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

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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 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

[1]For details, see Cassidy (2019).

[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.

[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.

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 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 prob-

[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.

lems. 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

[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).

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 (2019).

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 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

[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).

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 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

[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).

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).

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

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 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 (2019), 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 (2019).

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 (2019), 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 (2019)); 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 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 (2019) .

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

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

[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.

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 (2019), 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 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

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

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. 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.

[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.

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 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

[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.

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, 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 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

[21] See Angrist and Pischke (2008).

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 E.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 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

[22]By contrast, in Cassidy (2019), 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.

[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

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.)

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:

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

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.

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.

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 $R_d - P_dS^* > 0$, i.e., $\underbrace{-P_dS^*}_{\text{savings}} > \underbrace{-R_d}_{\text{resource loss}}$, $\frac{\partial S^*}{\partial d} > 0$. In this case, the lower cost of school more than offsets the resource loss, so schooling increases.

[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.

(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

[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.

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 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

[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.

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 day.

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

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

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].

[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.

[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—

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

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

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: $N_{1i} \geq N_{0i} \ \ \forall i$

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.

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.

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

[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.

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 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 $Z^{DTE}_{if,d}$ is:

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

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,

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

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 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].

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

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, 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 (2019), 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.

upon request.

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

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.

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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E Supplementary Tables

E.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

	Students			Episodes		
Year of Birth	Obs	Matched	Match Rate	Obs	Matched	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

Times Observed	Observations (Student-Years)			Students		
	(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

Notes.

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.

E.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.

E.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.20: 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.20 (Cont.): 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.27: 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.27 (Cont.): 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.

