ESSAYS ON MICROECONOMIC CAUSAL INFERENCE IN WELFARE, EDUCATION, AND HEALTH

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ABSTRACT OF THE DISSERTATION

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My dissertation consists of three applied microeconomic papers unified by the techniques of causal inference and the themes of welfare, education, and health. Family features prominently. The first two chapters study homeless families, while the third shifts focus to breastfeeding, a topic of general familial interest.

Chapter 1 investigates the educational effects of New York City's policy of placing homeless families in shelters near their children's schools. I find that proximity augments homeless students' educational outcomes. Homeless K–8 graders whose families are placed in shelters in their school boroughs have 8 percent (2.4 days) better attendance, are a third (18 percentage points) less likely to change schools, and exhibit higher rates of proficiency and retention. Homeless high schoolers have 5 percent (2.5 days) better attendance, 29 percent (10 pp) lower mobility, and 8 percent (1.6 pp) greater retention when placed locally. These results proceed from novel administrative data on homeless families observed in the context of a scarcity-induced natural experiment. A complementary instrumental variable strategy exploiting homeless eligibility policy reveals a subset of proximity-elastic students benefit considerably more. Panel evidence demonstrates homelessness does not cause educational impairment as much as reflect large preexisting deficits.

Chapter 2 situates neighborhood-based homeless shelter placements in the con-

text of whole-family outcomes. Again using an original administrative dataset in the context of a scarcity-based natural experiment in New York City, I find that families placed in shelters in their neighborhoods of origin remain there considerably longer than those assigned to distant shelters. Locally-placed families also access more public benefits and are more apt to work. A fixed effects model assessing multi-spell families confirms these main results. Complementary instrumental variable and regression discontinuity designs exploiting policy shocks and rules, respectively, suggest difficult-to-place families—such as those that are large, disconnected from services, or from neighborhoods where homelessness is common—are especially sensitive to proximate placements. Better targeting through improved screening at intake can enhance program efficiency. The practice of assigning shelter based on chance vacancies ought to be replaced with a system of evidence-based placements tailored to families' resources and constraints.

In Chapter 3, I retain emphases on families and education and study the long-term effects of breastfeeding. Despite consensus among medical authorities about the desirability of breastfeeding, causal evidence about its effects is surprisingly scant. Using a thorough collection of empirical approaches and detailed longitudinal data spanning five decades, I investigate a comprehensive set of outcomes with greater breadth and continuity than previous work. On average (per OLS), breastfeeding is associated with modest and persistent cognitive advantages from childhood through young adulthood—even after controlling for an extensive set of confounding forces. Accounting for breastfeeding duration strengthens these relationships and uncovers favorable labor market and fertility linkages as well. But there is no evidence for enduring health benefits. At the same time, a novel extended family fixed effects analysis comparing differentially breastfed siblings and cousins finds little association between breastfeeding and any outcome. I argue these findings are not mutually exclusive by providing evidence that, contrary to conventional wisdom, the divergent estimates are the consequence of considerable negative selection into the subset of families contributing to fixed effects identification.

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Dedication

To family—mine, and those that are homeless.

And especially to my grandparents—Irene and James Clooney and Frank and Gladys Cassidy—whose enthusiasm for my education made it easy to study hard.

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Introduction

I did not set out to write my dissertation about family, but I suppose it could not have been otherwise. Family has always been of unsurpassing importance to me. I was blessed to grow up in an extraordinarily tight-knit family. Prioritizing family remains an animating passion. But I did not necessarily think of it as a research topic.

What I did set out to do in graduate school was to acquire the analytic skills necessary to help others in a scientifically disciplined fashion. In my application essay, I wrote "My objective as a doctoral candidate at Rutgers will be to...use the theory and methods of economics to understand the causes and consequences of poverty and how best to ameliorate them." This ambition was shaped, in large part, by my first job out of college at the New York City Office of Management and Budget. Tasked with overseeing the City's social service agencies, I became increasingly frustrated by my inability to impart quantitative rigor to the policies and programs I was charged with evaluating. If I were to truly have a positive impact on the lives of the less fortunate, I needed to be sure my recommendations were actually helpful. I wanted evidence, not instinct.

Two graduate degrees, a pile of textbooks, and more than enough qualifying exams later, things have come full circle. My first two chapters use New York City administrative data to inform social policy. Fittingly, the beneficiaries are families—

homeless ones.

The policy in question is NYC's longstanding—but as yet unevaluated—strategy of placing homeless families in shelters near their children's schools. In Chapter 1, I study the effects of proximity on children's educational outcomes. In short, I find that being close to school helps. Students sheltered in their school boroughs have significantly better attendance and stability. They also have higher rates of proficiency and retention.

But rarely are policies unidimensional. In Chapter 2, I consider the effects of neighborhood-based shelter placements on whole-family outcomes. As with children, there are benefits to adults. Families placed in-borough work more, earn more, and access more public benefits. But there is an important cost, too: these families remain homeless longer.

In other words, as is often the case in economics, the big lesson is about incentives and trade-offs. Families given more desirable shelter placements will use more shelter. At the same time, familiarity and convenience breed gains in educational and financial wellbeing. These are insights hardly confined to homeless families; the behavioral implications of variation in the appeal of public benefits can inform the design of poverty alleviation programs more generally.

Appreciating that policies have multifaceted impacts is an important corrective to conventional narratives. But my results also hold an important secondary lesson about nuance: effects are heterogeneous. While averages are useful heuristics, parsimony ought not encourage neglect of distributional consequences. In the shelter placement context, families that are large, health-constrained, isolated, or from places where homelessness is common are especially sensitive to proximity. Embracing heterogeneity dictates that, under conditions of scarcity, evidence should guide resource allocations.

The theme of heterogeneity also pervades my third chapter. Here, I shift focus from homelessness to something relevant to families more generally: breastfeeding. I ask whether breastfeeding has long-term effects across a broad spectrum of outcomes, again focusing on education, labor, and welfare, as well as, in this case, health. I find that the answer is not unambiguous, and perhaps inherently so: separating the effect of breastfeeding from the broader context of parenting and formative environment is difficult, not in the least due to the mutability of breastfeeding response and availability of (dis)compensatory parenting behaviors. While breastfeeding is, on average, associated with meaningful cognitive benefits from childhood through the young adult years, comparisons among siblings and cousins fed differently during infancy yield no such contrasts. Both findings likely hold some truth, but is is equally probable that each is biased, at least to a degree, by confounding factors. Discerning causality from observational data is not a simple matter. The implication is that infant feeding is context-dependent; it follows that public health recommendations should respect the constraints and trade-offs parents face, furnishing supports to encourage desired behaviors as necessary.

Beyond a focus on family, my three chapters are united by emphases on causal inference and comprehensive empiricism. I use a range of econometric strategies—including multivariate regression, instrumental variables, regression discontinuity, and multidimensional fixed effects—and, in appreciation of the plausibility of uncovering genuinely heterogeneous responses with carefully structured analyses, generally interpret the (sometimes divergent) estimates they deliver as complements, not substitutes. At the same time, I highlight the assumptions of these methods, subjecting results to robustness checks where possible and recognizing the limitations of my claims where not. My unifying object is to ascertain causality (however complicated it may be), while remaining circumspect in acknowledging uncertainty and allowing for alternative readings of the evidence.

In this endeavor I am aided by good data—data, which, in their respective domains, offer both better detail and greater scope than those previously employed. Besides helping lay more credibly causal claims, novel data yield additional insights in their own rights. By observing homeless students' educational histories with expanse lacking in the extant literature, I am able to establish that educational deficits among homeless students are not the product of homelessness per se, but rather a manifestation of preexisting disadvantages. I construct a similarly broad view with regard to breastfeeding. By observing the same individuals from ages 5 through 25—a contiguous perspective heretofore absent—I am able to demonstrate that gaps in cognitive performance that emerge early in favor of breastfed individuals persist.

A final commonality across my chapters is an explicit eye toward policy relevance. In keeping with my mission to contribute to others' wellbeing, I strive to make plain the policy implications of my findings. It is my hope that those empowered to put research into practice take note. What I can say with certainty is that the lessons I distill about families—the role of incentives, the reality of trade-offs, the prevalence of heterogeneity, and the utility of taking the long view—have profound personal relevance, for it was during the writing of this dissertation that my wife Molly and I welcomed our son, Max, starting a family of our own.

Chapter 1

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

1.1 Introduction

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

It is well-known that unstably-housed students struggle in school, but evidence on policies to improve performance is scant. One such intervention is school-based shelter

[1]2014 and 2015 school year averages, excluding students in charter schools, alternative schools, and those in special programs for students with disabilities, derived from the homeless student panel described in Section 1.3.

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

placement. Since at least 1998, the City has maintained the explicit goal of placing homeless families in shelters in the boroughs of their youngest children's schools[3]. The theory is that minimizing educational disruption will improve academic outcomes; in addition, policymakers believe neighborhood continuity is generally beneficial for families, keeping them connected to economic and social supports[4].

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

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

The key behavioral insight is this: shelter conditions influence short-term con-

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

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

[5]Many of these students move outside NYC.

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

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

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

[6] I develop a formal model of homeless family educational consumption in Appendix A.2 and summarize it in Section 1.2.

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

[8] New York City Mayor's Office of Operations (2012, 2018). These numbers reflect fiscal years 2010–2011 and 2015–2016. Fiscal years run from July to June, and are named for the year in which they end, so they are coincident with school years, as I've defined them in this paper, though the

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

Nevertheless, it is also of interest to relax this assumption. There are two concerns: endogeneity and heterogeneity. Students whose families unobservably care more about education may (partially) self-select into treatment. Even if they do not, students may respond differently to local placement—a non-trivial issue given treatment scarcity.

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

I argue this IV approach complements, rather than supplants, OLS: operational

latter are named for their starting years.

[9] The product of a series of lawsuits emanating from the 1980's, this mandate, in large measure, explains the rapid expansion of the City's family homeless population at the core of my scarcity-based natural experiment. For a discussion, see Chapter 2.

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

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

Compliers also reap outsized rewards from local placement: in nearly all cases, my IV estimates indicate treatment effects substantially stronger in magnitude than the average treatment effects (ATE's) estimated by OLS. Primary school compliers experience attendance improvements on the order of a full month; compliant high schoolers see impressive gains in academic performance. Point estimates for other outcomes are similarly large, though imprecisely measured. In the absence of endogeneity—my preferred interpretation—these gaps between ATE's and LATE's illustrate the potential welfare gains of targeting interventions to the most receptive recipients, as well as the role of IV in identifying who they are. Eligibility policy tweaks have distributional consequences, both intended and not.

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

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

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

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

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

ample diversity in student experiences.

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

No study in economics has addressed the specific plight of homeless students. The few economics studies of homelessness have typically focused on single adults[11], macroeconomic issues[12], prevention[13], or theory[14], though several works—e.g., O'Flaherty (2004) and O'Flaherty (2010)—helpfully investigate the antecedents and attributes of family homelessness. O'Flaherty (2019) provides a summary of the recent literature; notably, education is not mentioned. The work perhaps most similar to my own is Cobb-Clark and Zhu (2017), who find that childhood homelessness in Australia is associated with lower educational attainment and less employment in adulthood.

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

My work confirms this impression, while also informing two related literatures in economics[16]. The first is that on neighborhood effects, which typically finds that,

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

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

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

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

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

[16] Appendix A.1.2 includes a much more comprehensive review of the literature.

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

Most pertinently, my results complement those in Chapter 2, where I find that families placed in shelters in their neighborhoods of origin remain in shelter 13 percent longer (about 50 days) and access more public benefits. Taken together, these two papers suggest proximity impacts homeless families' consumption choices. Local placements are preferred (in a reveal preference sense), so families consume more shelter when there. At the same time, local placements expand budget sets—through resource augmentation, decreased opportunity costs, or both—encouraging schooling consumption and leading to better attendance, fewer transfers, and improved academic performance.

There are three policy implications. The first is that shelter quality, often neglected, is an important policy parameter. Homelessness has been a priority for every recent mayor, but policy discussions typically focus on minimizing shelter stays, often through rental subsidies, or on avoiding them entirely, using prevention services. My results demonstrate that the quality of shelter stays—of which proximity is one

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

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

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

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

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

facet—can augment or impede objectives in economically meaningful ways. Whether other shelter attributes, such as orderliness, amenities, or services, have similar impacts is of interest.

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

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

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

1.2 Theory

In Appendix A.2, I use the framework of consumer theory to exposit a formal model characterizing the effects of school-based shelter placements on homeless students' educational outcomes. Here I summarize the key intuitions.

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

1.3 Policy Background and Data

1.3.1 Policy Background

Homeless families are perhaps the most invisible of society's most obviously afflicted populations. Unlike the single adult street homeless who dominate the popular consciousness, homeless families are not distinguished by substance abuse or mental illness but instead by a particularly pernicious form of poverty: the lack of regular places to call home.

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

Typically consisting of a high-school-educated, racial minority single mom with several young children previously living in overcrowded conditions, homeless families look like other poor families because they *are* like other poor families—albeit momentarily on the losing end of chance encounters with poverty's vicissitudes, such as health crises, job losses, or domestic disputes. Most recover quickly enough, and are sheltered for brief periods, never to return. Family homelessness is a phase, not a trait[24].

The consequences of poverty-induced residential instability are particularly pro-

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

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

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

nounced in New York City. A constellation of forces—a hospitable legal environment and notoriously expensive real estate market, in combination with a tradition of progressive politics, an enviable fiscal affluence, and an exceptionally mature municipal social service apparatus[25]—have made NYC home to one in four sheltered homeless families nationally (The U.S. Department of Housing and Urban Development, 2018). And while family homelessness has declined nationwide by a third since 2009, DHS' census of homeless families grew from 8,081 in March 2009 to 12,427 in March 2019, though down from its November 2018 peak of 13,164 (New York City Department of Homeless Services, 2019*a*). A large part of the explanation is that NYC is one of just two jurisdictions in the U.S. where families have a legal right to shelter[26].

Families presenting themselves as homeless must submit to an eligibility determination process. At minimum, they must have at least one member under 21 or pregnant and demonstrate that they have no suitable place to live[27]. Families are first screened for domestic violence and homeless prevention services (e.g., rent arrears payments); those unable to be diverted are interviewed by DHS caseworkers about their housing situations and granted conditional shelter stays for up to 10 days while investigation staff assess their claims. Those found eligible may remain in their initial shelter placements as long as necessary, while ineligible families may appeal their decisions through a fair hearing process or reapply, as many times as desired. Most ineligibilities occur due to failure to comply with the eligibility process or because other housing is found to be available. Families may also "make their own arrangements" and voluntarily withdraw (or fail to complete) their applications. Eligible families may request transfers to more suitable shelter units as they become

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

[26]The state of Massachusetts is the other. For details, see Chapter 2.

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

available.

The shelter system into which these families are placed is vast. Administered by DHS under the auspices of the Department of Social Services, it consists of more than 500 distinct shelter sites spread across the five boroughs (New York City Independent Budget Office, 2014; The City of New York, Mayor's Office, 2017). Although DHS runs several shelters directly, most day-to-day shelter operations are managed by contracted non-profit social service providers, as is the norm with human services in NYC (New York City Office of Management and Budget, 2018). The costs are substantial. In the fiscal year ending in June 2018, DHS spent $1.2 billion to shelter homeless families; the average cost per family *per day* in shelter was $192 (New York City Office of Management and Budget, 2019; New York City Mayor's Office of Operations, 2018)[28].

To help address the challenges homeless students face, the City has, since at least 1998, maintained the explicit goal of placing homeless families in shelters near their youngest children's schools[29]. In part, this neighborhood-based shelter placement policy facilitates compliance with the federal McKinney-Vento Homeless Assistance Act (42 U.S.C. 11431 et seq.), which requires local education agencies to provide the services necessary for homeless students to remain in their schools of origin, if desired. But increasingly the policy has come to reflect the conviction that keeping homeless families connected to their communities of origin—close not only to schools, but also to family, friends, jobs, places of worship, and other sources of support—is a means of expediting the return to more stable housing (The City of New York, Mayor's Office, 2017).

Officially, the placement target is the shelter nearest the child's school; in practice, DHS counts as successful any placement occurring in the youngest child's school bor-

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

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

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

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

1.3.2 Data and Sample

My data consists of an unbalanced panel covering the 2005–2016 school years among students whose families entered homeless shelter during calendar years 2010 to 2016, derived from linking administrative records maintained by DOE and DHS[30].

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

[30]Specifically, these students' families applied and were deemed eligible for homeless shelter between 1/1/2010 and 12/31/2016. For an extended discussion about the construction and content of the dataset, see Appendix A.1.3; for extensive detail on the DHS data specifically, see Chapter 2.

ing 6.5 times.

Table 1.1 describes the path from the full data to my analytical sample. I restrict the analysis to students in grades K–12 (pre-K is voluntary), during school years 2010–2015 (the school years with complete coverage in the DHS data), not enrolled in special school districts (charter schools, students with disabilities, alternative schools, or unknown), and who are enrolled in DOE prior to the date of shelter entry (to avoid spurious treatment among non-NYC residents). These remaining 216,177 student-school-year observations are a mix of school years prior to, during, and post shelter spells. Spells may begin at any time during the school year. Some spells span multiple school years. Some students have multiple spells.

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

In terms of content, the DHS portion of my data, adapted from Chapter 2, contains extensive detail about families' identities, compositions, and shelter stays. The raw data consists of individual-level records for all family members; it is these records that I use match homeless students to their educational histories. I rework these data such the unit of observation is the family-homeless-spell, defined as beginning with

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

a shelter entry more than 30 days subsequent to the end of a previous stay, which is natural in this setting[32]. To this core DHS data, I append information about homeless families' public benefit use maintained by HRA (Cash Assistance (CA); also known as "public assistance" or "welfare") and Food Stamps (formally, the Supplemental Nutrition Assistance Program (SNAP)), using probabilistic record linkage, as well as data on quarterly employment and earnings from the New York State Department of Labor (DOL), using a deterministic data linkage. For simplicity, I refer to the HRA and DOL data under the umbrella of "DHS" since the linkage is performed with the DHS data.

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

Correspondingly, DOE's data contains records for each student during each school year (the unit of observation is the student-school-year), with separate annual "topical" files for June biographical information (demographics, student characteristics, and enrollment details, including school ID and attendance; so named because records are reconciled at the end of the school year, in June), test scores (3rd–8th grade state standardized tests and Regents exams for high schoolers), and graduation (for high

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

schoolers). In addition to the topical files, there is also a separate transactions file detailing all admissions and discharges (including scheduled school level promotions to middle and high school, as well as non-normative transfers), and associated dates, over all school years in the sample. All variables are student-specific.

I match DHS' school-age family shelter residents with DOE records following standard City protocols for linking human service and education data. The match is probabilistic and based on first name, last name, date of birth, and sex. Overall, as described in Table A.1, approximately 87 percent of children age 5–18 in the DHS data are successfully linked to NYC public school records—which is about as high a rate as could be hoped, given not all children attend public schools during their shelter stays.

As detailed in Appendix A.1.3, which describes all data management tasks in greater detail, I also create a second broader "Complete" sample, summarized in Table 1.1, that encompasses housed students, in order to contextualize homeless student outcomes. These comparisons are presented in Appendices A.6.2 and A.7.1.

1.3.3 Treatment and Outcomes

Treatment

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

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

described in Appendix A.1.3, that parses the DOE transactions data to identify each student's original school for each school year.

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

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

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

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

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

percent and 8 percent, respectively.

Outcomes

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

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

I consider one academic performance measure common to all students: a promotion indicator equal to one in year t if either (a) a student's grade level in school year $t+1$ is greater, or (b) the student graduated in year t[35].

My other aptitude measures differ between my primary- and high-school samples. Students in grades 3–8 take NYS Math and English Language Arts (ELA) standardized tests[36]. Numeric scores are scaled for grade-year difficulty and translated to four

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

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

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

levels; students at levels 3 or 4 achieve proficiency[37]. I construct binary Math and English proficiency indicators consistent with this definition, modified such that students who miss a test (true of many homeless students) are classified as non-proficient. I also create an an overall proficiency indicator equal to one if a student scores 3 or higher on both tests.

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

1.4 Empirical Approach

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

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

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

[38]Regents are named for the board that oversees the NYS Education Department (NYSED). At least one of the five exams must be in each of the core subject areas: English Language Arts, Math (Algebra, Geometry, Trigonometry), Science (Living Environment, Chemistry, Earth Science, Physics), and Social Studies (Global History, U.S. History). NYSED may accept approved alternative subjects, such as a language exam, to fulfill one of the five tests. To graduate, students must also satisfy certain course credit requirements. Exams are given three times per year, in January, June, and August. They are graded on a scale of 0-100; passing is defined as 65 or higher. Students who pass nine exams receive an Advanced Regents diploma.

binary treatment case, two counterfactual states of the world

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

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

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

1.4.1 Conditional Independence and OLS

As shown in Section 1.5, the data suggests shelter scarcity—quasi-random assignment—does, as DHS suggests, play a leading role in determining which families end up where, conditional upon the shelter entry environment and factors expressly considered as placement criteria. Under this conditional independence assumption, OLS is a consistent estimator of treatment effects. Accordingly, I model outcomes as depending on treatment and covariates (both observed and unobserved) in a linear, separable fashion, while allowing for the possibility of heterogeneous treatment effects. My general estimating equation is:

$$Y_{ip} = \mathbf{X}_{ip}\boldsymbol{\beta} + \tau^{OLS} N_{ip} + \varepsilon_{ip} \tag{1.1}$$

Educational outcome Y for student i during spell p is a function of myriad individual and institutional characteristics, to be described below, including unobservables ε_{ip}. The parameter of interest is τ^{OLS}, the coefficient on the in-borough placement indicator, which gives the average effect being placed in a shelter in one's school borough, controlling for the covariates and fixed effects ($\mathbf{X}_{ip}$) included in the model, which will be discussed shortly. The estimand of interest is the average treatment effect (ATE) of local placement, which is the population mean difference in outcomes between in-borough and out-of-borough placements. Under conditional independence,

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

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

To add an additional layer of scrutiny, I also consider a **"Lag"** variant of my Main specification which includes days absent in the year prior to shelter entry. The idea is to proxy educational unobservables, and this is the outcome most consistently reported for all students. However, it is not my preferred specification for two reasons. First, for some students, prior year attendance is unobserved or unrepresentative,

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

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

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

which reduces my sample size considerably[42]. Second, lagged absences eat up much of the variation in the data. While this is an important observation—past student tendencies explain future patterns—the effects of other factors become imprecisely estimated[43]. I view omitting the lag as an acceptable omission, as in-borough and out-of-borough students are virtually identical in pre-shelter outcomes.

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

Throughout the main analysis, I estimate Equation 1.1 separately for primary school (grades K–8) and high school (grades 9–12) students. The reason is that the educational dynamics of high school, where students have greater independence, are categorically different than that of elementary and middle school, where parental volition exerts greater influence. As described in Section 1.3, I also restrict the analysis to the year of shelter entry for each student-spell.

To account for arbitrary covariances of unobservables among siblings, as well as for the presence of students with multiple spells, I cluster standard errors at the "family group" level. Family groups are clusters of families linked by at least one overlapping member, which I identify through a novel linking algorithm in Chapter 2. In most

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

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

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

cases, family groups are consistent with the DHS (and standard) definition of family; however, because homeless households are subject to compositional volatility (e.g., children may temporarily reside with relatives), this broader measure results in more conservative standard errors.

1.4.2 Instrumental Variables and Heterogeneity

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

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

On the other hand, if, as the evidence presented in Section 1.5 suggests, treatment assignment is truly random, but responses to it are diverse, IV does something more: it identifies the LATE among students whose treatment status is affected by the instrument. If this compliant subpopulation is also policy relevant, IV estimates can uncover policy insights the ATE obscures—even in the absence of endogeneity. Given local placements are scarce, understanding heterogeneous responses can help allocate slots in an aggregate welfare maximizing manner.

My instrument is the 15-day moving average of the initial ineligibility rate for rolling 30-day application periods. The City is legally required to provide shelter, but families are required to prove their need for housing. State rules and legal precedent regulate eligibility determinations, but City officials retain considerable discretion[45]. As described in Appendix A.3, which details the construction of my instrument, families may apply for shelter as many times as desired. These applications may be accepted, rejected, or voluntarily withdrawn (usually through non-completion). The 30-day periods reflect the agency view that repeat applications within a month reflect the same housing issue. A new period begins following a gap of more than 30 days from the date of a family's previous application; these periods are "rolling" in the sense that the 30-day clock resets with each application. "Initial" ineligibility refers to the disposition of a family's first application within each period. The 15-day moving averages smooth out noise in the data; they include each family's date of shelter entry and the 14 days prior, and are weighted in proportion to daily applications.

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

Fortunately, there is strong evidence that this sort of sample selection is not present. Simple time series trends demonstrate that the most pronounced swings in the ineligibility rate are coincident with policy changes. As shown in Figure 1.1, there is a striking discontinuity in eligibility in January 2014, when the Bloomberg administration was replaced by de Blasio mayoralty. In keeping with the latter's more generous stance towards the poor, ineligibility plummeted, only to rebound as

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

the shelter census expanded during the following year. Similar spikes and troughs are evident around the times DHS commissioner changes, as well as during other well-documented policy changes[46].

Even more convincingly, the average characteristics of students and their families do not appear to be influenced by the ineligibility rate. As shown in Table 1.2, students who enter shelter during periods of unusually high and low eligibility are similar in most observable respects. The table, which pools grades K–12, reports contrasts between students who enter shelter when the normalized ineligibility rate is one standard deviation (or more) below the mean those those entering when it is one standard deviation (or more) above the mean (students entering during more unremarkable times are omitted). Results are the average differences in characteristics between students entering in high versus low eligibility periods, after adjusting for Base covariates[47].

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

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

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

A key reason for this compositional uniformity is that most families eventually become eligible for shelter. Eligibility policy is mostly about the flow of shelter entrants, not the stock. Strict eligibility delays shelter entry rather than preventing it. A sizable share of families apply multiple times within a given time period before being found eligible. Using 30-day application periods, Figure 1.2 plots the quarterly mean of the 15-day moving average of the initial and final ineligibility rates[48]. The final ineligibility rate is lower and less volatile than the initial one. During my sample period, the initial ineligibility rate ranges from 11.6 percent to 34.7 percent, with a mean of 23.1 percent, while the final rate varies from 5.2 to 22.4 percent, with a mean of 13.1 percent. In part due to repeat applications, strict ineligibility lengthens the time it takes to become eligible, as shown on the right axis. During lenient times, the average family becomes eligible within 5 days of applying; during strict times, it can take more than 10 days. The slowing of shelter entry raises the chances of local placement, but without sample selection.

I discuss additional arguments in favor of instrument validity in Appendix A.3. Nevertheless, as a robustness check, I also use average days to eligibility as an alternative instrument[49]. The typical lag between initial application and eventual approval is, of course, related to the ineligibility rate. However, because approval lags don't directly "select" the sample in the same way as the ineligibility rate (days to eligibility are a characteristic of the eligible), it captures the part of eligibility policy plausibly least related to applicant unobservables.

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

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

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

squares, with Equation 1.1 the second stage and the first stage given by:

$$N_{ip} = \tau^1 Z_{ip} + \mathbf{X}_{ip}\boldsymbol{\beta}^1 + \boldsymbol{\varepsilon}^1_{ip} \tag{1.2}$$

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

1.4.3 Student Fixed Effects

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

I implement my student fixed effects estimator by modifying Equation 1.1 to include individual student dummies, α_i. That is, for student i during shelter spell p,

$$Y_{ip} = \alpha_i + \tau^{FE} N_{ip} + \mathbf{X}_{ip}\boldsymbol{\beta} + +\varepsilon_{ip} \tag{1.3}$$

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

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

1.5 Results

1.5.1 Descriptives and Randomization Check

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

Tables 1.3A and 1.3B compare mean characteristics of students placed in-borough (Local) and out-of-borough (Distant), separately for the primary and high school Main samples. The contrasts are obtained from bivariate regressions of each variable on an indicator for in-borough treatment, with standard errors clustered at the family group level[50]. Locally- and distantly-placed students are quite similar; even without adjusting for year, borough, or grade, the random assignment assumption is plausible. Due to the large sample size, contrasts are frequently statistically significant, but the associated magnitudes are small, generally not greater than a percentage point or two.

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

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

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

to be Hispanic and less likely to be placed in commercial hotels (by about 3 pp in each case), but these differences are likely attributable to borough and year of shelter entry.

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

Besides confirming the comparability of treated and untreated students, the remainder of Tables 1.3A and 1.3B provides rich detail about the characteristics of homeless students and their educational outcomes. Most notably, homeless students struggle in school. They are chronically absent, acutely non-proficient, and unstably schooled[51].

Figure 1.3 makes clear how these students compare with their housed peers. The figure presents kernel density plots of days absent, pooling across school years 2010–2015 in my Complete sample (which includes non-homeless students), separately for K–8 and high school. The average homeless primary school student misses 26.9 days per year, or 1.5 times the DOE standard of chronic absence (which is 10 percent, or approximately 18 days). Were this pattern to hold throughout grades K–8, such a student would miss well in excess of a full school year by high school. By comparison, the averaged housed student misses 10.9 days per year. Matters are even more extreme for homeless high school students, who are absent an average of 45.5 days per year, compared with 21.2 days among housed students—and here, estimates are biased downward as drop-outs are selected out of the sample.

[51] Appendix Table A.4 summarizes how key treatment and outcome measures vary by year of shelter entry. Tables A.5–A.12 present informative cross-tabulation-style summaries of sample shares, treatment, and selected outcomes by grade, borough, and year.

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

1.5.2 Primary School Main Results

Table 1.4 presents my main results for primary schoolers. Outcomes are listed in rows and specifications in columns; each cell corresponds to a separate regression. The first four columns give OLS treatment effects estimates, while the last four give IV. Standard errors clustered at the family group level are given in parentheses below each coefficient. The OLS cells also contain sample sizes in braces (analogous IV sample sizes are the same); IV cells present first-stage F-stats in brackets. Overall, the results show clearly that local shelter placement benefits homeless students educationally.

There is a major attendance impact. According to my Base specification (Column 1), which controls for year, month of shelter entry, borough, and grade, homeless students placed in shelters in their school boroughs miss 2.8 fewer school days in the year of shelter entry, compared with those placed out-of-borough. As expected given covariate balance, additionally controlling for student and family characteristics in my Main specification (Col 2) hardly changes the coefficient, which drops to 2.4 days, but remains highly significant. Including lagged prior-year absences (Col 3) or school and shelter fixed effects, along with time-varying school characteristics, (Col

[52]Additional tables and figures comparing homeless and housed students are shown in Appendices A.6.2 and A.7.1. Additional tables exhaustively describing homeless students are shown in Appendix A.6.1.

4) have no further effect. Compared with out-of-borough students, this is an absence reduction of 8.3 percent. Using the absence rate as the dependent variable yields the same conclusion. According to my preferred Main specification, which strikes a balance between extensive controls and overly-refining the unit of comparison, the absence rate improves by 1.5 pp (8.8 percent) with local placement.

