borough					
School Year	Manhattan	Bronx	Brooklyn	Queens	Staten Island	Total
2010	29.3	31.7	31.5	34.1	37.3	31.8
2011	30.2	30.2	31.6	33.2	36.3	31.3
2012	28.9	32.1	31.3	33.1	35.0	31.6
2013	31.5	32.9	34.6	34.0	41.1	33.6
2014	29.6	31.2	30.7	31.1	35.7	30.9
2015	28.1	27.7	28.6	28.1	36.1	28.4
Total	29.6	30.9	31.4	32.1	36.8	31.2

Table A.11: Summary Statistics by School Year of First Shelter Entry, Primary School (Grades K-8)

year	Students	In-Boro	Distance	Days Absent	School Change	Proficient	Promoted
2010	5,983	0.65	4.9	28.3	0.47	0.17	0.91
2011	5,483	0.56	5.7	27.5	0.47	0.19	0.91
2012	5,931	0.50	6.0	28.0	0.49	0.04	0.91
2013	5,564	0.53	5.9	30.1	0.48	0.05	0.92
2014	5,596	0.49	6.2	27.5	0.48	0.04	0.94
2015	6,025	0.46	6.6	25.7	0.44	0.07	0.94
Total	34,582	0.53	5.9	27.8	0.47	0.09	0.92

Data is Main sample, as defined in text.

Table A.12: Summary Statistics by School Year of First Shelter Entry, High School (Grades 9-12)

Year	Students	In-Boro	Distance	Days Absent	School Change	Took Regents	Passed Regents	Promoted
2010	1,551	0.60	5.3	45.5	0.38	0.64	0.42	0.68
2011	1,475	0.49	6.1	45.9	0.39	0.66	0.41	0.69
2012	1,474	0.48	6.1	46.1	0.40	0.63	0.38	0.68
2013	1,363	0.47	6.4	48.0	0.39	0.62	0.37	0.69
2014	1,471	0.43	6.6	44.1	0.39	0.67	0.40	0.72
2015	1,533	0.39	7.2	38.9	0.38	0.69	0.43	0.75
Total	8,867	0.48	6.3	44.6	0.39	0.65	0.40	0.70

Data is Main sample, as defined in text.

F.2 Complete Sample: Summary Statistics

Table A.13: Full DOE Data: Homeless and Housed Observations by Year

Year	Housed	Homeless	Total
2010	1,100,149	13,582	1,113,731
2011	1,103,439	16,130	1,119,569
2012	1,103,786	19,585	1,123,371
2013	1,110,184	21,867	1,132,051
2014	1,124,008	24,874	1,148,882
2015	1,135,739	25,458	1,161,197
Total	6,677,305	121,496	6,798,801

Homeless include only those students who enter shelter in school years 2010-2015.

Table A.14: All DOE Data: Housed and Homeless Students Key Outcomes by Year, Grades K-8

	2010	2011	2012	2013	2014	2015	Total
			Panel A: Housed Students				
Number of Students	648,907	648,073	645,746	644,097	638,562	636,278	3,861,663
Days Absent	11.9	10.6	10.8	11.5	10.8	10.0	10.9
ELL	0.16	0.17	0.16	0.16	0.16	0.17	0.16
IEP	0.16	0.15	0.17	0.18	0.19	0.19	0.17
Free or Reduced-Price Lunch	0.87	0.85	0.72	0.74	0.72	0.70	0.77
Black	0.27	0.26	0.25	0.23	0.22	0.21	0.24
Hispanic	0.41	0.41	0.41	0.41	0.42	0.42	0.41
White	0.16	0.16	0.16	0.16	0.17	0.17	0.16
Elementary School	0.67	0.68	0.68	0.68	0.68	0.68	0.68
Middle School	0.33	0.32	0.32	0.32	0.32	0.32	0.32
High School	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Manhattan	0.13	0.13	0.13	0.12	0.12	0.12	0.13
Bronx	0.22	0.21	0.21	0.21	0.21	0.21	0.21
Brooklyn	0.31	0.30	0.30	0.30	0.30	0.30	0.30
Queens	0.29	0.29	0.30	0.30	0.30	0.31	0.30
Staten Island	0.06	0.06	0.06	0.06	0.06	0.06	0.06
ELA Proficient	0.42	0.46	0.26	0.28	0.29	0.36	0.35
Math Proficient	0.57	0.59	0.30	0.33	0.34	0.34	0.41
Proficient	0.37	0.40	0.19	0.20	0.21	0.25	0.27
Promoted	0.97	0.97	0.97	0.98	0.98	0.98	0.98
Changed School	0.21	0.20	0.20	0.19	0.19	0.19	0.20
Left DOE	0.05	0.05	0.05	0.05	0.06	1.00	0.21
			Panel B: Homeless Students				
Number of Students	9,288	10,987	13,189	14,538	15,998	16,097	80,097
Days Absent	27.6	25.8	26.5	28.4	27.4	25.7	26.9
ELL	0.11	0.11	0.10	0.09	0.10	0.10	0.10
IEP	0.18	0.20	0.23	0.27	0.28	0.29	0.25
Free or Reduced-Price Lunch	0.99	0.99	0.99	1.00	1.00	1.00	0.99
Black	0.53	0.54	0.53	0.53	0.52	0.51	0.53
Hispanic	0.43	0.42	0.42	0.42	0.43	0.44	0.43
White	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Elementary School	0.74	0.72	0.72	0.74	0.76	0.76	0.74
Middle School	0.26	0.28	0.28	0.26	0.24	0.24	0.26
High School	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Manhattan	0.13	0.13	0.13	0.13	0.13	0.12	0.13
Bronx	0.40	0.41	0.42	0.44	0.46	0.46	0.44
Brooklyn	0.33	0.33	0.33	0.31	0.29	0.29	0.31
Queens	0.11	0.11	0.10	0.10	0.10	0.11	0.11
Staten Island	0.03	0.02	0.02	0.02	0.02	0.02	0.02
ELA Proficient	0.23	0.23	0.07	0.08	0.08	0.13	0.13
Math Proficient	0.30	0.31	0.07	0.09	0.09	0.10	0.15
Proficient	0.16	0.16	0.03	0.04	0.04	0.06	0.07
Promoted	0.91	0.91	0.91	0.92	0.93	0.94	0.92
Changed School	0.49	0.44	0.44	0.42	0.44	0.43	0.44
Left DOE	0.10	0.09	0.11	0.09	0.07	1.00	0.27

Data consists of all DOE students in grades K–8 during school years 2010–2015, excluding those in special school districts 75, 79, 84, and 88. Homeless defined as in DHS shelter during a given school year and includes only those students who enter shelter in school years 2010–2015. Housed are all other students, including any entering shelter pre-2010.

Table A.15: All DOE Data: Housed and Homeless Students Key Outcomes by Year, Grades 9-12

	2010	2011	2012	2013	2014	2015	Total
			Panel A: Housed Students				
Number of Students	307,802	304,036	298,326	293,984	292,377	290,413	1,786,938
Days Absent	22.7	21.6	21.7	21.5	20.2	19.5	21.2
ELL	0.12	0.13	0.12	0.12	0.11	0.11	0.12
IEP	0.12	0.12	0.14	0.15	0.15	0.16	0.14
Free or Reduced-Price Lunch	0.78	0.76	0.72	0.72	0.72	0.71	0.74
Black	0.32	0.31	0.30	0.29	0.29	0.28	0.30
Hispanic	0.39	0.39	0.39	0.39	0.40	0.40	0.39
White	0.13	0.13	0.13	0.13	0.13	0.14	0.13
Elementary School	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Middle School	0.00	0.00	0.00	0.00	0.00	0.00	0.00
High School	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Manhattan	0.20	0.20	0.21	0.21	0.21	0.20	0.21
Bronx	0.19	0.19	0.19	0.19	0.18	0.18	0.19
Brooklyn	0.29	0.29	0.29	0.28	0.29	0.28	0.29
Queens	0.26	0.26	0.26	0.26	0.26	0.27	0.26
Staten Island	0.06	0.06	0.06	0.06	0.06	0.06	0.06
ELA Proficient	1.00	.	.	.	.	.	1.00
Math Proficient	1.00	.	.	.	.	.	1.00
Proficient	.	.	.	.	.	.	.
Promoted	0.82	0.82	0.83	0.84	0.85	0.87	0.84
Changed School	0.27	0.26	0.26	0.26	0.26	0.26	0.26
Left DOE	0.26	0.27	0.26	0.27	0.26	1.00	0.38
			Panel B: Homeless Students				
Number of Students	2,277	2,871	3,370	3,709	4,212	4,185	20,624
Days Absent	46.2	46.5	44.8	46.6	46.8	42.6	45.5
ELL	0.09	0.12	0.11	0.09	0.09	0.09	0.10
IEP	0.17	0.19	0.24	0.25	0.26	0.26	0.23
Free or Reduced-Price Lunch	0.97	0.98	0.99	0.98	0.99	0.99	0.98
Black	0.56	0.56	0.56	0.58	0.56	0.56	0.56
Hispanic	0.40	0.40	0.40	0.38	0.39	0.39	0.39
White	0.02	0.02	0.02	0.02	0.03	0.03	0.03
Elementary School	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Middle School	0.00	0.00	0.00	0.00	0.00	0.00	0.00
High School	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Manhattan	0.19	0.20	0.22	0.21	0.21	0.19	0.20
Bronx	0.36	0.36	0.36	0.35	0.36	0.37	0.36
Brooklyn	0.31	0.30	0.30	0.31	0.30	0.31	0.30
Queens	0.11	0.12	0.11	0.11	0.11	0.11	0.11
Staten Island	0.03	0.02	0.02	0.02	0.02	0.02	0.02
ELA Proficient	.	.	.	.	.	.	.
Math Proficient	.	.	.	.	.	.	.
Proficient	.	.	.	.	.	.	.
Promoted	0.65	0.65	0.68	0.67	0.69	0.72	0.68
Changed School	0.38	0.36	0.38	0.36	0.38	0.38	0.37
Left DOE	0.25	0.26	0.25	0.26	0.24	1.00	0.40

Data consists of all DOE students in grades 9–12 during school years 2010–2015, excluding those in special school districts 75, 79, 84, and 88. Homeless defined as in DHS shelter during a given school year and includes only those students who enter shelter in school years 2010–2015. Housed are all other students, including any entering shelter pre-2010.