F Supplementary Figures

F.1 Stylized Facts

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

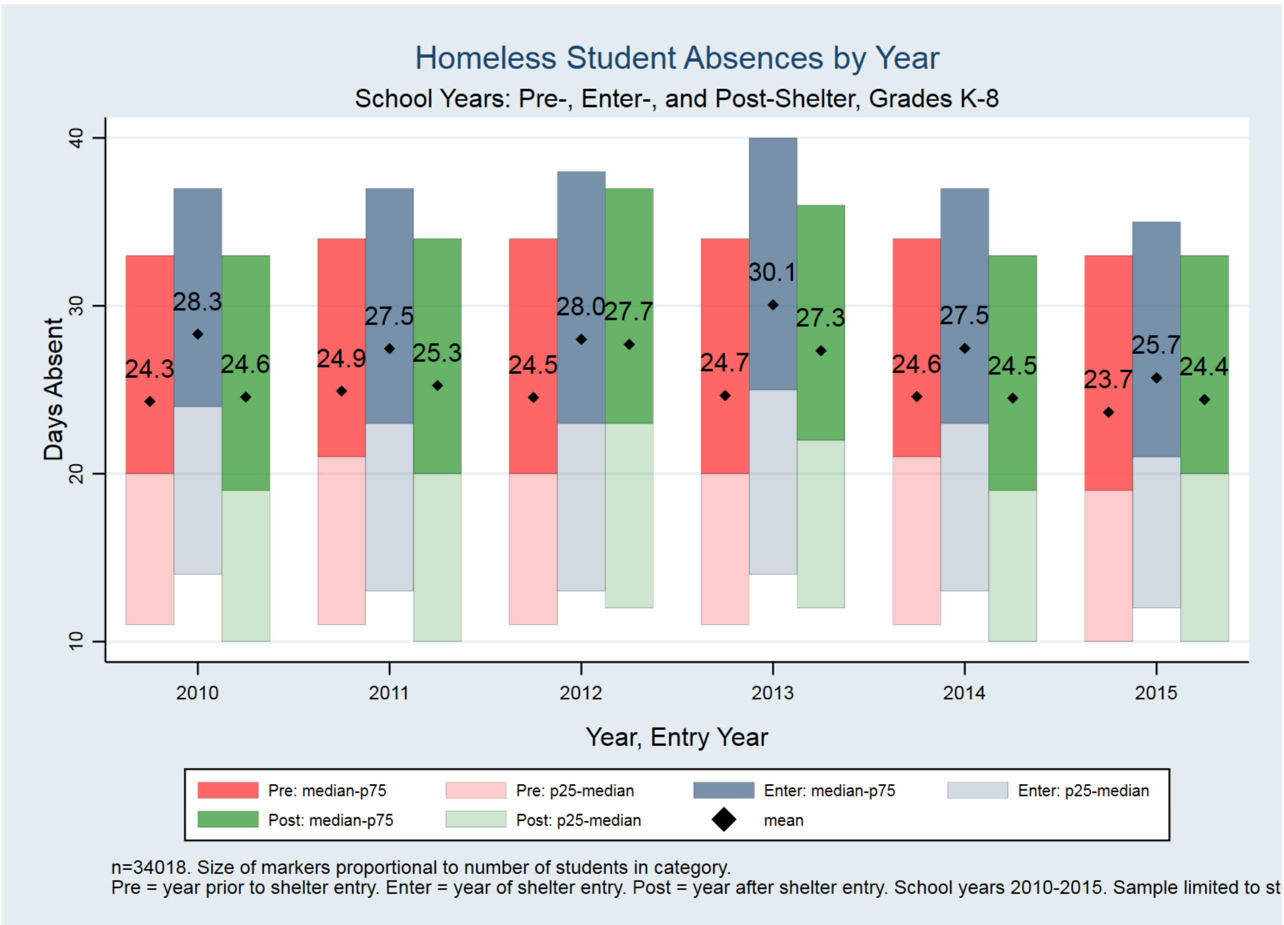


Figure A.2: Homeless High School Student Absences by Year