These are powerful effects. However, my IV results, presented in Cols 5–8, suggest these ATE's may, if anything, understate matters. Given the compelling evidence for random assignment in Tables 1.3A and 1.3B, my preferred IV interpretation is as indicative of treatment effect heterogeneity—the contrast between ATE's and LATE's. Nevertheless, skeptical readers may also regard my IV results as an endogeneity check.

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

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

Stability gains are equally impressive, on average. According to OLS, in-borough placement dramatically reduces the probability of transfer for the average student.

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

There is also evidence that in-borough placement improves academic performance. Per OLS, locally placed 3rd–8th graders are a statistically significant 0.9–1.3 pp more likely to be proficient in both Math and English. While small in absolute terms, 1 pp represents a 14.2 percent increase in the probability of proficiency, compared to the out-of-borough baseline of 7 percent. Most of this improvement is attributable to better Math performance. According to my preferred Main specification (Col 2), Math proficiency rates increase by 1.2 pp (an 8 percent increase relative to a baseline of 15 percent), while the differential in English performance is a statistically insignificant 0.8 pp. However, in the Base specification (Col 1), both coefficients are significant and of similar magnitude (0.014 for English and 0.016 for Math), so there is some evidence of across-the-board gains. The IV point estimates follow a similar pattern. Focusing on the Main specifications (Col 6), the probability of overall proficiency increases by 12.1 percentage points, with Math improving by 17.5 pp and ELA by 10.5 pp. However, the IV confidence intervals are wide and not exclusive of zero.

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

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

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

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

Identifying and characterizing compliers is straightforward in the textbook binary instrument case (Angrist and Pischke, 2008). Calculating complier shares and characteristics is more complicated when, as here, the instrument is continuous. To do so, I adopt the approach to discretizing the continuous instrument outlined in Dahl, Kostøl and Mogstad (2014) and Dobbie, Goldin and Yang (2018). Full methodological details are described in Appendix A.3; here I focus on results.

I estimate that compliers comprise 13 percent of my primary school sample (Table A.19). Table 1.5 describes their characteristics, as well as how they contrast with non-compliers (always- and never-takers). Standard errors (in parentheses) and t-statistics testing the difference in means (in brackets below group differences) are calculated using 200 bootstrap replications, clustering for family groups. For brevity, only the most interesting attributes are shown; Tables A.20A and A.20B include additional characteristics.

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

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

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

Geography is also pivotal. The majority of compliers (52 percent) are from the Bronx, versus a third of non-compliers. While this contrast narrowly misses statistical significance, related, more-precise results for other boroughs confirm this impression: just 6 percent of compliers, but 32 percent of non-compliers, come from Manhattan,

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

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

Queens, and Staten Island. This is in keeping with the scarcity story. When policy gets tighter, those best positioned to benefit are families from the Bronx, which is home to a plurality of the City's shelter units as well as the City's PATH intake center. Easy access facilitates multiple application rounds, yielding a competitive advantage vis-a-vis out-of-borough competition[55].

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

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

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

[56]See Chapter 2 for a longer discussion of this point.

1.5.3 High School Main Results

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

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

One striking departure from OLS, however, is academic performance. IV results imply a massive and statistically significant performance boost for compliers. Ac-

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

cording to my Main specification, compliers are 76 pp more likely to take a Regents and 72 pp more likely to pass one when placed locally. These results are suggestive of significant gains, even if the linearity assumptions embedded in 2SLS are too strong to be interpreted literally in this case.

To better understand these IV results—as well as their differences from the primary school pattern—it is helpful to return to Table 1.5 to look at the characteristics of high school compliers. While the small sample sizes preclude detecting many statistically significant differences, taking the point estimates at face value provides suggestive explanations.

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

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

1.5.4 Primary School Robustness

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

Table 1.7 provides robustness checks to address these three issues, with alternative treatments in supercolumns, identification assumptions (estimation methods) in columns, and time periods in panels. As before, each row considers a distinct outcome (the most important of those discussed earlier). Each cell is a separate regression, all of which consist of my preferred Main specification. I consider two alternative treatment definitions (school district and distance), one alternative instrument (days to eligibility), and one alternative treatment effect time period (the year post-shelter-entry[58]).

Panel A continues to assess outcomes during the year of shelter entry. The first three columns retain my main borough-based treatment definition. Columns 1 and 2 are repeated from Table 1.4 for completeness. Column 3 presents results for my alternative days to eligibility instrument and confirms my main IV results. Days to eligibility compliers have a statistically significant 15-day attendance improvement. Results are imprecise for proficiency and promotion but suggestive of small effects. Compliers are also a statistically significant 19 pp less likely to leave DOE, which is a stronger finding that that using the ineligibility rate. The days IV point estimate for school changes similarly indicates a larger benefit to compliers than does the ineligibility rate[59].

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

[59]As described in Appendix Tables A.26 and A.27A–A.27B, days to eligibility compliers do, in

The second set of columns considers an alternative treatment definition: placement within one's school district. Since the City is comprised of 32 school districts, this narrower unit of geography provides a more stringent treatment standard. Along with the change to the treatment indicator, Equation 1.1 is modified to include school district rather than school borough dummies.

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

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

fact, resemble ineligibility rate compliers: in particular, they come from medium-large families.

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

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

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

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

compliers to have a statistically significant 15 pp greater likelihood of proficiency.

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

Overall, my main results are robust to alternative treatments and identification strategies, and display explicable time dynamics. That the distance treatment measure confirms the official borough-based treatment definition is comforting: it demonstrates there is an underlying proximity effect, and not simply quirks of county[61]. Compliers in the days to eligibility IV substantially overlap ineligibility rate compliers; the former also helps guard against sample selection issues. Finally, treatment effects, while still present in the year after shelter entry, appear to attenuate quite rapidly, with the biggest enduring boon being school stability. To summarize, local placement benefits homeless primary school students, on average; some benefit tremendously.

1.5.5 High School Robustness

Table 1.8 assesses the robustness of these results to the same alternative treatments, identification strategies, and time periods as considered for primary schoolers.

The OLS findings in the year of shelter entry are confirmed (Panel A). Per school district treatment (Col 4), local placement reduces absences by an average of 2.7 days and the probability of changing schools by 10 pp, both on par with their borough treatment counterparts. Similarly, distance treatment (Col 7) demonstrates these

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

effects are continuous. Absences decrease by 0.27 days for each mile shelter is closer to school, while the probability of changing schools is reduced by 1.1 pp per mile. As with borough, other outcomes appear unaffected. One exception is that the reduced probability of leaving DOE in the borough case is not replicated with the other treatment definitions.

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

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

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

1.5.6 Panel Results: Student Fixed Effects and Event Study

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

First, a student fixed effects model permits a qualitatively different robustness check relying upon wholly alternative identification assumptions. Table 1.9 presents my student fixed effects results, which dispense with unobserved spell-invariant student heterogeneity, yielding a quite exacting comparison of same-student outcomes when placed locally or distantly. In the pooled K–12 Main sample, 3.8k students (8.7 percent) experience multiple homeless spells during the 2010–2015 period; 59.2 percent of these experience different treatment assignments (i.e., both in- and out-of-borough) during these stays. I consider five outcomes, all defined as before except proficiency, which, given the pooling of primary and high school homeless spells, is now the union of (a) joint English and Math proficiency for grades 3–8 and (b) passing any Regents for grades 8–12 (eighth graders are eligible to take Regents). As before, all outcomes correspond to the year of shelter entry. The first three columns present borough treatment and the following three assess distance.

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

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

Beyond delivering a student fixed effects ATE estimator, the longitudinal nature of my data also allows me to follow homeless students over the courses of their educational careers and thus provide a clear answer to the central causality question currently debated by homelessness researchers. While it is undeniably true that homeless students fare worse educationally than their housed peers (see Figure 1.3), it is not homelessness, nor entering the shelter system per se, that causes these unfortunate outcomes. Instead, homeless students' struggles begin *prior* to shelter.

To see this, Figure 1.4 returns to my Main K–8 sample but expanded to include a one-year window around shelter entry, summarizing treatment effect dynamics for five key outcomes—absences, school changes, promotion, proficiency, and leaving DOE. Because the data aggregates across years and grades, outcomes are first detrended and scaled to the 2014 third grade mean. Years are measured relative to first shelter entry. Only students whose educational records are observed in all three years are included, and only for their first observed homeless spell, in order to guard against selection bias[62].

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

Table 1.10 formalizes this event-study analysis. Each column presents predicted average outcomes in the year of shelter entry, as well as in the years preceding and succeeding it, separately for treated and untreated students[63]. Confirming the by-now familiar patterns, treated and untreated students are similar before and after their shelter experiences. However, during the year of shelter entry, absences for in-borough students are 2 days less and their probability of changing schools is 19 pp lower; both gaps reflect smaller increases relative to pre-shelter rather than absolute reductions. The relative reduced probability of school changes persists in the following year as well (by 5 pp). Other contrasts are imprecise, though there is suggestive evidence that proficiency slightly increases among treated students during the year of shelter

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

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

entry[64].

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

1.5.7 Extensions: Mechanisms

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

One important causal channel is length of stay in shelter (LOS). In Chapter 2, I demonstrate that families placed in their borough of prior residence stay in shelter considerably longer than those placed distantly. Table 1.11 confirms this finding, though here school defines borough of origin. The setup is the same as Table 1.4. I consider several length of stay measures.

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

[64]Discrepancies from the main analysis are due to the more rigid sample restriction that students be observed in all three years, which, for example, eliminates younger and older students.

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

in the revealed preference sense, to be placed locally; when they are, they stay.

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

Table 1.12 suggests length of stay does contribute to observed treatment effects. As in Table 1.7, this table assesses outcomes in the year *following* shelter entry (that is, year $t + 1$), but allows treatment effects to vary between students remaining in shelter (stayers) and those who've exited (leavers) by including an indicator for "still homeless" during this school year along with its interaction with in-borough placement[66]. The reason for considering outcomes in the year post-shelter-entry is that students are homeless for differing lengths of time during the year of shelter entry; continued homelessness in the following school year is thus a fairer proxy for length of stay, as all families have a least a full summer to navigate housing options. All results feature the Main K–8 sample and control for Main covariates.

Focusing on the OLS results in Panel A, continued homelessness, as expected, slightly negatively impacts educational outcomes, but treatment (in-borough placement) attenuates these effects. The most notable effects are with attendance. Students still homeless a year after shelter entry miss an additional 3.5 days of school,

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

compared with re-housed students (Col 1). Those who were placed in-borough, however, miss one day less. By contrast, there is no enduring attendance effect among students who've exited. Students remaining in shelter are also at an elevated risk for changing schools, by 4.2 pp; having been placed in-borough reduces this risk by 6.6 pp. There is no effect on school stability among leavers.

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

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

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

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

A second causal mechanism is school changes. Excess mobility has been established as an educational impediment in prior research. Table 1.13 confirms this is true in my data as well. Similar in setup to Table 1.12 but returning to outcomes in the year of shelter entry, Table 1.13 interacts treatment with the indicator for school changes (to this point considered as an outcome), thereby allowing placement effects to differ among students who transfer and those who stay put. OLS (Panel A) gives three key results. First, mobility is associated with impaired performance. Absences increase; proficiency, promotion, and retention decrease. While I can't be sure movers are similar to non-movers on unobservables, it is exceedingly likely, on the basis of the well-developed student mobility literature, that this relationship is causal.

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

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

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

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

1.6 Conclusion

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

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

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

School-based shelter placements have other effects as well. As I show in Chapter 2, families placed in shelters in their home boroughs remain in shelter longer, by about 13 percent, or roughly 50 days. At an average nightly cost of $200, this means students' educational gains cost the City about $10,000 per family, or, since the average family has two children in school, $5,000 per student. At the same time, families earn about 10 percent more when placed locally and also access more public benefits. For policymakers, one challenge is to determine the proper trade-off between these benefits and costs. More fundamentally, it is also necessary to understand whether longer shelter stays are themselves intrinsically valuable. Neighborhood-based placements are clearly expensive, but if, in addition to their educational merits, they enhance household and housing stability post-shelter, the additional upfront costs may be a wise investment.

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

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1.8 Tables

Table 1.1: Data and Sample Overview

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

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

Table 1.2: Ineligibility Instrument Shelter Entrants Comparison

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

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

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

Table 1.3A: Descriptives and Random Assignment

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

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

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

Table 1.3B: Descriptives and Random Assignment

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

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

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

Table 1.4: Primary School (K-8) Main Results

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

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

Table 1.5: Complier Characteristics, Ineligibility Rate Instrument

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

Main sample. Treatment is in-borough placement. Instrument is 15-day moving average of the initial ineligibility rate for 30-day application period. Compliers are those students placed in-borough when the ineligibility rate is high, but not otherwise. Non-compliers consist of always-takers and never-takers. Compiler and non-complier characteristics, adjusted for year and month of shelter entry, are estimated from the algorithm described in Appendix A.3.4. Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family.

Table 1.6: High School (9–12) Main Results

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

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

Table 1.7: Primary School (K-8) Robustness Checks

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

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

Table 1.8: High School (9–12) Robustness Checks

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

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

Table 1.9: Student Fixed Effects Results, Grades K–12

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

Setup follows to Table 1.4. Data consists of Main sample pooling grades K–12. Each cell reports the coefficient on in-borough shelter placement from a regression of the row-delineated outcome on the treatment indicator, controlling for the column-enumerated covariates, using the super-column-indicated treatment definition. All regressions include individual student fixed effects. The unit of observation is the student-school-year. Proficient is defined as having passed both ELA and Math State tests for grades 3–8, or having passed any Regents for grades 8–12. See the note for Table 1.4 and the text for additional detail. Standard errors clustered at family group level in parentheses. Number of observations in braces. * $p < 0.10$, ** $p < 0.05$

Table 1.10: Primary School (K-8) Event Study Results

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

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

Table 1.11: Primary School (K-8) Homelessness Outcomes

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

Setup is identical to Table 1.4, except outcomes assess student length of stay in shelter. Treatment is defined as shelter placement within one's school borough of origin. Each cell reports the coefficient on in-borough shelter placement from a regression of the row-delineated outcome on the treatment indicator, controlling for the column-enumerated covariates, using the super-column-indicated method. The unit of observation is the student-school-year. The sample is limited to shelter entry years for students in grades K–8 during school years 2010–2015. It excludes students in special school districts 75, 79, 84, and 88, as well as those enrolling in DOE subsequent to shelter entry. Observation counts are given for days absent regressions. Standard errors clustered at family group level in parentheses. First-stage F-stats in brackets. See the note for Table 1.4 and the text for additional detail. $p < 0.10$, ** $p < 0.05$.

Table 1.12: Primary School (K-8): Mediating Effects Remaining in Shelter on Post-Shelter-Entry Year Outcomes

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

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

Table 1.13: Primary School (K-8): Mediating Effects of School Changes

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

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

1.9 Figures

Figure 1.1: Instrument and Treatment Quarterly Time Series: Detrended

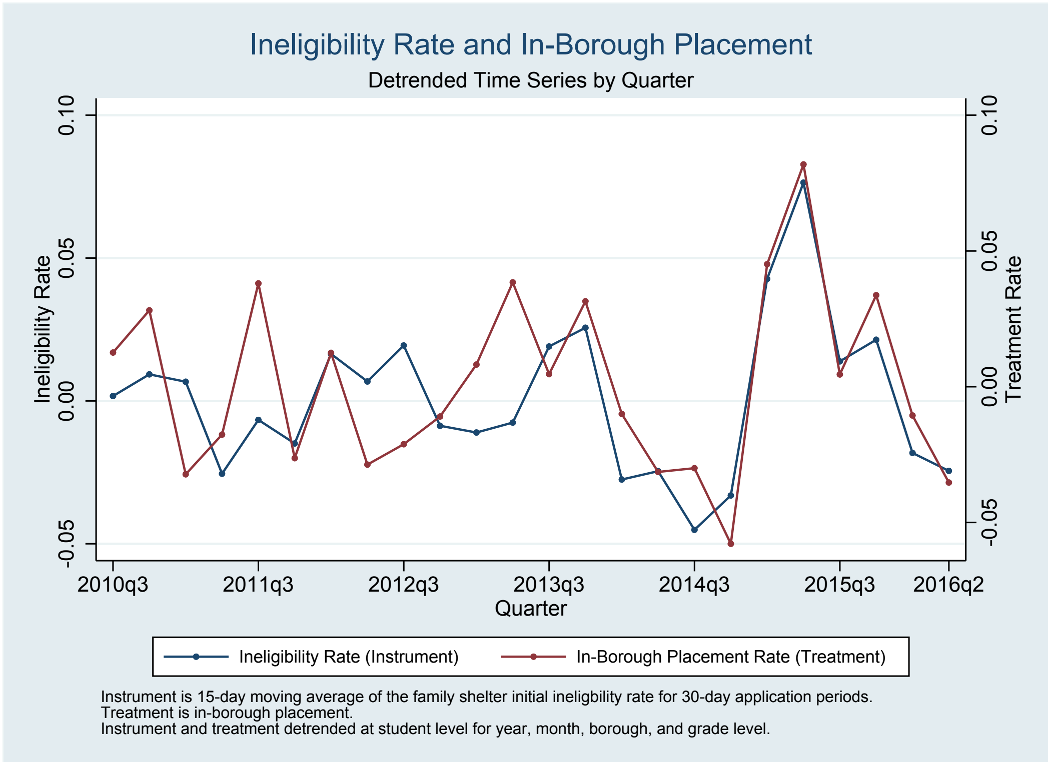


Figure 1.2: Family Shelter Ineligibility Rate

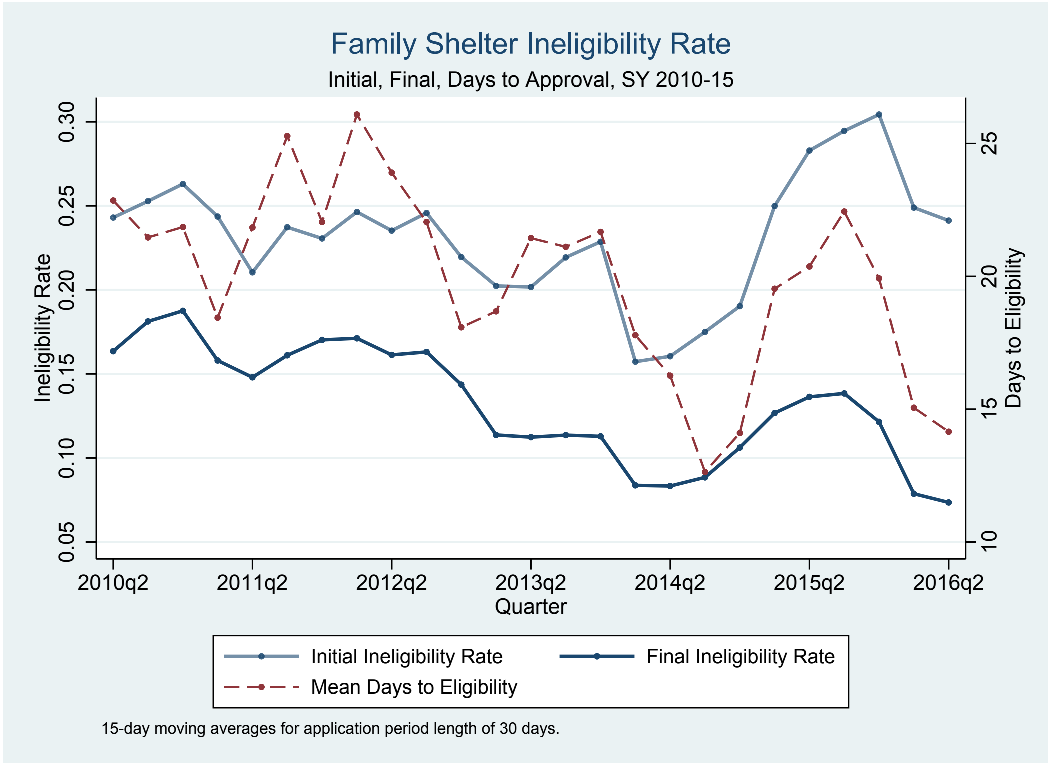
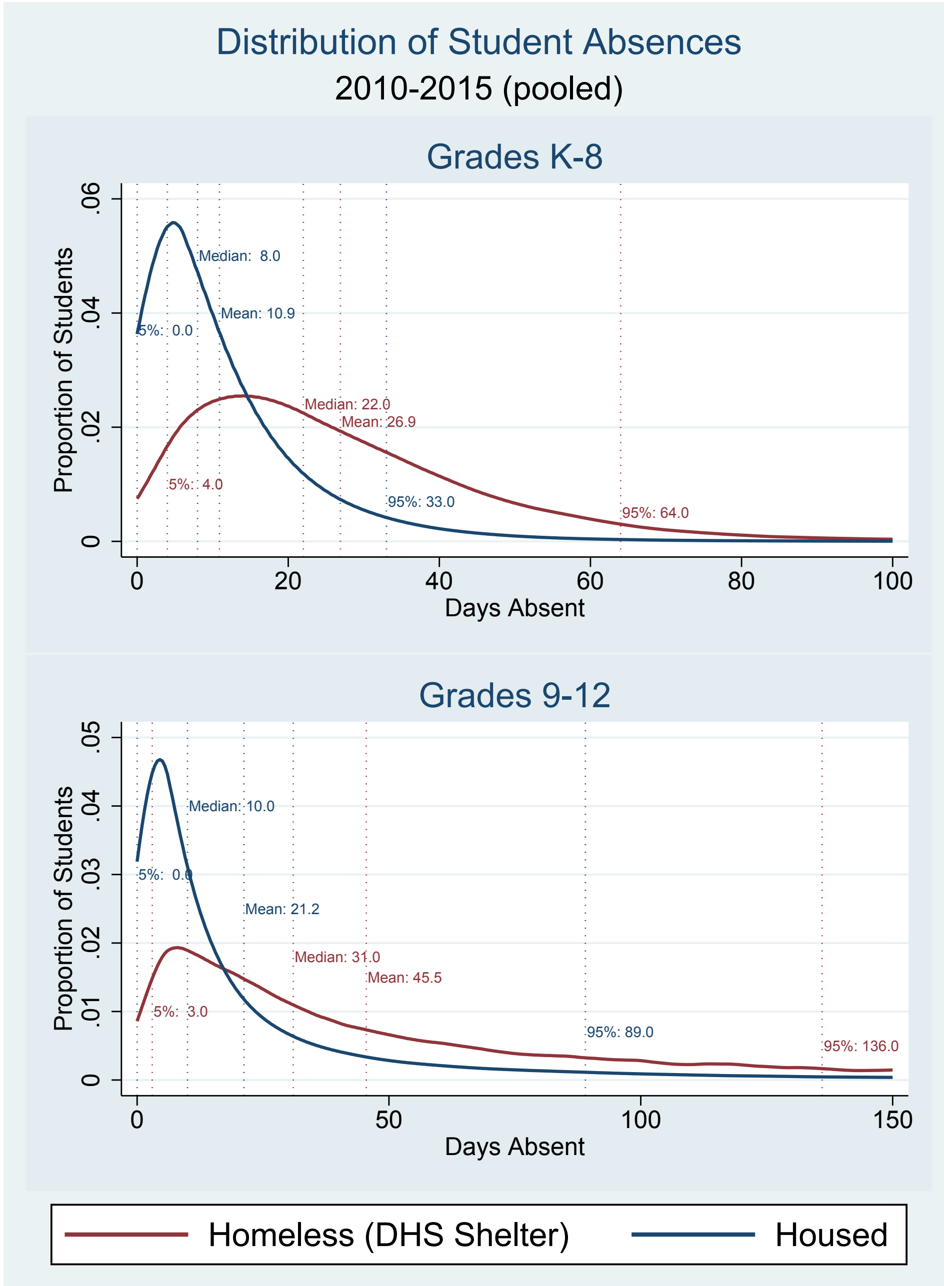
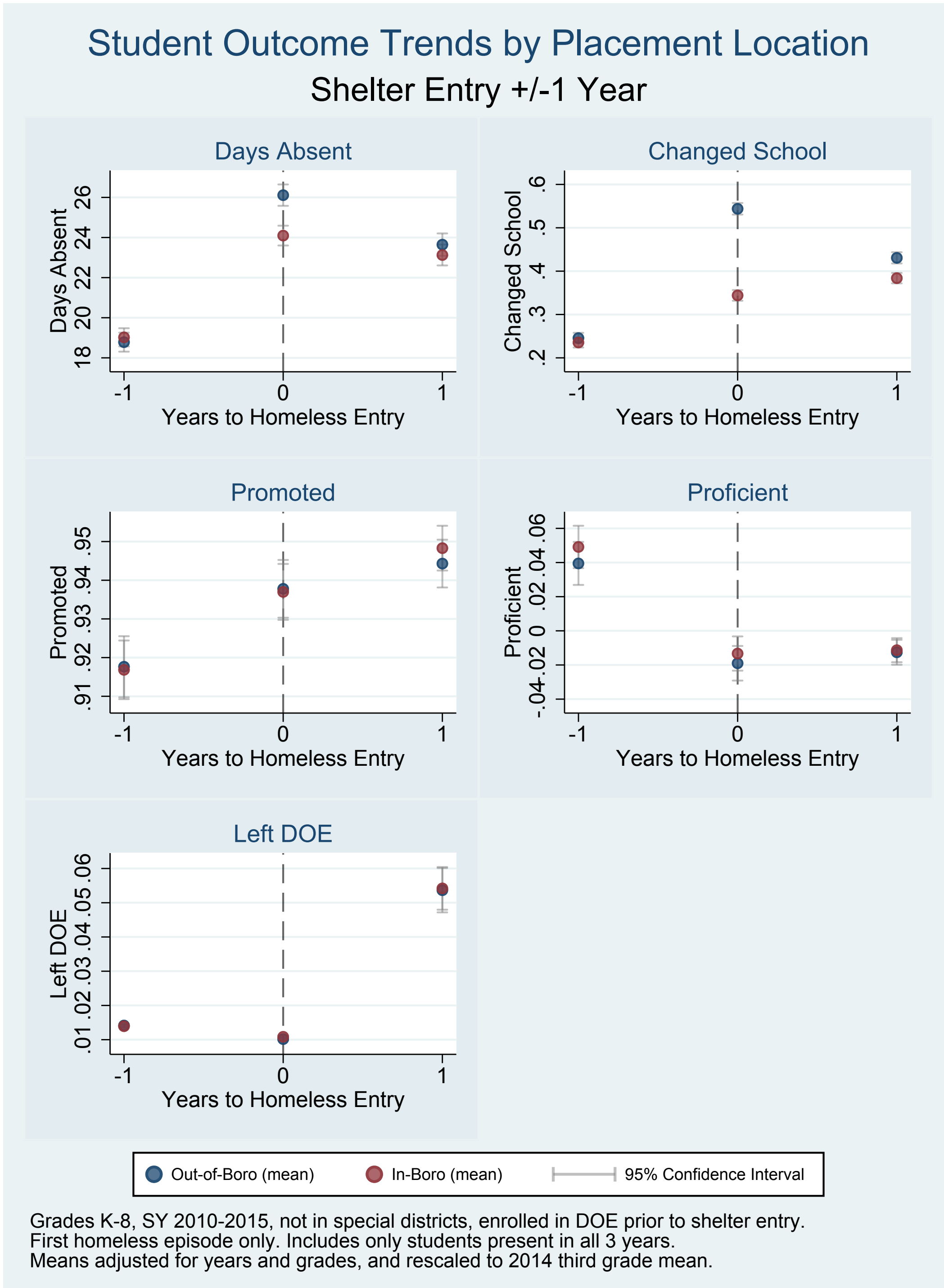


Figure 1.3: Distribution of Public Student Absences



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

Figure 1.4: Three-Year Student Outcome Trends by Placement



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

Appendix A

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

A.1 Policy, Literature, and Data Appendix

This section contains an expanded discussion of Section 1.3 in the main text. Portions are repeated for convenience.

A.1.1 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

[1]For details, see Chapter 2.

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

[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.

shelter stays for up to 10 days while dedicated investigation staff assess their claims, which may involve conversations with landlords and visits to prior addresses. Those found eligible may remain in their initial shelter placements as long as necessary, while ineligible families may appeal their decisions through a fair hearing process or reapply, as many times as desired. Most ineligibilities occur due to failure to comply with the eligibility process or because other housing is found to be available. Families may also "make their own arrangements" and voluntarily withdraw their applications. Eligible families may request transfers to more suitable shelter units as they become available.

The shelter system into which these families are placed is proportionately vast. Administered by the Department of Homeless Services under the auspices of the Department of Social Services[4], it consists of more than 500 distinct shelter sites spread across the five boroughs (New York City Independent Budget Office, 2014; The City of New York, Mayor's Office, 2017). Although DHS runs several shelters directly, most day-to-day shelter operations are managed by contracted non-profit social service providers, as is the norm with human services in NYC. Fully 82 percent of DHS' budget ($1.06 billion in FY19) is allocated to some 282 contracts for homeless family services (New York City Office of Management and Budget, 2018).

About three-fifths of families reside in one of the City's 169 traditional "Tier II" homeless shelters, which offer on-site social services and security but otherwise resemble the sorts apartment buildings typically found in low-income communities; indeed, landlords often convert private market buildings to shelters to cater to these more lucrative tenants[5]. The next most common form of temporary housing, comprising 276 sites and about a quarter of the population, are cluster, or scatter, units, so named

[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.

because they are localized groups of shelter apartments spread throughout otherwise private buildings in a given area and serviced by a single provider. The remaining 13 percent of families are placed in commercial hotels, which offer fewer services but a flexible way for the city to expand capacity to meet needs.

The costs are substantial. In the fiscal year ending in June 2018, DHS spent $1.2 billion to shelter homeless families; the average cost per family *per day* in shelter was $192 (New York City Office of Management and Budget, 2019; New York City Mayor's Office of Operations, 2018). And this and understatement, as it excludes administrative costs, prevention programs, and permanent housing subsidies, as well as services and benefits administered by other agencies.

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

To help address the challenges homeless students face, the City has maintained the explicit goal of placing homeless families in shelters near their youngest child's school since at least 1998 (The City of New York, Mayor's Office, 2017; New York City Mayor's Office of Operations, 2002; New York City Department of Education, 2019). In part, this neighborhood-based shelter placement policy facilitates compliance with the federal McKinney-Vento Homeless Assistance Act (42 U.S.C. 11431 et seq.), which requires local education agencies to provide the services necessary for homeless students to remain in their schools of origin, if desired[6]. But increasingly it has come to reflect the conviction that keeping homeless families connected to their communities of origin—close not only to schools, but also to family, friends, jobs, places of worship, and other sources of support—is a means of expediting the return to more stable housing (The City of New York, Mayor's Office, 2017).

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

[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).

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 Chapter 2.

A.1.2 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 Wein-

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

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

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

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

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

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

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

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 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 commu-

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

nities shape social and schooling opportunities, it is these more micro habitats that regulate educational results.

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

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

The evidence on mobility is even more nuanced. Changing schools tends to impede performance of movers and incumbents alike (Hanushek, Kain and Rivkin, 2004), especially in the short-run and when moves are intra-district; on the flip side, there is some evidence that 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.1.3 Data and Sample

DHS Data

One major contribution of this paper, along with its companion piece, Chapter 2, 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 Chapter 2.