F.3 Results Supplement

Table A.16: Descriptives and Random Assignment: Base Covariates

	Primary School (K-8)					High School (9-12)				
	Overall		Randomization Check			Overall		Randomization Check		
	Mean	SD	Distant	Local	Diff.	Mean	SD	Distant	Local	Diff.
2010	0.17	0.38	0.13	0.21	0.08**	0.17	0.38	0.13	0.22	0.08**
2011	0.16	0.37	0.15	0.17	0.02**	0.17	0.37	0.16	0.17	0.01
2012	0.17	0.38	0.18	0.16	-0.02**	0.17	0.37	0.17	0.17	0.00
2013	0.16	0.37	0.16	0.16	0.00	0.15	0.36	0.16	0.15	-0.01
2014	0.16	0.37	0.18	0.15	-0.03**	0.17	0.37	0.18	0.15	-0.03**
2015	0.17	0.38	0.20	0.15	-0.05**	0.17	0.38	0.20	0.14	-0.06**
School: Manhattan	0.12	0.32	0.18	0.06	-0.12**	0.19	0.39	0.29	0.08	-0.21**
School: Bronx	0.39	0.49	0.25	0.52	0.28**	0.34	0.47	0.20	0.49	0.29**
School: Brooklyn	0.33	0.47	0.31	0.34	0.03**	0.31	0.46	0.27	0.35	0.07**
School: Queens	0.13	0.34	0.20	0.07	-0.13**	0.13	0.34	0.18	0.08	-0.11**
School: Staten Island	0.03	0.17	0.06	0.01	-0.05**	0.03	0.16	0.05	0.00	-0.04**
Jan	0.08	0.28	0.09	0.08	-0.00	0.08	0.27	0.08	0.08	0.00
Feb	0.07	0.26	0.07	0.08	0.01**	0.07	0.25	0.06	0.08	0.01**
Mar	0.08	0.26	0.07	0.08	0.01**	0.08	0.27	0.08	0.08	0.00
Apr	0.07	0.26	0.06	0.08	0.02**	0.07	0.26	0.07	0.08	0.01**
May	0.07	0.26	0.07	0.07	0.00	0.07	0.26	0.07	0.07	0.00
Jun	0.07	0.25	0.07	0.07	-0.00	0.07	0.25	0.07	0.07	-0.00
Jul	0.09	0.29	0.10	0.09	-0.01**	0.09	0.29	0.09	0.10	0.00
Aug	0.10	0.31	0.12	0.09	-0.02**	0.11	0.31	0.12	0.09	-0.03**
Sep	0.11	0.31	0.11	0.11	0.00	0.11	0.31	0.11	0.11	-0.00
Oct	0.10	0.29	0.10	0.10	0.00	0.10	0.30	0.09	0.10	0.00
Nov	0.08	0.28	0.09	0.08	-0.00	0.08	0.27	0.08	0.08	-0.00
Dec	0.08	0.27	0.08	0.07	-0.01*	0.08	0.26	0.08	0.07	-0.00
Pre-K	0.00	0.00	0.00	0.00	0.00**	0.00	0.00	0.00	0.00	0.00**
Kindergarten	0.13	0.33	0.13	0.13	-0.00	0.00	0.00	0.00	0.00	0.00**
Grade 1	0.15	0.36	0.15	0.15	-0.00	0.00	0.00	0.00	0.00	0.00**
Grade 2	0.13	0.34	0.13	0.13	0.00	0.00	0.00	0.00	0.00	0.00**
Grade 3	0.12	0.32	0.12	0.12	-0.00	0.00	0.00	0.00	0.00	0.00**
Grade 4	0.11	0.31	0.11	0.10	-0.00	0.00	0.00	0.00	0.00	0.00**
Grade 5	0.09	0.29	0.10	0.09	-0.00	0.00	0.00	0.00	0.00	0.00**
Grade 6	0.09	0.29	0.09	0.10	0.01**	0.00	0.00	0.00	0.00	0.00**
Grade 7	0.09	0.28	0.09	0.09	0.00	0.00	0.00	0.00	0.00	0.00**
Grade 8	0.08	0.28	0.08	0.08	0.00	0.00	0.00	0.00	0.00	0.00**
Grade 9	0.00	0.00	0.00	0.00	0.00**	0.41	0.49	0.40	0.42	0.02
Grade 10	0.00	0.00	0.00	0.00	0.00**	0.28	0.45	0.28	0.29	0.01
Grade 11	0.00	0.00	0.00	0.00	0.00**	0.16	0.37	0.17	0.16	-0.01*
Grade 12	0.00	0.00	0.00	0.00	0.00**	0.14	0.35	0.15	0.13	-0.02**

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 A.17: Descriptives and Random Assignment: Main Covariates

	Primary School (K-8)					High School (9-12)				
	Overall		Randomization Check			Overall		Randomization Check		
	Mean	SD	Distant	Local	Diff.	Mean	SD	Distant	Local	Diff.
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**
White	0.02	0.15	0.03	0.02	-0.01**	0.02	0.15	0.03	0.02	-0.01**
Asian	0.01	0.10	0.01	0.01	-0.00**	0.01	0.11	0.01	0.01	-0.00
Native American	0.01	0.09	0.01	0.01	-0.00**	0.01	0.07	0.01	0.01	0.00
ELL	0.10	0.30	0.10	0.10	0.01	0.09	0.29	0.09	0.10	0.01
Non-English	0.17	0.38	0.17	0.18	0.01**	0.22	0.42	0.21	0.23	0.02**
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
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: Commerical 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 A.18: Descriptives and Random Assignment: Outcomes, Treatments, and Instruments

	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**
School Change Prior Year	0.35	0.48	0.36	0.34	-0.02**	0.35	0.48	0.35	0.36	0.01
Admission Prior Year	0.35	0.48	0.37	0.34	-0.03**	0.22	0.41	0.22	0.22	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	0.47	0.50	0.56	0.39	-0.17**	0.39	0.49	0.42	0.36	-0.06**
Admission	0.48	0.50	0.57	0.40	-0.17**	0.25	0.43	0.29	0.20	-0.09**
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 Boro	0.53	0.50	0.00	1.00	1.00	0.48	0.50	0.00	1.00	1.00
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	5.91	4.86	9.70	2.56	-7.14**	6.31	4.54	9.27	3.00	-6.27**
Ineligibility Rate (IV)	0.23	0.04	0.23	0.23	0.00**	0.23	0.04	0.23	0.23	0.00
Exits per Entrant (IV)	1.29	0.20	1.26	1.31	0.04**	1.29	0.21	1.27	1.31	0.04**
Days to Eligibility (IV)	6.30	1.91	6.39	6.23	-0.16**	6.27	1.90	6.37	6.18	-0.18**
Occupancy Rate (IV)	0.94	0.03	0.94	0.94	-0.01**	0.94	0.03	0.94	0.94	-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 A.19: Compliance Type Shares

	Primary School (K-8)			High School (9-12)		
	1%	1.5%	2%	1%	1.5%	2%
Compliers	0.13	0.13	0.12	0.12	0.12	0.11
Always-Takers	0.56	0.57	0.57	0.53	0.53	0.54
Never-Takers	0.30	0.31	0.31	0.34	0.35	0.35

Results from linear first-stage, controlling for year and month of shelter entry. Percentages in second row refer to percentiles used as thresholds to define low and high instrument values. See Appendix C.4 for estimation method details.

Table A.20A: Complier Characteristics, Ineligibility Rate Instrument

	Primary School (K-8)			High School (9-12)		
	Compliers	Non-Compliers	Diff.	Compliers	Non-Compliers	Diff.
Non-English	0.21	0.17	0.04	0.05	0.25	-0.20
	(0.004)	(0.000)	[0.69]	(0.026)	(0.000)	[-1.21]
Foreign-Born	0.05	0.05	-0.00	0.06	0.11	-0.05
	(0.001)	(0.000)	[-0.01]	(0.009)	(0.000)	[-0.53]
Student Age	8.97	9.54	-0.57	16.81	16.53	0.28
	(0.147)	(0.004)	[-1.46]	(0.120)	(0.002)	[0.79]
White	0.02	0.02	-0.00	0.05	0.02	0.03
	(0.001)	(0.000)	[-0.16]	(0.203)	(0.000)	[0.06]
Grade	3.19	3.58	-0.39	10.35	9.99	0.36
	(0.115)	(0.003)	[-1.15]	(0.092)	(0.001)	[1.17]
Absence Rate Prior Year	0.15	0.14	0.00	0.28	0.22	0.06
	(0.000)	(0.000)	[0.20]	(0.003)	(0.000)	[0.99]
Promoted Prior Year	0.91	0.92	-0.00	0.71	0.77	-0.05
	(0.002)	(0.000)	[-0.12]	(0.055)	(0.000)	[-0.23]
Proficient Prior Year	0.12	0.10	0.02	-0.25	0.12	-0.37
	(0.010)	(0.000)	[0.15]	(283.672)	(0.001)	[-0.02]
Took Regents Prior Year	-0.00	0.04	-0.04	0.63	0.52	0.11
	(0.171)	(0.027)	[-0.09]	(0.045)	(0.000)	[0.52]
Passed Regents Prior Year	.	.	.	0.39	0.33	0.06
	(.)	(.)	[.]	(0.040)	(0.000)	[0.31]
Changed School	0.52	0.48	0.04	0.18	0.31	-0.14
	(0.008)	(0.000)	[0.42]	(0.022)	(0.000)	[-0.91]
Promoted	0.89	0.93	-0.04	0.52	0.72	-0.21
	(0.001)	(0.000)	[-1.04]	(0.125)	(0.000)	[-0.59]
Left DOE	0.07	0.09	-0.02	0.33	0.16	0.17
	(0.002)	(0.000)	[-0.38]	(0.157)	(0.000)	[0.44]
Proficient	-0.06	0.10	-0.16	.	.	.
	(0.008)	(0.000)	[-1.79]	(.)	(.)	[.]
Regents Taken	0.17	0.07	0.10	0.76	0.64	0.13
	(26.582)	(0.000)	[0.02]	(0.026)	(0.000)	[0.76]
Regents Passed	0.14	0.05	0.09	0.29	0.42	-0.13
	(23.632)	(0.000)	[0.02]	(0.032)	(0.000)	[-0.71]
Placed in School Boro	0.00	0.61	-0.61	0.00	0.54	-0.54
	(0.000)	(0.000)	[-38.86]	(0.000)	(0.001)	[-22.33]
Days Absent	26.86	27.96	-1.11	56.26	42.99	13.27
	(9.751)	(0.260)	[-0.35]	(97.596)	(2.016)	[1.33]
Absence Rate	0.17	0.17	0.00	0.36	0.29	0.07
	(0.000)	(0.000)	[0.08]	(0.004)	(0.000)	[1.10]
School-Shelter Distance	5.31	6.01	-0.69	5.29	6.47	-1.19
	(0.126)	(0.006)	[-1.91]	(2.181)	(0.049)	[-0.79]
Ineligibility Rate (IV)	0.23	0.23	-0.00	0.22	0.23	-0.01
	(0.000)	(0.000)	[-0.24]	(0.000)	(0.000)	[-1.26]

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 A.20B: Complier Characteristics, Ineligibility Rate Instrument

	Primary School (K-8)			High School (9-12)		
	Compliers	Non-Compliers	Diff.	Compliers	Non-Compliers	Diff.
Family Size	4.85	4.38	0.47	.	.	.
	(0.205)	(0.005)	[1.02]	(.)	(.)	[.]
Students in Family	2.70	2.27	0.43	3.19	2.28	0.90
	(0.084)	(0.002)	[1.47]	(0.219)	(0.005)	[1.91]
Non-students in Family	1.92	2.14	-0.22	.	.	.
	(0.474)	(0.012)	[-0.32]	(.)	(.)	[.]
On CA	0.39	0.35	0.04	0.13	0.33	-0.20
	(0.009)	(0.000)	[0.40]	(0.030)	(0.000)	[-1.15]
Log Avg. Quarterly Earnings, Year Pre	2.40	2.70	-0.30	5.16	2.73	2.43
	(0.341)	(0.008)	[-0.51]	(1.618)	(0.025)	[1.90]
Head Age	33.85	34.52	-0.67	38.45	40.71	-2.26
	(1.266)	(0.035)	[-0.58]	(3.339)	(0.066)	[-1.22]
Partner Present	0.32	0.27	0.06	0.15	0.22	-0.07
	(0.006)	(0.000)	[0.73]	(0.018)	(0.000)	[-0.51]
Pregnant	0.05	0.04	0.01	0.06	0.02	0.04
	(0.001)	(0.000)	[0.16]	(0.002)	(0.000)	[0.83]
Head Education: Less Than High School	0.55	0.59	-0.05	0.67	0.57	0.11
	(0.008)	(0.000)	[-0.51]	(0.024)	(0.000)	[0.69]
Head Education: High School Grad	0.40	0.29	0.11	0.26	0.32	-0.06
	(0.008)	(0.000)	[1.24]	(0.030)	(0.000)	[-0.36]
Head Education: Some College	0.06	0.05	0.01	0.09	0.05	0.03
	(0.001)	(0.000)	[0.28]	(0.008)	(0.000)	[0.35]
Head Education: Unknown	0.02	0.07	-0.05	-0.01	0.06	-0.07
	(0.002)	(0.000)	[-1.23]	(0.007)	(0.000)	[-0.83]
Eligibility: Eviction	0.46	0.44	0.01	0.67	0.51	0.15
	(0.010)	(0.000)	[0.12]	(0.061)	(0.000)	[0.62]
Eligibility: Overcrowding	0.12	0.17	-0.05	0.06	0.17	-0.11
	(0.004)	(0.000)	[-0.82]	(0.029)	(0.000)	[-0.66]
Eligibility: Conditions	0.07	0.07	0.01	0.12	0.06	0.06
	(0.002)	(0.000)	[0.19]	(0.013)	(0.000)	[0.49]
Eligibility: DV	0.25	0.24	0.01	0.16	0.17	-0.01
	(0.006)	(0.000)	[0.13]	(0.021)	(0.000)	[-0.07]
Shelter Type: Tier II	0.61	0.54	0.07	0.65	0.52	0.13
	(0.006)	(0.000)	[0.90]	(0.027)	(0.000)	[0.81]
Shelter Type: Commercial Hotel	0.10	0.19	-0.09	-0.04	0.21	-0.26
	(0.004)	(0.000)	[-1.41]	(0.024)	(0.000)	[-1.67]
Shelter Type: Family Cluster	0.25	0.28	-0.03	0.23	0.28	-0.04
	(0.007)	(0.000)	[-0.35]	(0.025)	(0.000)	[-0.26]

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 A.21: Treatment Alternatives Summary

	Primary School (K-8)		High School (9-12)	
	N	Mean	N	Mean
Placed in School Boro	34,429	0.531	8,816	0.477
Placed in Home Boro	34,582	0.469	8,867	0.462
School-Shelter-Home Boro Treatment	29,147	0.492	7,570	0.427
Youngest Placed in Home Boro	34,491	0.469	8,841	0.464
Youngest Placed in School Boro	27,563	0.550	7,762	0.515
Youngest School-Shelter-Home Boro Treatment	23,326	0.493	6,647	0.459

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.