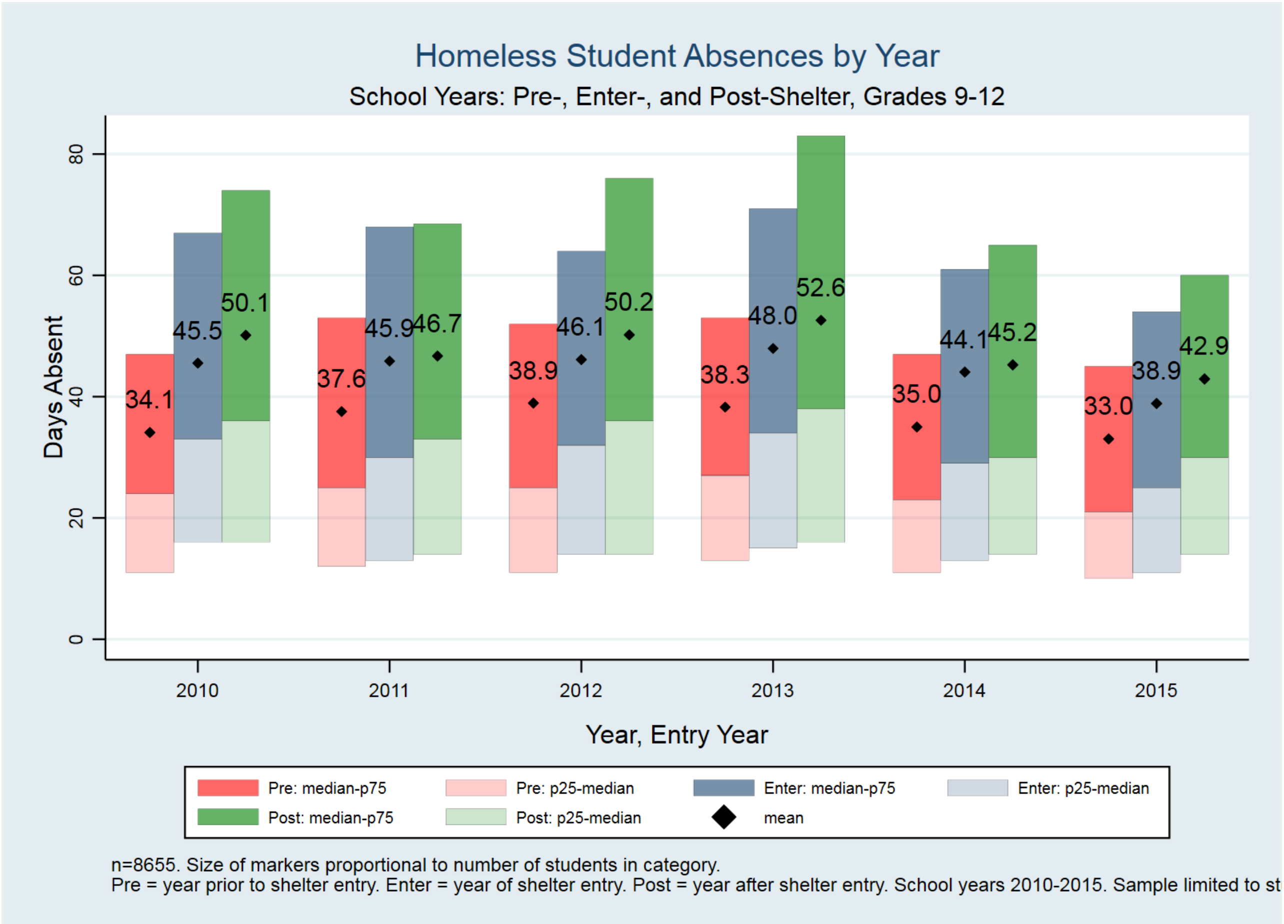


Figure A.3: Absence Persistence Summary

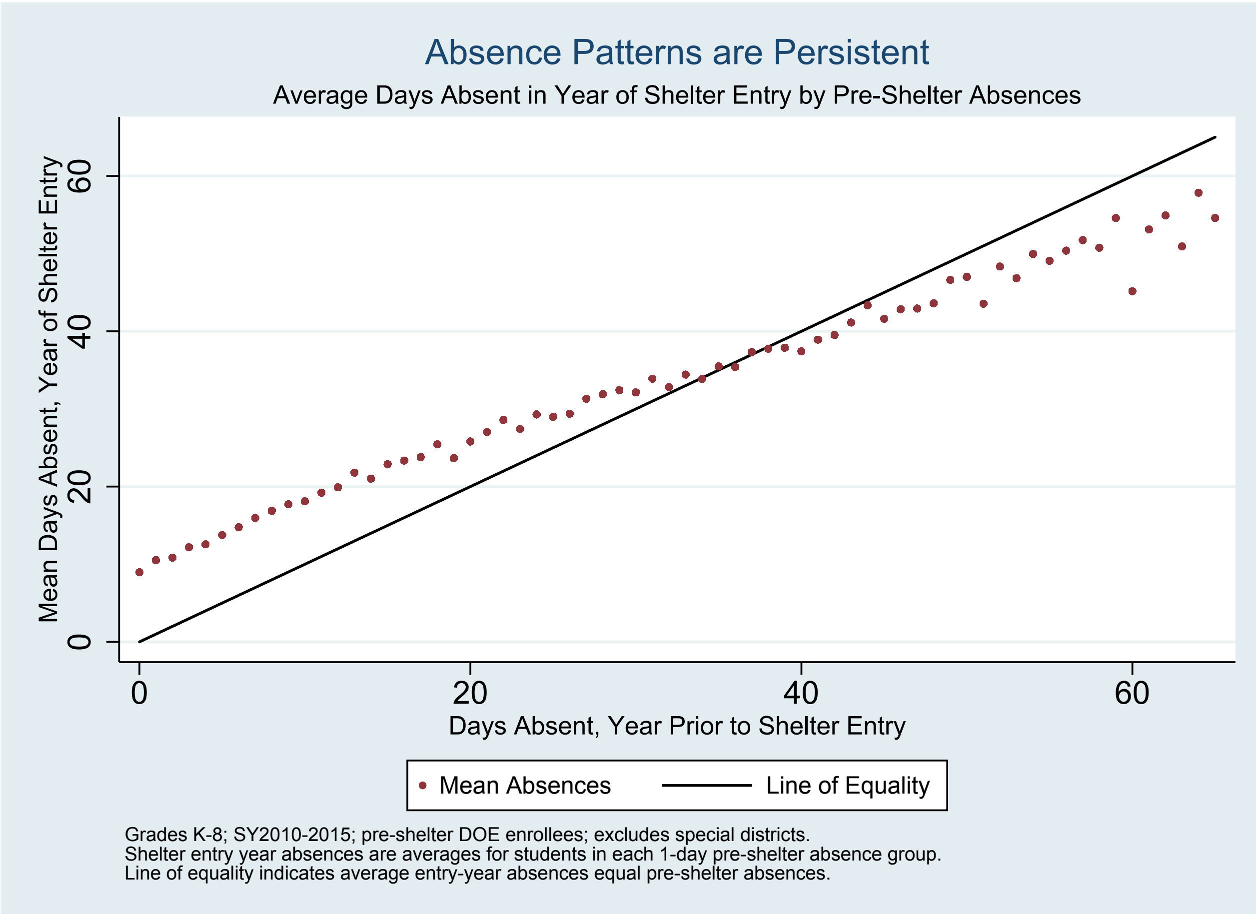


Figure A.4: Absence Persistence Detail

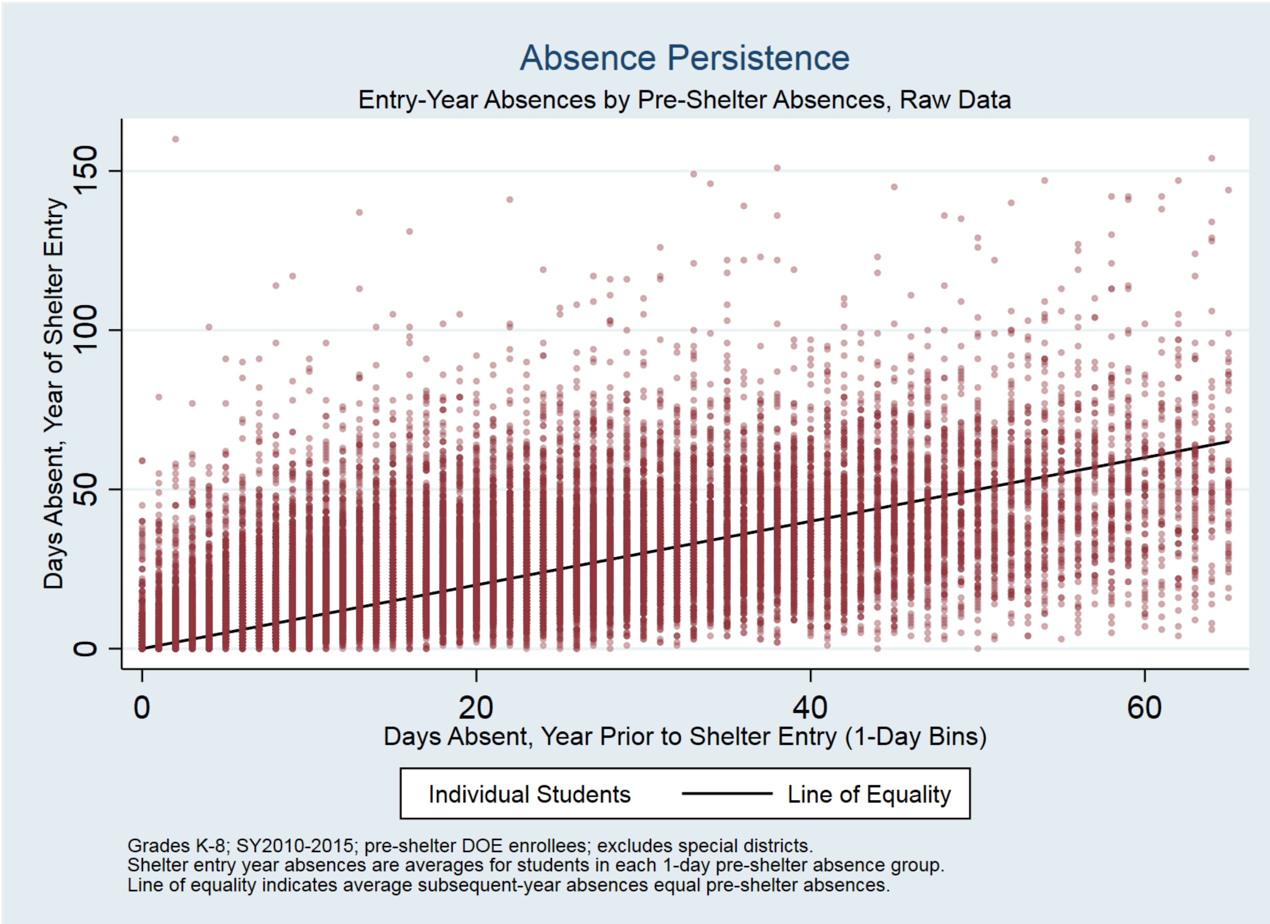