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 Chapter 2, 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 Chapter 2); 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 Chapter 2.

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,

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

using probabilistic matching techniques based on Social Security Number (SSN), first name, last name, and date of birth[13]. The HRA data also includes information on race and self-reported education. In a similar fashion, the New York State Department of Labor (DOL) provides data on quarterly employment and earnings, through a deterministic match on SSN[14].

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

For purposes of assessing family outcomes, as in Chapter 2, 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.

[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.

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

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.

DOE Data

I then match these candidate homeless students to a database of school records maintained by DOE, spanning school years 2005-06 to 2016-17 (my second foundational data source). DOE's database contains records for each student during each school year, with separate topical tables for June biographical information (demographics, student characteristics, and enrollment details, including school ID and attendance), test scores (3–8 grade state standardized tests and Regents for high schoolers), and graduation (for high schoolers). The biographical table is given the "June" designation because it is reconciled at the end of each school year, in June, and reflects each student's most up-to-date information as of then. 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 1.4. 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

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

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.

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

[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.

of shelter policy, my interest is in the first group.

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

Table A.1 details my match results by birth year, focusing specifically on children aged 5–18 during a shelter stay. Overall, 64,728 of 74,058 unique candidate students (87 percent), accounting for 78,465 of 88,582 student-homeless-episodes present in the DHS data (89 percent), have successful DOE matches[18]. For students in the "core" birth (calendar) years of 1995–2008, the match rate is 90 percent or greater; these children are in the prime schooling years during the 2010–16 period that comprises my homelessness window. As expected, match rates are lower for older and younger children[19].

[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.

[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.

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 1.1.

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.

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.

[20]The right to shelter applies regardless of whether prior residence was in NYC.
[21]See Angrist and Pischke (2008).

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 1.4.

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 1.1, 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 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 A.6.2 provides additional statistics describing this sample.

Additional Data

As described in Section 1.4, 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 1.4.

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].

[22]By contrast, in Chapter 2, 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

A.2 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 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].

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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 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.

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:

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

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

The Lagrangian for this problem is:

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

with first-order conditions[28]

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

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

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

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

[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.

statics exercise. Applying the implicit function theorem to Equation F,

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

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.

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

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

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

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

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

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

To summarize, school-predicated shelter placements affect the relative price of schooling that homeless families face. When school is closer, it becomes more attractive, but so do competing priorities, like seeing family or friends, or enjoying consumption goods in their presence. Consequently, the net price effect of neighborhood-based shelter assignments is theoretically ambiguous. What seems more clear—though it remains an assumption—is that distance reduces families' resources, by diminishing access to preexisting social supports and depreciating the value of neighborhood-specific human capital[30]. If distance increases the relative cost of schooling, price and

[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.

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.

A.3 Empirical Appendix

This section contains additional details about my empirical methods, described in Section 1.4 in the main text.

A.3.1 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 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.

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

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

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:

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

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

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

$$N_{ip} = \tau^1 Z_{ip} + \mathbf{X}_{ip}\boldsymbol{\beta}^1 + \boldsymbol{\varepsilon}_{ip}^1 \qquad \text{(first stage)} \tag{A.2}$$

[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—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.

$$Y_{ip} = \tau^{IV}\widehat{N}_{ip} + \mathbf{X}_{ip}\boldsymbol{\beta} + \boldsymbol{\varepsilon}_{ip} \qquad \text{(second stage)}$$

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

A.3.2 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 1.1 (detrended), which give the quarterly time series for the ineligibility rate and treatment, the first-stage relationship between the ineligibility rate and local placement is quite strong. Of particular note is how the relationship strengthens after detrending the instrument and treatment for base covariates, which is the econometrically relevant case. The picture is even clearer in the month-level scatterplots presented in Figures A.11 (raw) and A.12 (detrended): the linear first-stage relationship is much stronger once the year, seasonal, borough, and grade influences are removed. The probability of in-borough placement is considerably higher when the ineligibility rate is high (after adjusting for base trends).

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

Fortunately, as I argue in the main text, there is strong evidence that this sort of sample selection is not present, with Table 1.2 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 eligibil-

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

ity rate generally trends upwards, while the final ineligibility rate trends downward, though with small amplitude. At the same time, diversions increase in 2013 and decline after 2014, while own arrangements basically hold steady.

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

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

The lack of endogenous sampling can also be reconciled by appealing to theory. To illustrate this situation, label all family unobservables as "ability" and, for convenience, consider families of three types, low. medium, and high ability. Medium

[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.

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.

A.3.3 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.

A.3.4 Measuring and Describing Compliers

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

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

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

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

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

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

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

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

The first insight is that the mean of a binary characteristic X is a probability,

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

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

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

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

A.3.5 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.

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

A.4 Additional Results

A.4.1 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 Chapter 2, 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 1.4. Reassuringly, the main findings are confirmed[39]. According to OLS, students placed in their home boroughs miss 2.1 fewer school days, are 10.5 pp less likely to change schools, and are 0.9 pp less likely to leave DOE (controlling for Main covariates). Once again, the IV results delivering LATEs for compliers are mostly greater in magnitude and still statistically significant. Treated compliers miss 20.5 fewer days and are 17.2 pp less likely to leave DOE, though they appear no less likely to change schools.

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

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

A.4.2 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 1.7 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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A.6 Supplementary Tables

A.6.1 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

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

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

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

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

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

Table A.5: Grade and School Boro Sample Shares

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

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

Table A.6: Year and School Boro Sample Shares

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

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

Table A.7: Borough Treatment by Grade and Boro

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

Treatment defined as placed in school borough.

See note to Table A.4 for sample restrictions.

Table A.8: Days Absent by Grade and Boro

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

See note to Table A.4 for sample restrictions.

Table A.9: Borough Treatment by Year and Borough

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

Treatment defined as placed in school borough.

See note to Table A.4 for sample restrictions.

Table A.10: Days Absent by Year and Boro

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

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

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

Data is Main sample, as defined in text.

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

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

Data is Main sample, as defined in text.

A.6.2 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.

A.6.3 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 A.3.4 for estimation method details.

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

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

Main sample. Treatment is in-borough placement. Instrument is 15-day moving average of the initial ineligibility rate for 30-day application period. Compliers are those students placed in-borough when the ineligibility rate is high, but not otherwise. Non-compliers consist of always-takers and never-takers. Compiler and non-complier characteristics, adjusted for year and month of shelter entry, are estimated from the algorithm described in Appendix A.3.4. Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family.

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

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

Main sample. Treatment is in-borough placement. Instrument is 15-day moving average of the initial ineligibility rate for 30-day application period. Compliers are those students placed in-borough when the ineligibility rate is high, but not otherwise. Non-compliers consist of always-takers and never-takers. Compiler and non-complier characteristics, adjusted for year and month of shelter entry, are estimated from the algorithm described in Appendix A.3.4. 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 1.4, 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 1.4 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 1.4, 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 1.4 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 A.3.4 for estimation method details.

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

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

Repeats Table 1.5 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 A.3.4. Standard errors and differences in means are calculated from 200 bootstrap replications.

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

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

Repeats Table 1.5 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 A.3.4. Standard errors and differences in means are calculated from 200 bootstrap replications.

A.7 Supplementary Figures

A.7.1 Stylized Facts

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

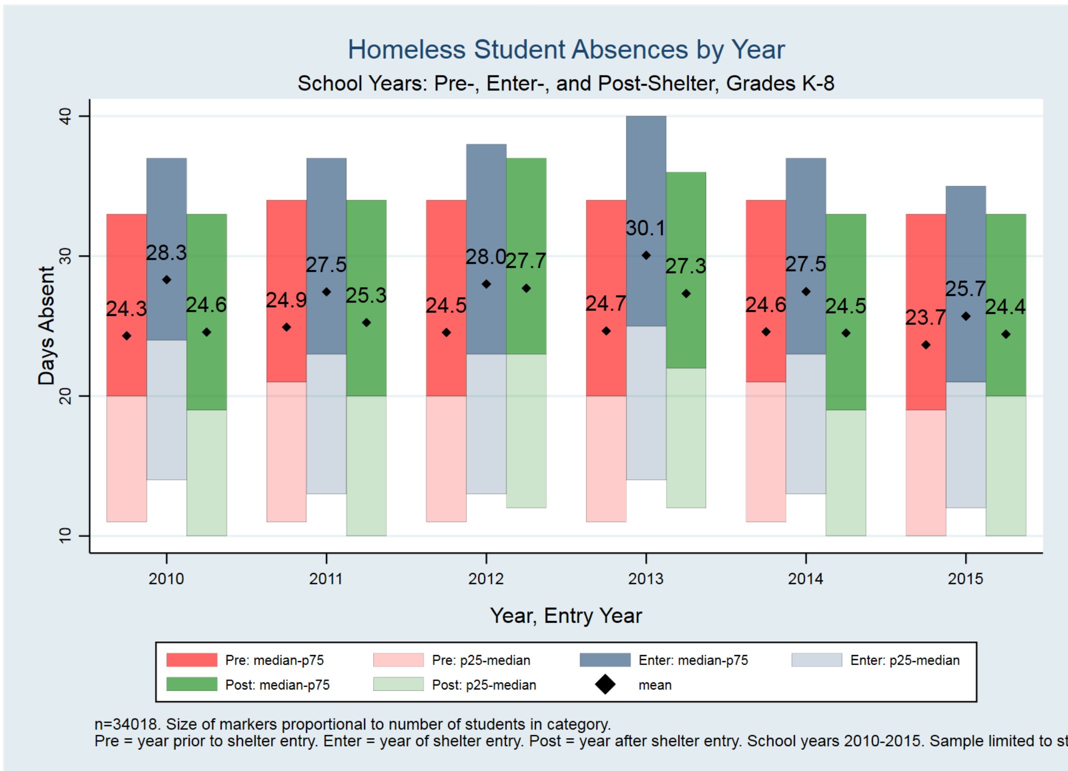


Figure A.2: Homeless High School Student Absences by Year

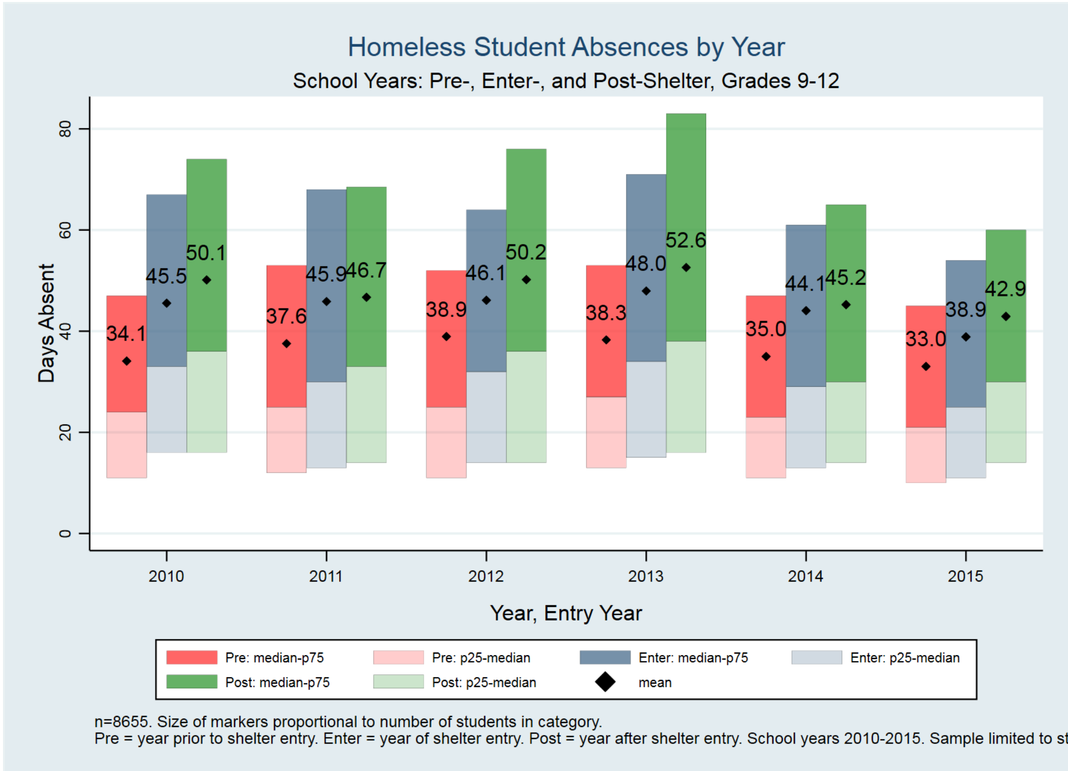


Figure A.3: Absence Persistence Summary

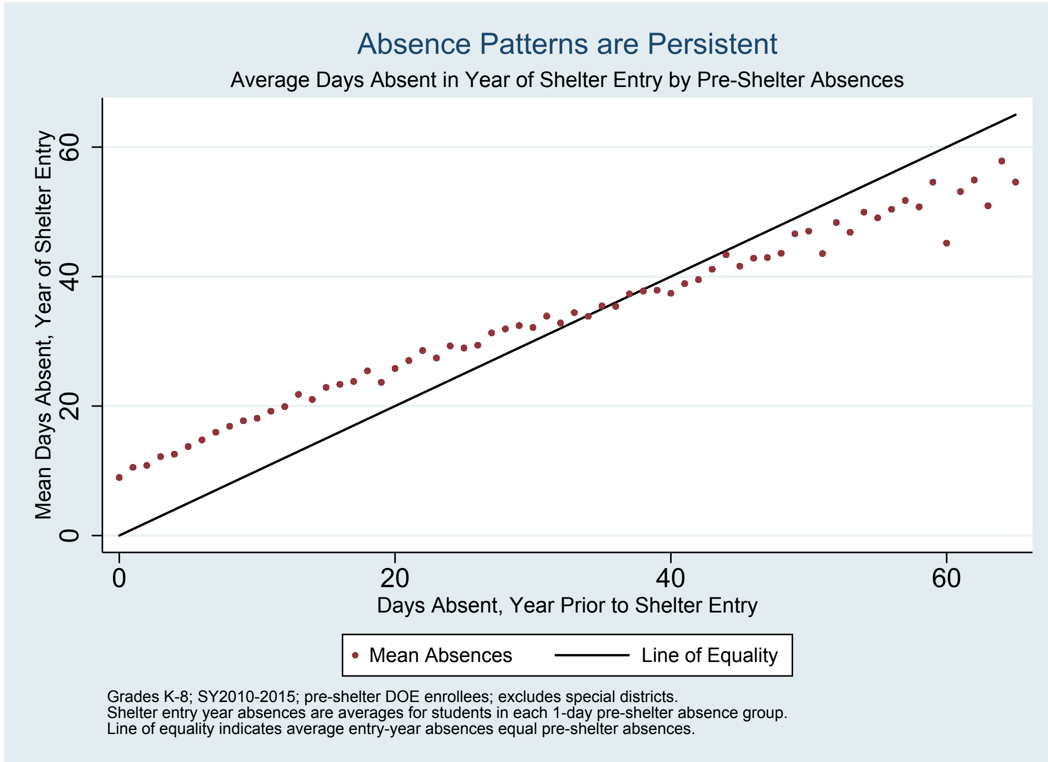


Figure A.4: Absence Persistence Detail

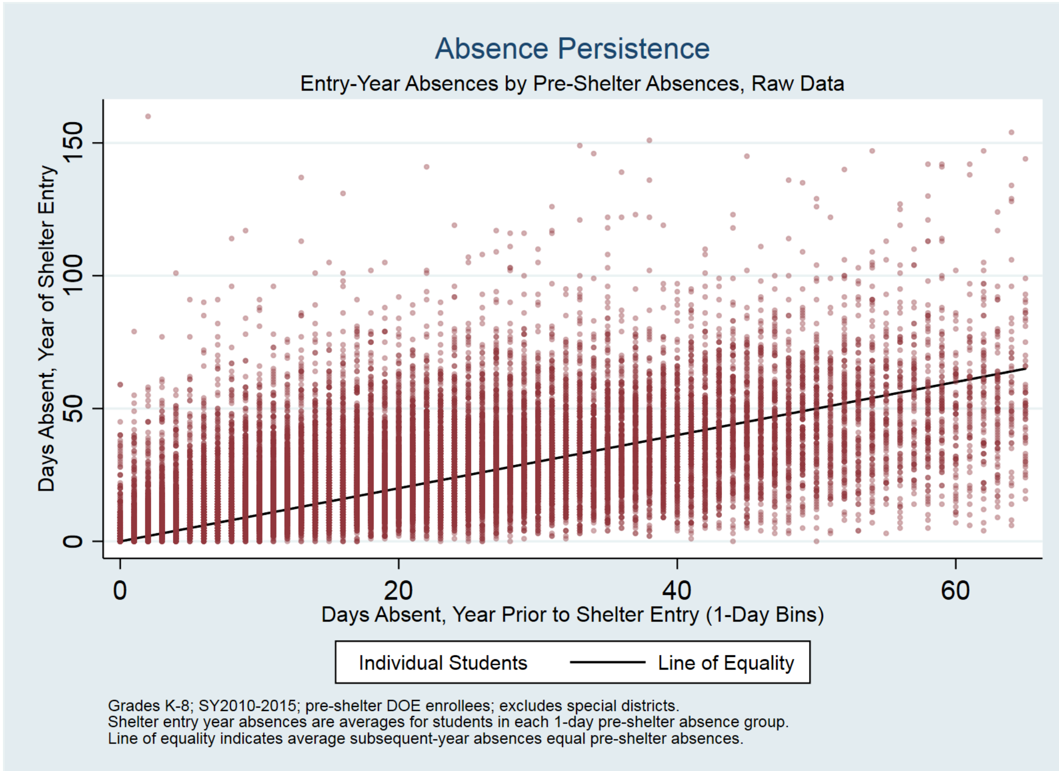


Figure A.5: Absences by Grade

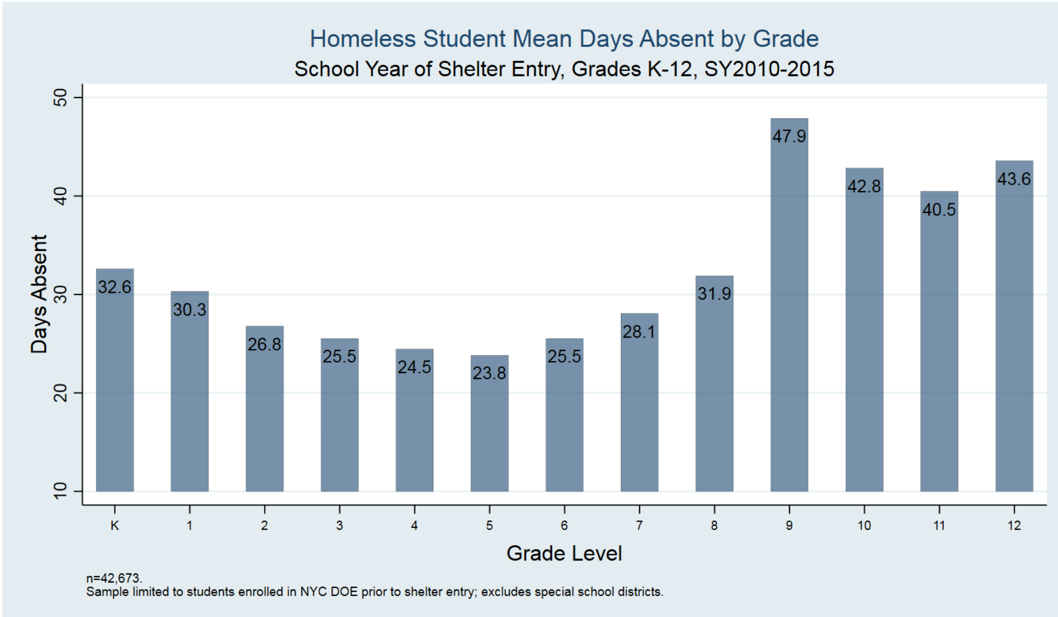


Figure A.6: Attendance and Proficiency

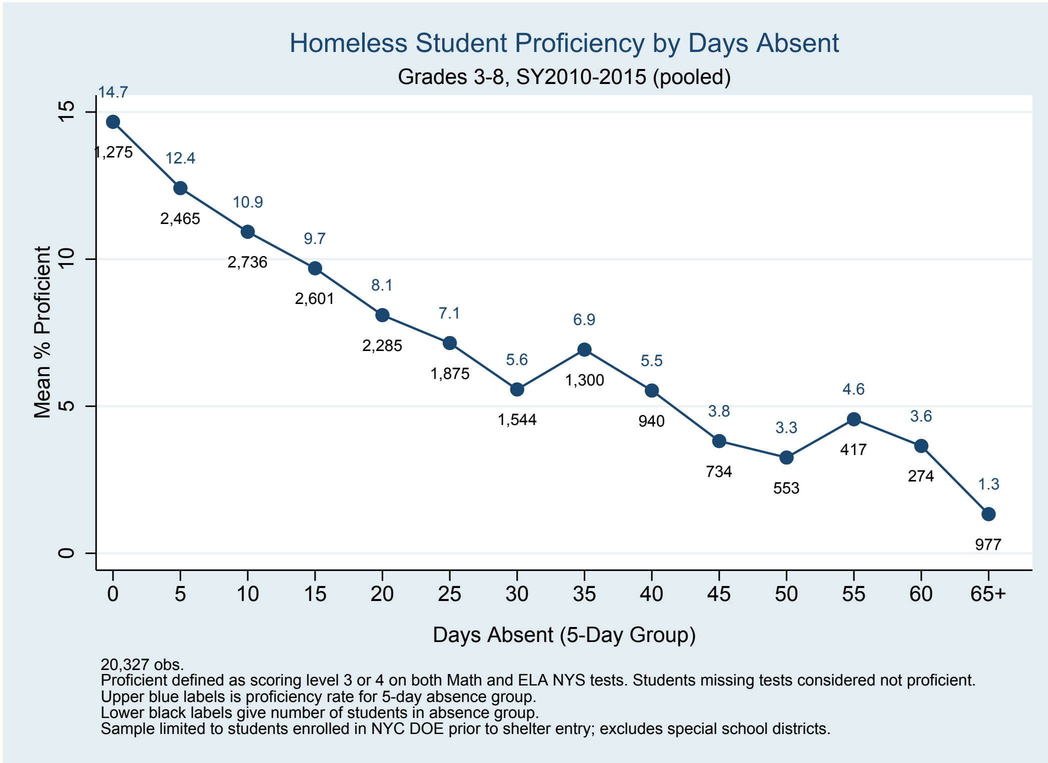


Figure A.7: Attendance and Promotion

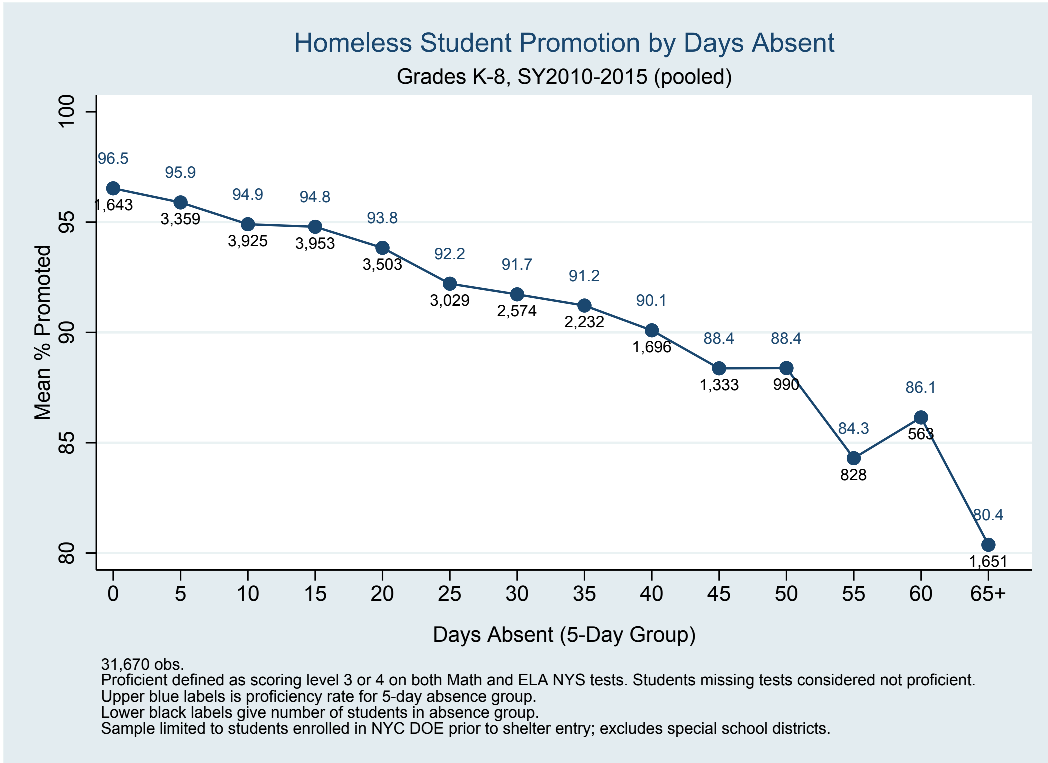


Figure A.8: NYC Public School Proficiency Rates

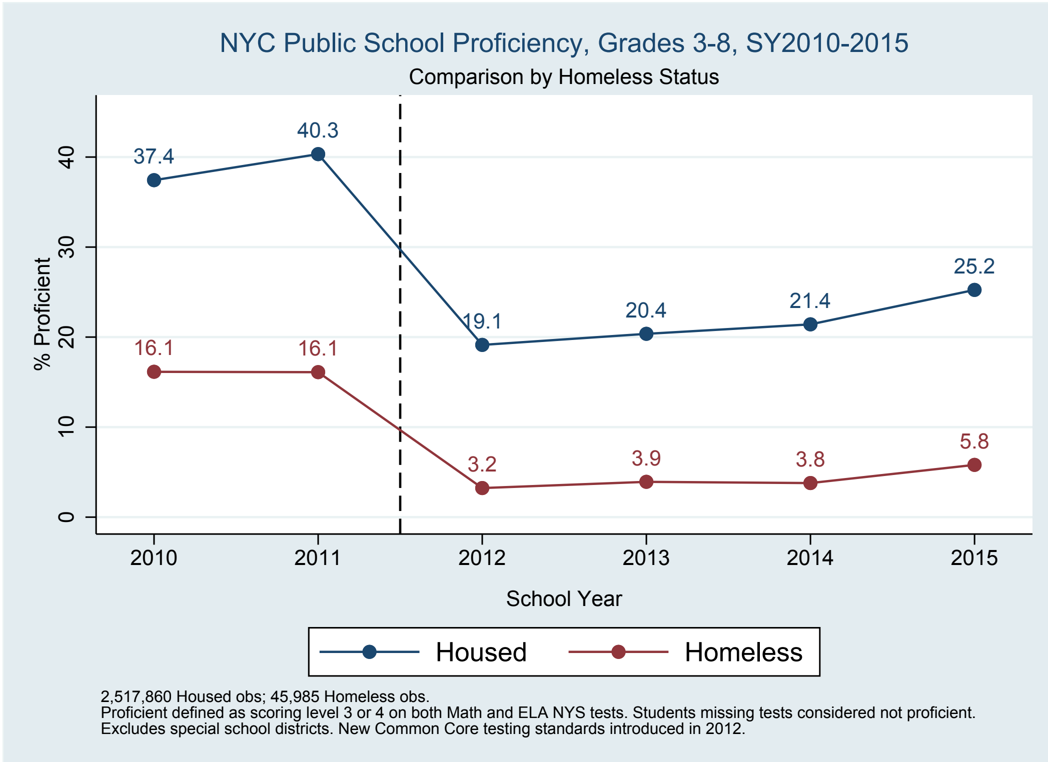
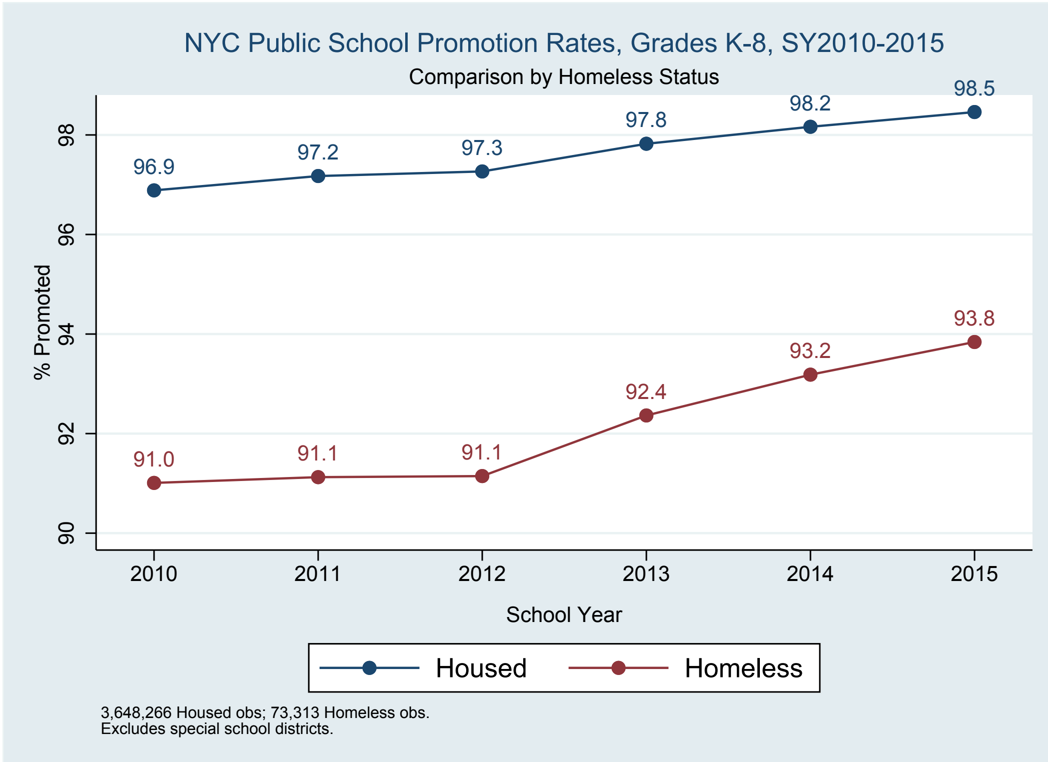