Table A.22: Treatment Correlations: Primary School (K–8)

	School	Home	All	Home (Y)	School (Y)	All (Y)
School	1.000					
Home	0.666	1.000				
All	0.904	0.877	1.000			
Home (Y)	0.666	1.000	0.877	1.000		
School (Y)	0.933	0.640	0.856	0.640	1.000	
All (Y)	0.875	0.883	0.974	0.883	0.881	1.000

School means placed in shelter in school borough (main treatment definition). Home means placed in shelter in borough of most recent residence. All means school, home, and shelter boroughs coincide. denotes treatment based on youngest student in family. Pairwise correlations shown.

Table A.23: Treatment Correlations: High School (9–12)

	School	Home	All	Home (Y)	School (Y)	All (Y)
School	1.000					
Home	0.582	1.000				
All	0.891	0.794	1.000			
Home (Y)	0.582	0.998	0.793	1.000		
School (Y)	0.838	0.609	0.783	0.610	1.000	
All (Y)	0.777	0.838	0.892	0.840	0.884	1.000

School means placed in shelter in school borough (main treatment definition). Home means placed in shelter in borough of most recent residence. All means school, home, and shelter boroughs coincide. denotes treatment based on youngest student in family. Pairwise correlations shown.

Table A.24: Primary School (K-8) School District Results

	OLS				IV			
	(1) Base	(2) Main	(3) Lag	(4) FE	(5) Base	(6) Main	(7) Lag	(8) FE
Days Absent	-3.0**	-2.6**	-2.3**	-2.6**	-150.8	-156.0	-198.2	-224.8
	(0.4)	(0.4)	(0.4)	(0.4)	(110.9)	(122.5)	(310.6)	(275.6)
	-	-	-	-	[2.1]	[1.8]	[0.4]	[0.7]
Absence Rate	-0.012**	-0.011**	-0.013**	-0.011**	-0.884	-0.940	-1.082	-1.315
	(0.003)	(0.003)	(0.002)	(0.003)	(0.668)	(0.753)	(1.712)	(1.637)
	-	-	-	-	[2.1]	[1.8]	[0.4]	[0.7]
Math Proficient	0.012	0.010	0.017*	0.012	0.698	0.754	0.832	0.663
	(0.009)	(0.009)	(0.010)	(0.009)	(0.714)	(0.702)	(1.646)	(0.775)
	-	-	-	-	[2.6]	[2.8]	[0.6]	[2.1]
ELA Proficient	0.010	0.004	0.012	0.004	0.348	0.428	0.274	0.315
	(0.008)	(0.008)	(0.009)	(0.008)	(0.585)	(0.574)	(1.224)	(0.632)
	-	-	-	-	[2.6]	[2.8]	[0.6]	[2.1]
Proficient	0.016**	0.013*	0.017**	0.013*	0.506	0.504	0.372	0.535
	(0.007)	(0.007)	(0.008)	(0.007)	(0.510)	(0.489)	(1.013)	(0.582)
	-	-	-	-	[2.6]	[2.8]	[0.6]	[2.1]
Admission	-0.066**	-0.072**	-0.122**	-0.074**	0.678	0.513	0.483	1.318
	(0.011)	(0.010)	(0.011)	(0.010)	(1.245)	(1.272)	(2.581)	(2.648)
	-	-	-	-	[1.8]	[1.5]	[0.4]	[0.6]
Promoted	-0.005	-0.005	-0.004	-0.004	0.504	0.605	0.514	1.176
	(0.005)	(0.005)	(0.006)	(0.005)	(0.603)	(0.713)	(0.971)	(1.826)
	-	-	-	-	[2.0]	[1.7]	[0.8]	[0.6]
Left DOE	0.006	-0.002	-0.011*	-0.001	-0.953	-1.194	-1.644	-1.972
	(0.007)	(0.006)	(0.006)	(0.006)	(0.977)	(1.206)	(2.850)	(2.905)
	-	-	-	-	[1.8]	[1.5]	[0.4]	[0.6]
Obs.	33,866	33,846	26,464	33,762	33,843	33,824	26,447	33,739
Base Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Main Covariates	No	Yes	Yes	No	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 treatment is defined as shelter placement within school district of origin. Each cell reports the coefficient on in-school-district 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 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 A.25: Primary School (K-8): Home Borough Treatment

	OLS				IV			
	(1) Base	(2) Main	(3) Lag	(4) FE	(5) Base	(6) Main	(7) Lag	(8) FE
Days Absent	-2.4**	-2.1**	-2.1**	-2.2**	-19.7**	-20.5**	-15.5**	-24.4**
	(0.3)	(0.3)	(0.3)	(0.3)	(7.2)	(7.2)	(6.3)	(9.8)
	-	-	-	-	[36.0]	[34.9]	[28.7]	[22.1]
Absence Rate	-0.016**	-0.013**	-0.014**	-0.014**	-0.106**	-0.112**	-0.072**	-0.135**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.042)	(0.042)	(0.035)	(0.057)
	-	-	-	-	[36.0]	[34.9]	[28.7]	[22.1]
Math Proficient	0.005	0.004	0.003	0.004	0.078	0.119	0.056	0.138
	(0.006)	(0.006)	(0.007)	(0.007)	(0.127)	(0.127)	(0.141)	(0.168)
	-	-	-	-	[21.4]	[21.3]	[17.2]	[14.1]
ELA Proficient	0.007	0.003	0.002	0.005	0.062	0.109	0.070	0.115
	(0.006)	(0.006)	(0.006)	(0.006)	(0.122)	(0.121)	(0.134)	(0.161)
	-	-	-	-	[21.4]	[21.3]	[17.2]	[14.1]
Proficient	0.003	0.001	0.001	0.001	0.095	0.116	0.075	0.159
	(0.005)	(0.005)	(0.005)	(0.005)	(0.093)	(0.094)	(0.104)	(0.129)
	-	-	-	-	[21.4]	[21.3]	[17.2]	[14.1]
Changed School	-0.124**	-0.105**	-0.107**	-0.105**	-0.042	-0.018	0.077	0.124
	(0.008)	(0.008)	(0.008)	(0.008)	(0.165)	(0.166)	(0.186)	(0.219)
	-	-	-	-	[35.7]	[34.4]	[27.8]	[21.4]
Promoted	0.002	0.001	0.001	0.003	0.066	0.074	0.030	0.117
	(0.004)	(0.004)	(0.004)	(0.004)	(0.075)	(0.077)	(0.083)	(0.111)
	-	-	-	-	[33.2]	[32.0]	[24.8]	[18.2]
Left DOE	-0.008*	-0.009**	-0.006	-0.009*	-0.152*	-0.172*	-0.079	-0.244*
	(0.004)	(0.004)	(0.004)	(0.005)	(0.090)	(0.092)	(0.091)	(0.128)
	-	-	-	-	[35.7]	[34.4]	[27.8]	[21.4]
Obs.	28,932	28,918	23,663	28,829	28,918	28,904	23,653	28,814
Base Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Main Covariates	No	Yes	Yes	No	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 treatment is defined as shelter placement within residential borough of origin, defined by most recent address. 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 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 A.26: Compliance Type Shares: Days to Eligibility Instrument

	Primary School (K-8)			High School (9-12)		
	1%	1.5%	2%	1%	1.5%	2%
Compliers	0.14	0.14	0.13	0.14	0.13	0.13
Always-Takers	0.62	0.62	0.63	0.59	0.59	0.59
Never-Takers	0.24	0.24	0.24	0.28	0.28	0.28

Repeats Table A.19 for days-to-eligibility instrument. Main sample. Results from linear first-stage, controlling for year and month of shelter entry. Percentages in second row refer to percentiles used as thresholds to define low and high instrument values. See Appendix C.4 for estimation method details.

Table A.27A: Complier Characteristics, Days-to-Eligibility Instrument

	Primary School (K-8)				High School (9-12)			
	Compliers	Non-Compliers	Diff.	T-Stat	Compliers	Non-Compliers	Diff.	T-Stat
Elementary School	0.74	0.73	0.01	0.21	.	.	.	.
	(0.002)	(0.000)			(.)	(.)		
Middle School	0.26	0.27	-0.01	-0.21	.	.	.	.
	(0.003)	(0.000)			(.)	(.)		
Promoted Prior Year	0.96	0.91	0.05	1.30	0.83	0.75	0.08	0.24
	(0.002)	(0.000)			(0.117)	(0.000)		
Proficient Prior Year	0.13	0.10	0.03	0.41	-0.14	0.10	-0.24	-0.18
	(0.006)	(0.000)			(1.748)	(0.001)		
School Change Prior Year	0.23	0.37	-0.14	-1.61	0.31	0.36	-0.05	-0.35
	(0.007)	(0.000)			(0.019)	(0.000)		
Admission Prior Year	0.23	0.37	-0.13	-1.63	0.14	0.23	-0.09	-0.79
	(0.007)	(0.000)			(0.012)	(0.000)		
Took Regents Prior Year	0.00	0.04	-0.04	-0.01	0.61	0.52	0.09	0.70
	(20.721)	(0.014)			(0.016)	(0.000)		
Passed Regents Prior Year	.	.	.	.	0.47	0.31	0.16	1.30
	(.)	(.)			(0.014)	(0.000)		
On CA	0.42	0.35	0.08	0.91	0.27	0.32	-0.05	-0.34
	(0.007)	(0.000)			(0.022)	(0.000)		
On SNAP	0.74	0.71	0.03	0.32	0.66	0.68	-0.02	-0.16
	(0.007)	(0.000)			(0.022)	(0.000)		
Employed	0.44	0.37	0.08	0.87	0.55	0.39	0.17	1.04
	(0.008)	(0.000)			(0.025)	(0.000)		
Head Education: Less Than High School	0.45	0.61	-0.16	-1.73	0.66	0.57	0.10	0.61
	(0.008)	(0.000)			(0.024)	(0.000)		
Head Education: High School Grad	0.50	0.27	0.23	2.34	0.31	0.31	-0.00	-0.01
	(0.009)	(0.000)			(0.023)	(0.000)		
Head Education: Some College	0.05	0.05	-0.00	-0.09	0.05	0.06	-0.01	-0.08
	(0.001)	(0.000)			(0.005)	(0.000)		
Head Education: Unknown	0.02	0.07	-0.05	-1.15	-0.01	0.06	-0.07	-1.11
	(0.002)	(0.000)			(0.004)	(0.000)		
Health Issue	0.33	0.33	-0.00	-0.01	0.51	0.36	0.15	0.99
	(0.005)	(0.000)			(0.023)	(0.000)		
Partner Present	0.26	0.28	-0.02	-0.26	0.27	0.21	0.06	0.48
	(0.005)	(0.000)			(0.015)	(0.000)		
Pregnant	0.05	0.04	0.01	0.25	0.01	0.03	-0.02	-0.48
	(0.001)	(0.000)			(0.002)	(0.000)		
Eligibility: Eviction	0.43	0.45	-0.02	-0.21	0.66	0.51	0.15	1.00
	(0.009)	(0.000)			(0.023)	(0.000)		
Eligibility: Overcrowding	0.10	0.18	-0.08	-1.35	0.03	0.18	-0.15	-1.22
	(0.004)	(0.000)			(0.016)	(0.000)		
Eligibility: Conditions	0.12	0.06	0.06	1.60	0.10	0.07	0.04	0.46
	(0.001)	(0.000)			(0.007)	(0.000)		
Eligibility: DV	0.24	0.24	0.00	0.06	0.20	0.17	0.03	0.30
	(0.005)	(0.000)			(0.013)	(0.000)		
ELL	0.09	0.10	-0.01	-0.32	0.02	0.11	-0.09	-1.07
	(0.002)	(0.000)			(0.007)	(0.000)		
Non-English	0.19	0.17	0.02	0.33	0.12	0.24	-0.11	-0.90
	(0.003)	(0.000)			(0.015)	(0.000)		
Foreign-Born	0.05	0.05	-0.01	-0.15	0.04	0.11	-0.07	-0.80
	(0.001)	(0.000)			(0.008)	(0.000)		
IEP	0.26	0.23	0.03	0.52	0.24	0.22	0.02	0.15
	(0.002)	(0.000)			(0.015)	(0.000)		
Female	0.48	0.51	-0.02	-0.36	0.46	0.55	-0.09	-0.67
	(0.004)	(0.000)			(0.019)	(0.000)		
Black	0.47	0.54	-0.06	-0.79	0.49	0.58	-0.09	-0.55
	(0.006)	(0.000)			(0.024)	(0.000)		
Hispanic	0.48	0.42	0.06	0.79	0.41	0.39	0.02	0.12
	(0.006)	(0.000)			(0.031)	(0.000)		
White	0.00	0.03	-0.02	-0.90	0.06	0.02	0.04	0.88
	(0.001)	(0.000)			(0.002)	(0.000)		

Repeats Table 5 for days-to-eligibility instrument. Main sample. Treatment is in-borough placement. Instrument is 15-day moving average average days to eligibility for 30-day application period. Compliers are those students placed in-borough when DTE 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 and differences in means are calculated from 200 bootstrap replications.