Figure A.5: Absence Persistence Detail

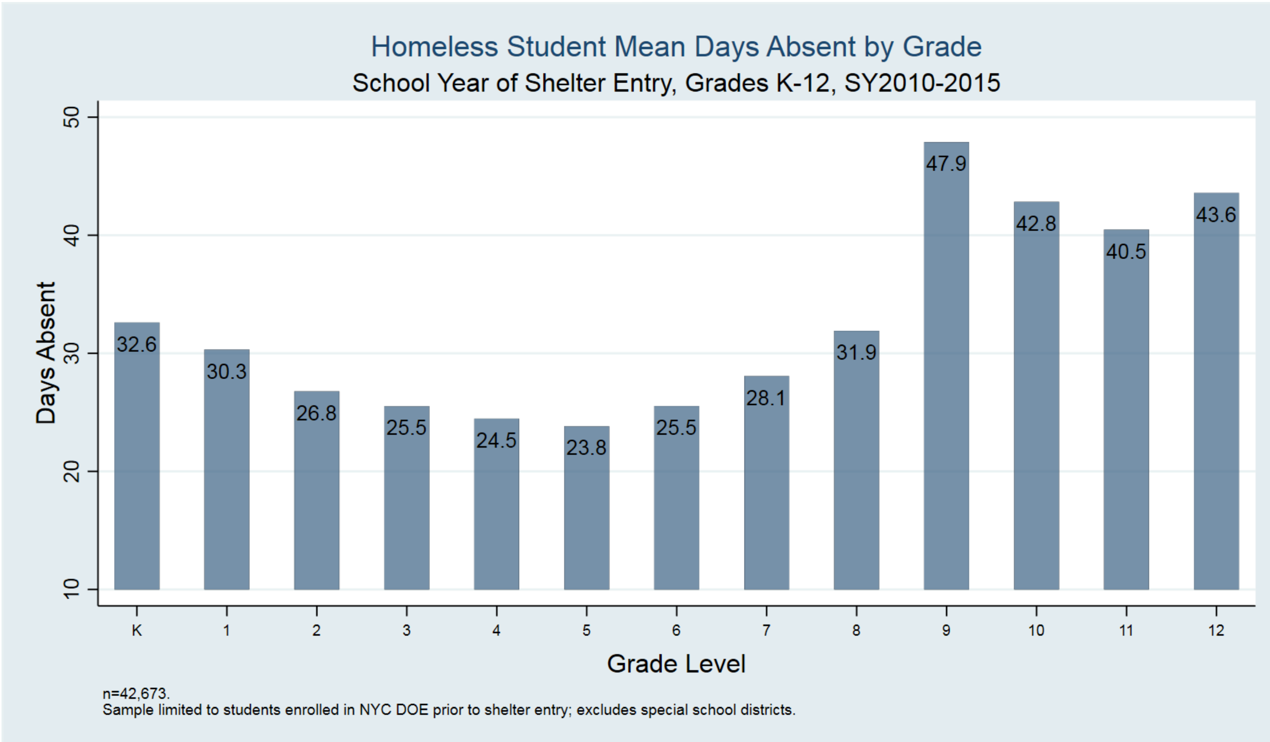


Figure A.6: Attendance and Proficiency

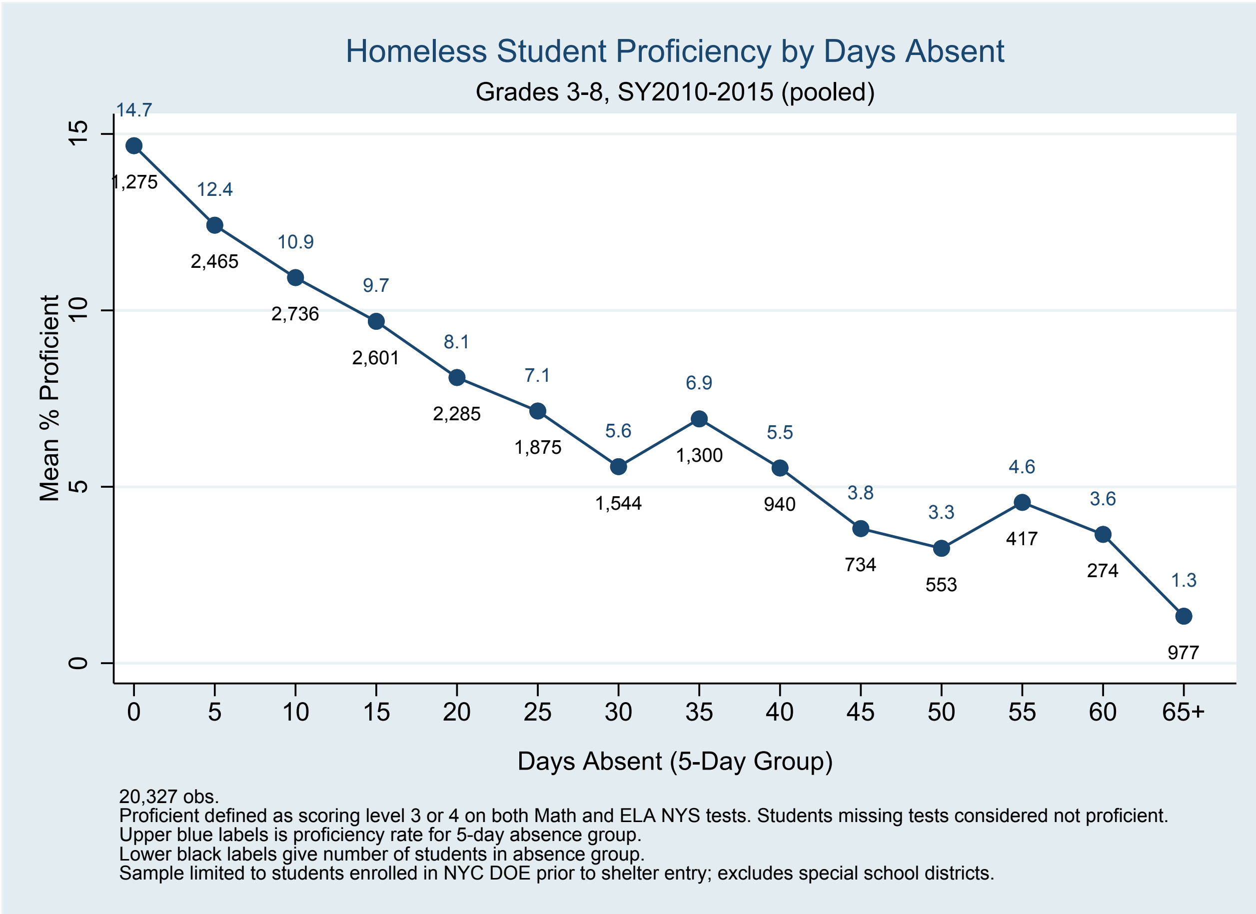