Figure A.9: NYC Public School Promotion Rates



A.7.2 Instrument Assessment

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

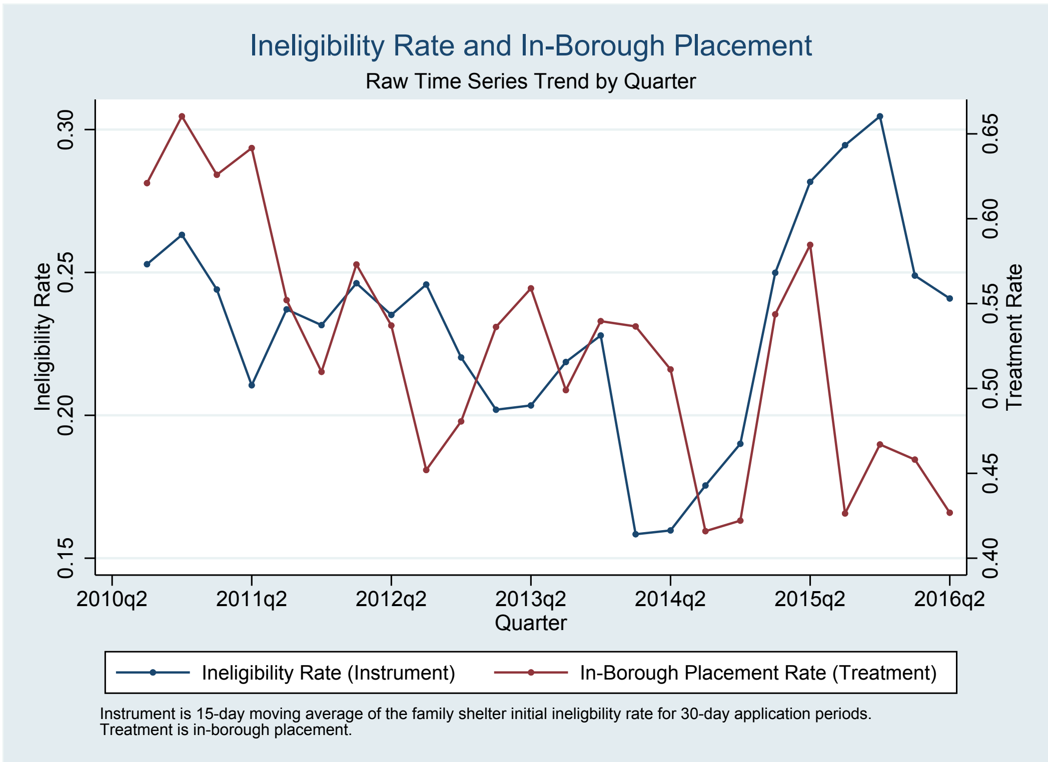


Figure A.11: Instrument and Treatment: Raw

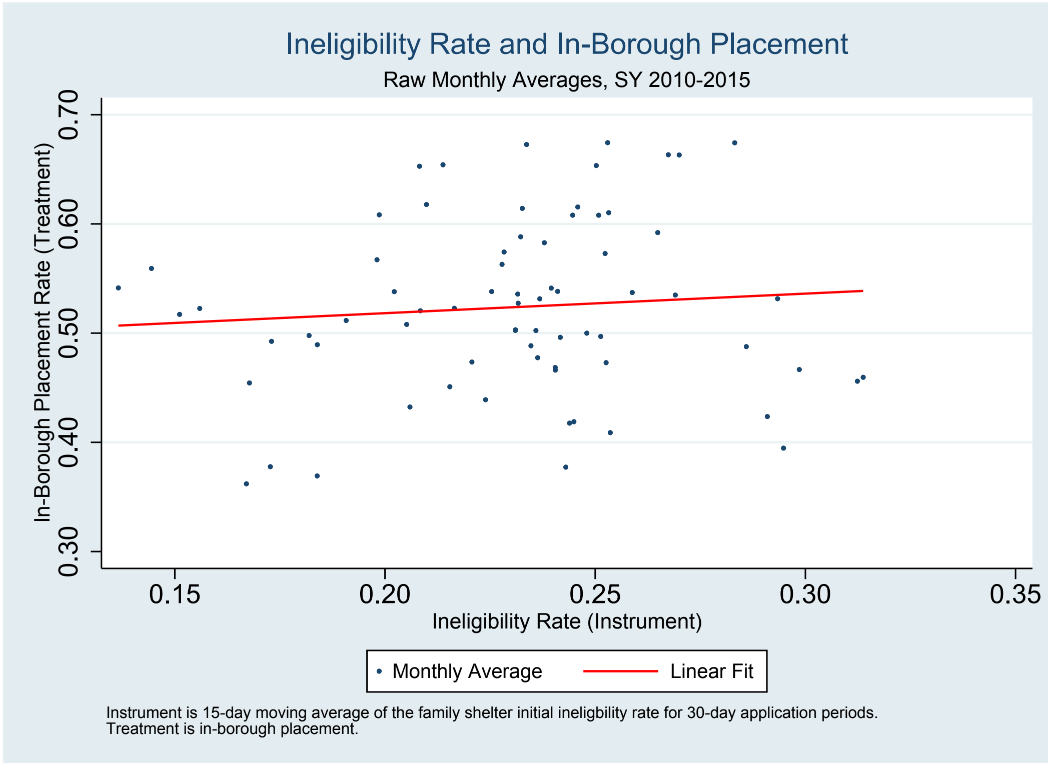


Figure A.12: Instrument and Treatment: Detrended

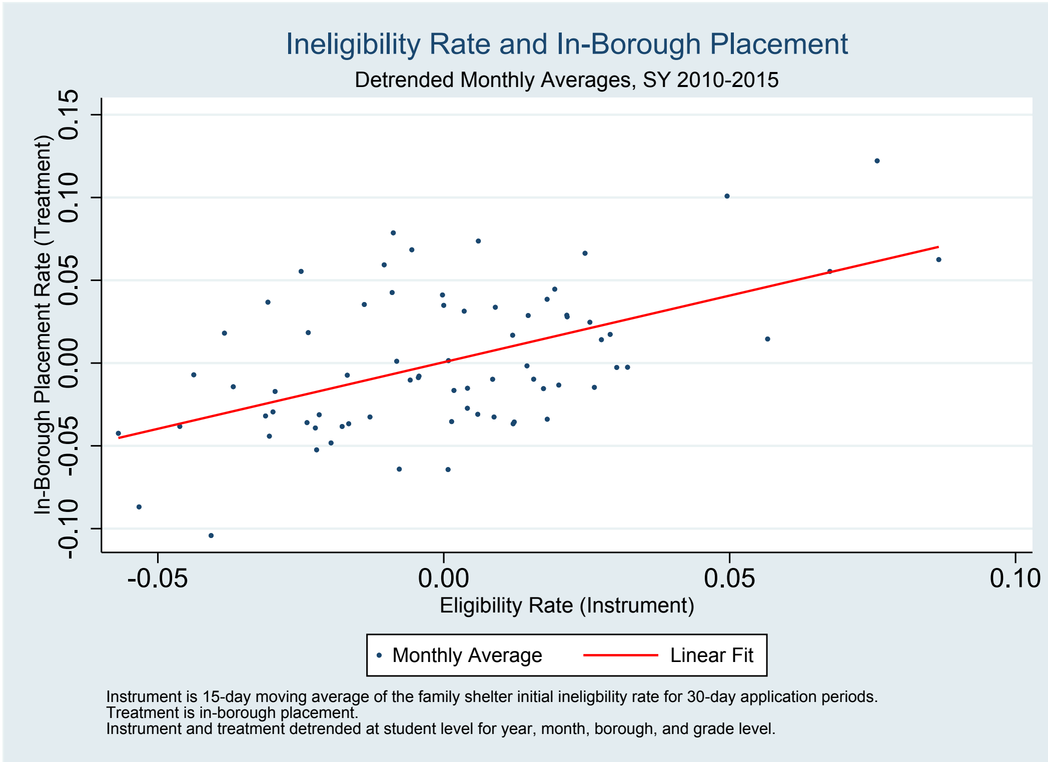


Figure A.13: Family Shelter Application Outcomes

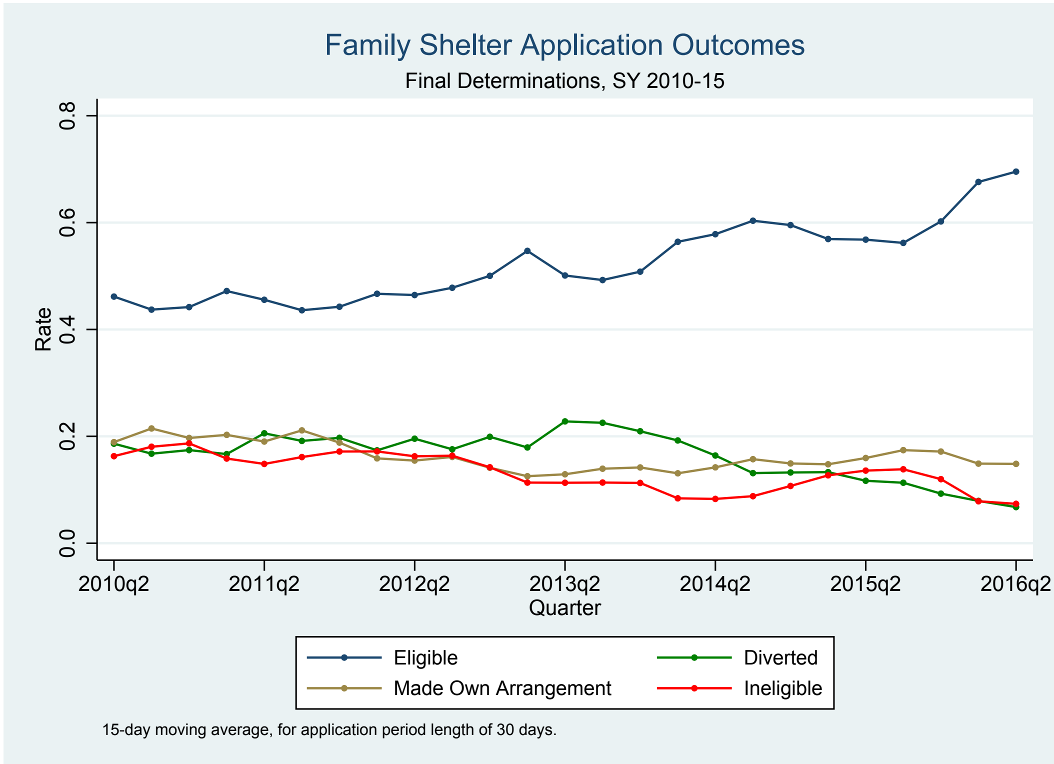


Figure A.14: Initial Ineligibility and Final Eligibility

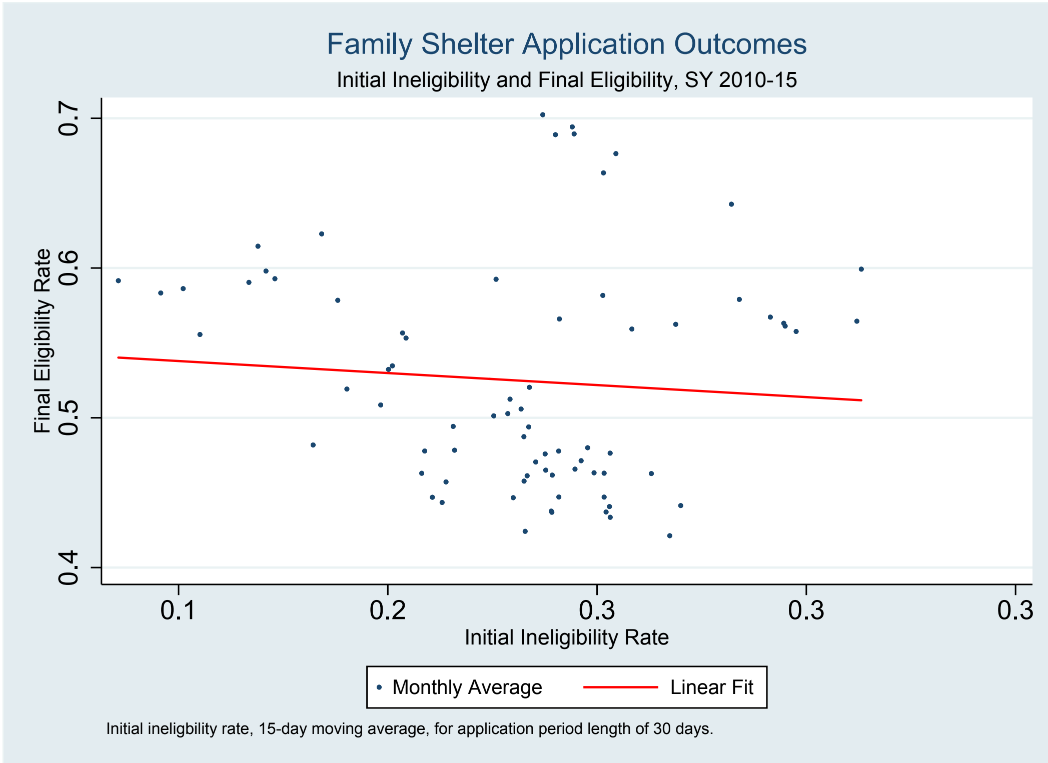


Figure A.15: Final Ineligibility and Final Eligibility

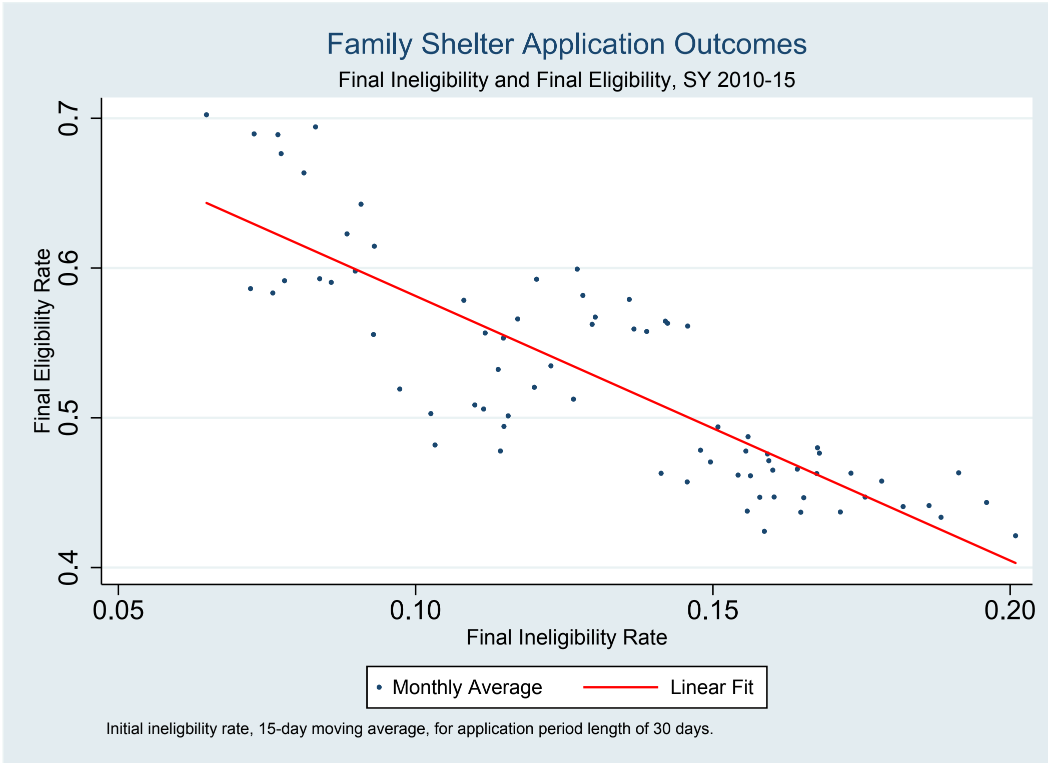
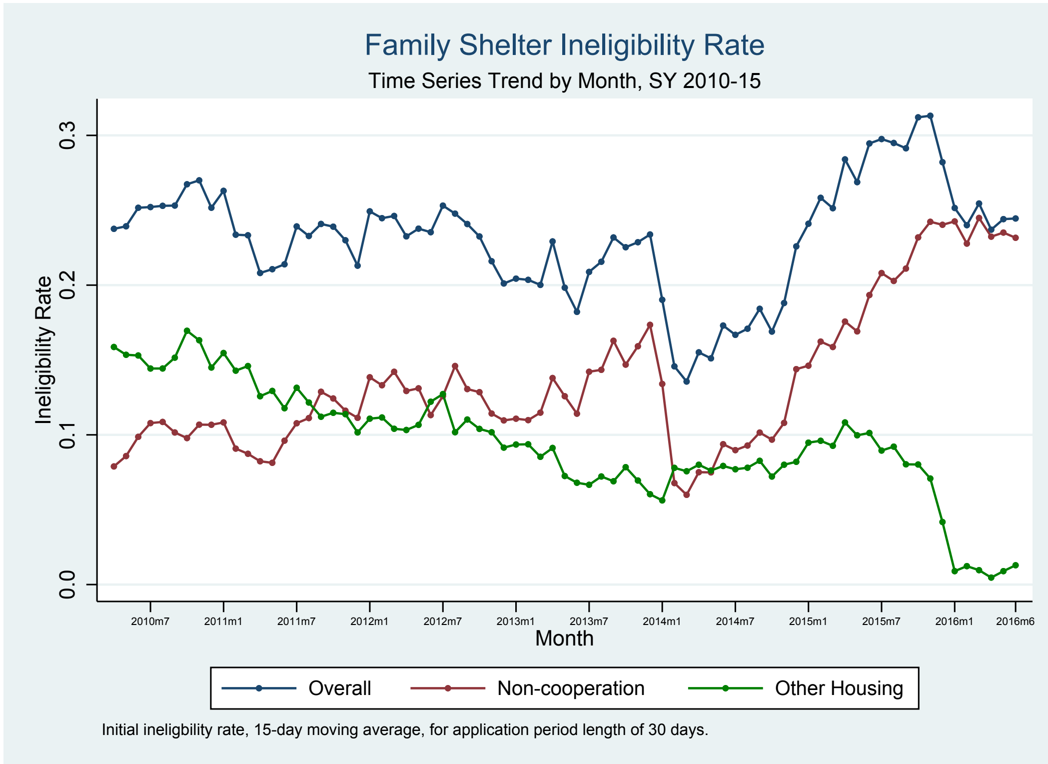
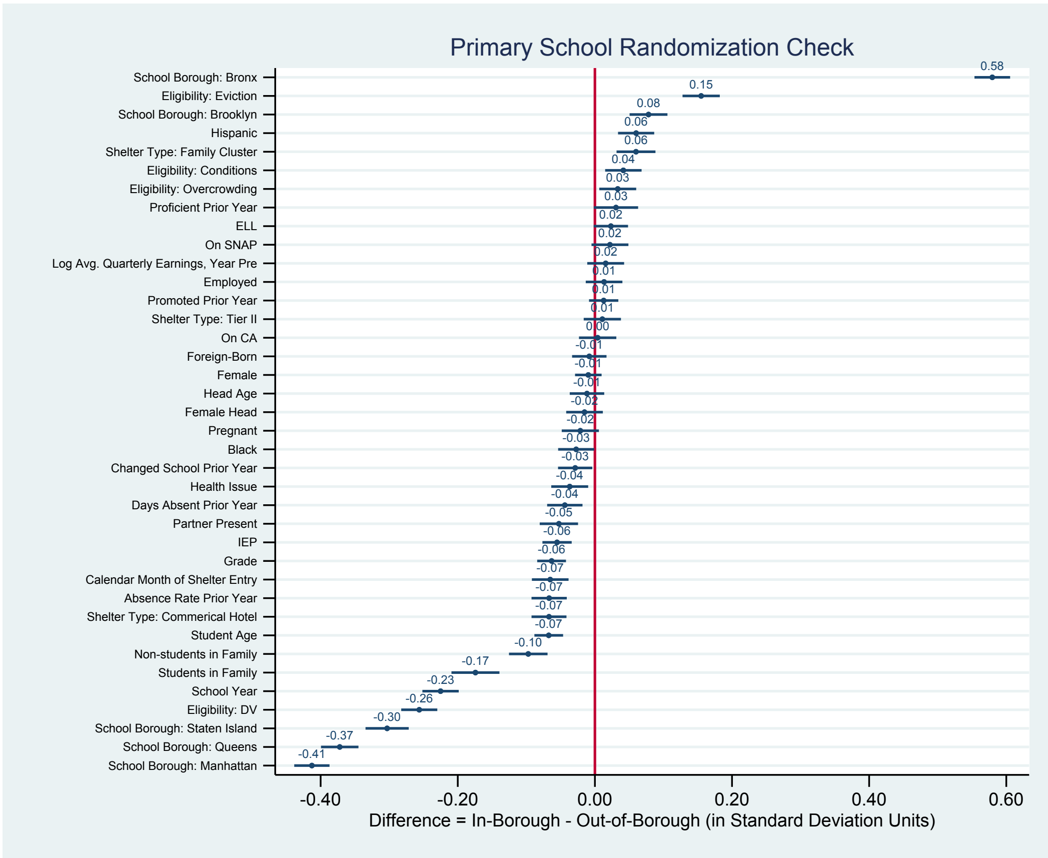


Figure A.16: Ineligibility Rate Details



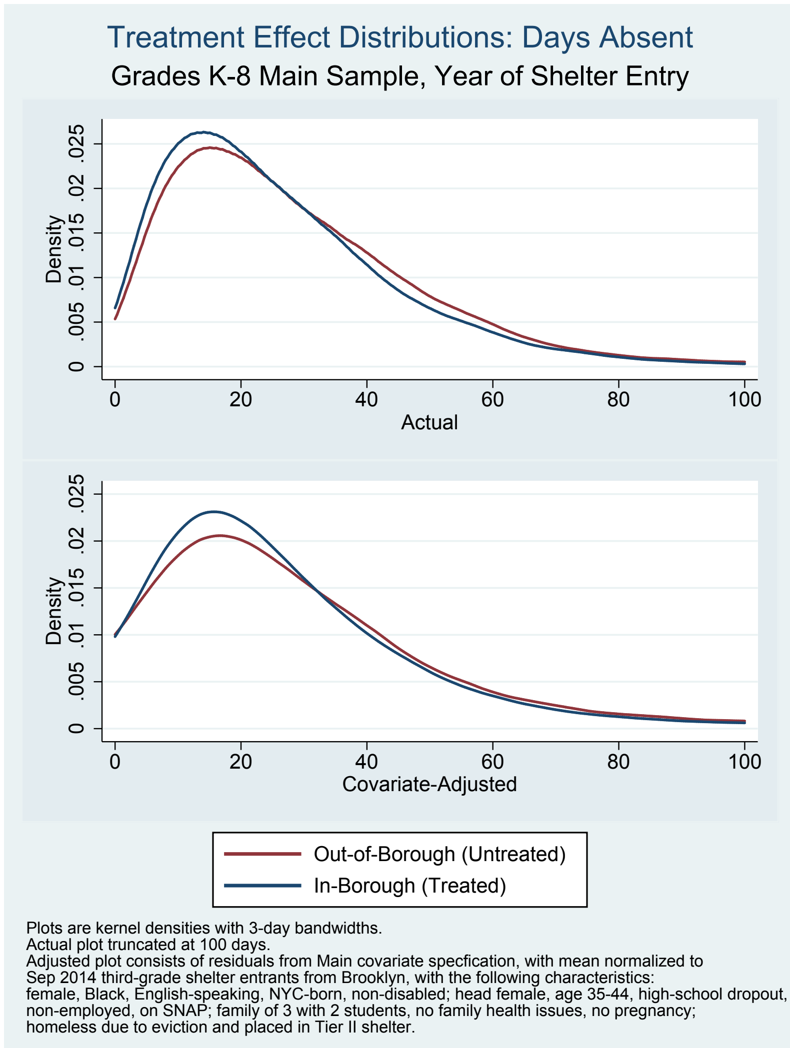
A.7.3 Results Supplement

Figure A.17: Randomization Check



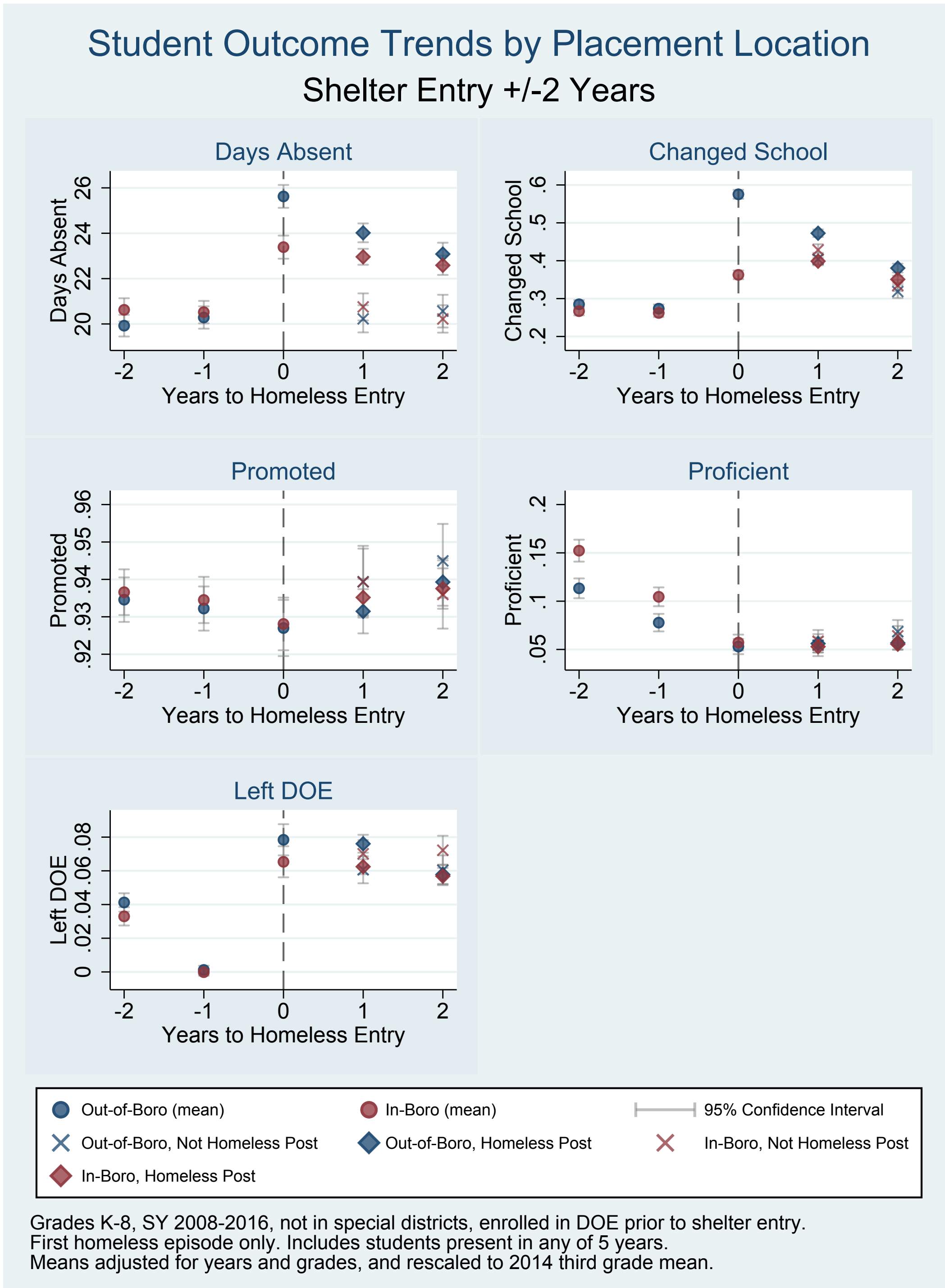
Notes: Graphical depiction of primary school results from Table 1.3A. Plot gives coefficient on in-borough treatment indicator, scaled in standard deviation units, from separate bivariate OLS regressions of each characteristic on the treatment indicator. Bars give 95 percent confidence intervals; standard errors clustered at the family group level.

Figure A.18: Days Absent Treatment Effect Distribution



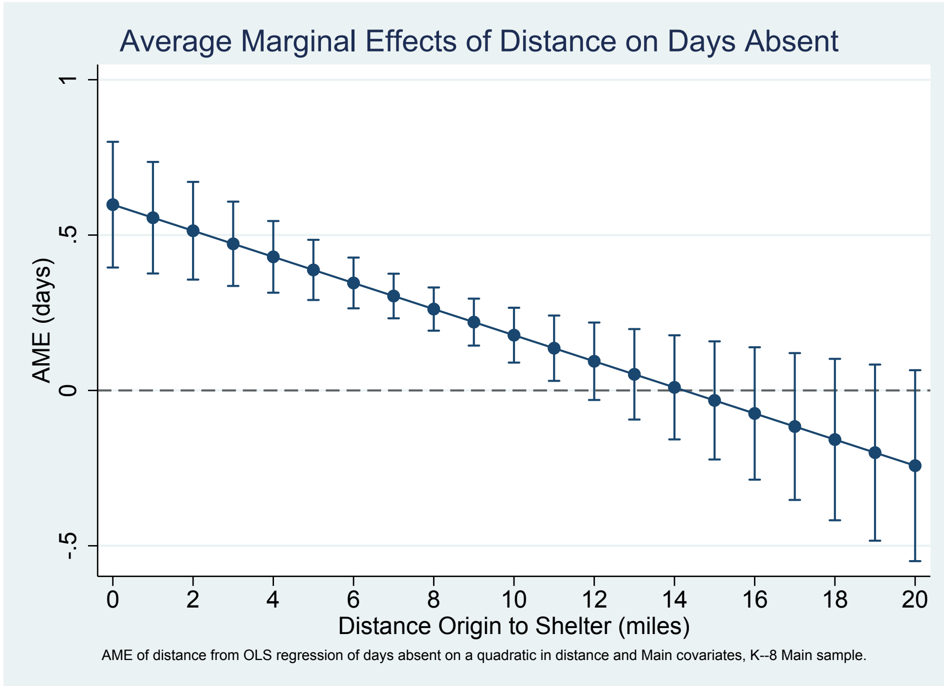
Notes: Plots are kernel densities with 3-day bandwidths. Main sample, grades K–8. Actual plot truncated at 100 days. Adjusted plot consists of residuals from Main covariate specification, with mean normalized to Sep 2014 third-grade shelter entrants from Brooklyn, with the following characteristics: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



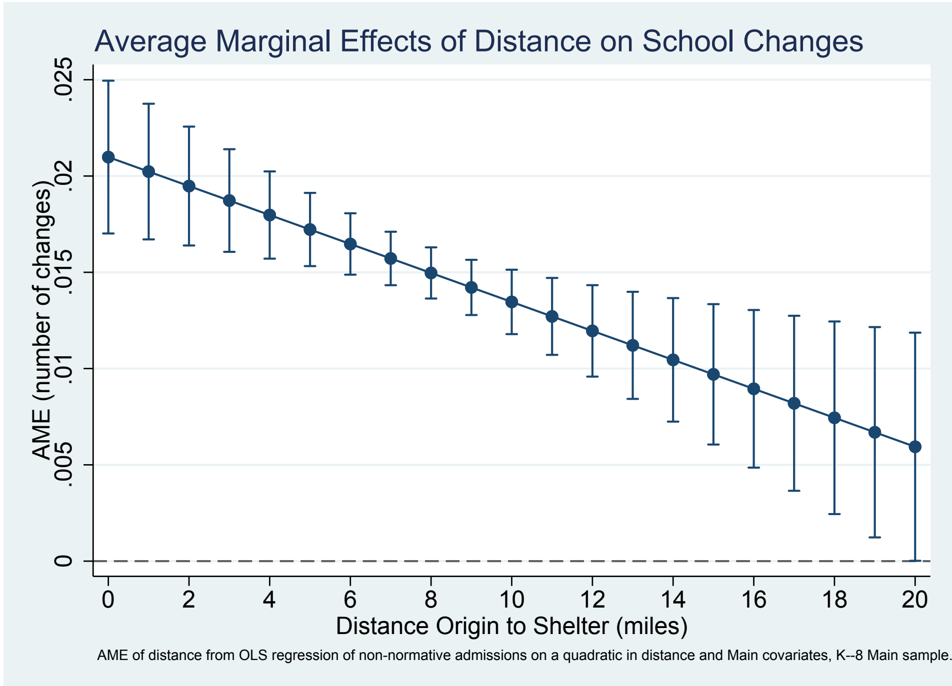
Notes: Grades K-8, SY 2008-2016, not in special districts, enrolled in DOE prior to shelter entry. First homeless episode only. Includes students present in any of 5 years. Means adjusted for years and grades, and rescaled to 2014 third grade mean.