Table A.27B: Complier Characteristics, Days-to-Eligibility Instrument

	Primary School (K-8)				High School (9-12)			
	Compliers	Non-Compliers	Diff.	T-Stat	Compliers	Non-Compliers	Diff.	T-Stat
Shelter Type: Tier II	.	.	.	.	0.62	0.52	0.09	0.65
	(.)	(.)			(0.020)	(0.000)		
Shelter Type: Commerical Hotel	0.14	0.18	-0.04	-0.80	-0.06	0.22	-0.28	-1.90
	(0.003)	(0.000)			(0.021)	(0.000)		
Shelter Type: Family Cluster	0.18	0.29	-0.10	-1.33	0.31	0.26	0.05	0.37
	(0.006)	(0.000)			(0.019)	(0.000)		
School Borough: Manhattan	0.04	0.13	-0.10	-2.08	0.23	0.18	0.05	0.41
	(0.002)	(0.000)			(0.012)	(0.000)		
School Borough: Bronx	0.40	0.39	0.01	0.10	0.32	0.34	-0.03	-0.17
	(0.007)	(0.000)			(0.025)	(0.001)		
School Borough: Brooklyn	0.42	0.31	0.11	1.41	0.32	0.31	0.02	0.11
	(0.006)	(0.000)			(0.024)	(0.000)		
School Borough: Queens	0.09	0.14	-0.04	-0.89	-0.01	0.16	-0.16	-1.57
	(0.002)	(0.000)			(0.011)	(0.000)		
School Borough: Staten Island	0.01	0.03	-0.02	-1.37	0.05	0.02	0.03	0.95
	(0.000)	(0.000)			(0.001)	(0.000)		
Household Size: 1-3	0.29	0.34	-0.05	-0.77	0.36	0.39	-0.03	-0.19
	(0.004)	(0.000)			(0.023)	(0.000)		
Household Size: 4-5	0.62	0.40	0.21	2.35	0.56	0.38	0.18	1.18
	(0.008)	(0.000)			(0.024)	(0.000)		
Household Size: 6+	0.10	0.25	-0.15	-1.81	0.12	0.23	-0.10	-0.75
	(0.007)	(0.000)			(0.018)	(0.000)		
1 Student in Family	0.24	0.30	-0.06	-0.93	0.20	0.30	-0.11	-0.81
	(0.004)	(0.000)			(0.017)	(0.000)		
$>$ 1 Students in Family	0.75	0.70	0.05	0.78	0.81	0.70	0.11	0.84
	(0.004)	(0.000)			(0.017)	(0.000)		

Repeats Table 5 for days-to-eligibility instrument. Main sample. Treatment is in-borough placement. Instrument is 15-day moving average average days to eligibility for 30-day application period. Compliers are those students placed in-borough when DTE 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 and differences in means are calculated from 200 bootstrap replications.