Figure A.7: Attendance and Promotion

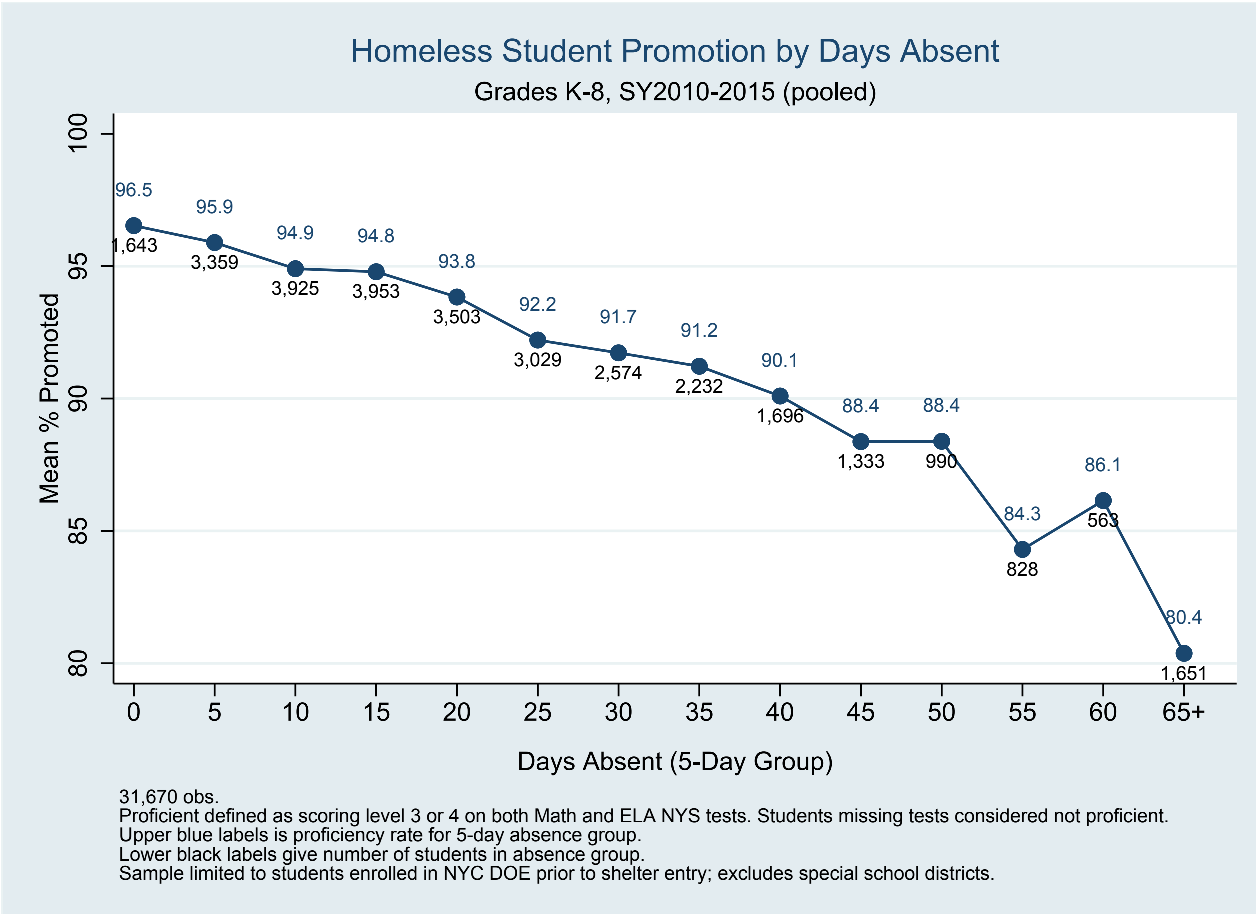


Figure A.8: NYC Public School Proficiency Rates

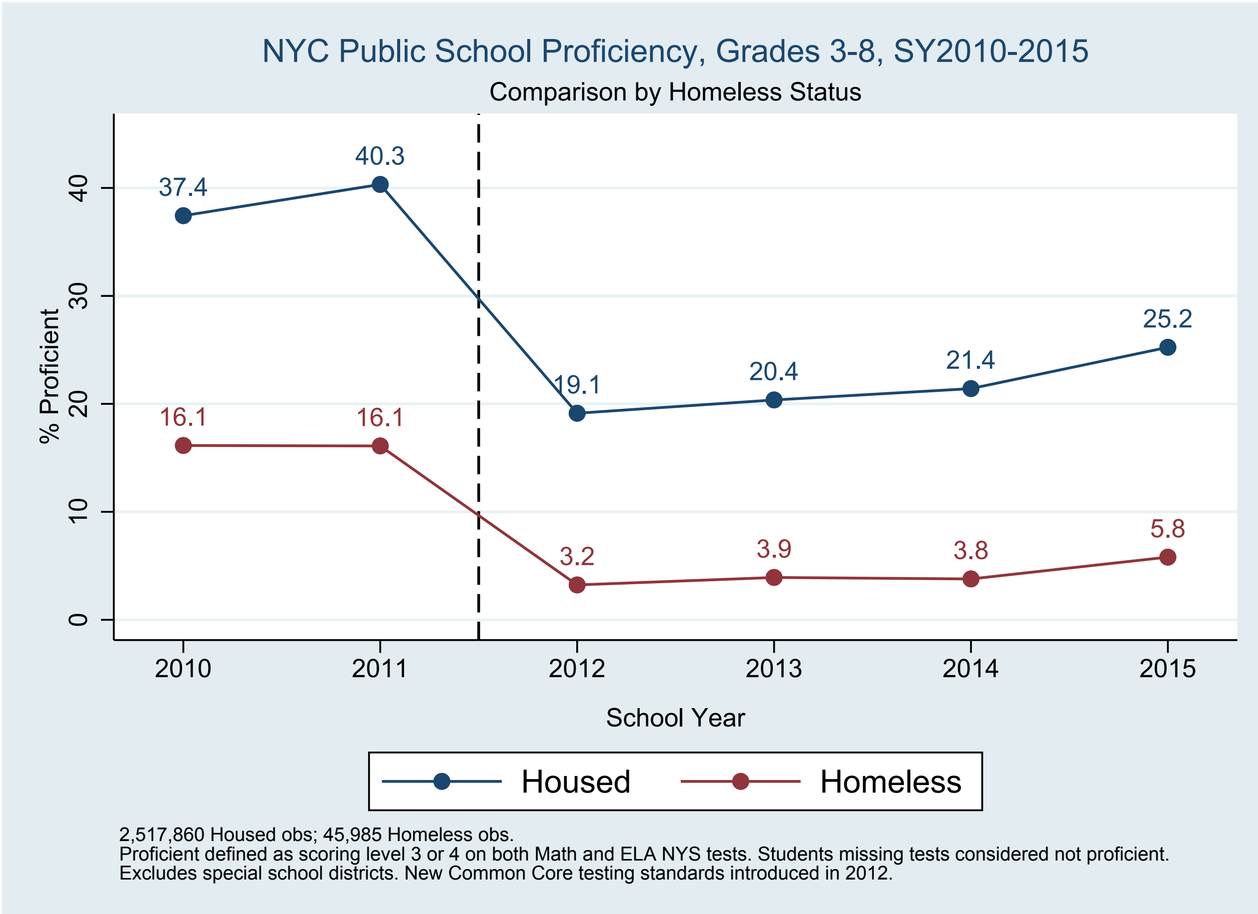
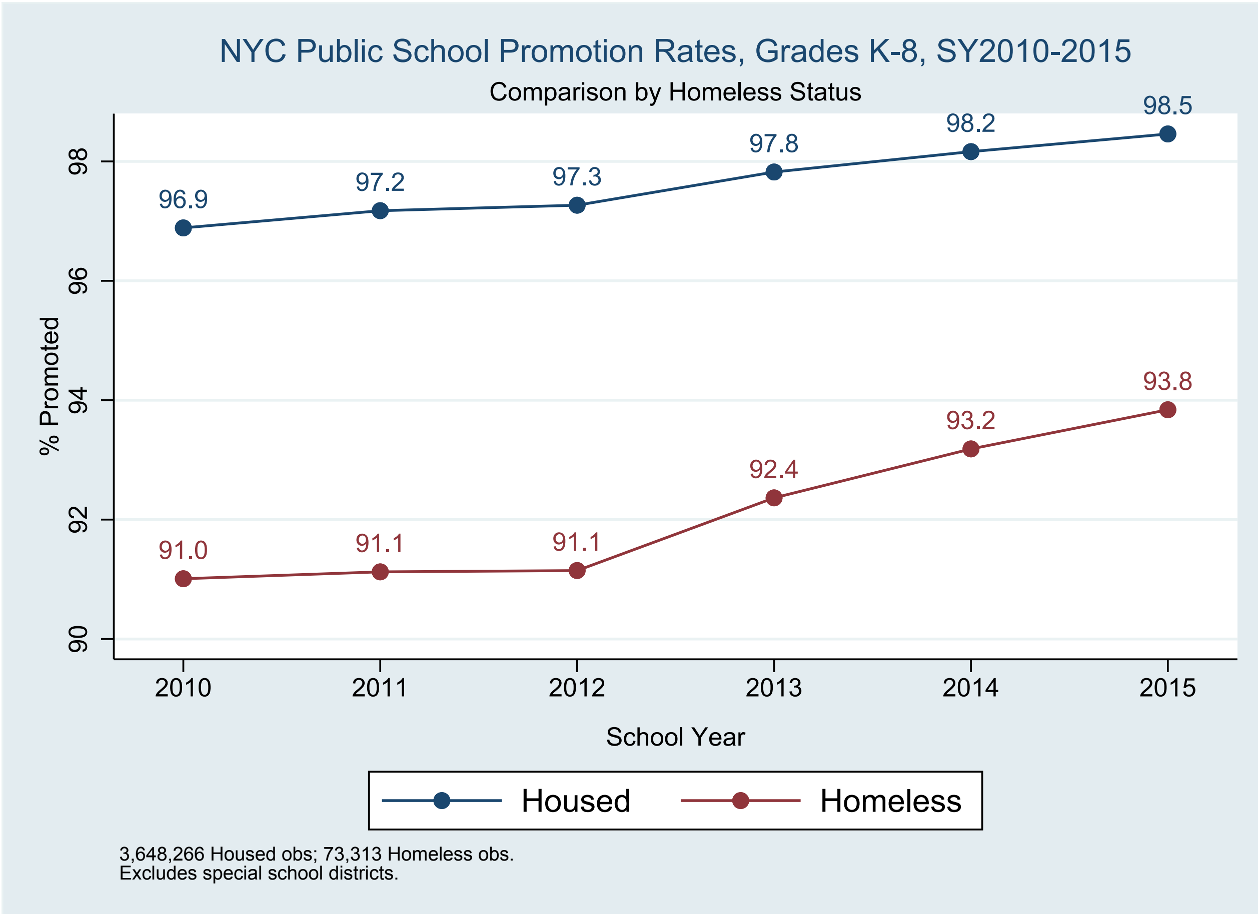