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.

Chapter 2

Short Moves and Long Stays: Homeless Family Responses to Exogenous Shelter Assignments in New York City

2.1 Introduction

Housing is the most essential good people consume, besides, perhaps, food. Despite this, homeless families remain curiously ignored by economists. Housing instability is associated with worse physical and mental health, greater food insecurity, less labor market success, and more poverty (O'Flaherty, 2019; Ellen and O'Flaherty, 2010). Homeless children struggle in school (Buckner, 2008; Miller, 2011; Samuels, Shinn and Buckner, 2010). While causality primarily derives from deeper determinants Chapter 1, these compound challenges nevertheless mark homeless families as a population especially deserving of attention.

Nationwide, more than a third of America's homeless—some 180,413 individuals—

are people in families (U.S. Department of Housing and Urban Development, 2018). Unlike the single adult street homeless who loom large in the public consciousness, homeless families—typically, young, African-American and Hispanic single moms with several kids and high school educations—reside out of view in government-provided homeless shelters often indistinguishable from the sorts of marginal housing stock from whence they came. Most of these families are neither addicted nor ill, but rather poor and unlucky[1].

Nowhere are the manifestations more obvious than in New York City, where the confluence of a legal right to shelter, high housing costs, and progressive governance (NYC Mayor's Office, 2017) led the shelter census to rise from 8,081 families in March 2009 to 13,164 in November 2018 (NYC Department of Homeless Services, 2019*b*)[2]. Sheltering these families costs taxpayers more than $1.2 billion annually (NYC Office of Management and Budget, 2019).

Reducing homelessness, a municipal priority for decades, has taken on increased urgency. The City maintains myriad programs intended to minimize shelter stays. Prevention services forestall entries. Rental subsidies speed exits. Traditional public assistance and work supports fill gaps. But accepting that some homelessness is unavoidable, a central element of the City's strategy is to make homeless spells less disruptive for families through neighborhood-based shelter placements. Since at least the late-1990's, the City has maintained a policy of placing families in shelters in the boroughs of their youngest children's schools. While the policy is predicated on minimizing educational hardship, community continuity—keeping families connected to friends, relatives, jobs, and places of worship—has increasingly been seen as a way of improving overall well-being and expediting returns to permanent housing (NYC Mayor's Office, 2017).

[1]O'Flaherty (2019); Evans, Philips and Ruffini (2019); O'Flaherty (2010); Culhane et al. (2007); Shinn et al. (1998); Curtis et al. (2013).

[2]This includes only families sheltered by the Department of Homeless Services (DHS). Since 2018, the family census has stabilized, standing at 12,195 as of September 2019.

In this study, I evaluate how families assigned to shelters in their neighborhoods of origin fare compared to those situated in less proximate shelters. I find that local placements result in considerably longer shelter stays. Proximity also promotes access to public benefits, as well as gains in employment and earnings. In other words, families do better when placed locally, but they remain homeless longer.

It is not immediately obvious that this would be the pattern of results. One could envision an alternative scenario where proximity-propagated labor market success is associated with shorter stays. Instead, the evidence suggests the comforts of being placed near one's networks (which encourage longer stays) outweigh any resource-augmentation they produce (which encourage shorter ones). Shelter satisfaction is more receptive to the effects of proximity than is labor, or at least more promptly so.

I explain my findings with a "search effort model of family homelessness," in which sheltered families choose how to allocate effort between housing search and other activities they value. Local placement is preferable, so families assigned there remain in shelter longer, diverting time that they would otherwise spend on housing search to other activities, like work and school. Locally-placed families may also require additional incentives—rental subsidies—to leave. Optimal search effort is increasing in family resources; the greater supports, or fewer constraints, a family is endowed, the less it gives up by searching.

My empirical results proceed from analysis of a novel administrative panel of all eligible families with children who entered the NYC Department of Homeless Services (DHS) family shelter system from 2010 to 2016. I construct it by linking Department records detailing family characteristics and shelter experiences with data on public benefit use and labor market experiences maintained by other agencies.

At the core of my research design is a natural experiment. Policy objectives notwithstanding, severe capacity limitations—the vacancy rate for traditional shelters was below 1 percent in 2016 (NYC Mayor's Office, 2017)—have meant that local placement is challenging to achieve. In 2010, 66 percent of families were placed in shelters in their boroughs of prior residence; by 2016, the local placement rate had dropped to 38 percent[3]. According to program administrators, conditional upon factors implicated as placement criteria—family size, health constraints, safety, and having a school-aged child—which families are placed locally is largely a matter of chance: what suitable units are available at the time of application[4].

I demonstrate that this random assignment characterization is empirically apt. Assuming the same is true of unobservables, I can give causal interpretation to differences in average outcomes between locally- and distantly-placed families, after adjusting for placement factors. Nevertheless, I supplement OLS analysis with three complementary quasi-experimental identification strategies: instrumental variables, regression discontinuity, and family fixed effects. These can be viewed as guarding against endogeneity or as local average treatment effects reflecting heterogeneous responses.

The first strategy is an instrumental variable (IV) approach exploiting exogenous policy shocks. Although NYC has a legal right to shelter, families must prove their needs through a rigorous application process. City officials retain considerable discretion in making these determinations. The more lenient is eligibility policy, the faster the rate of shelter entry and the more competitive are local placements. Hence, my first instrument is the ineligibility rate: the higher is this rate, the better are the chances of in-borough placement for accepted families. While the applicant mix can influence the ineligibility rate, the most notable swings occur with changes of administration or other well-publicized policy initiatives. My second instrument, which I refer to as the "aversion ratio," extends the first by giving the rate of shelter stays

[3]Calculations based on my sample and treatment definition. Officially, the City reports having placed 84 percent of families in the boroughs of their youngest children's schools in fiscal year 2010, declining to a range of 49–53 percent between FY15 and FY19 (NYC Mayor's Office of Operations, 2012; New York City Mayor's Office of Operations, 2019).

[4]During their stays, families may be offered transfers to more proximate shelters. Because these moves are at families' discretion, my treatment definition is based on initial assignment.

averted—through ineligible applications and subsidized exits—per new entrant. During my study period, the City initiated and ended several rental assistance programs; as with eligibility, subsidy availability depends upon political priorities and budgetary constraints. I use these instruments separately, each characterizing an experiment influencing the treatment statuses of treatment-marginal families whose local placement responses may be different than average.

My second identification strategy takes advantage of exogeneity embedded in the neighborhood placement policy itself, isolating responses along a different margin. It is a regression discontinuity (RD) design based upon oldest children's ages. Neighborhood-based shelter placement is, first and foremost, an education policy, and so families with school-age children receive priority for in-borough placement. Because the timing of shelter entry is partly beyond families' control, those who enter shelter prior to their oldest children starting school (and are ineligible for the local placement boost) are counterfactuals for those who enter shelter after (and are eligible).

My third identification strategy is a family fixed effects approach. Repeat spells of homelessness are common. So long as outcome-relevant unobservables are spell-invariant, families who enter shelter multiple times with varying treatment assignments are counterfactuals for themselves.

Neighborhood placements have powerful impacts. Per OLS, families placed in-borough remain in shelter 12.7 percent longer, equivalent to about 50 days. Locally-placed families also access more public benefits and are better connected to the labor market. During the year following shelter entry, they are 1.4 percent (1.1 percentage points) more likely to receive Cash Assistance, 2.1 percent (1.0 pp) more likely to be employed, and have 9.9 percent higher earnings[5]. Elevated benefit use continues post-

[5]These outcomes may well be related. Longer stays allow more time for benefit and employment effects to percolate; at the same time, better connections to jobs and supports may encourage longer stays. In addition, Cash Assistance comes with work requirements and work supports.

shelter. In-borough families are 4.6 percent (1.8 pp) more likely to exit shelter with a rental subsidy, and Cash Assistance receipt continues to be 2.3 percent (1.7 pp) higher during the ensuing year. However, labor market effects attenuate. Given capacity-based random assignment is the most broadly applicable experiment—all families are affected—these are my preferred estimates of *average* treatment effects (ATE's). Family fixed effects results—which are also informed by the natural experiment of shelter scarcity—reinforce these findings, with modestly larger effect estimates across outcomes.

On the other hand, my IV and RD results indicate that OLS may understate the potential of neighborhood-based placements. In the context of quasi-random assignment, I interpret IV and RD as dually-layered natural experiments identifying local average treatment effects (LATE's) among difficult-to-treat subgroups: "compliers" who are placed locally only when conditions are especially fortuitous[6]. I show that compliers exhibit characteristics one would expect of families facing augmented barriers to proximate placements: on average, complier families are large, young, disconnected from services, and from neighborhoods where homelessness is common. They are also more responsive to treatment. Ineligibility rate, aversion ratio, and school-starting compliers stay in shelter an order of magnitude longer than average homeless families when placed locally. They are as much as doubly likely to receive Cash Assistance compared to when they are placed out-of-borough. The evidence on labor market outcomes is more mixed. Policy compliers see large boosts to employment and earnings, while school-starters see diminished job prospects, especially post-shelter. The gap between ATE's and LATE's demonstrates the difference between average and marginal policy impacts. In other words, by carefully choosing

[6]For further details on the LATE concept, as introduced by Imbens and Angrist (1994) and Angrist, Imbens and Rubin (1996), see Angrist and Pischke (2008). Heckman and Vytlacil (1999, 2005, 2007), Vytlacil (2002), Heckman, Urzua and Vytlacil (2006), and Heckman (2010) show LATE's can be constructed from a choice-theoretic primitive—the marginal treatment effect (MTE)—which has the additional appeal of unifying the treatment effects literature.

policymaker-controlled instruments that affect treatment participation margins, I am able to identify policy relevant treatment effects in the spirit of Heckman and Vytlacil (1999, 2001, 2005, 2007).

Alternatively, under the assumption of constant treatment effects, another interpretation of IV estimates larger in absolute value than OLS is as evidence of endogeneity: OLS coefficients biased toward zero by unobservables correlated with treatment. In this telling of it, in-borough families are disproportionately those who would have left sooner on their own; they are also less likely to have their public benefit use or employment patterns impacted by local placement. One story consistent with these results is that unobservably well-resourced (or minimally-constrained) families are systematically more likely to secure favorable placements.

These findings complement those in Chapter 1, where, studying the same neighborhood placement policy, I find that local shelter assignment significantly improves homeless students' attendance, stability, and test scores. This pair of papers are the first (to my knowledge) to situate homeless families in an expressly microeconomic framework and assess, empirically, how they respond to the incentives of the shelter services system—as well as how their shelter usage patterns relate to labor supply, education, and participation in other government benefit programs.

Besides Chapter 1, the works most similar to my own are Curtis et al. (2013), who study health as an exogenous shock to family homelessness, Collinson and Reed (2018), who use a randomized judge design to study the effect of evictions on homelessness in NYC, Cobb-Clark et al. (2016), who use econometric methods to study homeless duration, and Cobb-Clark and Zhu (2017), who find that early-life homelessness is associated with worse education and employment outcomes in adulthood[7].

[7]In contrast, most previous economic studies of homelessness have focused on one of five themes: macro issues (Cragg and O'Flaherty, 1999; O'Flaherty and Wu, 2006; Gould and Williams, 2010; Corinth, 2017); single adults (Allgood and Warren, 2003; Allgood, Moore and Warren, 1997); theory (Glomm and John, 2002; O'Flaherty, 1995; O'Flaherty, 2004, 2009); description (Shinn et al., 1998; Culhane et al., 2007; Ellen and O'Flaherty, 2010); prevention and prediction (Goodman, Messeri and O'Flaherty, 2014; Goodman, Messeri and O'Flaherty, 2016; Evans, Sullivan and Wallskog, 2016;

My work also contributes to two other literatures. The first is neighborhood effects[8]. The best studies have used natural experiments—typically the allocation of housing subsidies through lotteries—and have tended to find negligible effects on most economic outcomes[9]. However, some recent evidence suggests residential mobility improves families' contemporaneous physical and mental health and subjective well-being, as well as longer-term educational and labor market outcomes for children[10]. My work is the first to examine the effects of neighborhood specifically in the context of homeless families, a group less well-off than the low- and moderate-income families typically featured.

Second, an understanding of homeless family behavior can inform the design of poverty alleviation programs generally. Optimal programs must strike a balance between helping the truly needy and minimizing moral hazard[11]. My findings inform this trade-off. Abstractly, capacity-constrained shelter placements are exogenous variation in a public benefit program. Families that luck their way into more generous benefits have less incentive to give up those benefits and, simultaneously, wider latitude to pursue utility-augmenting possibilities.

But are neighborhood-based shelter placements a good idea? My findings indicate the answer is not unambiguous. When placed locally, homeless families will remain homeless longer (generally regarded as welfare-reducing) but they will be better connected to government services, jobs, and their children's schools (generally regarded as welfare-enhancing). In other words, the two current pillars of New York City's family homeless policy—stays that are short and comfortable—are not complemen-

O'Flaherty, Scutella and Tseng, 2018*a*; O'Flaherty, Scutella and Tseng, 2018*b*); or housing stability interventions (Wood, Turnham and Mills, 2008; Gubits et al., 2016). O'Flaherty (2019) and Evans, Philips and Ruffini (2019) provide two recent and comprehensive summaries of this literature from the perspective of economists.

[8]Topa, Zenou et al. (2015) summarize this literature.

[9]Oreopoulos (2003); Jacob (2004); Kling, Liebman and Katz (2007); Ludwig et al. (2008); Sanbonmatsu et al. (2011); Jacob and Ludwig (2012); Jacob, Kapustin and Ludwig (2015); Galiani, Murphy and Pantano (2015).

[10]Ludwig et al. (2012, 2013); Chetty, Hendren and Katz (2016); Andersson et al. (2016).

[11]Nichols and Zeckhauser (1982); Besley and Coate (1992, 1995).

tary. Nor are these stays cheap. At the City's average shelter cost of about $200 per family per day (NYC Mayor's Office of Operations, 2018), neighborhood-based placements cost the City an additional $10,000 per family. It is an open question whether 10 percent gains in school attendance and earnings are the best uses of the City's next $10,000.

Recognizing these trade-offs is important. But complicated questions of budgetary optimization are not the first step; the more immediate point is that there remains ample room to enhance the efficiency of neighborhood placements. Outcomes among homeless families are highly variable. My IV and RD compliers—marginally-treated families—are highly policy elastic. This suggests potential gains from better targeting local placements to families most likely to benefit. Policy-relevant heterogeneity should be better screened at intake and explicitly factored into placement decisions using predictive models. Special attention should be afforded families who are difficult to place: my results suggest it is these families whose outcomes will be most sensitive to their assignments. Integrated support services should be correspondingly customized to families' comparative advantages and limitations, while respecting the influence placement proximity (and other characteristics) will have on families' incentives.

These insights derive from a natural experiment in shelter assignment. That experiment should be replaced with evidenced-based placements designed to allocate scarce resources in a welfare-maximizing manner.

2.2 Policy Background

Neither homelessness nor poverty among families are foreign to municipalities anywhere in the United States, but in few places is the intersection starker than in New York City. Since 1994, New York's homeless census has nearly tripled, from 24,000 to 60,000 in 2019. Two-thirds are people in families (NYC Department of Homeless Services, 2019*b*). Overall, NYC accounts for about a quarter of sheltered homeless families in the U.S. (NYC Department of Homeless Services, 2019*d*; U.S. Department of Housing and Urban Development, 2018; Coalition for the Homeless, 2019)[12].

Family homelessness is particularly pronounced in New York City for two reasons. First, unique among U.S. cities, NYC has a legal right to shelter, the consequence of a series of consent decrees originating in the 1980s[13]. The City is legally obligated to provide emergency accommodations to any family able to demonstrate it has no suitable alternative. The second factor is NYC's relentless real estate market. In the decade ending in 2015, median rent in NYC grew three times the pace of median incomes (18.3 percent versus 6.6 percent). Vacancy rates are consistently below 4 percent (NYU Furman Center, 2016). According the City, demand for affordable apartments exceeds supply by a factor of two; approximately half of renters in the City are rent-burdened, defined as allocating more than 30 percent of household income to rent (NYC Mayor's Office, 2017).

Responsibility for managing shelters and supports for homeless families and individuals falls primarily to DHS, an agency under the purview of the City's much larger Department of Social Services (DSS)[14]. Families apply for shelter at a central intake center in the Bronx. The eligibility determination process requires families to prove they have no suitable housing alternative. State guidelines and court orders govern these determinations, but City policymakers retain considerable discretion. Families deemed eligible are given shelter assignments by dedicated placement staff,

[12]Los Angeles, which has a fifth the number of homeless families as NYC, has the second largest homeless family population among U.S. cities; 21 percent are unsheltered (U.S. Department of Housing and Urban Development, 2018).

[13]The state of Massachusetts also has such a right. See NYC Independent Budget Office (2014) and University of Michigan Law School (2017) for more detail.

[14]DHS was originally a part of DSS, but was spun off as an independent agency in 1993. In 2016, the two agencies were again consolidated under a single commissioner. Nevertheless, it remains conventional to refer to the departments as distinct. See NYC Department of Homeless Services (2019*a*) for more detail. DSS is also known as the Human Resources Administration (HRA). Accordingly, I use "DSS" and "HRA" interchangeably when referring to this agency.

who take into account such criteria as family size, health issues, safety, and proximity to children's schools[15].

The shelter system these families enter is vast and complex, consisting of traditional Tier II shelters[16], as well as "cluster" apartments scattered in otherwise private buildings and commercial hotels enlisted to expand capacity on-demand. In recent years, vacancy rates have hovered around one percent NYC Mayor's Office (2017). Expanding capacity is complicated by the virulent community opposition that typically greets proposals for new shelters[17].

Shelter is also expensive. During fiscal year 2018, the average cost of sheltering one family for one night (inclusive of rent and services) was $192. Overall, DHS spent $1.2 billion on family homeless shelter—and this excludes administrative costs, prevention programs, and rental subsidies, as well as welfare benefits administered by other agencies (NYC Office of Management and Budget, 2019; NYC Mayor's Office of Operations, 2018). While DHS does manage some shelters directly, most homeless services provision is carried out through contracts with community-based non-profit organizations who operate shelters[18].

Throughout this period, a pillar of the City's homelessness strategy has been community continuity. To the extent capacity and other constraints allow, the City endeavors to place families in their neighborhoods of origin. Predicated on the goal of keeping children in their home schools, the policy reflects a more general premise—that families are better positioned to expeditiously return to permanent housing when they remain connected to their support networks, including relatives, friends, and places of work and worship (NYC Mayor's Office, 2017). Since at least 1997, the

[15]For more, see NYC Department of Homeless Services (2019*c*); NYC Independent Budget Office (2014). Additionally based on author's conversations with City officials.

[16]These are apartment buildings exclusively designated to serve homeless families.

[17]See, e.g, Stewart (2017).

[18]82 percent of DHS' budget consists of such contracts. This service arrangement is not unique to homeless services; most social service programs in the City are administered this way (NYC Mayor's Office of Operations, 2017).

share of families placed in shelters according to their youngest child's school has been a DHS performance indicator. The official placement objective is the shelter nearest the youngest child's school, but in practice DHS counts any placement within the school borough as successful (NYC Mayor's Office of Operations, 2018). According to DHS officials, which families are given preferential local placement is essentially a function of what units are available at the time a family applies.

In recent years—after my study period—the emphasis on local placement has become even stronger, with the introduction of the School Proximity Project, through which DHS and DOE share data to identify homeless students and offer their families transfers to shelters closer to their schools.

2.3 Theory

Homeless families' most pressing objective is to find permanent housing. Hence it is natural to adapt search theory to their context[19]. A *search effort model of family homelessness* parsimoniously characterizes my main results and offers generalizable insights.

Agents are homeless families, indexed by i and inhabiting a static, one period environment[20]. They start the period in shelter. Families value two goods, housing (H) and "consumption" (C), an aggregate good comprising everything besides housing, including leisure and work, that families value. Shelter (S) is a particular type of housing—namely, the least valuable kind: $S = \underline{H}$.

Families are endowed with a single resource: their own effort (e). Effort is normal-

[19] Search theory, which typically considers job search, was pioneered by McCall (1970). Important contributions relevant for present purposes include Mortensen and Pissarides (1999); Pissarides (2000); Eckstein and Van den Berg (2007); Cahuc, Carcillo and Zylberberg (2014). Given that homeless families are in the receipt of government benefits (shelter) as they search for a good (housing), particularly useful are the insights of the unemployment duration and optimal unemployment insurance literatures (Chetty, 2008; Chetty and Finkelstein, 2013; Katz and Meyer, 1990; Lalive, Van Ours and Zweimüller, 2006; Spinnewijn, 2013).

[20] In what follows, I often omit the subscript i for simplicity.

ized to a 0–1 scale, where 0 represents no effort expenditure and 1 represents maximal effort. A family's decision problem is to choose how to allocate effort[21] between housing search (e_S) and consumption ($e_C = 1 - e_S$). In choosing e_S, a family is choosing the probability it finds permanent housing.

Families' preferences are described by a continuously twice differentiable utility function $u(H, C)$, strictly increasing ($u_H, u_C > 0$) and strictly concave ($u_{HH}, u_{CC} < 0$) in both arguments (with subscripts denoting partial derivatives). In words, families value housing and consumption, there is diminishing marginal utility, and families are risk adverse. Also assume complementarity (or supermodularity), $u_{HC} > 0$. The pleasure of consumption increases with better housing, and housing is more satisfying when consumption is greater. Since shelter is the worst form of housing, it follows that $u_C(H, c) > u_C(S, c)$.

Neighborhoods affect families' valuation of homeless shelter as a housing good. The utility of families in shelter is $u(S(N), C)$, with N an indicator for local placement. I assume families prefer to be placed in their pre-shelter neighborhoods, so $u(S(N = 1), C) > u(S(N = 0), C)$.

Putting it all together, homeless families choose their housing search effort to maximize expected within-period utility:

$$\begin{aligned}\max_{0 \leq e_S \leq 1} (1 - e_S) u(S(N), C) + e_S u(H, C) \\ \text{subject to} \\ C \leq w(1 - e_S)\end{aligned}$$

where w denotes the "wage" or, more generally, the return to effort not expended on housing search, inclusive of opportunity costs.

Assuming that the consumption constraint binds with equality at an interior solu-

[21] "Effort" does not imply that the object upon which it is expended is not enjoyable; excess can be thought of as being allocated to leisure. No effort is ever wasted.

tion, optimal housing search effort, e_S^* is implicitly defined by the first-order condition:

$$u(H,C) - u(S(N),C) - (1-e_S^*)wu_C(S(N),C) - e_S^*wu_C(H,C) = 0$$

Rearranging, I get the following expression, which makes makes the optimality condition intuitive to interpret.

$$\underbrace{u(H,C) - u(S(N),C)}_{\text{expected gain from search}} = \underbrace{w[(1-e_S^*)u_C(S(N),C) + e_S^*u_C(H,C)]}_{\text{expected loss from search}}$$

Families choose housing search effort so as to equate the (expected) benefit of search ($u(H,\cdot) - u(S(N),\cdot)$) with the expected utility cost of search, which is the product of the marginal opportunity cost of search (w) and the expected marginal utility of consumption, which depends on if the search is successful ($(1-e_S^*)u_C(S(N),C) + e_S^*u_C(H,C)$).

Of primary interest is how this optimal effort changes based upon shelter neighborhood. Using the implicit function theorem, the comparative statics of neighborhood placement are straightforward to derive (with F denoting the implicit function defined by the FOC):

$$\frac{\partial e_S^*}{\partial N} = -\frac{\frac{\partial F}{\partial N}}{\frac{\partial F}{\partial e_S^*}} = \frac{\frac{\partial u(S)}{\partial S}\frac{\partial S}{\partial N} + w(1-e_S^*)\frac{\partial u_C(S)}{\partial S}\frac{\partial S}{\partial N}}{-w(\frac{\partial u(H)}{\partial C} - \frac{\partial u(S)}{\partial C})} = \frac{+}{-} < 0$$

where the consumption arguments in the utility function are suppressed for clarity and $\frac{\partial C}{\partial e_S^*} = -w$. Since the numerator is positive (being placed locally increases the marginal utility of being housed in shelter, and the marginal utility of consumption increases with being placed locally) and the denominator is negative (by exerting effort to search for housing, families give up consumption, which is valued more when in permanent housing), optimal search effort decreases when families are placed in their neighborhoods of origin[22].

Intuitively, families prefer permanent housing to shelter, but being placed in a local shelter narrows the gap. Thus, when placed locally, families have less incentive to search. Because e_S^* measures the probability of finding permanent housing,

$$E(Y) = \frac{1}{e_S^*}$$

gives the expected duration (length of stay) of the shelter spell. The model predicts families placed locally will remain in shelter longer because they allocate less effort to search.

On the other hand, since $e_C = 1 - e_S$, the effect of local shelter placement on "consumption" outcomes—of which labor market earnings and benefit receipt are of greatest interest—is positive.

$$\frac{\partial e_C^*}{\partial N} = -\frac{\partial e_S^*}{\partial N} > 0$$

That is, when families devote less effort to housing search, more effort is available to pursue earnings opportunities or apply for government benefits, like Cash Assistance.

I can also rearrange the FOC to get an expression for optimal search effort e_S^* in terms of the primitives of the model:

$$e_S^* = \frac{u(H) - u(S) - wu_C(S)}{w(u_C(H) - u_C(S))}$$

It is easy to show, given my assumptions, that this expression is strictly positive. Further, the following is a necessary and sufficient condition for an interior solution

[22] Note that, in this setup, the level of intra-period consumption is the same whether or not families are successful at finding permanent housing.

(i.e., optimal search effort less than unity):

$$wu_C(H) > u(H) - u(S)$$

In words, families will not spend all their effort on housing search when the utility of consumption they must give up to do so exceeds the utility of housing they gain[23].

A simple way to introduce heterogeneity is by allowing w, the opportunity cost of search, to depend on family characteristics $\mathbf{X}$. For simplicity, consider $\mathbf{X} = X$, a one-dimensional measure of resources (e.g., extended family support or savings); equivalently, it can be interpreted as an absence of constraints (e.g., having a small family). Assume that $\partial w/\partial X < 0$. The opportunity cost of search decreases with resources. The more supports or fewer constraints a family has, the less consumption it gives up by devoting effort to search. For any level of housing search effort, high resource families consume more.

Of primary interest is how optimal effort changes with resources. Differentiating the expression for e_S^* with respect to X,

$$\frac{\partial e_S^*}{\partial X} = \frac{(-w_X u_C(S))(w(u_C(H) - u_C(S))) - (u(H) - u(S) - wu_C(S))(w_X(u_C(H) - u_C(S))}{(w(u_C(H) - u_C(S))^2}$$
$$= \frac{+}{+} > 0$$

where, as before, subscripts represent partial derivatives. The first term in the numerator is positive, as $w_X < 0$, as is the second term, given that the FOC implies $u(H) - u(S) > wu_C(S)$. The denominator is obviously positive, which means $\partial e_S^*/\partial X > 0$. Optimal search effort increases with resources; equivalently, it decreases with constraints.

[23]The term for consumption utility in shelter does not enter into the equation, as maximal search effort implies finding housing with certainty.

2.4 Data and Sample

My data derives from administrative records linked across several City and State agencies. The main source is DHS' Client Assistance and Rehousing Enterprise System (CARES), the City's management information system of record for homeless families. My base data consists of all eligible family shelter entrants—adult(s) with one or more children under 21, or pregnant—who applied, were found eligible, and began their shelter stays in the period beginning January 1, 2010 and ending December 31, 2016. CARES provides detailed information characterizing family attributes and shelter stays. To this core DHS data, I append data on public benefit use and labor market experiences maintained by other agencies.

My unit of analysis is the *family-spell*. A homeless spell is defined as a shelter stay uninterrupted by a break of more than 30 days[24]; families returning after 30 days are considered to have begun a new spell. Many families experience multiple spells during the sample period. After removing from the raw data records with decisively missing data[25], my complete sample consists of 68,584 family-spells. This is a near-census of family homelessness. As shown in Table 2.1, my analytical sample shrinks for three reasons. First, 7,178 families originate from outside NYC. Another 286 spells lack data on borough of origin[26]. Finally, I limit my analytical sample to those families whose oldest child is under 18 years of age[27]. Henceforth I refer to these remaining 59,253 family-spells as my "Full Sample."

As robustness checks, I also consider three alternative samples: a "Non-DV" sample consisting of families eligible for shelter for reasons other than domestic violence (many DV families are deliberately placed out-of-borough for safety reasons), a "Pre-

[24] This is the definition DHS conventionally uses in its own reporting.

[25] The unit of observation in the raw CARES data is the individual. Decisive fields include family identifier, entry dates, and the presence of children.

[26] My preferred measure of address of origin are geocoded addresses. 5,395 spells fail to geocode due to data entry errors. A redundant CARES "NYC Borough" field allows me to recover borough for 5,109 of these spells.

[27] Individuals 18 and over can be a head of household.

2015" sample consisting of all spells in the 2010–2014 period (to minimize censoring issues), and a "One School-Age Child" sample (to address potential multi-child confounding in my RD design).

Most variables are defined and measured at the time of shelter entry. For group characteristics shared by family members, like shelter assignment, I assign the shared value to the family. For individual characteristics that vary among members, such as age or sex, I assign the family the value of its (initial) head. For aggregate characteristics, like family size, I violate the "at-entry" rule and assign the family its maximum for the spell, to better reflect true composition.

In the remainder of this section, I discuss key variables conceptually and define their implementations in the data. Additional detail can be found in the Appendix.

2.4.1 Outcomes

The outcomes I assess are comprehensive, spanning shelter experiences, public benefit use, and employment. The most proximate and policy salient is length of stay (LOS) in shelter—a measure, in days, of the time between a family's entry into shelter and its exit, including gaps of up to 30 days[28]. As the most *immediate* shelter outcome, length of stay is the one most likely to be impacted by neighborhood placement; in turn, it impacts—and is impacted by—other outcomes, including families' experiences in the markets for labor and government benefits. In my analysis, I take the natural log of this duration.