G Supplementary Figures

G.1 Stylized Facts

Figure A.1: Homeless Primary School Student Absences by Year

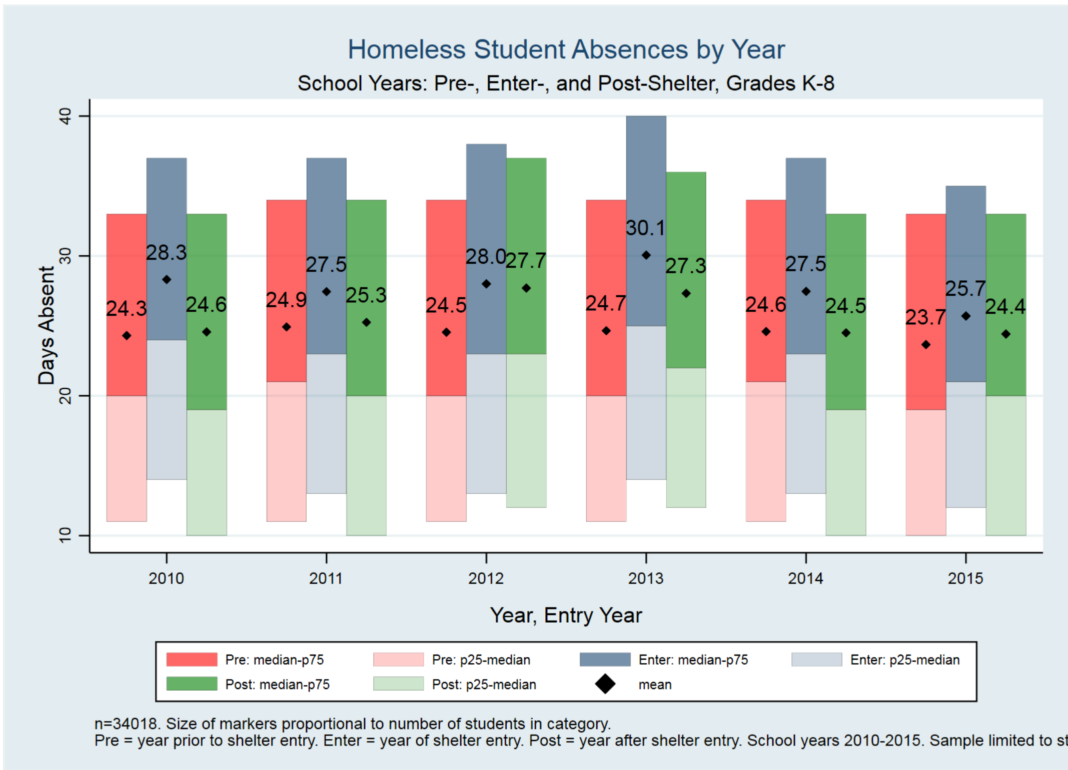


Figure A.2: Homeless High School Student Absences by Year

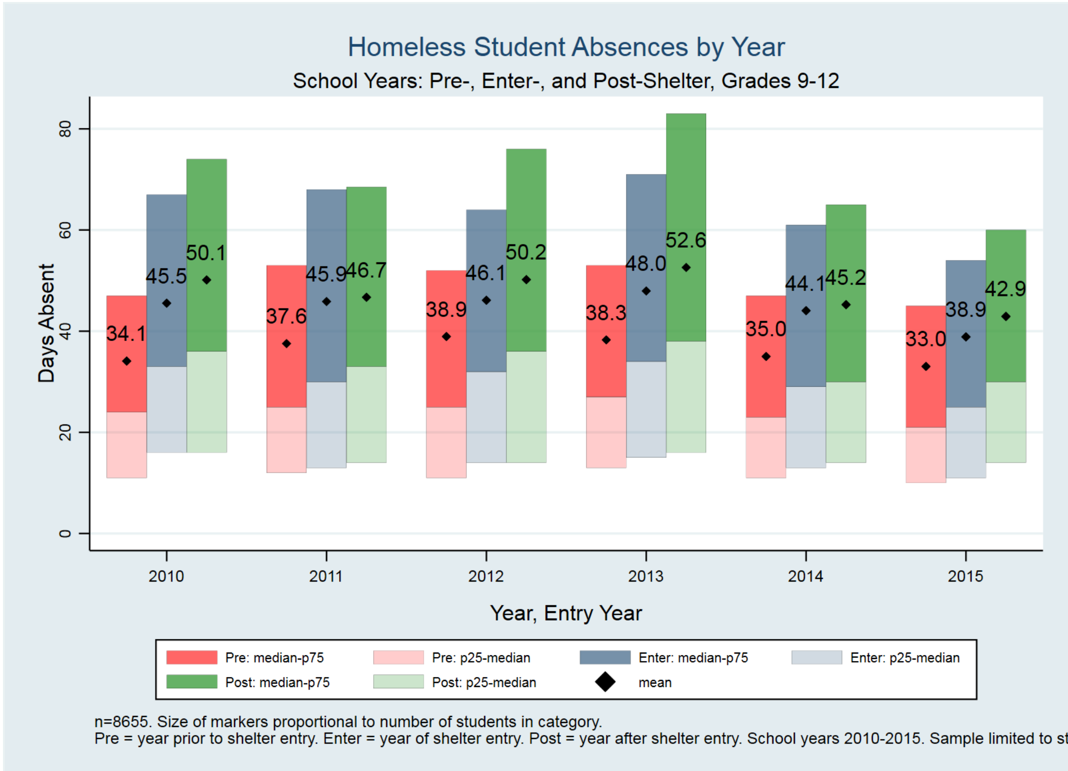


Figure A.3: Absence Persistence Summary

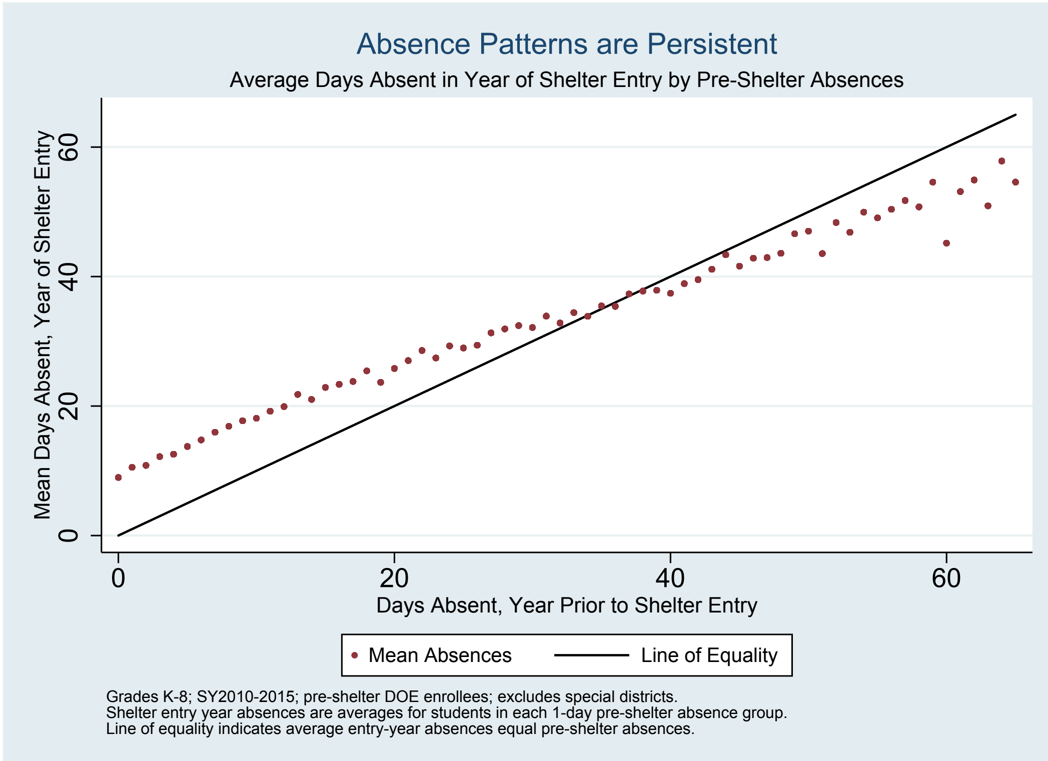


Figure A.4: Absence Persistence Detail