Figure A.9: NYC Public School Promotion Rates



F.2 Instrument Assessment

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

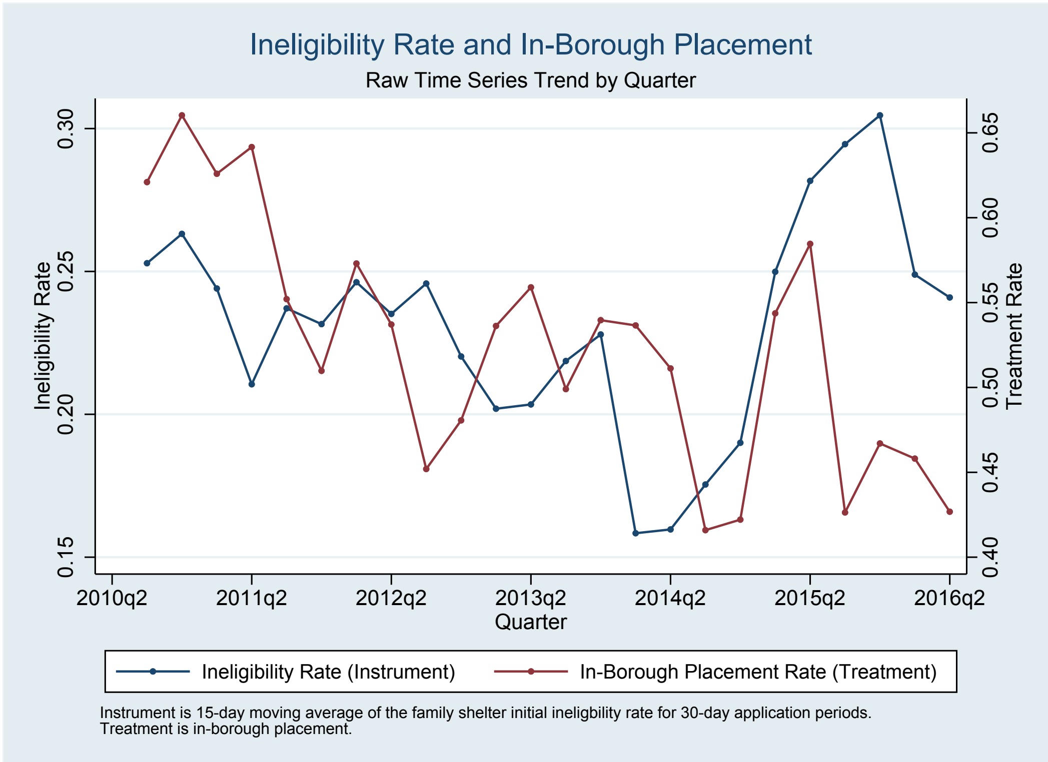


Figure A.11: Instrument and Treatment: Raw

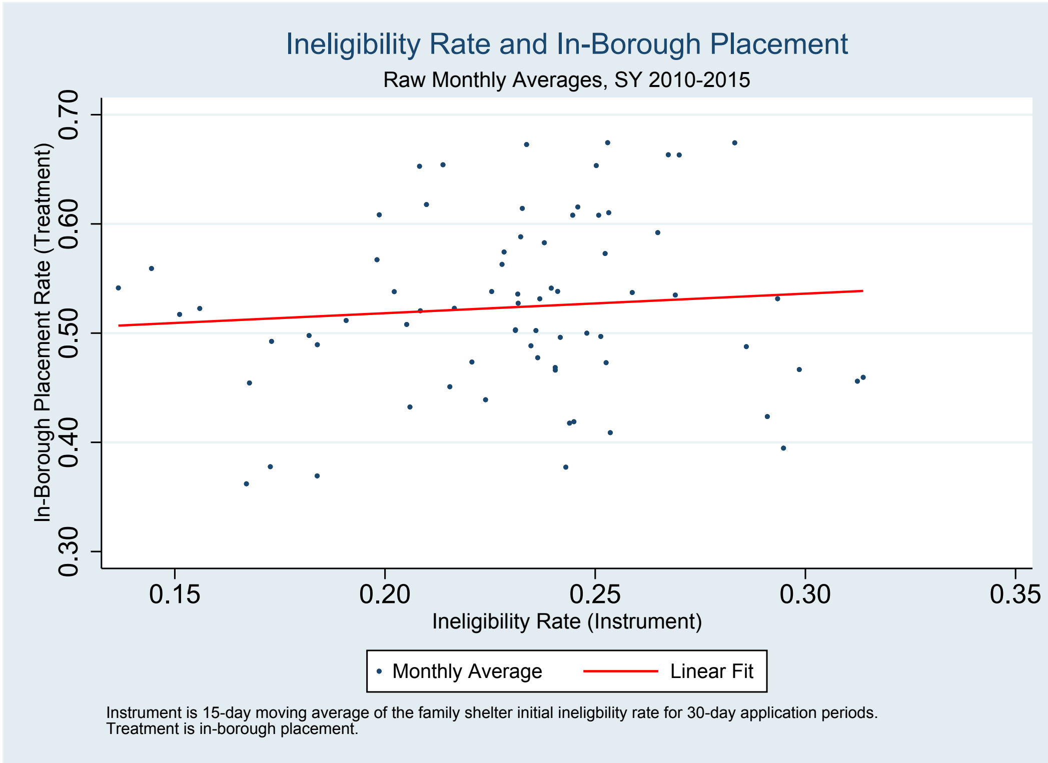


Figure A.12: Instrument and Treatment: Detrended

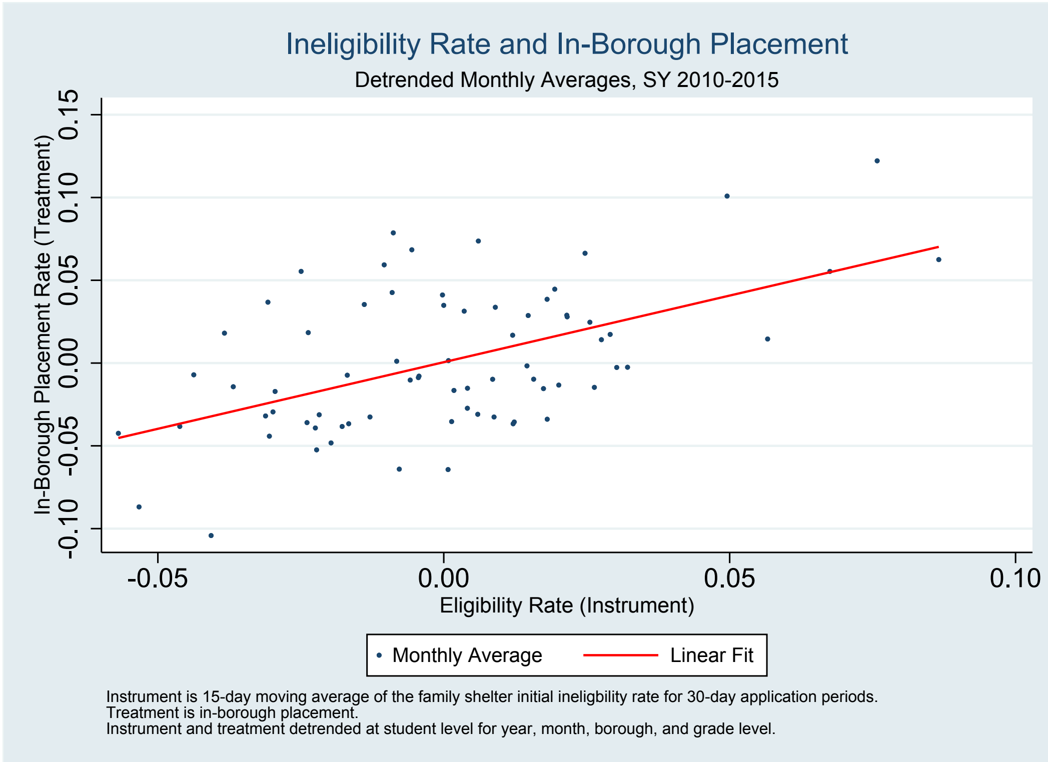