Shelter exits must balance speed-of-transition with stability. A second outcome—return to shelter within a year of exit (after having been out of shelter for more than 30 days)—quantifies at this objective. My third outcome is an indicator for subsidy receipt; the presence of rental assistance is perhaps the most policy-relevant way to

[28]In DHS parlance, this is known as "system" LOS, because it reflects a family's overall attachment to the homeless services system, regardless transient absences. It is not uncommon for families to leave shelter for a few days, then return. An alternative duration measure, "shelter" LOS, excludes the interludes from the count. The measures produce similar results.

characterize shelter departures. I observe families' stays, exits, and returns through May 2019.

I also consider economic outcomes beyond housing: public benefit use and labor market experiences. The former, non-housing public benefits, derive from records maintained by the City's Department of Social Services (DSS), spanning 2001–2016. DSS, the City's designated Local Social Service Agency, oversees virtually all aspects of the social safety net, including the two most important income supports for homeless families: Cash Assistance[29] and Food Stamps[30]. I measure Cash Assistance and Food Stamps use with indicators for active cases at any time during a period of interest. I focus on two periods: the year post-shelter entry and the year post-shelter exit.

To assess labor market outcomes, I use quarterly earnings records from the New York State Department of Labor (DOL) spanning the first quarter of 2004 to the first quarter of 2017. Again focusing the years post-entry and post-exit, I construct indicators for positive earnings during any quarter as my measures of employment[31]. Correspondingly, my measure of earnings is log average quarterly earnings[32].

Public benefit and labor market outcomes require cross-agency data matches.

[29]Cash Assistance consists of Temporary Assistance for Needy Families (TANF), which, in New York, is referred to as Family Assistance, and its State counterpart for single adults and TANF time-limited families, Safety Net Assistance. Sometimes described as "public assistance" or "welfare," Cash Assistance provides unrestricted monetary transfers to poor individuals and families. Eligibility is limited to the very poorest and imposes work requirements. Benefits are similarly tight, topping out at $789 a month for a three-person family. 332,407 New York City residents were actively receiving Cash Assistance as of August 2019 (Cohen and Giannarelli, 2016; New York State Office of Temporary and Disability Assistance, 2016*b*, 2015*b*, 2017; NYC Human Resources Administration, 2019).

[30]Food Stamps, officially known as the Supplemental Nutrition Assistance Program (SNAP), provides low-income families with monthly dollars that must be spent on food. SNAP eligibility standards are less strict than Cash Assistance; correspondingly, its caseloads are much larger. In 2019, a family of three receives $509 monthly. 1.5 million NYC residents received SNAP as of August 2019 (New York State Office of Temporary and Disability Assistance, 2019; NYC Human Resources Administration, 2019).

[31]DOL data lacks information on work hours.

[32]Average quarterly earnings themselves are in real 2016 dollars, are inclusive of all quarters, whether working or not, and have one dollar added, so as to avoid omitting families with zero earnings when taking logs. For partially-censored spells, the earnings denominator is the minimum of four quarters or the number of quarters before censoring.

Because individual identifiers vary by program (and are subject to administrative error), I use probabilistic matching techniques to link DHS and DSS data[33]. The DHS-DOL link is deterministic based on Social Security Number (SSN). I assume that non-linkages between DHS families and DSS/DOL records mean that families are truly not receiving benefits or not working.

2.4.2 Treatment

In my leading case, I define treatment as in-borough placement[34]. Origin address is defined as the family's "last known address" reported to DHS[35]. A small share of families (less than 4 percent) report other shelters as their prior addresses. In light of this, and given that unstably housed family may move frequently, it is best to interpret origin addresses as places where families have preexisting community ties. Correspondingly, I define shelter neighborhoods in terms of *initial* shelter assignments. During their stays, families may be offered transfers to more proximate shelters; because within-spell moves are at families' discretion, I consider only initial assignment. Since some "control" families end up treated, this will have the effect, if any, of attenuating my results. In my Full sample, 51 percent of families are placed in their boroughs of origin.

For robustness, I also consider a continuous treatment definition: Euclidean (straight-line) distance, in miles, between origin and shelter addresses[36]. The average in-borough family is placed in a shelter 2.7 miles from its previous address, while the average out-of-borough one is placed 9.3 miles away. As a second check, I define

[33]There are several so-called "fuzzy matching" techniques standard in the computer science and statistics literatures. In this study, I primarily rely upon the user-written Stata command `reclink2`, which utilizes a bigram (two-character) string comparator (Wasi, Flaaen et al., 2015).

[34]NYC is comprised of five boroughs, which are analogous to counties: Manhattan, The Bronx, Brooklyn, Queens, and Staten Island.

[35]After cleaning, standardizing, and parsing addresses into distinct fields, I use the NYC Department of City Planning's Geosupport Desktop Edition application (GBAT), version 17.1, to classify origin and shelter addresses by borough, school district, and spatial X-Y coordinates.

[36]This measure is calculated from Cartesian geospatial coordinates.

neighborhoods in terms of the City's 32 geographical school districts, which are administrative boundaries for the public school system. 10 percent of families are placed in their neighborhood of origin by this standard.

2.4.3 Covariates

The extensive detail in my linked administrative data allows me to control for a rich set of observables. I group my covariates into three sets: placement characteristics, family characteristics, and shelter characteristics. Together, I refer to the complete collection of these variables as *Main covariates*.

Placement characteristics are factors upon which the natural experiment is conditioned. A cubic in year of shelter entry controls for time trends. Month fixed effects control for seasonal trends. Borough-of-origin dummies address systematic geographical disparities in treatment probabilities (i.e., boroughs are equal neither in shelter capacity nor shelter entrants). I also control for the four factors expressly considered as placement criteria. Family size is an integer count of unique individuals present at any time during a shelter stay. Number of children under 18 is analogously defined (both include non-relative case members). Health issue is a dummy equal to one if any family member has a medical, mental health, or substance abuse issue, and is based on screenings performed by DHS and providers at intake and during shelter stays[37]. Official eligibility reason is a set of six dummies: eviction, overcrowding, housing conditions, domestic violence, other, and unknown. DV status is particularly relevant to shelter placements, as safety concerns are paramount. I also include an integer count of oldest child's (potential) grade—my RD running variable—both to ensure comparability between estimation methods and because this age factors into placement decisions.

[37] I interpret missing values of the health issues indicator as indicative of good health; families not receiving a screening are assumed not to have significant limitations. This assumption is strengthened by the fact that my data derives from authoritative administrative records.

Family characteristics describe families' compositions and circumstances, while proxying for unobservables. Female is a dummy that is equal to one for female head of family and zero otherwise. Age is a continuous measure, in years, of the duration between a head's date of birth and shelter entry date. Race consists of six mutually exclusive categories: White, Black, Hispanic, Asian, Other, and Unknown (if race is refused or missing). Partner present is a dummy equal to one if a head's significant other is present in shelter, whether or not such a partner is a married spouse. Pregnancy is a dummy equal to one if a family indicates a pregnant member at shelter entry. Education consists of four mutually exclusive categories: no degree (less than high school), high school graduate, some college or more, and unknown[38]. On Cash Assistance and On Food Stamps are dummies equal to one if a family has an active benefit case in the respective program at the time of shelter entry. Log average quarterly earnings in the year prior to shelter entry is analogous to the earnings outcomes defined above.

The final category of controls are *shelter characteristics*: variables related to a family's shelter assignment. These include four categories of facility type (Tier II shelter, cluster unit, commercial hotel, and other) and five dummies for shelter borough. In my "Shelter" specification, I also include dummies for the 271 individual "facilities" into which families in my sample are placed. These dummies proxy for unobservable shelter and provider characteristics[39].

2.4.4 Censoring

My analysis is complicated by the flow nature of my sample. I do not observe all families for the same length of time, and some outcomes for some families are right-

[38]Education level derives from DSS records. While some families do not report education, non-matches between DHS and DSS account for most of the unknown cases.

[39]Given facility codes for cluster units encompass many distinct buildings, the latter interpretation of these fixed effects as indicative of provider influences is probably more accurate. Six facilities have singleton observations and are dropped from the Full sample in this specification.

censored. For outcomes derived from DHS records (length of stay, subsidized exits, and one-year returns), this issue is minimal, as my CARES data extends through May 2019. Only 2 percent of my sample have censored stays. Slightly more, 5 percent, are not observed for a full year following shelter exit (see Table B.1).

However, my DSS data only extends through 2016 and my DOL data through the first quarter of 2017. Thus, for these outcomes, I take care to define censoring-resilient measures, focusing on one-year windows following shelter entry and exit, so as to put families on as equal footing as the data allows[40]. Because observations can still be censored within these year intervals, I also prioritize indicator or rate variables, which can at least be partially defined during partially-censored years. Nevertheless, I do not observe a full year of post-entry public benefit outcomes for 16 percent of family-spells. Post-exit, 34 percent of family-spells have incompletely observed benefit outcomes; 30 percent have censored labor market results.

The vast majority of this censoring occurs for family spells beginning in 2015 or 2016. Since the censoring mechanism is primarily an artifact of the data collection process, I make the standard assumption that it is as-good-as random and therefore will primarily attenuate my results toward zero. This assumption will hold so long as longer-staying early-year family shelter entrants are representative of longer-staying later-year ones. Nevertheless, for robustness, I replicate most of my main analyses for a sample of pre-2015 entrants[41].

[40]When quarters are the unit of time, all such periods are defined as excluding the quarter of transition and inclusive of the following four quarters. When days are the time unit, periods begin on the day of transition and extend for the the next 365 days, inclusive. I also follow the same approach when controlling for pre-shelter earnings, considering the year prior to shelter entry.

[41]An earlier version of this paper, based on entirely on data observed through 2016, included an extensive discussion about the the econometrics of censoring and presented results for a variety of censoring methods, including survival analysis and selection models. The major prediction was that treatment effects would be attenuated in the presence of censoring, and indeed that is what I find. The earlier version of the paper is available upon request.

2.5 Empirical Approach

2.5.1 OLS: A Shelter Scarcity Experiment

In my main analysis, I define treatment for family i during homeless spell p as an indicator in-borough placement, $N_{ip} = \mathbf{1}\{boro_{ip,origin} = boro_{ip,shelter}\}$. Correspondingly, Y_{Nip} is a potential outcome for family i. If as DHS suggests, shelter assignments are truly quasi-random once shelter entry contexts and placement criteria are taken into account, I can make the conditional independence assumption $\{Y_{ip0}, Y_{ip1}\} \perp N_{ip}|\mathbf{X}_{ip}$, where $\mathbf{X}_{ip}$ includes all covariates (including fixed effects and a constant) in a particular model. My general estimating equation is:

$$Y_{ip} = \mathbf{X}_{ip}\boldsymbol{\beta} + \tau^{OLS} N_{ip} + \varepsilon_{ip} \tag{2.1}$$

Under the CIA, unobservables, ε_{ip}, are unrelated to treatment ($E[\varepsilon_{ip}|\mathbf{X}_{ip}] = 0$), and so OLS consistently estimates the *average treatment effect* (ATE) of neighborhood placement, $ATE = E[Y_{1ip} - Y_{0ip}|\mathbf{X}_{ip}] = \tau^{OLS}$.

I focus on four covariate specifications, the components of which are described in Section 2.4. My *Base* specification is a simple bivariate mean comparison. My *Placement* specification controls for factors expressly implicated in families' placement assignments. My *Main* (preferred) specification augments the Placement specification with additional family and shelter characteristics. My *Shelter* specification includes facility fixed effects and narrows the unit of comparison to distantly- and locally-placed families in the same shelter. I cluster standard errors at the "family group" level[42].

[42]Family groups, which I define with an algorithm linking all families with at least one overlapping member, address the evolution of family structures during my sample period as well as multi-spell families.

2.5.2 Instrumental Variables: Exogenous Policy Shocks

In Section 2.6, I present evidence in favor of random assignment. But even when OLS consistently estimates ATE's, it is silent on response heterogeneity, τ_{ip}, which is particularly policy-relevant when resources are scarce. Instrumental variables identify natural experiments in their own right, estimating *local* average treatment effects (LATE's) among compliers whose treatment statuses are affected by the instrument[43]. By isolating impacts among families at various treatment margins (which, in general, differ by instrument), these localized experiments can reveal the distributional aspects of policy.

At the same time, the evidence for random assignment is favorable, but not dispositive; family unobservables, which even detailed administrative data cannot inform, may still bias results. Thus, IV can also play its more traditional role of guarding against endogeneity. The difference is one of interpretation.

My IV approach exploits exogenous variation in the City's homeless policy writ large. Neighborhood-based shelter placements are but one element of the City's complex and perpetually-evolving homelessness strategy. Front-door policies, like those influencing eligibility determinations, affect the pace of shelter entry, while back-door approaches, like rental subsidies, impact exit rates[44]. These flows influence the likelihood of local placement: the faster is the entry current or the slower is the exit stream, the worse is an eligible family's chance of a well-matched placement. Equally important, front-door and back-door policies are driven by political, budgetary, and operational considerations independent of families' potential outcomes and treatment statuses. In other words, these policy changes are exogenous shocks—a second layer

[43]For details on LATE's, introduced by Imbens and Angrist (1994) and Angrist, Imbens and Rubin (1996), see Angrist and Pischke (2008). Heckman and Vytlacil (1999, 2005, 2007), Vytlacil (2002), and Heckman, Urzua and Vytlacil (2006) show LATE's, as well as ATE's and other conventional treatment effect parameters, can be derived as weighted averages of underlying marginal treatment effects (MTE's).

[44]Also important are shelter conditions, but these are harder to measure.

of quasi-random variation—to doubly justify the natural experiment assumption.

I consider two such instruments. The first, borrowed from Chapter 1, focuses on the front door: the family shelter ineligibility rate. Although the City is legally required to house needy families, the rigor of the application process provides ample room for administrative discretion, typically with regard to the stringency with which disqualifying rules are enforced[45]. As can be seen in the top panel of Figure 2.1, the large changes in the ineligibility rate are associated with new commissioners, and the most striking shift came when Bill de Blasio replaced Mike Bloomberg as mayor in 2014. Other big swings coincide with well-publicized policy initiatives, such as the City-negotiated modifications to State eligibility rules that took place between September 2015 and November 2016[46]. The figure also makes plain the strong relationship between eligibility policy and in-borough placement[47].

Specifically, my first instrument is the 15-day moving average of the initial ineligibility rate for rolling 30-day application periods[48]. For family i entering shelter on day $D = d$, my instrument Z_{id}^{IE} is defined as average ineligibles divided average applications:

$$Z_{id}^{IE} = \frac{\frac{1}{15}\sum_{D=(d-14)}^{d}\sum_{i\in D}\mathbf{1}\{O_i = inel\}}{\frac{1}{15}\sum_{D=(d-14)}^{d}\sum_{i\in D}1}$$

[45]Families are deemed ineligible for two broad reasons—failure to comply with application procedures or availability of other housing—both of which, in part, are subject to interpretation. For more detail, see the discussions in NYC Independent Budget Office (2014); Routhier (2017*a*); Harris (2016).

[46]O'Flaherty (2019) discusses these policy changes in detail. See also: Jorgensen (2017); New York State Office of Temporary and Disability Assistance (2016*a*); New York State Office of Temporary and Disability Assistance. (2015*a*); Fermino (2016*a*); Eide (2018); New York Daily News Editorial (2014); Fermino (2016*b*); Katz (2015); Routhier (2017*b*).

[47]Figure B.1 gives seasonally-detrended versions of these graphs, which makes the relationship even clearer.

[48]Families can apply for shelter multiple times; a month is the conventional agency standard for defining discrete spells of housing instability. New periods begin following gaps of more than 30 days from families' previous applications. Periods "roll" by resetting the 30-day clock with each application. "Initial" refers to the outcome of a family's first application within a period. The 15-day moving average includes each family's date of shelter entry and the 14 days prior, weighted in proportion to daily applications; it is simply a device to smooth out noise.

with $\mathbf{1}\{\cdot\}$ the indicator function and O_i a random variable denoting family i's application outcome, which may be eligible, ineligible, diversion, or made own arrangement (voluntarily withdrawn or incomplete).

For the ineligibility rate instrument to be valid, it must satisfy four well-known conditions. First-stage relevance is empirically obvious. Monotonicity follows from the reasonable assumption that less competition means better chances of local placement for all families.

Independence requires that the ineligibility rate not influence the mix of shelter entrants; because ineligibility policy can select the eligible families who comprise my sample, this is a nontrivial concern. In Chapter 1, I present detailed evidence that this sort of sample selection does not take place. Families entering during periods of high and low eligibility are remarkably similar. A major reason why is that families may apply for shelter as many times as desired. Even in strict policy environments, most are eventually determined eligible; tight policy operates primarily by slowing the pace of shelter entry rather than preventing entries completely.

Exclusion correspondingly demands that the effect of eligibility policy on outcomes operates entirely through its impact on local placement. One challenge is that eligibility policy may be correlated with other policy changes. I address this concern by including a cubic in years in all of my regressions, so as to capture general contextual trends without overfitting. What's more, eligibility policy is the most direct front-door intervention, so coincident policy changes that are part of the same broad homelessness strategy eligibility policy reflects can reasonably be seen as supplemental contributors. To err on the side of caution, I interpret my IV results as weakly satisfying the exclusion restriction: approximations of true LATE's that may be mildly influenced by the directs effects of related policies.

My second instrument, original to this paper, elaborates on the first by incorporating back-door policies—specifically, subsidized shelter exits. In an effort to shorten stays and strengthen housing stability, the City has implemented a variety of rental assistance programs over the years. Typically offering time-limited benefits and requiring family contributions, these programs, which are often conditioned on criteria such as employment and income, help families transition to permanent housing.

I refer to my second instrument as the "aversion ratio," Z_{id}^{AR}. It gives the shelter census averted by policy normalized by the number of entrants:

$$Z_{id}^{AR} = \frac{\overline{SE} + \overline{IN}}{\overline{EL}}$$

where SE is a count of subsidized exits, IN is a count of ineligible families, EL is a count of eligible families, and the bars denote 15-day moving averages, e.g., $\overline{SE} = \frac{1}{15}\sum_{D=(d-14)}^{d} SE_d$. As shown in the bottom panel of Figure 2.1, the aversion ratio has an even tighter correspondence with movements in the probability of in-borough placement than does ineligibility alone; accounting for both front- and back-door policies makes the instrument stronger. The arguments required to justify independence and exclusion are similar to before, with the obvious extension that the absence or presence of rental assistance programs doesn't alter potential outcomes except through their influence on treatment probabilities. As with front-door policies, the availability of rental assistance programs depends largely on political and budgetary factors orthogonal to family characteristics. For example, the primary rental assistance program during the Bloomberg years ended with great fanfare in 2011 due to funding dispute between the City and State (Secret, 2011; Edwards, 2012), while the the de Blasio administration was quick to roll out its successor, Living in Communities (LINC) upon taking office in 2014 (NYC Mayor's Office, 2017).

I use the ineligibility rate and aversion ratio instruments separately in standard two-stage least squares (2SLS) estimation, with Equation 2.1 representing the second stage (with actual treatment, N_{ip} replaced by first-stage predicted treatment, $\widehat{N_{ip}}$)

and first stages given by:

$$N_{ip} = \mathbf{X}_{ip}\boldsymbol{\pi}_0 + \pi_1 Z_{ip} + \nu_{ip} \tag{2.2}$$

where Z_{ip} is either of the instruments and ν_{ip} is the error.

The resulting estimates of τ^{IE} and τ^{AV} are LATE's among their respective compliant subpopulations. Given the variation that produces these localized experiments stems from big-picture homeless strategy, these instruments isolate treatment effects among families, who as a logical matter, face augmented barriers to local placement: they are treated only when the policy environment makes doing so especially easy. If, as might be anticipated, the responses of these marginally-treated homeless families are distinct from the average responses OLS identifies, it is of considerable interest to understand who these families are. Put differently, carefully chosen instruments—i.e., policy variables that influence treatment participation margins—can identify treatment effects that are policy relevant in the sense of Heckman and Vytlacil (1999, 2001, 2005, 2007).

Accordingly, I supplement my IV analysis with additional exercises characterizing compliers. While it is fundamentally impossible to identify individual compliers, it is possible to estimate their average characteristics. Angrist and Pischke (2008) show how to do this in the canonical binary instrument case; Dahl, Kostøl and Mogstad (2014) and Dobbie, Goldin and Yang (2018) implement an analogous procedure for continuous instruments. In Chapter 1, I extend this work to incorporate explicit hypothesis tests and continuous characteristics. I follow the same procedure here[49].

[49]In brief, this algorithm uses first-stage regressions and convenient conditional probability equivalences to estimate the relative prevalences of traits in the compliant subpopulation; standard errors are calculated through bootstrap resampling. Details are provided in the empirical appendix of Chapter 1.

2.5.3 Regression Discontinuity: A Boost at School-Starting

A complementary identification strategy exploits policy rules native to the neighborhood placement policy itself. The policy is, expressly, an educational policy: the explicit goal is to place families near their youngest children's *schools*[50]. This lends itself to a regression discontinuity design[51]. Families whose oldest children are younger than school age have a less compelling case for local placement than do those with school-age children. While DHS seeks to place all families in their origin boroughs, those with student members get priority.

My RD setup is both discrete and fuzzy, which introduces several non-standard issues[52]. My running variable is the potential grade attained by a family's oldest child during the year of shelter entry: $A_{ip} = \lfloor \frac{EOY - DOB}{365.25} - 5 \rfloor$, where EOY is December 31 of the shelter entry year, DOB is date of birth, and the L-brackets indicate the floor operator. In, NYC, children are eligible for, and required to, attend kindergarten in the calendar years they turn five, so this assignment variable gives families' oldest children's potential grades, normalized so that zero is kindergarten. Policy dictates this running variable be discrete: age matters in years. There are 16 support points, $A_{ip} \in \{-3, -2, \ldots, 11, 12\}$[53].

Because having a school-age child increases the chances of local placement but does not guarantee it, my RD is fuzzy. What changes sharply at the school starting threshold is treatment assignment, not treatment status. It follows that school-age threshold crossing, $T_{ip} = \mathbf{1}\{A_{ip} \geq 0\}$, is an instrument for local placement.

Discrete fuzziness dictates my RD analysis reduces to standard IV (Angrist and Pischke, 2008). Traditional RD concerns—local polynomial choice and bandwidth

[50]Most students in NYC attend their residentially-zoned school, so placement near a youngest child's school usually means older siblings are near their schools as well.

[51]For details on RD, see, e.g., Hahn, Todd and Van der Klaauw (2001); Imbens and Lemieux (2008); Lee and Lemieux (2010); Cattaneo, Idrobo and Titiunik (2018, 2017)

[52]See Kolesár and Rothe (2018); Lee and Card (2008); Dong (2015); Frandsen (2017).

[53]I exclude $A_{ip} = \{-5, -4\}$ because families who enter shelter during children's birth years or soon thereafter have idiosyncratic outcomes.

selection—are simplified. I estimate two categories of models, which I refer to as "Wald" and "Linear." The general form of my Wald equation is

$$
\begin{aligned}
N_{ip} &= \mathbf{X}_{ip}\boldsymbol{\pi}_0 + \pi_1 T_{ip} + \nu_{ip} \implies \widehat{N}_{ip} && \text{(first stage)} \\
Y_{ip} &= \mathbf{X}_{ip}\boldsymbol{\pi}_1 + \tau^{RDW}\widehat{N}_{ip} + \varepsilon_{ip} && \text{(second stage)}
\end{aligned}
\tag{2.3}
$$

The Wald setup is based on local randomization approach to RD inference (Cattaneo, Idrobo and Titiunik, 2018). The key assumption is that treatment assignment is as-good-as-random in some neighborhood of the assignment cutoff. Rather than make any assumptions about functional forms in the neighborhood of the cutoff, I simply pool the running variable for a limited set of support points at or near the threshold.

I vary this model across three dimensions: bandwidth, threshold, and covariates. For bandwidths, I use both the narrowest possible comparison, $A_{ip} \in \{-1, 0\}$, as well as a "wide Wald" frame expanded to two support points on either side of the threshold. Second, to address variability in school-starting age (discussed below), I variously include and exclude families at the $A_{ip} = 0$ threshold. Exclusion yields a potentially sharper comparison, at the risk of being less representative. Finally, I present estimates both with and without Main covariates, with the following adjustment. My running variable is highly collinear with family size, number of children under 18, and head of household's age, so I replace the continuous measures with indicators for whether a family is above-median in these characteristics; I refer to this modified set as "Main RD" covariates.

More common than local randomization, RD proceeds from continuity assumptions: namely, that conditional expectations of treatment and outcomes, as functions of the running variable, are smooth on either side of the cutoff, with any discontinuity in extrapolated intercepts attributed to the effect of threshold-crossing (Cattaneo, Idrobo and Titiunik, 2017). My "Linear" models are rooted in this framework. I allow the slopes to differ on either side of the threshold, estimating the following set of equations by 2SLS:

$$
\begin{aligned}
N_{ip} &= \mathbf{X}_{ip}\boldsymbol{\pi}_{10} + \pi_{11}T_{ip} + \pi_{12}A_{ip} \\
&\quad + \pi_{13}(A_{ip} \times T_{ip}) + \nu_{1ip} \implies \widehat{N}_{ip} && \text{(first stage)} \\
N_{ip} \times A_{ip} &= \mathbf{X}_{ip}\boldsymbol{\pi}_{20} + \pi_{21}T_{ip} + \pi_{22}A_{ip} \\
&\quad + \pi_{23}(A_{ip} \times T_{ip}) + \nu_{2i} \implies \widehat{N_{ip} \times A}_{ip} && \text{(first stage)} \\
Y_{ip} &= \mathbf{X}_{ip}\boldsymbol{\pi}_{30} + \tau^{RDL}\widehat{N}_{ip} + \pi_{32}A_{ip} \\
&\quad + \pi_{33}(\widehat{A_{ip} \times N}_{ip}) + \varepsilon_{ip} && \text{(second stage)} \qquad (2.4)
\end{aligned}
$$

Given the normalization of the running variable, τ^{RDL} gives the estimated treatment effect at the threshold. As with the Wald estimates, I present several specifications, estimating the model (a) for global ($[-3, 12]$) and local ($[-3, 3]$) bandwidths, (b) including and excluding the threshold, and (c) with and without Main RD covariates.

For both Wald and Linear RD inference, I continue to cluster standard errors at the family group level. Following Lee and Card (2008), conventional practice for discrete RD has been to cluster on the running variable. However, recent research by Kolesár and Rothe (2018) demonstrates that these standard errors can be substantially too small, especially when, as here, there is a limited number of support points[54]. Since their results show traditional heteroskedasticity-robust standard errors are about as good as the more elaborate bias-corrected variants they propose, I stick with family group clustering, which, in any event, is standard in IV estimation and thus ensures comparability with my non-RD IV results.

For my RD to be valid, it must satisfy standard IV assumptions. Discontinuous treatment probabilities at the threshold (i.e., first-stage relevance) is empirically clear

[54]In related work, Dong (2015) offers corrections when the running variable is a discretized version of a continuous variable. Though my running variable falls in this category, I do not pursue it here, as the discrete age is the policy-relevant attribute.

and monotonicity is uncontroversial. The exclusion restriction is the highest hurdle. It must be that school starting affects potential outcomes only through its influence on treatment probabilities. In the homeless shelter context, preferential placements based on school enrollment make shelter assignments a major channel through which school-agedness effects are transmitted. But having a child start school frees up time that would otherwise be spent on child care for work and leisure. From a time allocation perspective, one would expect families with kindergärtners would have higher rates of employment and shorter shelter stays. On the other hand, stays could lengthen if desires to not disrupt school motivate families to delay move-outs.

Another challenge for validity is that school-starting age is itself fuzzy. Although most children attend kindergarten during their age-five years, parents may optionally enroll their children in prekindergarten at age four or defer school-starting until first grade at age six[55]. In addition, families may enter shelter at any time during their children's school-starting years, including prior to school enrollment (i.e., January–June). Setting age five (i.e., potential grade zero) as the strict treatment assignment threshold will thus impart some degree of misclassification. Fortunately, this is a minor concern. As the graphical evidence presented in Section 2.6 demonstrates, age five is the empirically obvious discontinuity point: while the probability of treatment rises about two percentage points per year up until age four, it gets a five percentage point bump at age five (from 46.9 percent to 52.0 percent). Part of the reason for the sharp divide is that my running variable, which is based on calendar year, captures four-year-olds in the second halves of their pre-K-eligible school years as among those assigned to treatment. My threshold-omitting and linear specifications, which are less sensitive to blurry treatment assignment, offer even sharper contrasts[56].

[55]Based on data from Chapter 1, I estimate at least 93 percent of homeless children start school by kindergarten; of these, slightly more than half attend pre-K. The City's introduction of universal pre-K in 2014 guaranteed all four-year-olds public pre-kindergarten spots. Prior to 2014, only a quarter of four-year-olds attended full-day public pre-K (NYC Mayor's Office, 2014).

[56]In principle, school-starting fuzziness could be remedied with data on children's actual enrollment statuses, which I lack due to confidentiality restrictions. However, actual enrollment status is

2.5.4 Family Fixed Effects: Multi-Spell Counterfactuals

My third identification strategy relies on the panel nature of my data. Repeat spells of homelessness are not uncommon. A fifth (10,390) of families in my sample have multiple stays during my study period (see Table B.2). When these families' treatment statuses vary across these stays, comparing own outcomes when placed locally and distantly is an exacting way to estimate treatment effects. Implementing my family fixed effects estimator is a straightforward modification of Equation 2.1 to include individual student dummies, α_i. For family i in shelter spell p,

$$Y_{ip} = \alpha_i + \tau^{FE} N_{ip} + \mathbf{X}_{ip}\boldsymbol{\beta} + \varepsilon_{ip} \tag{2.5}$$

I continue to cluster standard errors at the family group level.

Consistency relies upon the assumption of no spell-varying unobservables. This assumption is strengthened by the presence of administrative covariates capturing broad classes of cross-spell variation. In addition, the underlying quasi-randomness of shelter scarcity continues to apply to each spell.

Prior research suggests homeless spells among low-income families are largely based on luck (O'Flaherty, 2010). It follows that those with multiple bad hands are representative of homeless families in general. At the least, their findings generalize to the considerable subsample of multi-spell families.

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potentially less desirable as an instrument, as school-starting is subject to parental choice endogeneity.

2.6 Results

2.6.1 Descriptives and Randomization Check

My first empirical task is to assess the plausibility of the natural experiment assumption. Is shelter assignment truly determined by a scarcity-based queuing?

Table 2.2 formally tests this proposition, while also descriptively summarizing the Full sample. The randomization check consists of separate bivariate regressions of baseline covariates and pre-shelter outcomes on an indicator for in-borough placement. The difference between treated (in-borough) and untreated (out-of-borough) families is the coefficient on treatment. If placements are truly random, these characteristics should be approximately balanced.

Due to the large sample size, group contrasts are often statistically significant, but they are rarely economically meaningful. Families placed in- and out-of-borough are virtually identical in family composition and education, as well as pre-shelter public benefit use, employment, and earnings.

The big differences are innocuous and expected. There is systematic variation in treatment probability by year, month, and borough. Shelter is relatively more abundant in the early years of my sample (when the homeless population is smaller), during the early months of the year (when fewer families enter shelter), and in the Bronx (where a plurality of shelters are located). Along related lines, treated families are more likely to be placed in cluster units (which are more common in the Bronx and earlier in the sample), while their untreated counterparts are more likely to be assigned to commercial hotels (which are more common in the other boroughs and later in the sample).