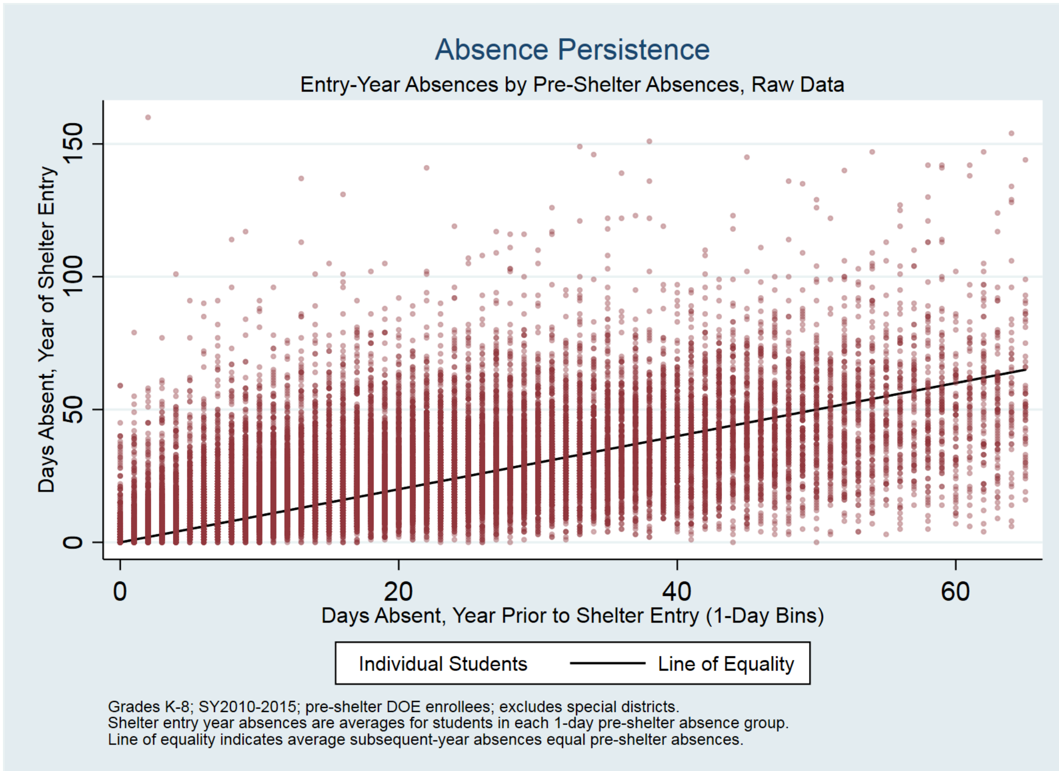


Figure A.5: Absences by Grade

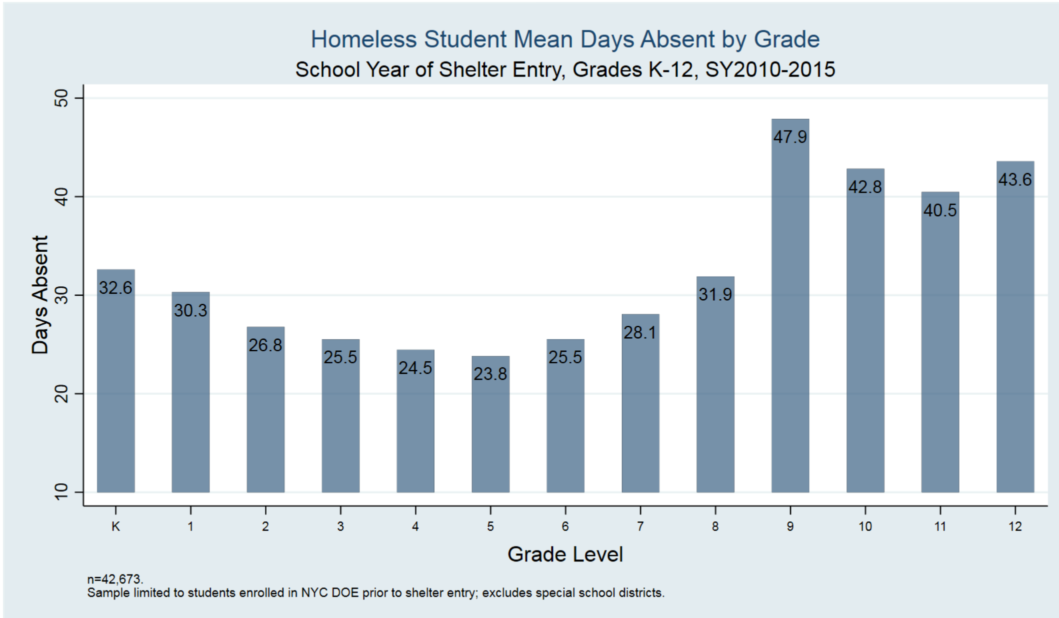


Figure A.6: Attendance and Proficiency

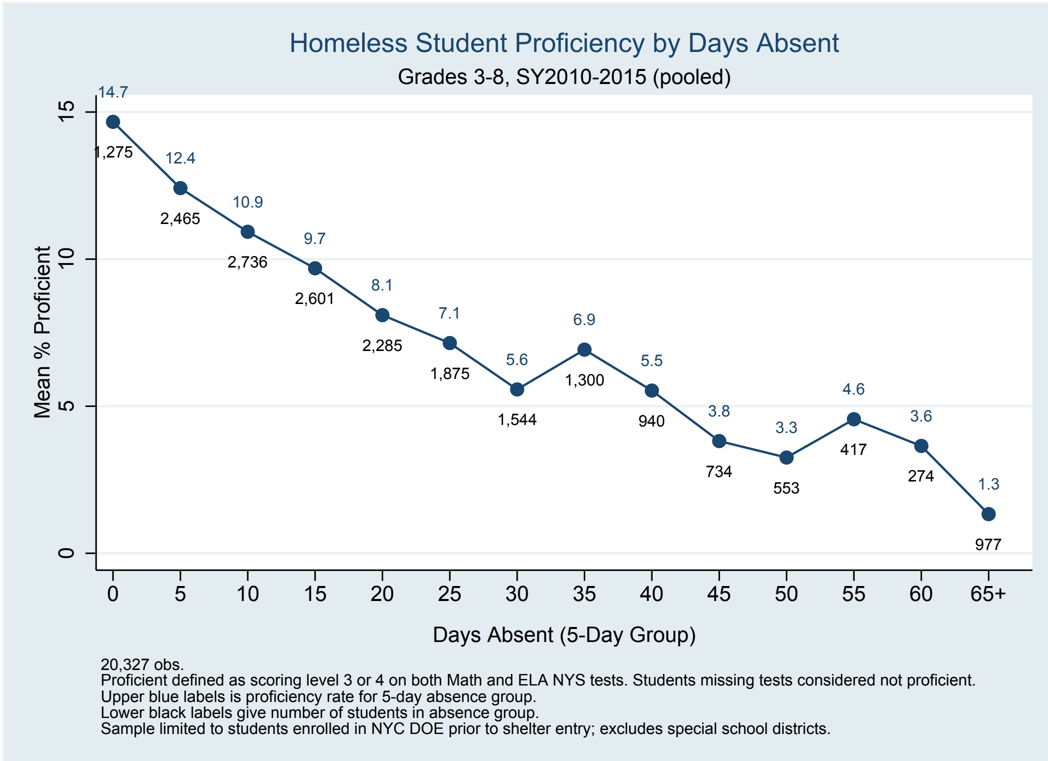


Figure A.7: Attendance and Promotion

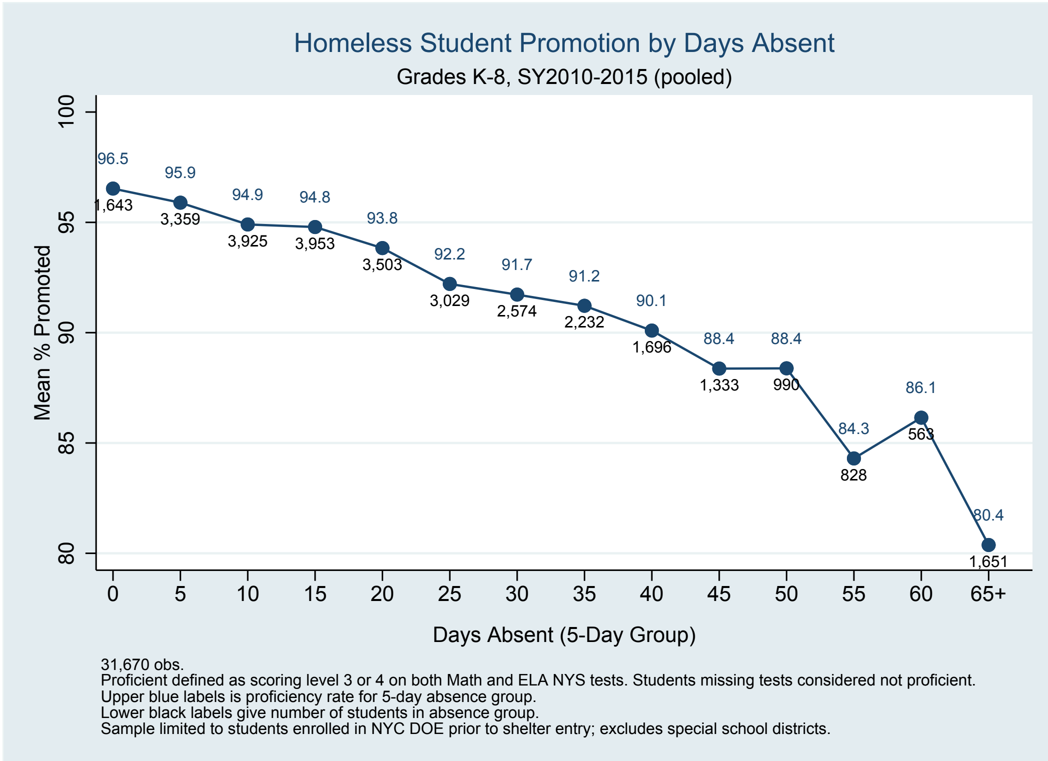


Figure A.8: NYC Public School Proficiency Rates

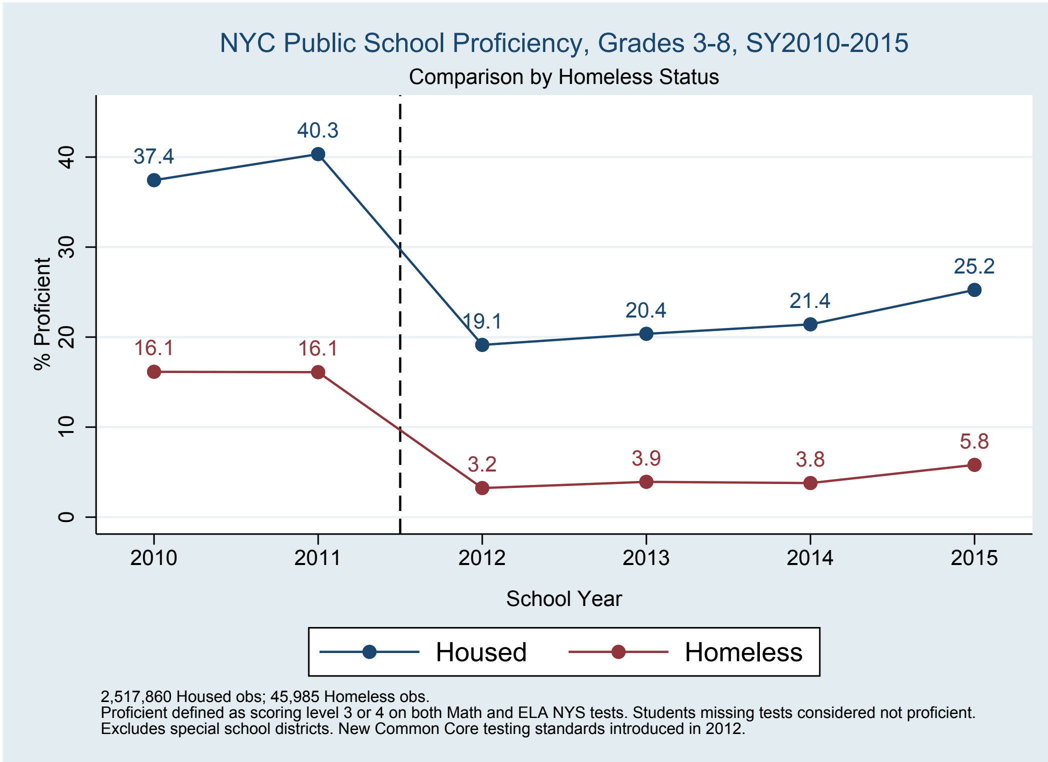
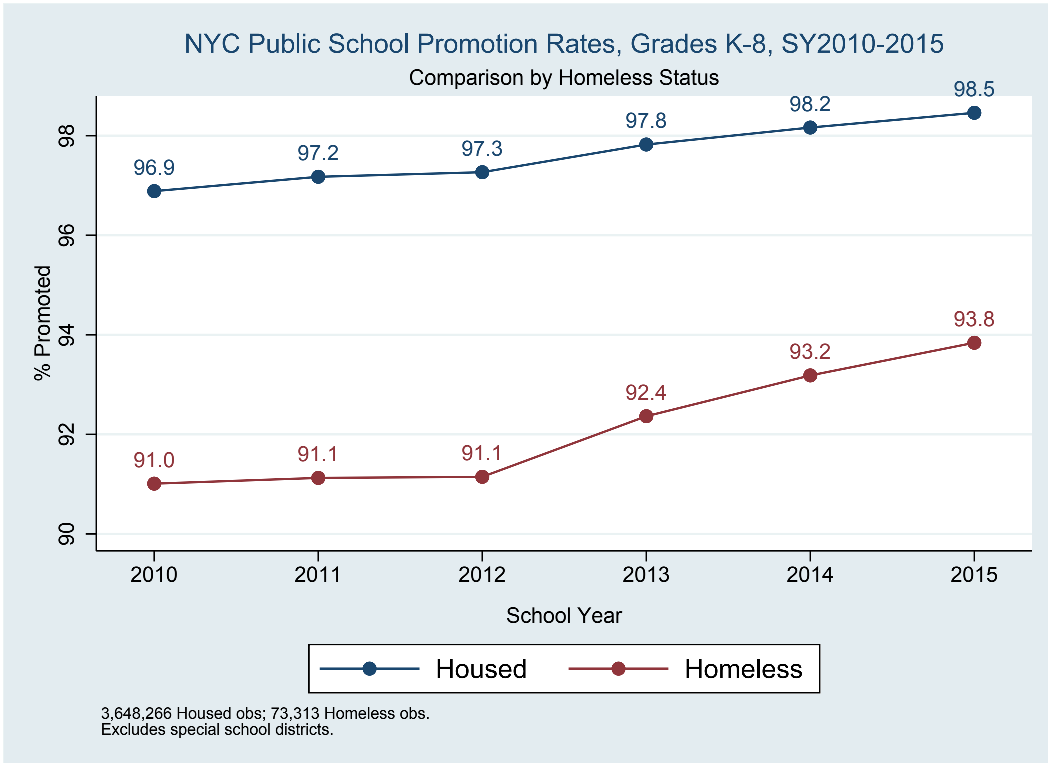