Figure A.13: Family Shelter Application Outcomes

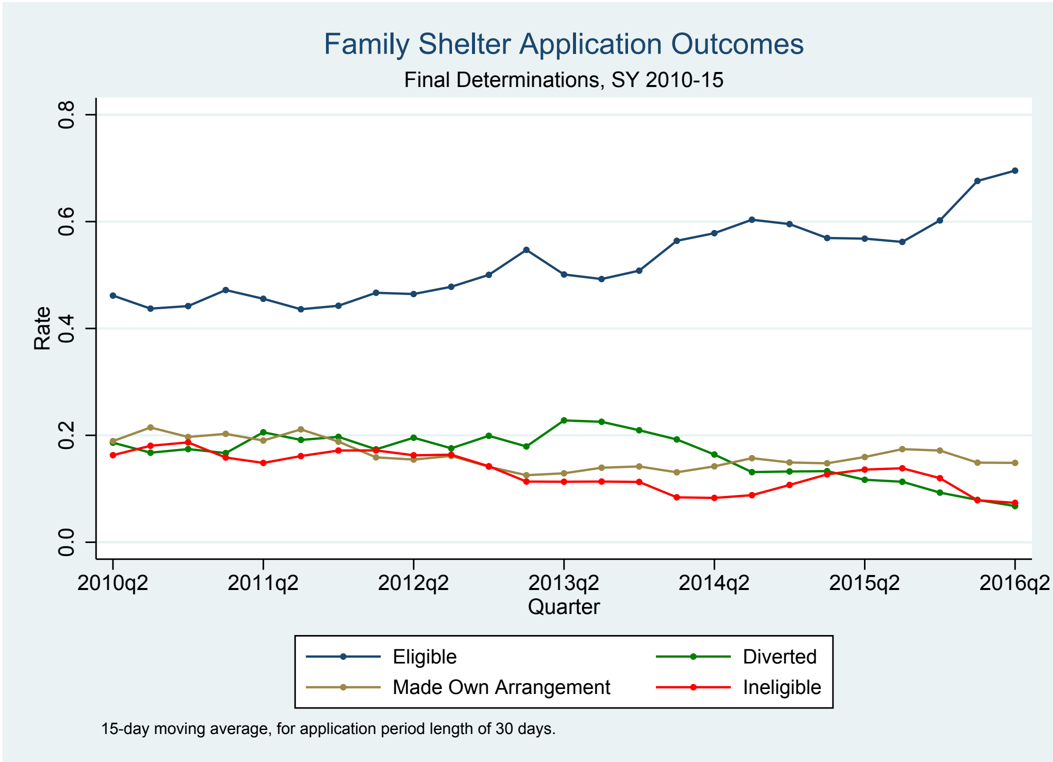


Figure A.14: Initial Ineligibility and Final Eligibility

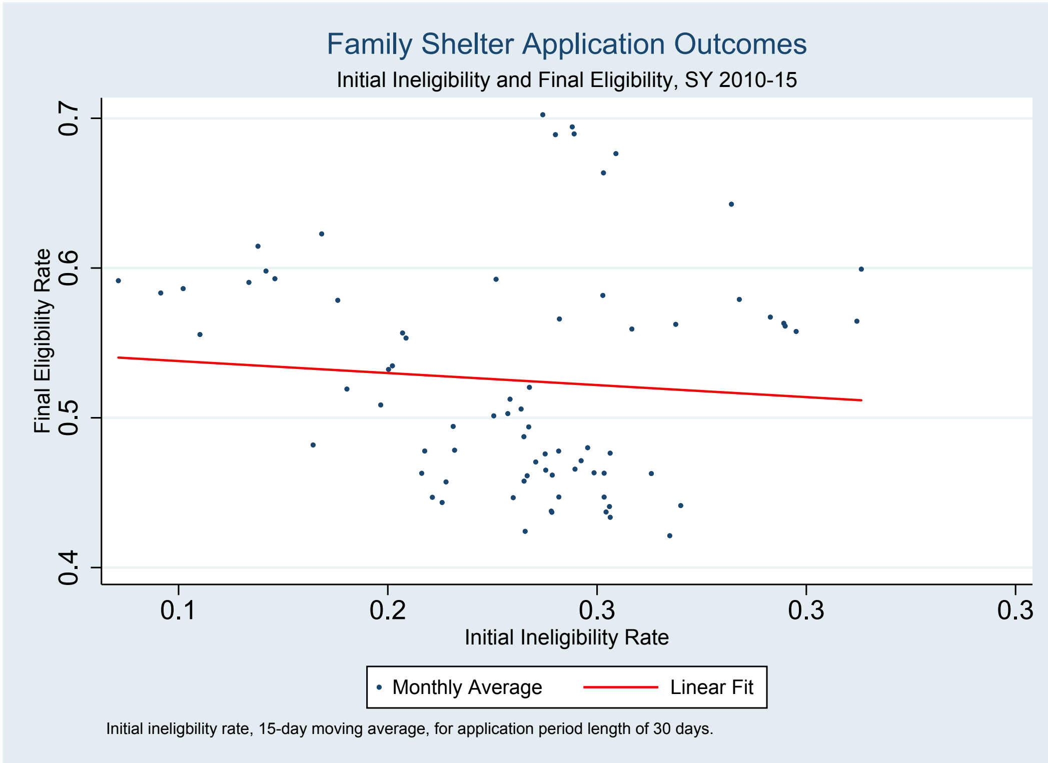


Figure A.15: Final Ineligibility and Final Eligibility

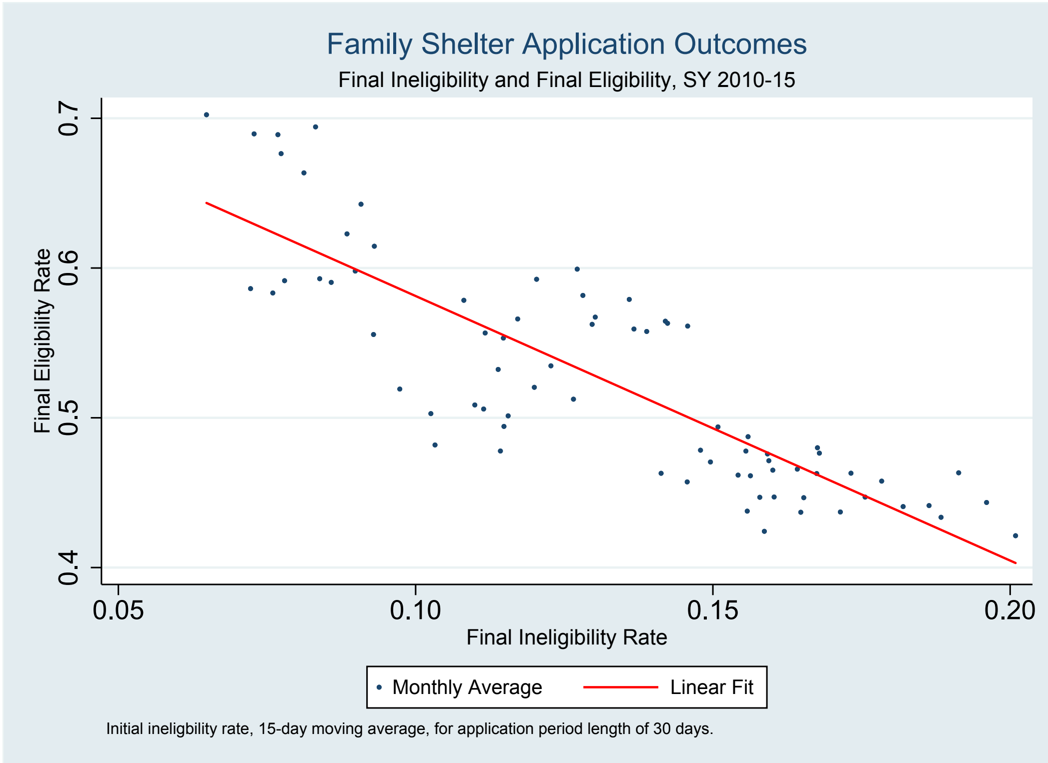
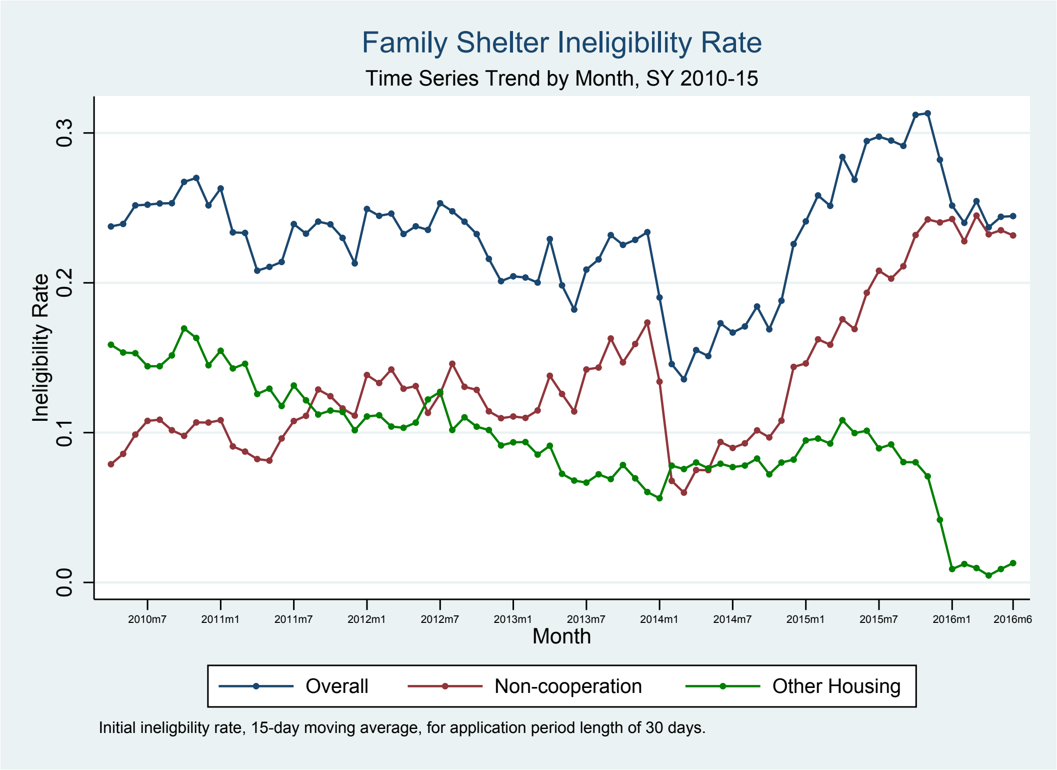
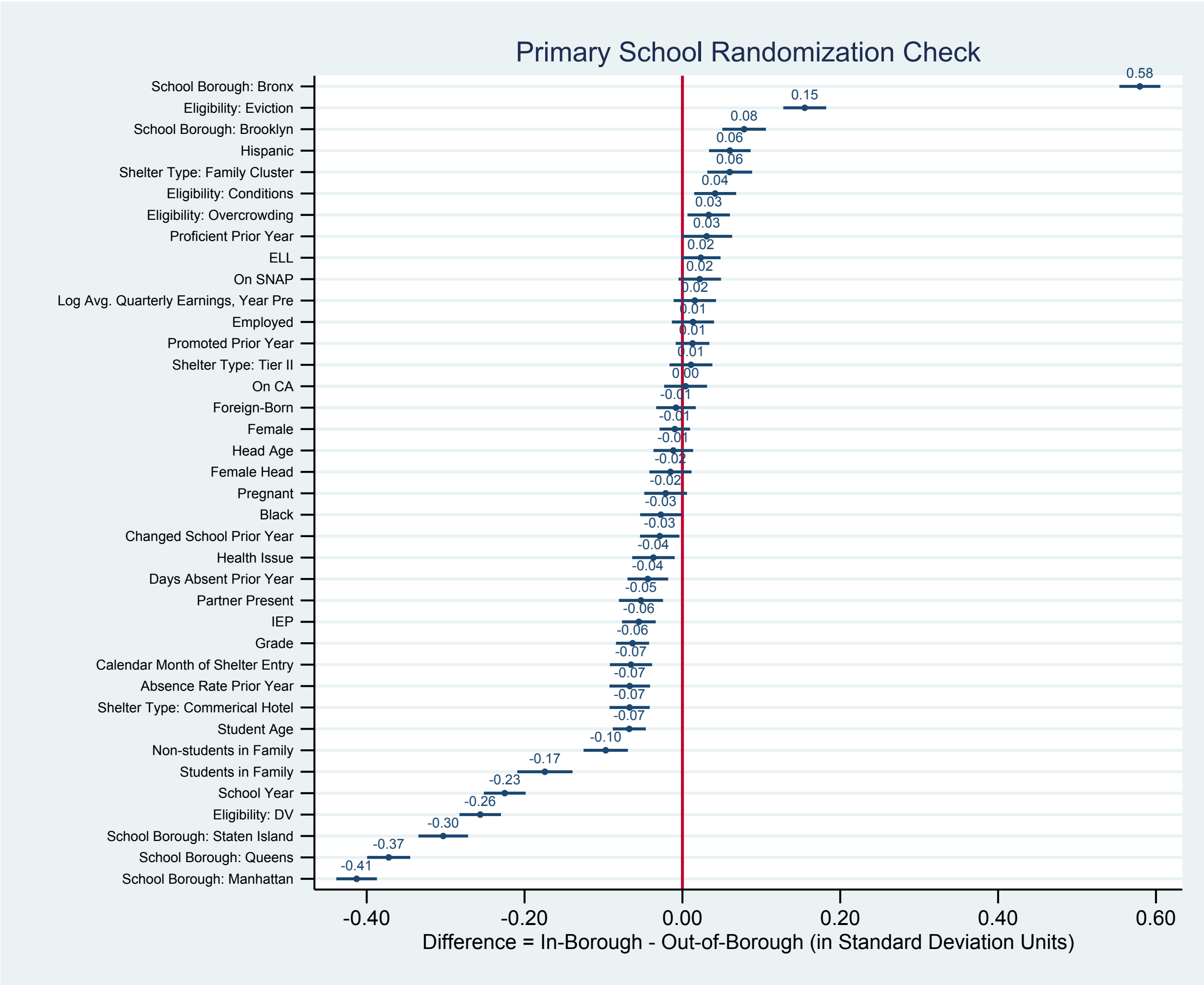