Other placement criteria matter, too. Due to safety concerns, families experiencing domestic violence are considerably less likely to be treated, accounting for 22 percent of in-borough placements but 37 percent of out-of-borough ones. Conversely, evictions are more common in-borough. Families with health limitations are also more challenging to place: 32 percent of out-of-borough families have health issues, compared with 28 percent of in-borough ones. In-borough families have older oldest children, averaging third grade, versus the out-of-borough average of second. Family heads are older, too.

Overall, the data supports the administrative impression that shelter placements depend upon availability, conditioned on placement criteria.

2.6.2 OLS Results

Tables 2.3A and 2.3B present my main OLS results. Given the evidence for conditional random assignment, these are my preferred ATE estimates. Each cell gives the coefficient on in-borough placement from a separate regression. Outcomes are listed in rows and organized into three panels. Panel A in Table 2.3A analyzes stays and returns—the most salient outcomes in the homeless services domain. Table 2.3B is split into two panels: year post-entry outcomes (B1), which refer to the year following a family's shelter entry (and is typically, but not always, spent in shelter), and year post-exit outcomes (B2), which refer to the year following shelter exit (and is typically, though not always, spent out of shelter). Column 1 gives outcome means. Columns 2–5 present sequentially more stringent covariates for the Full sample. Columns 6 (Non-Domestic-Violence) and 7 (Pre-2015) consider alternative samples for robustness. My preferred estimates are those in Column 4, which include Main covariates for the Full sample. Family-group clustered standard errors are given in parentheses. Sample sizes are given in braces under the first outcome in each panel, as well as for subsequent within-panel outcomes where the sample size differs from the first due to censoring.

As would be expected under random assignment, covariates beyond placement factors make little difference in the results. Focusing on Panel A's Main estimates

(Col 4), families assigned in-borough stay 12.7 percent longer than those placed out-of-borough. With lengths of stay averaging 424 days, this implies in-borough families remain in shelter 54 days longer, though, the log specification acknowledges these effects may be non-linear. In-borough families are also 1.8 pp (4.6 percent) more likely to exit with a rental subsidy. They do not appear any more likely to return to shelter.

Panel B1 (Table 2.3B) shows that, during their years of shelter entry, in-borough families are 1.1 pp (1.4 percent) more likely to receive Cash Assistance. They are also 1.0 pp (2.1 percent) more likely to be employed and have 9.9 percent higher quarterly earnings. It is not clear whether the labor boost is due to preserving existing employment relationships or through new opportunities fostered by retained social ties. There is no impact on Food Stamps, likely because almost all homeless families receive it. Panel B2 illustrates that elevated Cash Assistance recipiency continues in the year post-shelter exit, by 1.7 pp (2.3 percent). During this year, the benefits connection extends to Food Stamps as well, by 0.8 pp (0.9 percent). But employment effects disappear.

This pattern of outcomes is consistent with the search effort model of shelter behavior. Homeless families respond to program incentives by allocating effort to their highest-value priorities. In-borough placement is preferred, so families stay longer and require additional impetus—rental assistance—to leave. Time otherwise spent on housing search is instead allocated to labor and consumption[57].

These findings remain consistent in my Shelter specification (Col 5), which controls for provider quality, as well as in the Non-DV (Col 6) and Pre-2015 (Col 7) samples, suggesting neither eligibility reasons nor censoring issues are driving my results.

[57]One concern with this behavioral interpretation is that City rental assistance policy could be driving length of stay. If the City prioritized out-of-borough families for subsidies, longer stays for in-borough families would be an artifact of subsidy queuing. In Appendix B.3 and Table B.6, I provide evidence that this is not the case: the effect of in-borough placement on length of stay is, if anything, strengthened when accounting for subsidies. Table B.7 confirms this is also true of my IV results.

Tables 2.4A and 2.4B present additional robustness checks, examining the same outcomes for treatment defined as school district placement and school-shelter distance, in miles, controlling for Main covariates. School district treatment (Col 1) confirms my Full sample results for length of stay (8.5 percent longer), entry-year employment (+1.8 pp), and entry-year earnings (+13 percent). However, other results are near zero or imprecise, likely for two reasons. First, only a small minority of families are placed in their school districts. Second, the stakes are higher for borough treatment: untreated families by the school district standard can still be quite close to their prior addresses. Being very close to home may be more important for jobs than it is for other outcomes.

Distance treatment broadly confirms my main findings, demonstrating that genuine proximity effects—rather than borough quirks—are at play. The Full sample (Col 4) results show that families stay 1.4 percent longer for every mile they are placed closer to their prior residences. At the average borough treatment distance gap of 6.6 miles, this translates to 9.4 percent longer stays. The probability of subsidized exit increases by 0.26 pp per mile closer to school, while the likelihood of Cash Assistance receipt increases 0.15 pp/mile post-entry and 0.16 pp/mile post-exit. Entry-year employment increases by 0.20 pp/mile closer and earnings by 1.6 percent/mile[58].

2.6.3 IV Results

Although I believe my OLS results credibly describe average policy responses in my quasi-experimental setting, prudent skepticism nevertheless dictates—and policy exogeneity permits—alternative identification strategies. Tables 2.5A and 2.5B present my main policy IV results. Similar in organization to Tables 2.3A and 2.3B, the first three columns assess the ineligibility rate instrument while the latter three analyze

[58] Table B.8 repeats Tables 2.3A and 2.3B for several alternative outcome definitions.

the aversion ratio.

Both instruments are very strong. First-stage F-stats, given in brackets (for the first outcome in each panel, as well as for subsequent outcomes with censored samples), are consistently above 20 for the ineligibility rate and double that for the aversion ratio. As expected, policy strictness increases the likelihood of local placement. A 10 pp increase in the ineligibility rate increases the chances of in-borough placement by 3.0 pp (Col 2), while an additional averted stay per unit entrant raises treatment probability by 6.1 pp (Col 5).

Length of stay continues to exhibit the most striking findings. LATE's for compliers are in the direction of OLS ATE's but an order of magnitude larger (Panel A). Per my Main specification (the point estimates for the Placement and Shelter specifications are similarly precise and slightly smaller in magnitude), families placed in-borough when the ineligibility rate is high but not otherwise stay four times longer (Col 2). Aversion ratio compliers (Col 5) stay 2.6 times longer when placed locally. Ineligibility rate compliers are also 29 pp more likely to return to shelter. The largest departure from OLS is that policy compliers are substantially less likely to exit with a subsidy: by 79 pp for the ineligibility rate and by 33 pp for the aversion ratio.

Compliers' use of other public benefits (Panels B1 and B2) are also more strongly influenced by proximity than homeless families overall. Continuing to focus on Main covariate specifications (Cols 2 and 5), ineligibility rate compliers are 65 pp more likely to receive Cash Assistance during their shelter entry years, and 43 pp more likely to receive it post-exit. LATE's for aversion ratio compliers are slightly smaller—34 pp entry year Cash Assistance, 27 pp exit year Cash Assistance—but still huge. As with OLS, there appears to be little effect on compliers' use of Food Stamps either during or after shelter. Unlike OLS, labor market impacts for compliers arise after shelter. There are no statistically significant effects for either instrument during the year post-entry. Post-exit, however, ineligibility rate compliers are 40 pp more likely to be employed. Aversion ratio compliers have a 34 pp employment boost—and earn seven times more.

These coefficients are large, but not implausible. Outcomes among homeless families have wide variation. A 400 percent increase in length of stay takes families from the median (294 days) to about the 95th percentile (1,246 days); the fifth percentile is just 20 days (see Figure B.6). Similarly, only a third of families are on Cash Assistance at shelter entry and just 43 percent work in the prior year, so the room for impact is large.

What's more, compliers—-who are placed in-borough *only* when policy makes it easy to do so—are families with considerable barriers to local placement. These constraints, discussed below, may also make it more difficult to find permanent housing, as well as generate inertial incentives to stick with in-borough shelter apartments that are nontrivial to obtain. Consequently, length of stay increases, allowing more time for other treatment effects to percolate. About 8 percent of my Full sample are ineligibility rate compliers and 10 percent comply with the aversion ratio (see Tables B.9 and B.10).

Tables 2.6A and 2.6B compare the average characteristics of ineligibility rate compliers with non-compliers, using the Dahl, Kostøl and Mogstad (2014) and Dobbie, Goldin and Yang (2018) procedure with a modified first-stage controlling for time trends and seasonality[59]. The most notable contrast is borough of origin. 57 percent of compliers are from the Bronx, compared with 39 percent of non-compliers[60]. Compliers also tend to be medium-large families: 39 percent have four or five members, compared with 28 percent of non-compliers. Other comparisons are imprecisely estimated. It should also be noted that these complier characteristics are indicative

[59]See Tables B.22 and B.23 for comparisons of additional characteristics.

[60]Compliers are also less likely to be African-American (43 percent vs. 57 percent) or sheltered in commercial hotels (8 percent vs. 29 percent), though these contrasts are likely explained by borough. Only 45 percent of Bronx entrants are Black, versus 55 percent of shelter entrants overall; likewise, just 21 percent of Bronx placements are in commercial hotels.

but not unqualified: majorities of large, young, and Bronx families are non-compliers, after all, so unobservables and characteristic interactions are clearly implicated[61].

Aversion ratio compliers (Tables 2.7A and 2.7B), like ineligibility rate ones, are more likely than non-compliers to originate from the Bronx (55 percent vs. 39 percent). The family size contrast loses statistical precision, though the point estimate (+6 pp for family size of 4–5) is similar. What becomes more notable is Cash Assistance receipt. Just 23 percent of aversion ratio compliers are on CA at shelter entry, compared with 37 percent of non-compliers[62].

Large families from the Bronx disproportionately benefit when eligibility policy gets tighter or move-outs more common. The Bronx is where 41 percent of homeless families originate—by far the most of any borough—and also where the most out-of-borough families are placed (29 percent). Not uncoincidentally, PATH, the City's central intake center for homeless families, is also located there. When eligibility gets strict, applications become more labor-intensive; Bronx families have easier access, gaining an advantage as the out-of-borough flow slows. Large families also benefit from less congestion. The bigger a family, the harder is it to find suitable units; less competition improves the odds.

It is reasonable that large Bronx families also be especially responsive to local placement. The Bronx is small, isolated, and poor (U.S. Census Bureau, 2018), so treatment is more meaningful. In-borough placements are closer and out-of-borough ones further than non-Bronx averages. Competition for high-quality, affordable housing is fierce. Bronx families, especially large ones, fortunate to secure local placements thus have less incentive to leave.

[61] Differences between this depiction of ineligibility rate compliers and that discussed in Chapter 1 are likely due to the facts that the latter (implicitly) weights results at the child level, includes only school-age children, and covers fewer years. In addition, that paper defines borough of origin in terms of school address.

[62] Further, 35 percent have health limitations, compared with 29 percent of non-compliers; while this contrast narrowly misses statistical significance, it is indicative of the finding in Chapter 1, where the unit of complier comparison is school-age children. Aversion ratio compliers are also less likely to be in commercial hotels (−16 pp) or Black (−9 pp), with the latter marginally insignificant.

At the same time, aversion ratio LATE's are generally 50–60 percent the magnitudes of their ineligibility rate counterparts. The difference in pre-shelter CA receipt may help explain why. As reflected by their lower reliance on public benefits—as well as large, precise post-shelter employment responses—aversion compliers would seem to be drawn from the higher end of the self-sufficiency spectrum.

A conservative perspective suggests interpreting these IV results as upper bounds. Both instruments are based on time variation and may pick up the effects of complementary policies (e.g., improved shelter quality). In my main results, I control for macro patterns with a year cubic. Table B.15 and Figures B.3 and B.4 detail a time trend sensitivity analysis. The OLS results are little changed. Sufficiently flexible trends absorb much variation in IV reduced forms, but robust first stages suggest overfitting rather than exclusion restriction violations is to blame: to the extent time trends capture correlated policy changes, these correlated changes appear small and eligibility policy remains independently informative[63].

For the skeptical reader inclined to think in terms of homogeneous effects and endogeneity, my IV results suggest OLS, if anything, is understating true policy impacts. But heterogeneity seems the more parsimonious story consistent with facts.

2.6.4 RD Results

Having a school-age child is a third instrument, with its own population of compliers: families placed locally only when they have school-age children. Figures 2.2–2.4 show how treatment and outcomes vary according to the running variable, oldest child's (potential) grade. Each graph plots mean outcomes and 95 percent confidence intervals by grade, along with linear trends fit separately on either side of the threshold. Left of the threshold, the regression is fit on the [-3,-1] interval and extrapolated from

[63]Additional robustness checks for the ineligibility and aversion instruments are detailed in Tables B.13 and B.14, respectively. My main results are confirmed.

-5 to 0; the above-threshold regression is fit on the full [0,12] interval[64].

The top left panel of Figure 2.2 shows the fuzzy RD first-stage is strong. Although the probability of in-borough placement increases at young ages, there is an unmistakable boost when families' oldest children reach school age. Families whose oldest children are six are about 8 pp (17 percent) more likely to be placed in-borough than those whose oldest are four. Treatment probabilities remain basically flat at older ages, though there may be a slight bump around middle school starting (grade six)[65]. Length of stay exhibits an even starker discontinuity at school starting (Figure 2.2, top right). Exits and returns do not display decisive breaks (bottom panels) .

Figures 2.3 and 2.4 show entry- and exit-year benefit and employment outcomes, respectively. These results are, in general, nosier and treatment effects more muted. Cash Assistance displays the clearest discontinuity around school starting, with notable increases during the kindergarten ($A_{ip} = 0$) and first-grade ($A_{ip} = 1$) years, both during and following shelter (top left panels). Food Stamps appear unrelated to school-starting (top rights). Labor market outcomes are more nuanced (bottom panels). During the year of shelter entry, employment and earnings drop noticeably among families whose oldest children are in first or second grade, but hold steady, or even slightly increase, among those with kindergarten-age children. Post-shelter, there is slightly stronger evidence of an adverse labor market impact, especially with earnings, though it is difficult to disentangle discontinuities from general patterns of less employment among those who enter shelter with older children.

Tables 2.8A and 2.8B formalize the RD analysis, confirming the visual impression. As before, results are grouped into three panels, with each row considering a separate outcome. Column 1 gives Wald estimates for immediately adjacent threshold points ($A_{ip} = \{-1, 0\}$), while Column 2 excludes the threshold in assessing a symmetric

[64]Negative "grades" should be interpreted as years before conventional school starting age. I exclude -5 and -4 in fitting the below-threshold regression due to unrepresentative patterns among families with very young oldest children.

[65]Figure B.8 shows an analogous pattern holds for distance treatment.

two-year window ($A_{ip} = \{-2, -1, 1, 2\}$). Columns 3 and 4 assess global linear fits, the latter controlling for Main RD covariates.

Families whose treatment status is affected by having a school-age child stay about 3–7 times longer when placed in-borough (Table 2.8A)[66]. They are 35–66 pp more likely to leave shelter with a subsidy. But there is little evidence of impact on shelter returns.

There are few clear entry-year impacts on benefits and employment (Table 2.8B, Panel B1). The exception, as might be anticipated, is Cash Assistance, which has generally large positive coefficients, precisely estimated in the covariate-adjusted global linear specification (Col 4), suggesting a 18 pp increase in the probability of Cash Assistance receipt among compliers. Food Stamps and employment effects are unclear, though the balance of evidence for the latter is suggestive of mild negative impacts.

Exit-year effects are generally sharper (Table 2.8B, Panel B2). Cash Assistance is again the most striking result, with compliers 14–40 pp more likely to receive it, significant in all specifications. At the same time, local placement appears to adversely impact compliers' post-exit labor market outcomes. Point estimates for both employment and earnings are uniformly negative, though statistically significant only in the highly-powered wide Wald case (Col 2; -29 pp employment decrease; 4.5 times fewer earnings). Food Stamps impacts remain difficult to discern[67].

Correct inferences depend on whether families who enter shelter with young oldest children are suitable counterfactuals for those with school-age ones. Families congregating on either side of the threshold would be evidence of deliberate sorting that would invalidate RD identification. The histogram in Figure 2.5 demonstrates this is not the case: the frequency of shelter entry is smooth around the treatment threshold.

[66] To see this, note that $e^{1.065} = 2.9$ and $e^{1.986} = 7.3$.

[67] Tables B.16 and B.17 provide additional Wald and Linear specification permutations, respectively. Table B.18 reproduces the RD analysis for my three alternative samples. Table B.19 replicates the RD analysis distance treatment across all four samples. Table B.20 repeats Tables 2.8A and 2.8B for an alternative running variable: "potential grade" defined based on school years, starting in July and ending in June. The main conclusions remain unchanged.

The formal Frandsen (2017) test for the manipulation of a discrete running variable confirms this impression, delivering a maximum p-value of 0.832, which cannot nearly reject the null of no sorting.

A second implication of random assignment is that families below and above the treatment threshold be similar in baseline covariates and pre-shelter outcomes. To assess this proposition, Figures 2.6–2.8 repeat the RD plots for these characteristics, while Tables B.21A–B.21B provide the formal regression analysis[68]. The presence of threshold-crossing induced treatment effects for any of these "outcomes" is evidence that the RD independence and exclusion assumptions may be violated.

There are no discontinuities for most variables, including pre-shelter public benefit use and labor market outcomes, though employment and earnings peak among families whose oldest children are five. On the other hand, year of shelter entry (families with older oldest children enter in later years), housing conditions as an eligibility reason (less likely with school-age children), and education (those with school-age children are more highly educated) do have discontinuities at the threshold[69]. Overall, families around the school-starting threshold are comparable; most differences are expected.

A perhaps more important caution relates to representativeness: I estimate school-starting compliers constitute about one percent of my sample, or about a tenth the size of my IV complier populations. Nevertheless, school-starting families are an important subpopulation in their own right[70].

[68]Figures B.11–B.13 give the three-year window versions.

[69]In addition, there are threshold kinks in shelter locations, but these are expected given most homeless families originate from the Bronx and Brooklyn and those with school-age children are prioritized for in-borough placement. Boroughs of origin show no such patterns.

[70]Table B.25 provides a formal complier characterization exercise, finding compliers disproportionately have Bronx and Brooklyn origin, Tier II placements, fewer members, and younger heads.

2.6.5 Family FE Results

My final identification strategy capitalizes on a different sort of natural experiment: multiple homeless spells. Tables 2.9A and 2.9B summarize the analysis. The first four columns assess the Full sample. Columns 5 and 6 consider robustness-check subsamples. The results are virtually identical to OLS; if anything, they slightly strengthen key findings. Per my Full sample Main specification (Col 3), families stay 17 percent longer when placed in-borough. Public benefit use is greater as well. They are 2.6 pp more likely to exit with a subsidy and 1.6–1.7 pp more likely to receive Cash Assistance during and after shelter. Entry-year employment increases by 1.7 pp and quarterly earnings by 15 percent. There is no evidence of impacts for Food Stamps or post-shelter labor market outcomes. The length of stay, subsidized, and entry-year Cash Assistance results hold for both alternative subsamples. The entry-year earnings finding holds for the Pre-2015 sample and the exit-year Cash Assistance result holds for the Non-DV sample. Other subsample point estimates are in the expected directions.

2.7 Conclusion

Homeless families placed in shelters in their neighborhoods of origin remain in shelter longer and are better connected to public benefits. Per the natural experiment of shelter scarcity—which justifies OLS identification and facilitates family fixed effects as well—*average* families stay 13–17 percent longer when assigned in-borough. They are about 5 percent more likely to exit shelter with a rental subsidy, and have 2 percent greater propensities to receive Cash Assistance, both during and after shelter. They also work more, with 10–15 percent higher earnings during the year of shelter entry when placed locally, though labor market effects attenuate post-shelter.

These are meaningful impacts. Yet they pale in comparison to effects among *marginally-treated* families—those who, due to such factors as geography, composition, or children's ages, tend to secure in-borough placements only when conditions are favorable. Both policy (IV) and school-starting (RD) compliers stay on the order of four times longer when placed in-borough. Both are overwhelmingly—by roughly 40 pp—more likely to receive Cash Assistance, with policy compliers having more pronounced effects during shelter and school-starting compliers exhibiting greater returns following it. Similarly large, but divergent, labor market impacts arise post-shelter, with policy compliers seeing 30-pp boosts in employment and school-starting compliers experiencing equally pronounced declines.

These results complement those in Chapter 1, where I find that homeless students placed in shelters in their school boroughs have markedly better attendance, performance, and stability. As with their families as a whole, students with especially challenging placement limitations exhibit greater policy responsiveness.

The challenge for policymakers is partly philosophical. The current policy objective is to place all families locally, to the extent constraints allow. But other objectives are possible. For example, if the goal is to minimize shelter use, then policy designs that make program participation less pleasant (such as distant placements) are likely to be effective. On the other hand, if the aim is to maximize the well-being of participants while they are participating, then loosening resource constraints through benefit enhancements (such as local placements) is preferable. Of course, long-term consequences matter, too. While this study is unable to assess such outcomes, the findings of generally smaller differences between treated and untreated families post-shelter, combined with the empirical regularity that most homeless families do not become long-term homeless, suggest modest increases in benefit generosity are unlike to be harmful.

Given finite resources, some families will inevitably be served suboptimally. In this context, my results suggest distinct priorities for differentially-situated groups is desirable. If locally-placed families are more apt to work, but less likely to seek housing, they should be targeted for supplemental housing search assistance. Correspondingly, distantly-placed families may have greater difficultly forging labor market ties; they should be prioritized for job training services and transit subsidies. In general, supplementary services should complement families' comparative advantages in manners compatible with their incentives.

If all homeless families were the same, there would not be much more to the story. But the theme of heterogeneity underscores a more primitive point: the potential gains from better targeting local placements. The most immediate question is not whether $10,000 is the right price to pay for, on average, 10 percent gains in earnings and school attendance, but instead how those costly shelter slots can be more efficiently allocated to the families poised to benefit the most. I find that difficult-to-place families are particularly sensitive to their shelter assignments; this "resistance" to treatment is partly predictable from administrative observables, including families' aptitudes for navigating the application process. Screening practices should be augmented to better identify high responders. Counterintuitively, the families perceived to be the most challenging to place proximately should have their slots prospectively reserved. Services better tailored to family needs should generate surpluses that can be used to compensate families given less desirable assignments.

At the core of my study is a natural experiment. Shelter assignment location is essentially random. It should not be.

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2.9 Tables

Table 2.1: Data and Sample Overview: Eligible NYC DHS Family Shelter Entrants, 2010–2016

Family Spells	Count	Percent
All	68,584	1.00
NYC Entrants	61,406	0.90
Full Sample with Borough Treatment Status	61,120	0.89
Full Sample with Treatment and Running Variable[a]	59,253	0.86
Non-DV Sample	43,235	0.63
Pre-2015 Sample	41,717	0.61
One School-Age Child Sample	40,779	0.59

Unit of observation is family shelter spell. Data from NYC administrative records, as described in text. Indentation indicates cumulative refinement.

[a] Running variable is oldest child's potential grade level for children under 18 years of age. Families whose oldest children are 19–21 years are excluded.

Table 2.2: Descriptives and Random Assignment

	Overall		Randomization Check		
Variable	Mean	SD	Out-of-Boro	In-Boro	Diff.
Year Entered Shelter	2013.01	2.07	2013.38	2012.65	-0.72**
Month Entered Shelter	6.52	3.40	6.78	6.28	-0.50**
Manhattan Origin	0.12	0.33	0.16	0.09	-0.07**
Bronx Origin	0.41	0.49	0.33	0.49	0.16**
Brooklyn Origin	0.32	0.47	0.31	0.32	0.01**
Queens Origin	0.12	0.33	0.15	0.10	-0.06**
Staten Island Origin	0.03	0.16	0.05	0.01	-0.04**
Family Size	3.35	1.39	3.34	3.36	0.02*
Family Members Under 18	1.97	1.19	1.95	1.99	0.04**
Oldest Child's Grade	2.57	5.32	1.95	3.18	1.23**
Health Issue Present	0.30	0.46	0.32	0.28	-0.04**
Eligibility: Eviction	0.33	0.47	0.28	0.39	0.10**
Eligibility: Overcrowding	0.18	0.38	0.17	0.19	0.02**
Eligibility: Conditions	0.08	0.28	0.08	0.09	0.01**
Eligibility: Domestic Violence	0.30	0.46	0.37	0.22	-0.15**
Eligibility: Other	0.11	0.31	0.10	0.11	0.01**
Female	0.92	0.28	0.92	0.91	-0.01**
Age	31.54	8.86	30.94	32.13	1.20**
Partner/Spouse Present	0.26	0.44	0.27	0.24	-0.03**
Pregnant	0.07	0.25	0.07	0.06	-0.01**
Black	0.56	0.50	0.57	0.55	-0.02**
White	0.03	0.16	0.03	0.02	-0.01**
Hispanic	0.38	0.48	0.36	0.39	0.03**
No Degree	0.57	0.50	0.56	0.58	0.01**
High School Grad	0.32	0.47	0.32	0.32	-0.01*
Some College or More	0.05	0.22	0.05	0.05	-0.00
Unknown Education	0.06	0.24	0.06	0.06	-0.00
On Cash Assistance	0.35	0.48	0.36	0.35	-0.01**
On Food Stamps	0.73	0.44	0.73	0.73	0.00
Employed Year Pre	0.43	0.50	0.44	0.43	-0.01**
Log AQ Earnings Year Pre	3.01	3.58	3.02	2.99	-0.03
Tier II Shelter	0.55	0.50	0.55	0.55	0.01**
Commercial Hotel	0.28	0.45	0.30	0.25	-0.05**
Family Cluster Unit	0.16	0.37	0.14	0.19	0.05**
Manhattan Shelter	0.18	0.39	0.27	0.09	-0.18**
Bronx Shelter	0.39	0.49	0.29	0.49	0.20**
Brooklyn Shelter	0.27	0.44	0.22	0.32	0.11**
Queens Shelter	0.15	0.36	0.21	0.10	-0.11**
Staten Island Shelter	0.01	0.09	0.01	0.01	-0.01**
School District Placement	0.10	0.30	0.00	0.19	0.19**
Placement Distance (miles)	5.89	4.65	9.27	2.66	-6.61**
Borough Placement	0.51	0.50	0.00	1.00	1.00

Treatment defined as placed in-borough. Group contrasts obtained from separate bivariate OLS regressions of each characteristic 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. Unit of observation is family-spell. Full Sample: 59,253 observations. See Appendix for additional covariates. * $p < 0.10$, ** $p < 0.05$

Table 2.3A: OLS Main Results

Outcome	Full Sample					Non-DV	Pre-2015
	Outcome Mean (1)	Base (2)	Placement (3)	Main (4)	Shelter (5)	Main (6)	Main (7)
A. Stays and Returns							
Log Length of Stay	5.501	0.139**	0.107**	0.120**	0.115**	0.085**	0.125**
	(1.241)	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)	(0.013)
	{59,253}	{59,253}	{59,253}	{59,253}	{59,247}	{41,744}	{41,717}
Subsidized Exit	0.392	0.007*	0.021**	0.018**	0.017**	0.017**	0.016**
	(0.488)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
	{57,962}	{57,962}	{57,962}	{57,962}	{57,954}	{40,766}	{41,420}
Returned to Shelter	0.151	-0.025**	-0.005	-0.005	-0.004	-0.000	-0.007*
	(0.358)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
	{52,274}	{52,274}	{52,274}	{52,274}	{52,271}	{36,768}	{40,552}
Placement Controls		No	Yes	Yes	Yes	Yes	Yes
Family & Shelter Controls		No	No	Yes	Yes	Yes	Yes
Shelter Fixed Effects		No	No	No	Yes	No	No

Each cell reports the coefficient on in-borough shelter placement from a separate OLS regression of the row-delineated outcome on the treatment indicator, controlling for the column-enumerated covariates. Placement covariates are dummies for shelter entry month, borough of origin, health issue, and eligibility reason, as well as a cubic polynomial in year of shelter entry and linear controls for family size, number of family members under 18, and oldest child's grade. Main covariates are placement covariates plus family and shelter covariates. Family covariates are dummies for head gender, race, partner presence, education category, Cash Assistance receipt, and Food Stamps receipt, as well continuous controls for head age and log average quarterly earnings. Shelter covariates are dummies for shelter type and shelter borough. All covariates are defined at shelter entry or as near as possible. Supercolumns give samples. Standard errors clustered at family group level in parentheses. Number of observations given in braces below first outcome in each panel, as well as for any subsequent outcome where the sample size differs from the first due to censoring. * $p < 0.10$, ** $p < 0.05$

Table 2.3B: OLS Main Results

	Full Sample					Non-DV	Pre-2015
Outcome	Outcome Mean (1)	Base (2)	Placement (3)	Main (4)	Shelter (5)	Main (6)	Main (7)
B1. Year Post-Entry Outcomes							
Cash Assistance	0.782	0.019**	0.015**	0.011**	0.010**	0.011**	0.013**
	(0.413)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
	{59,253}	{59,253}	{59,253}	{59,253}	{59,247}	{41,744}	{41,717}
Food Stamps	0.896	0.010**	0.006**	0.003	0.003	-0.000	0.003
	(0.306)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Employed	0.479	0.006	0.012**	0.010**	0.010**	0.009*	0.010**
	(0.500)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Log Avg. Quarterly Earnings	3.377	0.088**	0.108**	0.094**	0.086**	0.087**	0.085**
	(3.679)	(0.031)	(0.032)	(0.028)	(0.028)	(0.033)	(0.033)
B2. Year Post-Exit Outcomes							
Cash Assistance	0.738	0.014**	0.019**	0.017**	0.016**	0.021**	0.016**
	(0.440)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
	{48,082}	{48,082}	{48,082}	{48,082}	{48,076}	{33,761}	{39,974}
Food Stamps	0.884	0.010**	0.011**	0.008**	0.008**	0.003	0.008**
	(0.321)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Employed	0.455	0.005	0.009*	0.003	0.002	0.005	0.003
	(0.498)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
Log Avg. Quarterly Earnings	3.268	0.094**	0.084**	0.043	0.036	0.050	0.041
	(3.732)	(0.034)	(0.036)	(0.033)	(0.033)	(0.040)	(0.036)
Placement Controls		No	Yes	Yes	Yes	Yes	Yes
Family & Shelter Controls		No	No	Yes	Yes	Yes	Yes
Shelter Fixed Effects		No	No	No	Yes	No	No

Each cell reports the coefficient on in-borough shelter placement from a separate OLS regression of the row-delineated outcome on the treatment indicator, controlling for the column-enumerated covariates. Placement covariates are dummies for shelter entry month, borough of origin, health issue, and eligibility reason, as well as a cubic polynomial in year of shelter entry and linear controls for family size, number of family members under 18, and oldest child's grade. Main covariates are placement covariates plus family and shelter covariates. Family covariates are dummies for head gender, race, partner presence, education category, Cash Assistance receipt, and Food Stamps receipt, as well continuous controls for head age and log average quarterly earnings. Shelter covariates are dummies for shelter type and shelter borough. All covariates are defined at shelter entry or as near as possible. Supercolumns give samples. Standard errors clustered at family group level in parentheses. Number of observations given in braces below first outcome in each panel, as well as for any subsequent outcome where the sample size differs from the first due to censoring. * $p < 0.10$, ** $p < 0.05$