Figure A.9: NYC Public School Promotion Rates



G.2 Instrument Assessment

Figure A.10: Instrument and Treatment Quarterly Time Series: Raw

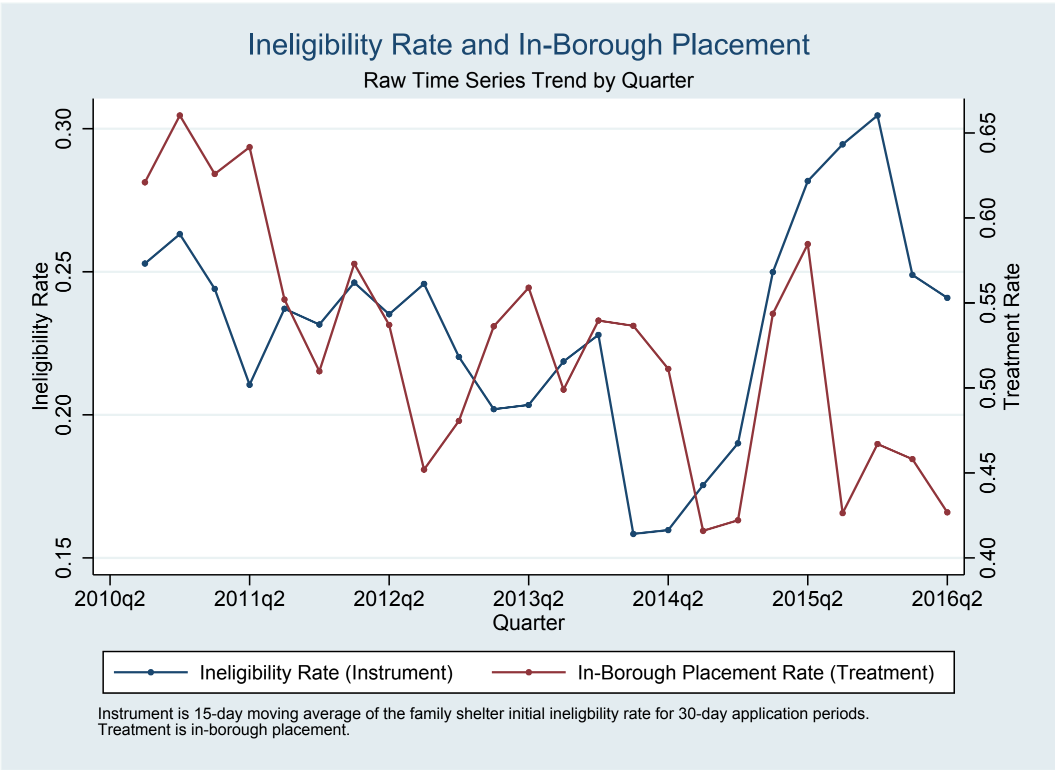


Figure A.11: Instrument and Treatment: Raw

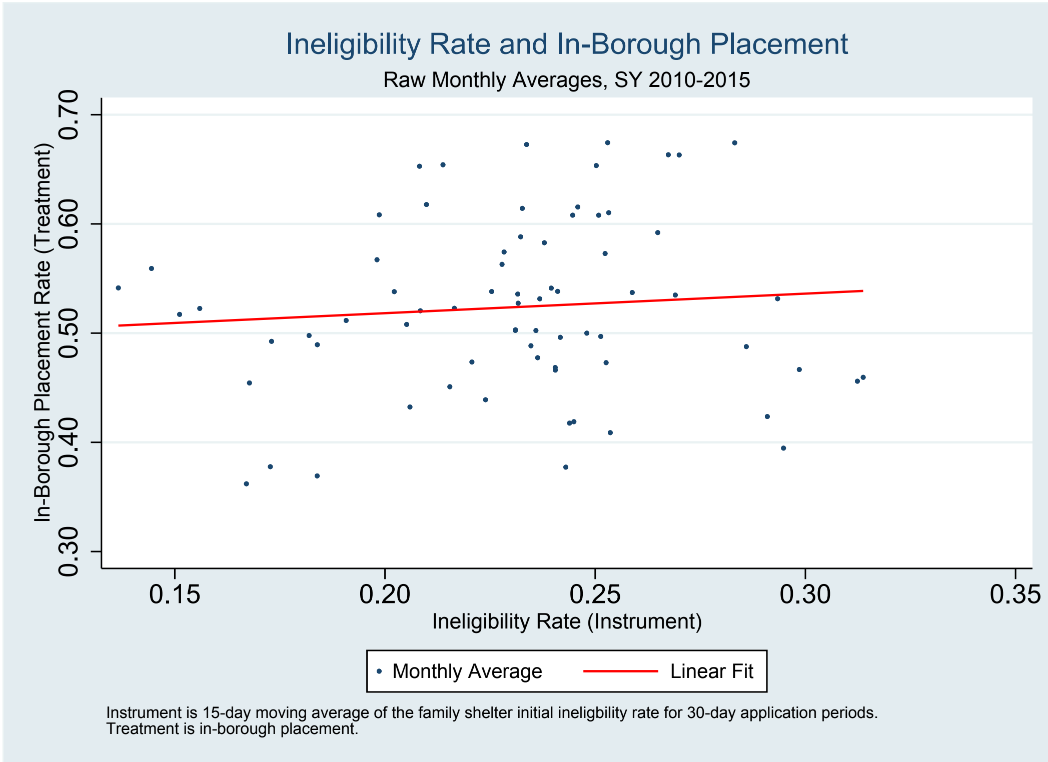


Figure A.12: Instrument and Treatment: Detrended

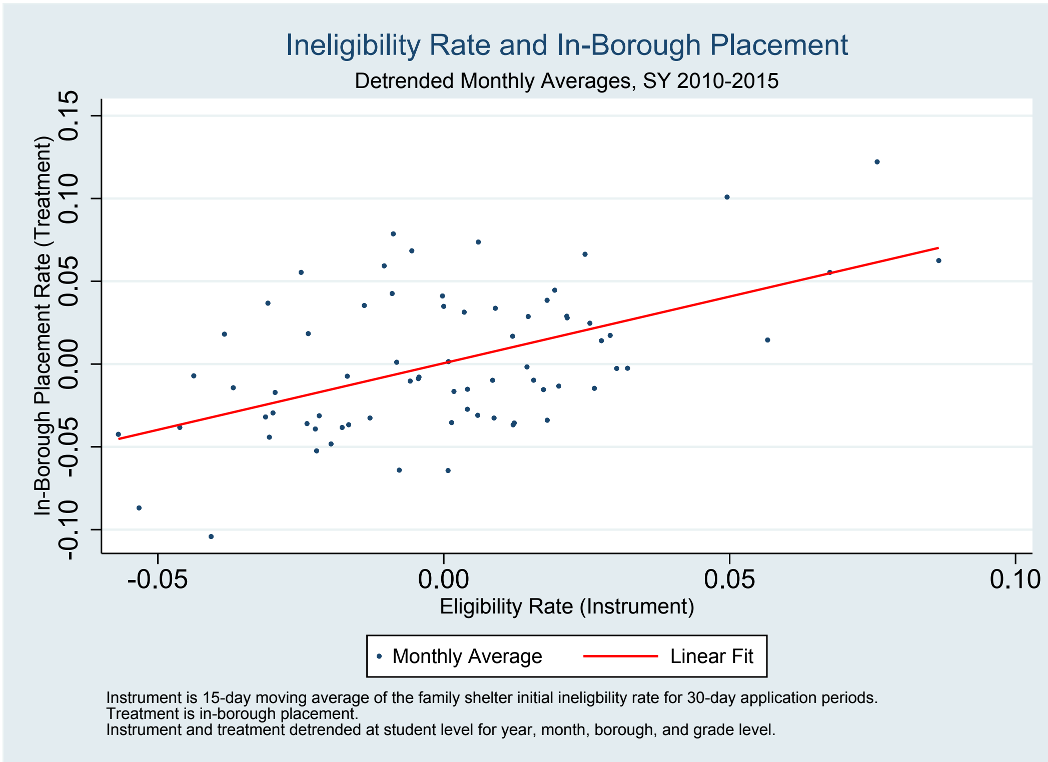


Figure A.13: Family Shelter Application Outcomes

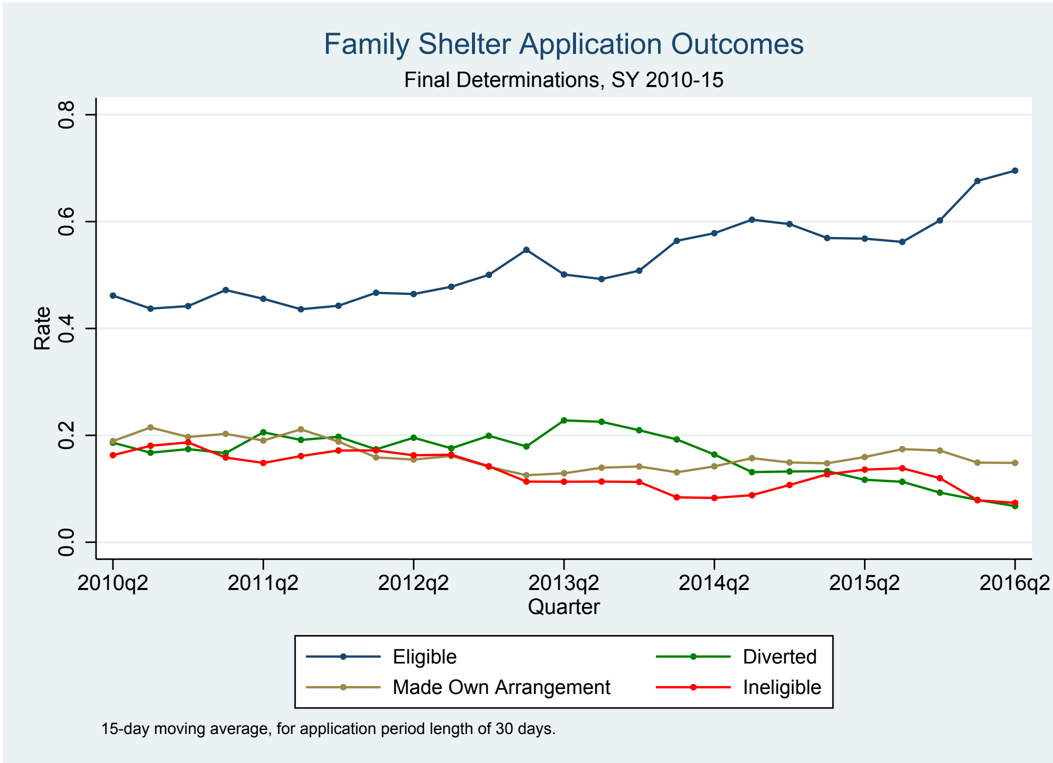


Figure A.14: Initial Ineligibility and Final Eligibility

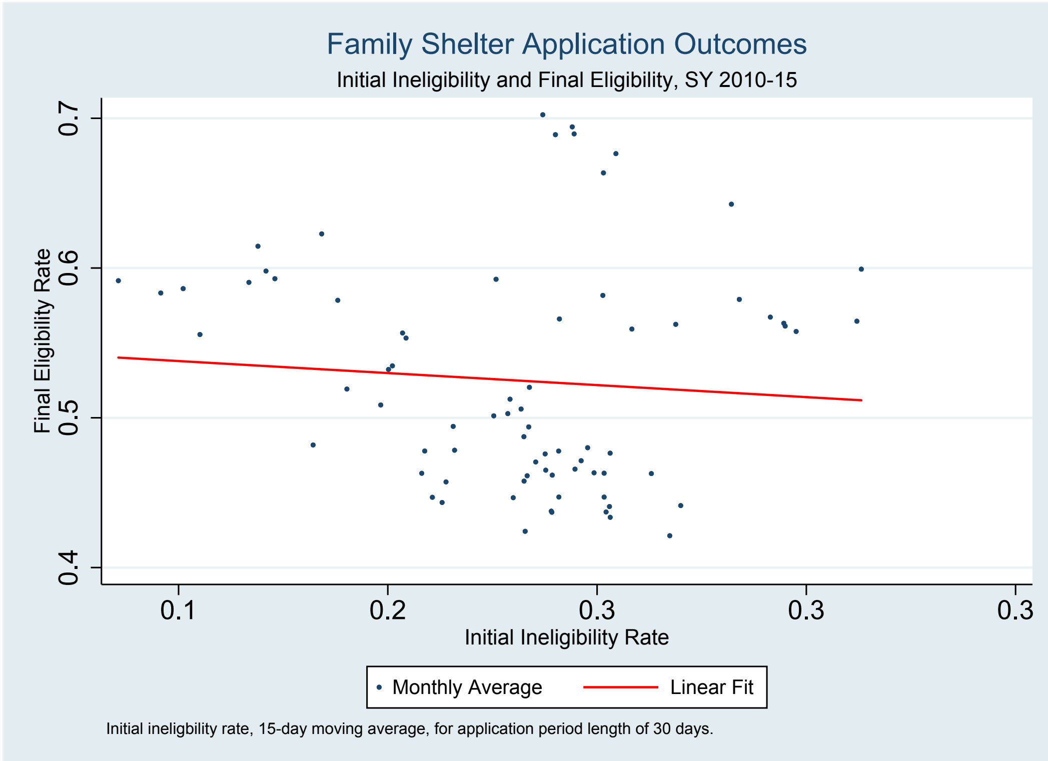


Figure A.15: Final Ineligibility and Final Eligibility

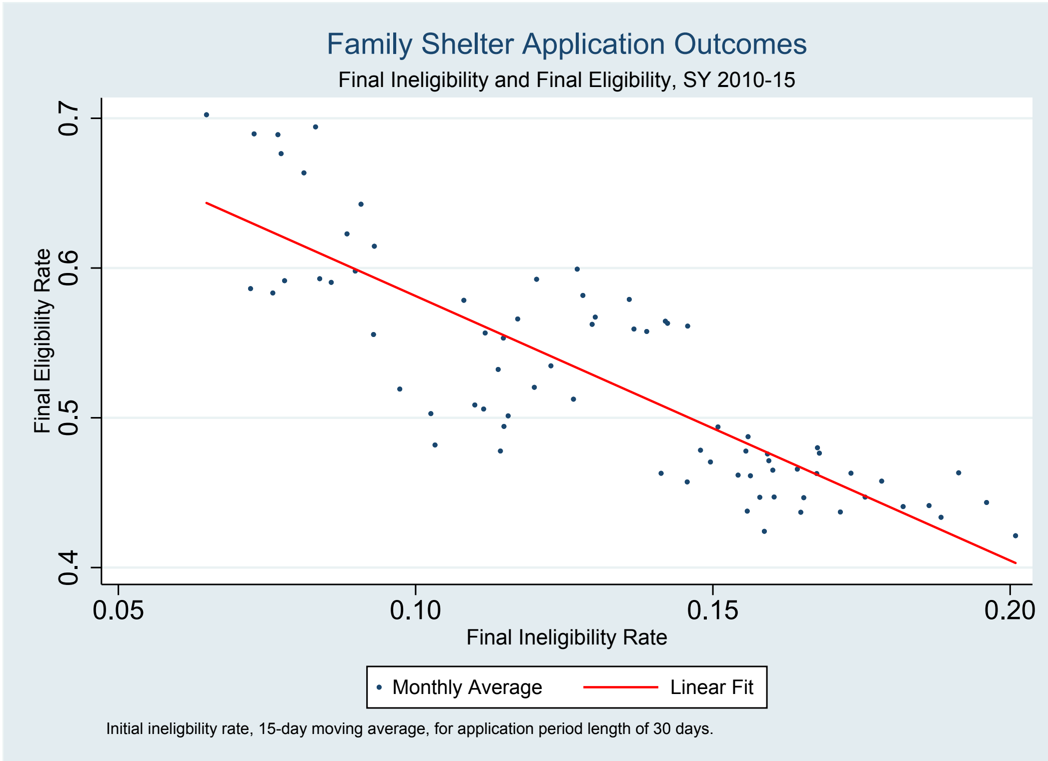
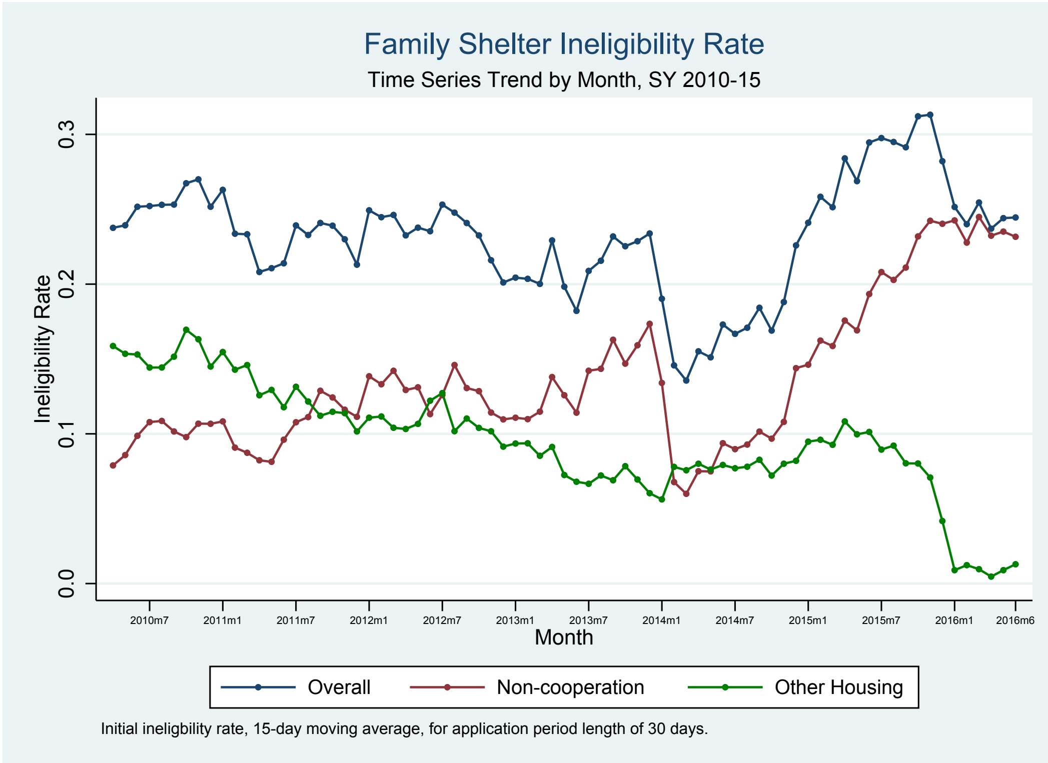
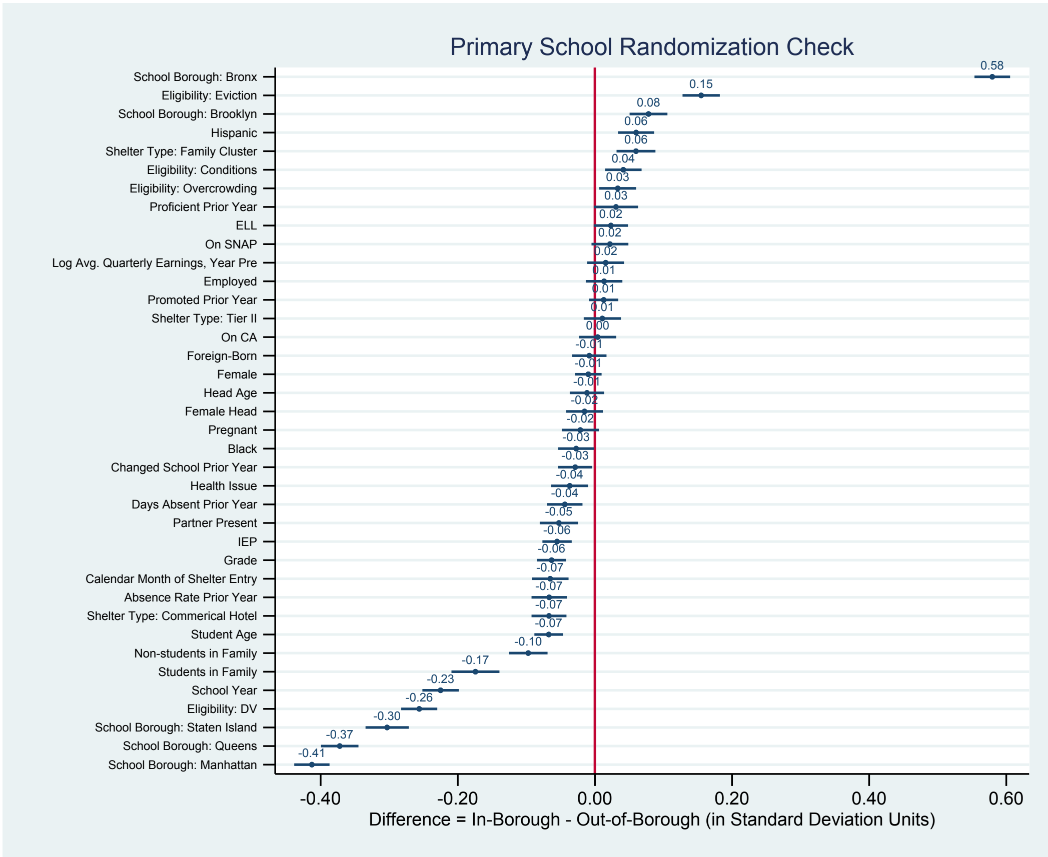