Figure A.16: Ineligibility Rate Details



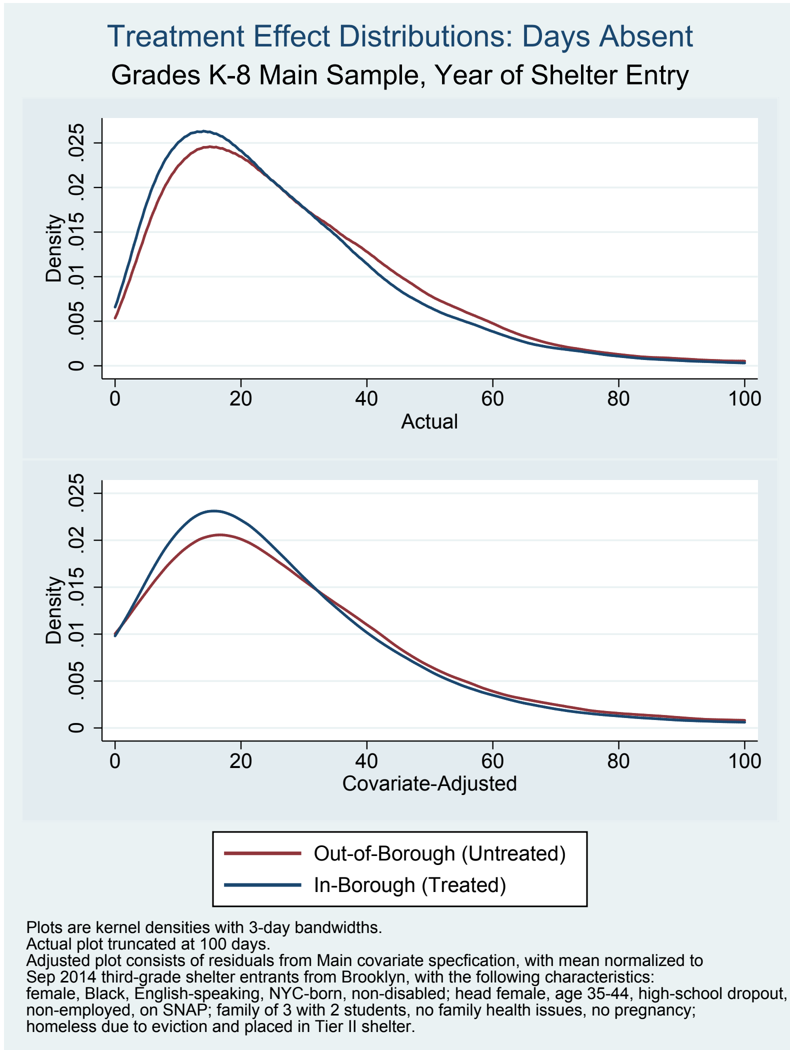
F.3 Results Supplement

Figure A.17: Randomization Check



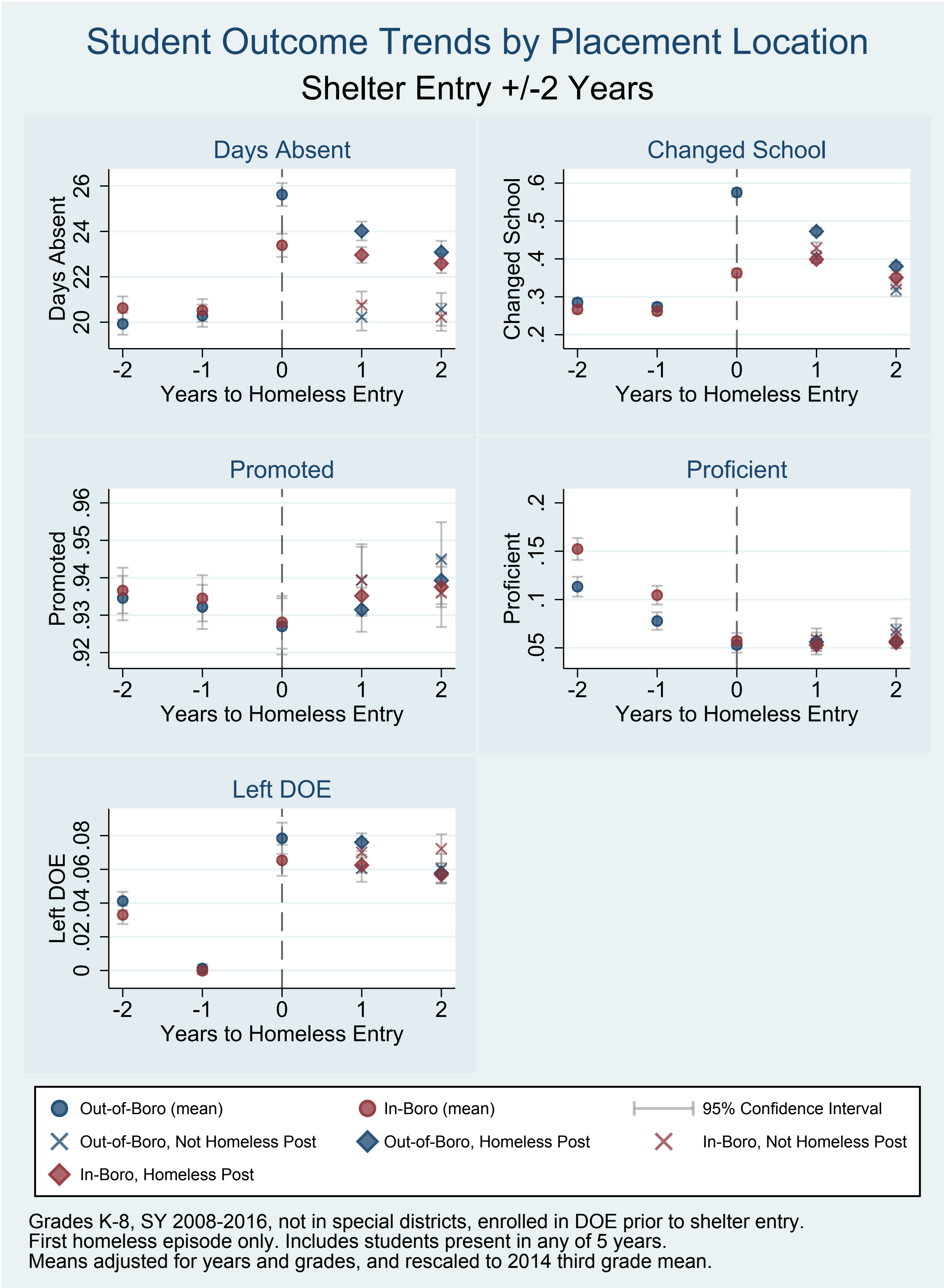
Notes: Graphical depiction of primary school results from Table 3A. Plot gives coefficient on in-borough treatment indicator, scaled in standard deviation units, from separate bivariate OLS regressions of each characteristic on the treatment indicator. Bars give 95 percent confidence intervals; standard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

Figure A.18: Days Absent Treatment Effect Distribution



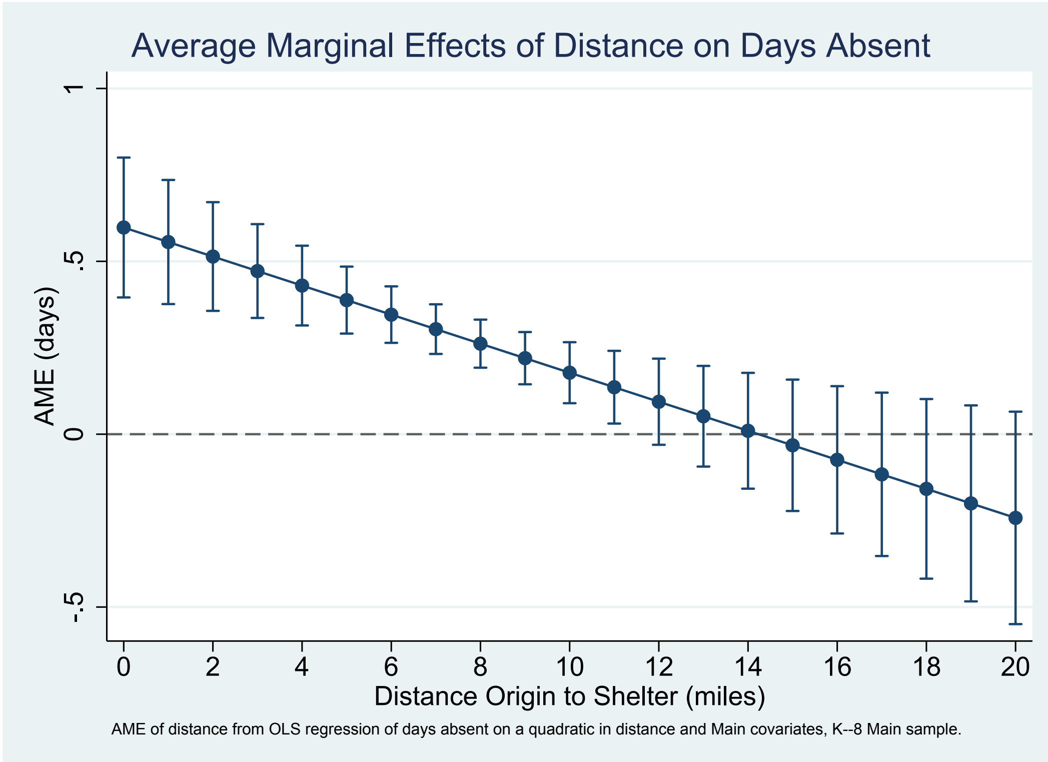
Notes: Plots are kernel densities with 3-day bandwidths. Main sample, grades K–8. Actual plot truncated at 100 days. Adjusted plot consists of residuals from Main covariate specification, with mean normalized to Sep 2014 third-grade shelter entrants from Brooklyn, with the following characteristics: 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



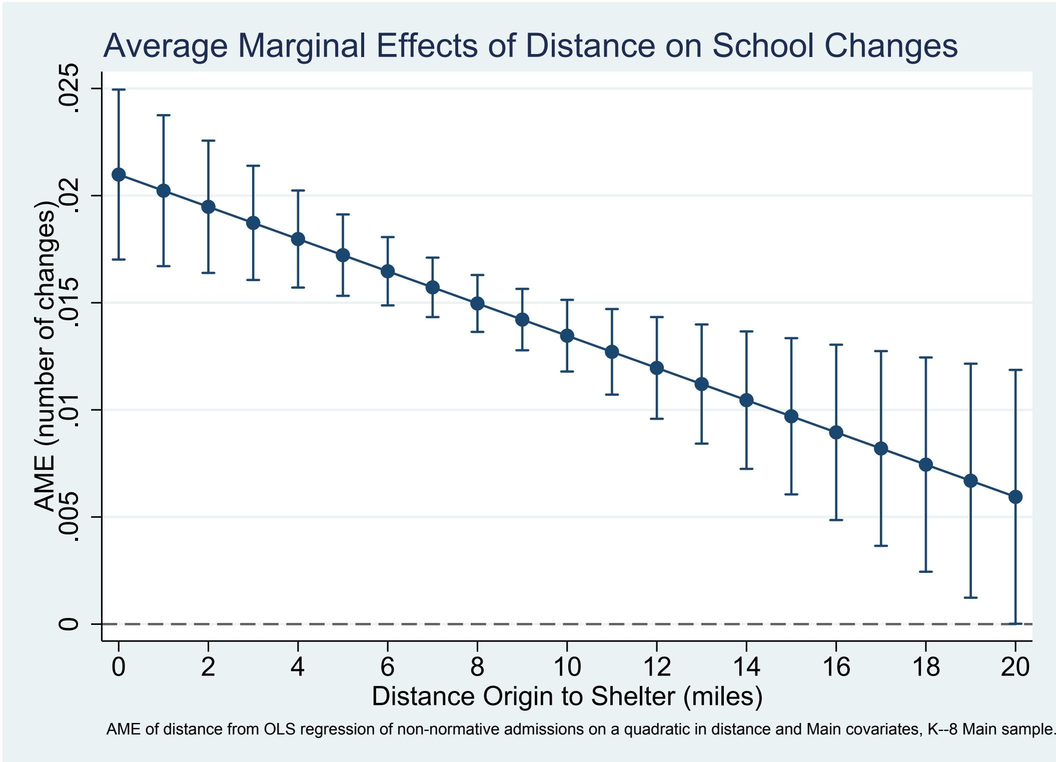
Notes: Grades K-8, SY 2008-2016, not in special districts, enrolled in DOE prior to shelter entry. First homeless episode only. Includes students present in any of 5 years. Means adjusted for years and grades, and rescaled to 2014 third grade mean.

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.