Figure A.16: Ineligibility Rate Details



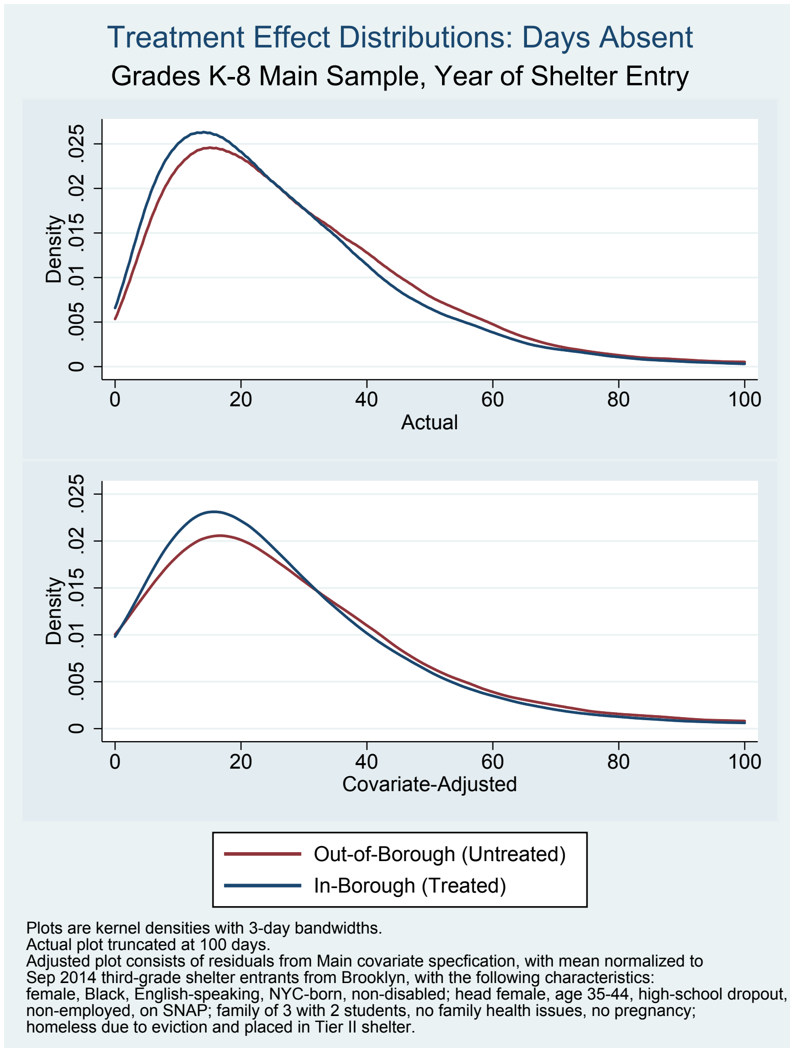
G.3 Results Supplement

Figure A.17: Randomization Check



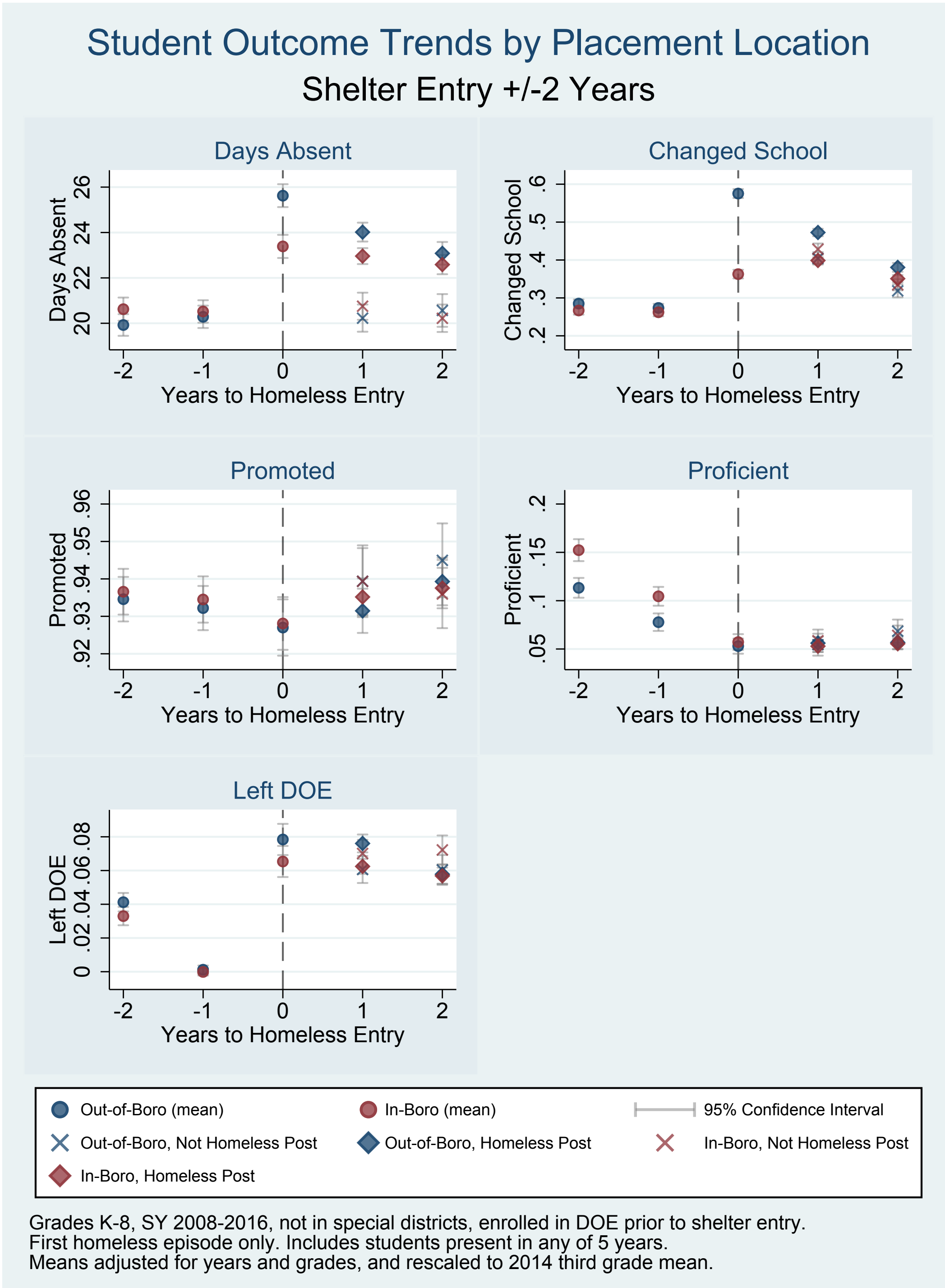
Notes: Graphical depiction of primary school results from Table 3A. Plot gives coefficient on in-borough treatment indicator, scaled in standard deviation units, from separate bivariate OLS regressions of each characteristic on the treatment indicator. Bars give 95 percent confidence intervals; standard errors clustered at the family group level.

Figure A.18: Days Absent Treatment Effect Distribution



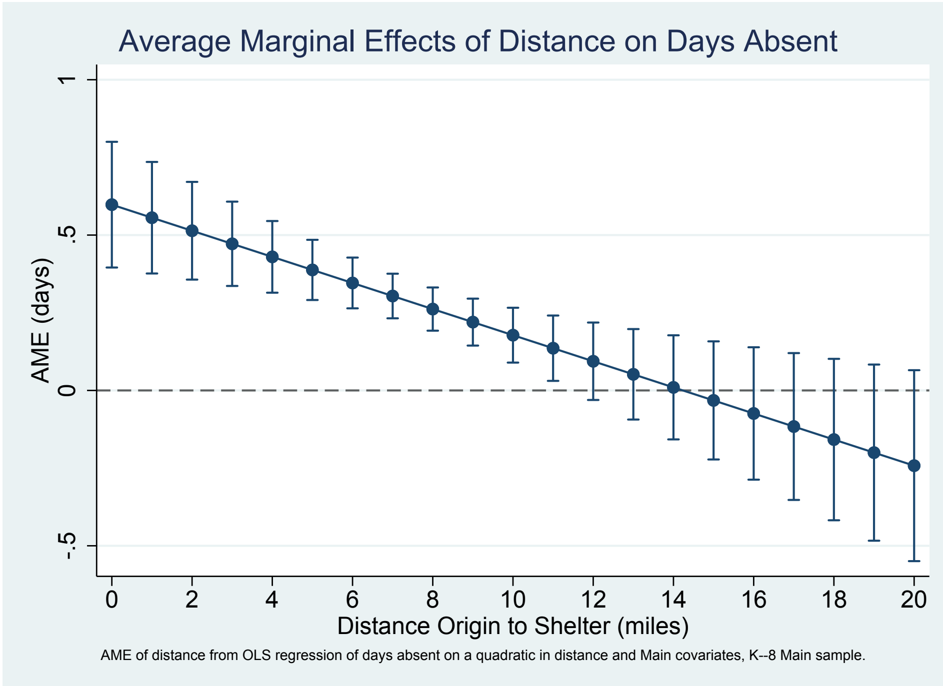
Notes: Plots are kernel densities with 3-day bandwidths. Main sample, grades K–8. Actual plot truncated at 100 days. Adjusted plot consists of residuals from Main covariate specification, with mean normalized to Sep 2014 third-grade shelter entrants from Brooklyn, with the following characteristics: female, Black, English-speaking, NYC-born, non-disabled; head female, age 35-44, high-school dropout, non-employed, on SNAP; family of 3 with 2 students, no family health issues, no pregnancy; homeless due to eviction and placed in Tier II shelter.

Figure A.19: Five-Year Student Outcome Trends by Placement



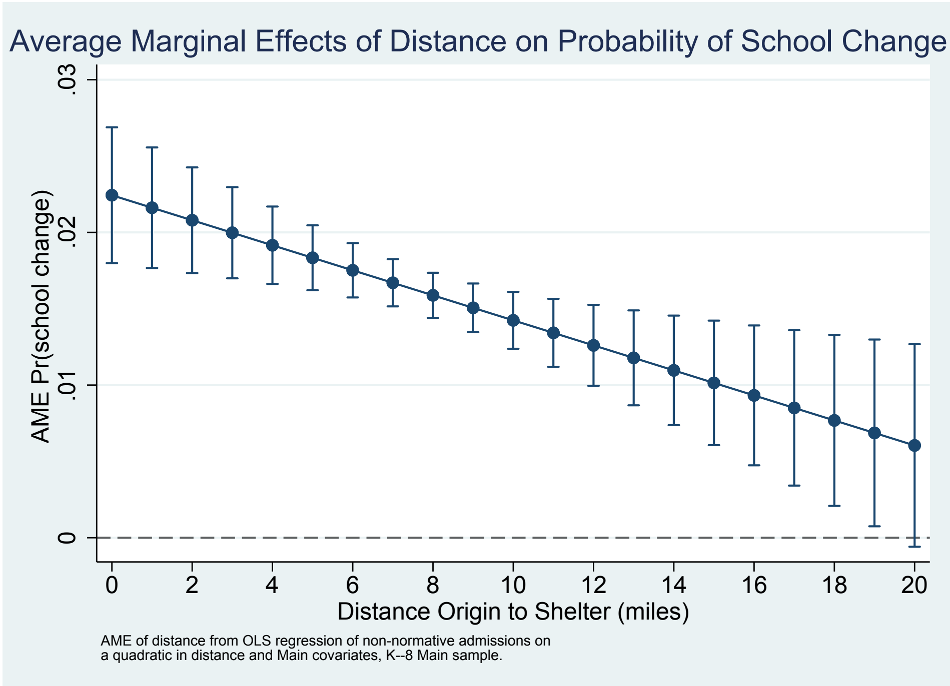
Notes: Grades K-8, SY 2008-2016, not in special districts, enrolled in DOE prior to shelter entry. First homeless episode only. Includes students present in any of 5 years. Means adjusted for years and grades, and rescaled to 2014 third grade mean.

Figure A.20: Average Marginal Effects of Distance on Days Absent



Notes: Plot presents average marginal effects of school-shelter distance from OLS regression of days absent on a quadratic in distance and Main covariates, using K-8 Main sample. Standard errors clustered at family group level. Bars indicate 95 percent confidence intervals.

Figure A.21: Average Marginal Effects of Distance on School Changes



Notes: Plot presents average marginal effects of school-shelter distance from OLS regression of an indicator for school change on a quadratic in distance and Main covariates, using K-8 Main sample. Standard errors clustered at family group level. Bars indicate 95 percent confidence intervals.