Table 2.4A: OLS Robustness

	School District			Distance		
	Full (1)	Non-DV (2)	Pre-2015 (3)	Full (4)	Non-DV (5)	Pre-2015 (6)
A. Stays and Returns						
Log Length of Stay	0.0814**	0.0559**	0.0757**	-0.0143**	-0.0108**	-0.0141**
	(0.0162)	(0.0169)	(0.0193)	(0.0012)	(0.0013)	(0.0016)
	{54,306}	{38,587}	{38,053}	{54,306}	{38,587}	{38,053}
Subsidized Exit	0.0009	-0.0036	0.0011	-0.0026**	-0.0023**	-0.0019**
	(0.0068)	(0.0077)	(0.0078)	(0.0005)	(0.0006)	(0.0006)
	{53,121}	{37,687}	{37,789}	{53,121}	{37,687}	{37,789}
Returned to Shelter	-0.0066	-0.0037	-0.0105*	0.0004	0.0002	0.0005
	(0.0054)	(0.0059)	(0.0059)	(0.0004)	(0.0005)	(0.0005)
	{47,858}	{33,963}	{36,991}	{47,858}	{33,963}	{36,991}
Placement Controls	Yes	Yes	Yes	Yes	Yes	Yes
Family & Shelter Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shelter FE	No	No	No	No	No	No

Each cell reports the coefficient on local shelter placement from a separate OLS regression of the row-delineated outcome on the treatment, controlling for Main covariates, described in Table 2.3A. Columns give samples; supercolumns give treatment definitions. Standard errors clustered at family group level in parentheses. Number of observations given in braces below first outcome in each panel, as well as for any subsequent outcome where the sample size differs due to censoring. * $p < 0.10$, ** $p < 0.05$

Table 2.4B: OLS Robustness

	School District			Distance		
	Full (1)	Non-DV (2)	Pre-2015 (3)	Full (4)	Non-DV (5)	Pre-2015 (6)
B1. Year Post-Entry Outcomes						
Cash Assistance	0.0013	-0.0031	0.0014	-0.0015**	-0.0015**	-0.0014**
	(0.0051)	(0.0057)	(0.0060)	(0.0004)	(0.0004)	(0.0004)
	{54,306}	{38,587}	{38,053}	{54,306}	{38,587}	{38,053}
Food Stamps	0.0042	-0.0012	0.0046	-0.0005**	-0.0001	-0.0003
	(0.0032)	(0.0036)	(0.0036)	(0.0002)	(0.0003)	(0.0003)
Employed	0.0181**	0.0129*	0.0146*	-0.0020**	-0.0018**	-0.0015**
	(0.0063)	(0.0070)	(0.0075)	(0.0004)	(0.0005)	(0.0005)
Log Avg. Quarterly Earnings	0.1262**	0.0945*	0.0895*	-0.0164**	-0.0150**	-0.0117**
	(0.0451)	(0.0503)	(0.0535)	(0.0031)	(0.0037)	(0.0038)
B2. Year Post-Exit Outcomes						
Cash Assistance	-0.0047	-0.0053	-0.0066	-0.0016**	-0.0019**	-0.0016**
	(0.0065)	(0.0074)	(0.0072)	(0.0005)	(0.0006)	(0.0005)
	{43,981}	{31,172}	{36,453}	{43,981}	{31,172}	{36,453}
Food Stamps	0.0018	-0.0048	0.0005	-0.0002	0.0003	-0.0001
	(0.0043)	(0.0049)	(0.0046)	(0.0003)	(0.0004)	(0.0003)
Employed	-0.0081	-0.0092	-0.0102	-0.0006	-0.0005	-0.0007
	(0.0073)	(0.0081)	(0.0080)	(0.0005)	(0.0006)	(0.0006)
Log Avg. Quarterly Earnings	-0.0441	-0.0556	-0.0617	-0.0060	-0.0040	-0.0055
	(0.0536)	(0.0601)	(0.0586)	(0.0037)	(0.0045)	(0.0042)
Placement Controls	Yes	Yes	Yes	Yes	Yes	Yes
Family & Shelter Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shelter FE	No	No	No	No	No	No

Each cell reports the coefficient on local shelter placement from a separate OLS regression of the row-delineated outcome on the treatment, controlling for Main covariates, described in Table 2.3A. Columns give samples; supercolumns give treatment definitions. Standard errors clustered at family group level in parentheses. Number of observations given in braces below first outcome in each panel, as well as for any subsequent outcome where the sample size differs due to censoring. * $p < 0.10$, ** $p < 0.05$

Table 2.5A: IV Main Results

	Ineligibility Rate			Aversion Ratio		
Outcome	Placement (1)	Main (2)	Shelter (3)	Placement (4)	Main (5)	Shelter (6)
A. Stays and Returns						
Log Length of Stay	1.121**	1.367**	1.151**	0.781**	0.946**	0.765**
	(0.403)	(0.527)	(0.471)	(0.282)	(0.342)	(0.331)
	[42.9]	[28.8]	[33.3]	[80.7]	[60.8]	[61.0]
Subsidized Exit	-0.581**	-0.789**	-0.664**	-0.244**	-0.331**	-0.291**
	(0.186)	(0.257)	(0.224)	(0.120)	(0.147)	(0.145)
	[39.7]	[26.2]	[30.6]	[75.4]	[55.8]	[55.8]
Returned to Shelter	0.219*	0.287*	0.272*	0.058	0.088	0.093
	(0.130)	(0.166)	(0.156)	(0.088)	(0.104)	(0.106)
	[36.0]	[25.2]	[28.0]	[71.5]	[55.7]	[54.2]
First Stage Instrument Coefficient	0.387**	0.303**	0.330**	0.073**	0.061**	0.062**
	(0.059)	(0.056)	(0.057)	(0.008)	(0.008)	(0.008)
Placement Controls	Yes	Yes	Yes	Yes	Yes	Yes
Family & Shelter Controls	No	Yes	Yes	No	Yes	Yes
Shelter FE	No	No	Yes	No	No	Yes

Each cell reports the coefficient on in-borough shelter placement from a separate 2SLS regression of the row-delineated outcome on the treatment indicator, controlling for the column-enumerated covariates, described in Table 2.3A. Instruments are indicated by supercolumns. Standard errors clustered at family group level in parentheses. First-stage F-stats given in brackets below first outcome in each panel, as well as for any subsequent outcome where the sample size differs due to censoring. All results are for Full sample; number of observations given in Tables 2.3A and 2.3B. * $p < 0.10$, ** $p < 0.05$

Table 2.5B: IV Main Results

	Ineligibility Rate			Aversion Ratio		
	Placement	Main	Shelter	Placement	Main	Shelter
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
B1. Year Post-Entry Outcomes						
Cash Assistance	0.529**	0.651**	0.579**	0.292**	0.338**	0.317**
	(0.152)	(0.183)	(0.162)	(0.101)	(0.105)	(0.104)
	[42.9]	[28.8]	[33.3]	[80.7]	[60.8]	[61.0]
Food Stamps	-0.137	-0.142	-0.095	-0.088	-0.100	-0.069
	(0.100)	(0.093)	(0.085)	(0.073)	(0.064)	(0.063)
Employed	-0.101	-0.020	-0.022	0.066	0.116	0.102
	(0.157)	(0.171)	(0.159)	(0.115)	(0.118)	(0.118)
Log Avg. Quarterly Earnings	0.264	1.245	1.035	0.650	1.085	0.903
	(1.152)	(1.243)	(1.148)	(0.847)	(0.851)	(0.846)
B2. Year Post-Exit Outcomes						
Cash Assistance	0.394**	0.428**	0.420**	0.267**	0.265**	0.288**
	(0.189)	(0.210)	(0.195)	(0.126)	(0.129)	(0.132)
	[27.4]	[20.3]	[23.2]	[56.6]	[46.4]	[45.4]
Food Stamps	-0.023	-0.064	-0.051	0.048	0.023	0.040
	(0.130)	(0.130)	(0.120)	(0.091)	(0.086)	(0.087)
Employed	0.386*	0.397*	0.363*	0.395**	0.338**	0.330**
	(0.211)	(0.232)	(0.214)	(0.147)	(0.149)	(0.150)
Log Avg. Quarterly Earnings	2.515	2.508	2.317	2.591**	2.035*	2.003*
	(1.562)	(1.673)	(1.551)	(1.093)	(1.078)	(1.090)
First Stage Instrument Coefficient	0.387**	0.303**	0.330**	0.073**	0.061**	0.062**
	(0.059)	(0.056)	(0.057)	(0.008)	(0.008)	(0.008)
Placement Controls	Yes	Yes	Yes	Yes	Yes	Yes
Family & Shelter Controls	No	Yes	Yes	No	Yes	Yes
Shelter FE	No	No	Yes	No	No	Yes

Each cell reports the coefficient on in-borough shelter placement from a separate 2SLS regression of the row-delineated outcome on the treatment indicator, controlling for the column-enumerated covariates, described in Table 2.3A. Instruments are indicated by supercolumns. Standard errors clustered at family group level in parentheses. First-stage F-stats given in brackets below first outcome in each panel, as well as for any subsequent outcome where the sample size differs due to censoring. All results are for Full sample; number of observations given in Tables 2.3A and 2.3B. * $p < 0.10$, ** $p < 0.05$

Table 2.6A: Complier Characteristics: Ineligibility Rate Instrument

	Compliers	Non-Compliers	Diff.
Manhattan Origin	0.00	0.14	-0.13
	(0.003)	(0.000)	[-2.56]
Bronx Origin	0.57	0.39	0.18
	(0.006)	(0.000)	[2.28]
Brooklyn Origin	0.25	0.32	-0.07
	(0.005)	(0.000)	[-0.99]
Queens Origin	0.10	0.13	-0.02
	(0.003)	(0.000)	[-0.45]
Staten Island Origin	0.02	0.03	-0.00
	(0.000)	(0.000)	[-0.33]
Health Issue Present	0.33	0.30	0.04
	(0.004)	(0.000)	[0.61]
Eligibility: Eviction	0.29	0.34	-0.05
	(0.005)	(0.000)	[-0.67]
Eligibility: Domestic Violence	0.30	0.30	0.00
	(0.004)	(0.000)	[0.01]
Female	0.97	0.91	0.06
	(0.002)	(0.000)	[1.26]
Partner/Spouse Present	0.31	0.25	0.06
	(0.004)	(0.000)	[0.99]
Black	0.43	0.57	-0.14
	(0.006)	(0.000)	[-1.79]
Hispanic	0.46	0.37	0.09
	(0.006)	(0.000)	[1.18]
White	0.06	0.02	0.04
	(0.001)	(0.000)	[1.57]
No Degree	0.61	0.57	0.05
	(0.005)	(0.000)	[0.67]
High School Grad	0.30	0.32	-0.02
	(0.005)	(0.000)	[-0.29]
Some College or More	0.06	0.05	0.01
	(0.001)	(0.000)	[0.21]

Full sample. Treatment is in-borough placement. Instrument is 15-day moving average of the initial ineligibility rate for 30-day application period. Compliers are families 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 Chapter 1. Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family group.

Table 2.6B: Complier Characteristics: Ineligibility Rate Instrument

	Compliers	Non-Compliers	Diff.
On Cash Assistance	0.30	0.36	-0.06
	(0.005)	(0.000)	[-0.78]
On Food Stamps	0.75	0.73	0.02
	(0.006)	(0.000)	[0.29]
Employed Year Pre	0.39	0.44	-0.05
	(0.005)	(0.000)	[-0.67]
Tier II Shelter	0.63	0.54	0.08
	(0.004)	(0.000)	[1.27]
Commercial Hotel	0.08	0.29	-0.21
	(0.005)	(0.000)	[-3.08]
Family Cluster Unit	0.19	0.16	0.02
	(0.003)	(0.000)	[0.46]
Family Size 1–3	0.54	0.64	-0.11
	(0.006)	(0.000)	[-1.43]
Family Size 4–5	0.39	0.28	0.11
	(0.004)	(0.000)	[1.68]
Family Size 6+	0.07	0.08	-0.01
	(0.002)	(0.000)	[-0.27]
Age	31.69	31.53	0.16
	(1.350)	(0.013)	[0.14]
Log AQ Earnings Year Pre	2.73	3.03	-0.30
	(0.259)	(0.002)	[-0.60]

Full sample. Treatment is in-borough placement. Instrument is 15-day moving average of the initial ineligibility rate for 30-day application period. Compliers are families 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 Chapter 1. Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family group.

Table 2.7A: Complier Characteristics: Aversion Ratio Instrument

	Compliers	Non-Compliers	Diff.
Manhattan Origin	0.09	0.13	-0.04
	(0.001)	(0.000)	[-1.18]
Bronx Origin	0.55	0.39	0.16
	(0.004)	(0.000)	[2.67]
Brooklyn Origin	0.20	0.33	-0.13
	(0.003)	(0.000)	[-2.29]
Queens Origin	0.12	0.13	-0.01
	(0.002)	(0.000)	[-0.17]
Staten Island Origin	0.03	0.03	0.01
	(0.000)	(0.000)	[0.51]
Health Issue Present	0.35	0.29	0.06
	(0.002)	(0.000)	[1.27]
Eligibility: Eviction	0.33	0.34	-0.01
	(0.003)	(0.000)	[-0.13]
Eligibility: Domestic Violence	0.27	0.30	-0.03
	(0.002)	(0.000)	[-0.59]
Female	0.94	0.91	0.03
	(0.001)	(0.000)	[0.82]
Partner/Spouse Present	0.28	0.25	0.03
	(0.002)	(0.000)	[0.61]
Black	0.47	0.57	-0.09
	(0.004)	(0.000)	[-1.51]
Hispanic	0.43	0.37	0.06
	(0.003)	(0.000)	[0.98]
White	0.05	0.02	0.03
	(0.000)	(0.000)	[1.46]
No Degree	0.59	0.57	0.02
	(0.003)	(0.000)	[0.41]
High School Grad	0.31	0.32	-0.01
	(0.002)	(0.000)	[-0.21]
Some College or More	0.07	0.05	0.02
	(0.001)	(0.000)	[0.76]

Full sample. Treatment is in-borough placement. Instrument is 15-day moving average of the aversion ratio. Compliers are families placed in-borough when the aversion ratio 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 Chapter 1. Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family group.

Table 2.7B: Complier Characteristics: Aversion Ratio Instrument

	Compliers	Non-Compliers	Diff.
On Cash Assistance	0.23	0.37	-0.14
	(0.003)	(0.000)	[-2.43]
On Food Stamps	0.70	0.74	-0.04
	(0.003)	(0.000)	[-0.63]
Employed Year Pre	0.39	0.44	-0.05
	(0.003)	(0.000)	[-0.91]
Tier II Shelter	0.59	0.55	0.04
	(0.003)	(0.000)	[0.82]
Commercial Hotel	0.14	0.29	-0.16
	(0.003)	(0.000)	[-2.83]
Family Cluster Unit	0.19	0.16	0.02
	(0.001)	(0.000)	[0.63]
Family Size 1–3	0.60	0.64	-0.04
	(0.003)	(0.000)	[-0.76]
Family Size 4–5	0.35	0.28	0.06
	(0.003)	(0.000)	[1.26]
Family Size 6+	0.05	0.08	-0.02
	(0.001)	(0.000)	[-0.82]
Age	32.20	31.47	0.73
	(0.949)	(0.014)	[0.74]
Log AQ Earnings Year Pre	2.76	3.03	-0.27
	(0.147)	(0.002)	[-0.71]

Full sample. Treatment is in-borough placement. Instrument is 15-day moving average of the aversion ratio. Compliers are families placed in-borough when the aversion ratio 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 Chapter 1. Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family group.

Table 2.8A: Regression Discontinuity Main Results

	(1)	(2)	(3)	(4)
A. Stays and Returns				
Log Length of Stay	1.986**	1.612**	1.357**	1.065**
	(0.705)	(0.271)	(0.436)	(0.331)
	{7,679}	{14,925}	{50,480}	{50,480}
Subsidized Exit	0.353*	0.661**	0.622**	0.370**
	(0.211)	(0.106)	(0.171)	(0.126)
	{7,548}	{14,642}	{49,334}	{49,334}
Returned to Shelter	-0.067	-0.247**	0.013	-0.042
	(0.153)	(0.075)	(0.120)	(0.101)
	{6,798}	{13,268}	{44,574}	{44,574}
First Stage	0.051**	0.089**	0.051**	0.058**
	(0.011)	(0.008)	(0.013)	(0.012)
	[20.4]	[117.8]	[89.6]	[104.1]
Order	Wald	Wald	Linear	Linear
Bandwidth	{-1,0}	{-2,-1,1,2}	[-3,12]	[-3,12]
Threshold	Yes	No	Yes	Yes
Covariates	No	No	No	Yes

The table presents fuzzy regression discontinuity analysis using families' oldest children's potential grades (end-of-calendar-year age year minus five) as the running variable. Each cell reports the coefficient on in-borough shelter placement from a separate 2SLS regression of the row-delineated outcome on the treatment indicator, using as the instrument an indicator for whether a family's oldest child's potential grade is zero or greater. Columns 1 and 2 give Wald estimates pooling the running variable for the given bandwidth; coefficients are thus instrumented mean comparisons between families without and with school-aged children. Columns 3 and 4 fit linear regressions on the running variable for the given bandwidths, allowing for different slopes on either side of the threshold; the coefficients are the differences in intercepts at the threshold. Column 4 controls for Main RD covariates. Standard errors clustered at family group level in parentheses. Number of observations given in braces below first outcome in each panel, as well as for any subsequent outcome where the sample size differs due to censoring. First-stage given for in-borough placement indicator. First-stage F-stat, in brackets, given for log length of stay regressions. * $p < 0.10$, ** $p < 0.05$

Table 2.8B: Regression Discontinuity Main Results

	(1)	(2)	(3)	(4)
B1. Year Post-Entry Outcomes				
Cash Assistance	0.223	0.025	0.170	0.183**
	(0.188)	(0.076)	(0.128)	(0.092)
	{7,679}	{14,925}	{50,480}	{50,480}
Food Stamps	0.070	-0.137**	-0.037	0.009
	(0.130)	(0.056)	(0.090)	(0.055)
Employed	0.001	-0.268**	-0.123	-0.081
	(0.223)	(0.098)	(0.156)	(0.114)
Log Avg. Quarterly Earnings	0.881	-1.131	-0.568	-0.277
	(1.623)	(0.690)	(1.124)	(0.815)
B2. Year Post-Exit Outcomes				
Cash Assistance	0.403**	0.138*	0.398**	0.347**
	(0.191)	(0.084)	(0.152)	(0.120)
	{6,295}	{12,246}	{41,110}	{41,110}
Food Stamps	0.212	-0.107*	0.091	0.071
	(0.130)	(0.059)	(0.100)	(0.073)
Employed	-0.147	-0.287**	-0.219	-0.135
	(0.203)	(0.099)	(0.162)	(0.128)
Log Avg. Quarterly Earnings	-0.901	-1.533**	-1.606	-0.909
	(1.485)	(0.714)	(1.192)	(0.935)
	{6,295}	{12,246}	{41,110}	{41,110}
First Stage	0.051**	0.089**	0.051**	0.058**
	(0.011)	(0.008)	(0.013)	(0.012)
	[20.4]	[117.8]	[89.6]	[104.1]
Order	Wald	Wald	Linear	Linear
Bandwidth	{-1,0}	{-2,-1,1,2}	[-3,12]	[-3,12]
Threshold	Yes	No	Yes	Yes
Covariates	No	No	No	Yes

The table presents fuzzy regression discontinuity analysis using families' oldest children's potential grades (end-of-calendar-year age year minus five) as the running variable. Each cell reports the coefficient on in-borough shelter placement from a separate 2SLS regression of the row-delineated outcome on the treatment indicator, using as the instrument an indicator for whether a family's oldest child's potential grade is zero or greater. Columns 1 and 2 give Wald estimates pooling the running variable for the given bandwidth; coefficients are thus instrumented mean comparisons between families without and with school-aged children. Columns 3 and 4 fit linear regressions on the running variable for the given bandwidths, allowing for different slopes on either side of the threshold; the coefficients are the differences in intercepts at the threshold. Column 4 controls for Main RD covariates. Standard errors clustered at family group level in parentheses. Number of observations given in braces below first outcome in each panel, as well as for any subsequent outcome where the sample size differs due to censoring. First-stage given for in-borough placement indicator. First-stage F-stat, in brackets, given for log length of stay regressions. * $p < 0.10$, ** $p < 0.05$

Table 2.9A: Family Fixed Effects Results

	Full Sample				Non-DV	Pre-2015
	Base	Placement	Main	Shelter	Main	Main
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
A. Stays and Returns						
Log Length of Stay	0.091**	0.156**	0.158**	0.149**	0.093**	0.133**
	(0.024)	(0.024)	(0.025)	(0.025)	(0.030)	(0.034)
	{20,149}	{20,149}	{20,149}	{20,125}	{11,134}	{12,570}
Subsidized Exit	-0.020**	0.029**	0.026**	0.024**	0.023*	0.033**
	(0.009)	(0.008)	(0.009)	(0.009)	(0.012)	(0.011)
	{19,659}	{19,659}	{19,659}	{19,633}	{10,850}	{12,467}
Returned to Shelter	0.011	-0.013	-0.007	-0.004	-0.001	-0.005
	(0.011)	(0.011)	(0.011)	(0.011)	(0.015)	(0.014)
	{17,464}	{17,464}	{17,464}	{17,444}	{9,597}	{12,089}
Placement Controls	No	Yes	Yes	Yes	Yes	Yes
Family & Shelter Controls	No	No	Yes	Yes	Yes	Yes
Shelter FE	No	No	No	Yes	No	No

Each cell reports the coefficient on in-borough shelter placement from a separate OLS regression of the row-delineated outcome on the treatment indicator, controlling for family fixed effects. Standard errors clustered at family group level in parentheses. Number of observations given in braces below first outcome in each panel, as well as for any subsequent outcome where the sample size differs from the first due to censoring. * $p < 0.10$, ** $p < 0.05$

Table 2.9B: Family Fixed Effects Results

	Full Sample				Non-DV	Pre-2015
	Base	Placement	Main	Shelter	Main	Main
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
B1. Year Post-Entry Outcomes						
Cash Assistance	0.017**	0.016**	0.017**	0.019**	0.016*	0.013*
	(0.006)	(0.006)	(0.006)	(0.006)	(0.009)	(0.008)
	{20,149}	{20,149}	{20,149}	{20,125}	{11,134}	{12,570}
Food Stamps	-0.001	-0.002	-0.002	-0.001	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Employed	0.010	0.014*	0.017**	0.016**	0.010	0.013
	(0.007)	(0.007)	(0.008)	(0.008)	(0.011)	(0.010)
Log Avg. Quarterly Earnings	0.062	0.109**	0.137**	0.129**	0.055	0.119*
	(0.049)	(0.050)	(0.052)	(0.053)	(0.073)	(0.066)
B2. Year Post-Exit Outcomes						
Cash Assistance	0.013*	0.013*	0.016**	0.016**	0.026**	0.012
	(0.007)	(0.007)	(0.008)	(0.008)	(0.011)	(0.009)
	{15,585}	{15,585}	{15,585}	{15,569}	{8,498}	{11,820}
Food Stamps	0.006*	0.004	0.006	0.006	0.009	0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.004)
Employed	-0.005	-0.004	0.001	0.004	0.005	-0.000
	(0.008)	(0.009)	(0.009)	(0.009)	(0.013)	(0.011)
Log Avg. Quarterly Earnings	-0.037	-0.002	0.025	0.047	0.013	0.027
	(0.057)	(0.059)	(0.061)	(0.063)	(0.088)	(0.071)
Placement Controls	No	Yes	Yes	Yes	Yes	Yes
Family & Shelter Controls	No	No	Yes	Yes	Yes	Yes
Shelter FE	No	No	No	Yes	No	No

Each cell reports the coefficient on in-borough shelter placement from a separate OLS regression of the row-delineated outcome on the treatment indicator, controlling for family fixed effects. Standard errors clustered at family group level in parentheses. Number of observations given in braces below first outcome in each panel, as well as for any subsequent outcome where the sample size differs from the first due to censoring. * $p < 0.10$, ** $p < 0.05$

2.10 Figures

Figure 2.1: Policy Instruments Time Series

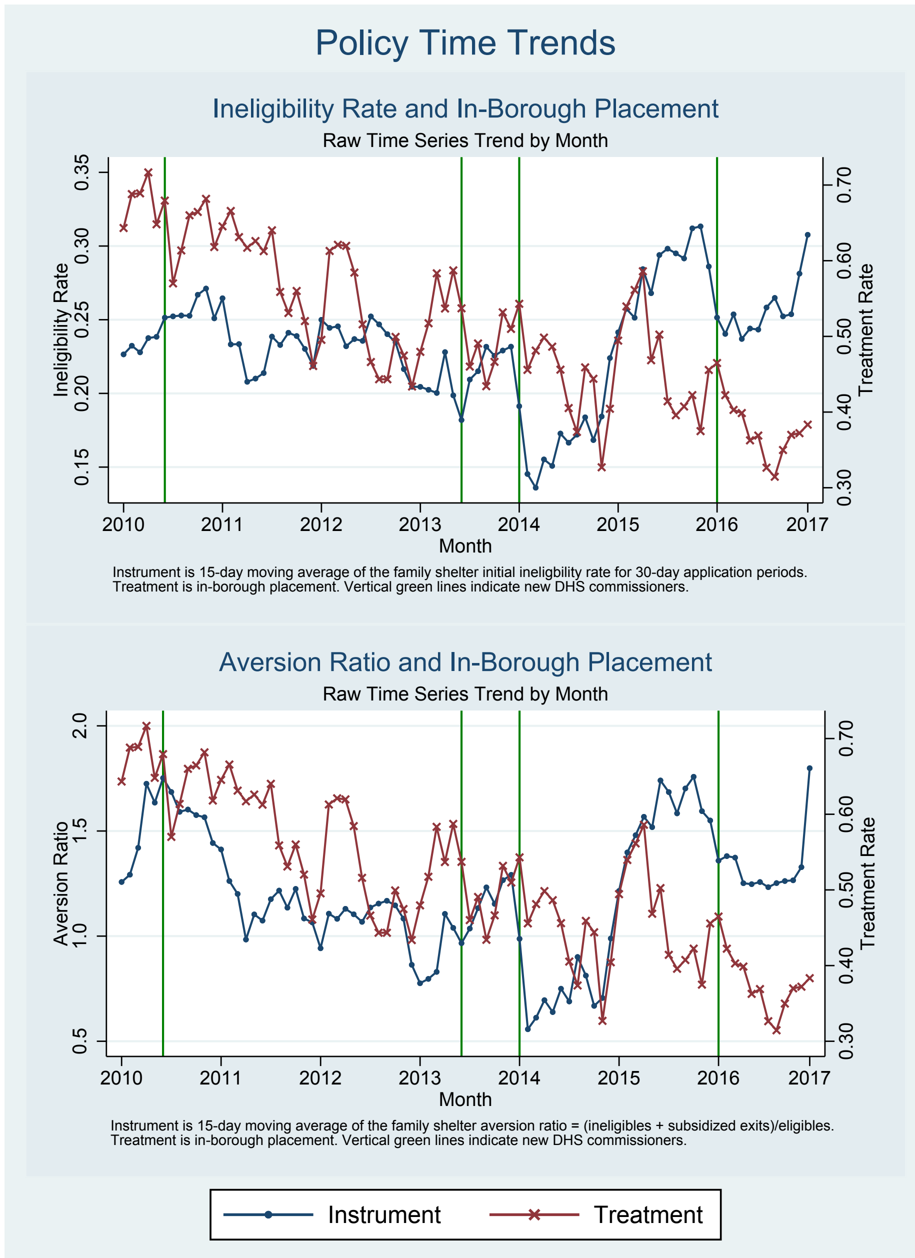


Figure 2.2: Regression Discontinuity Treatment, Stays, and Returns

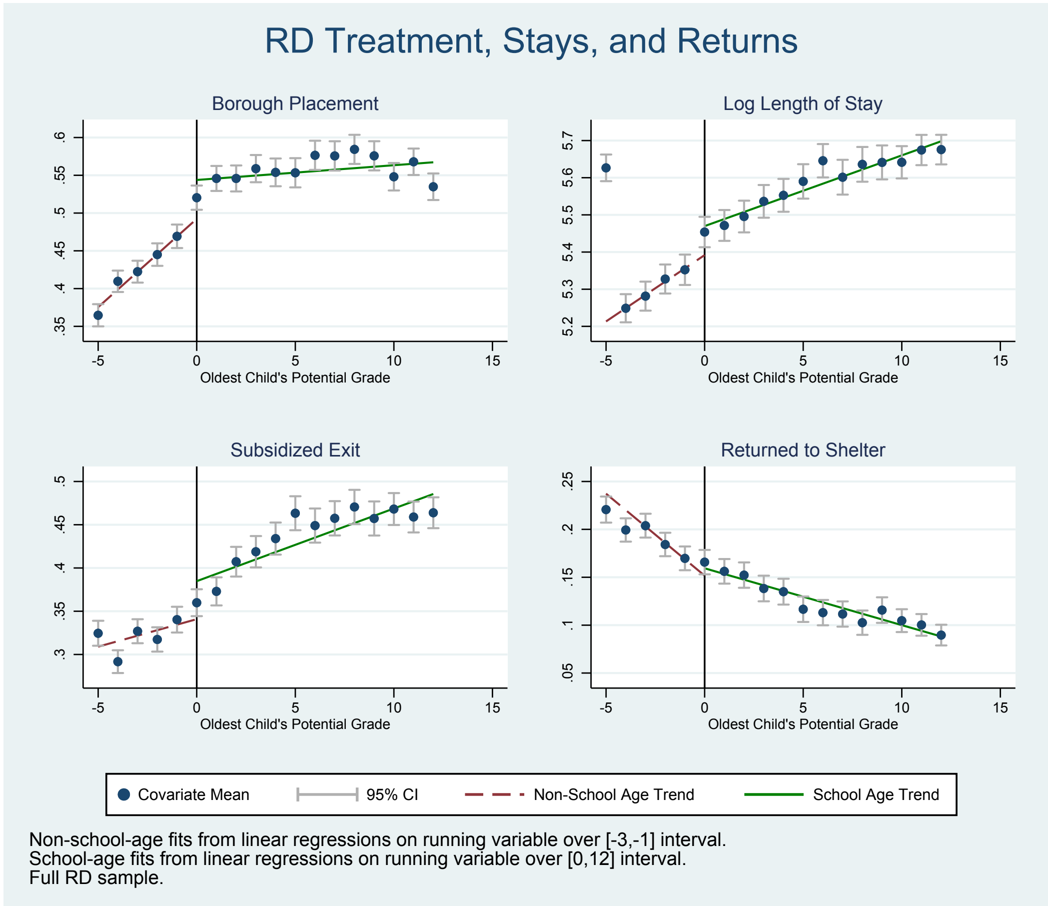


Figure 2.3: Regression Discontinuity Entry Year Outcomes

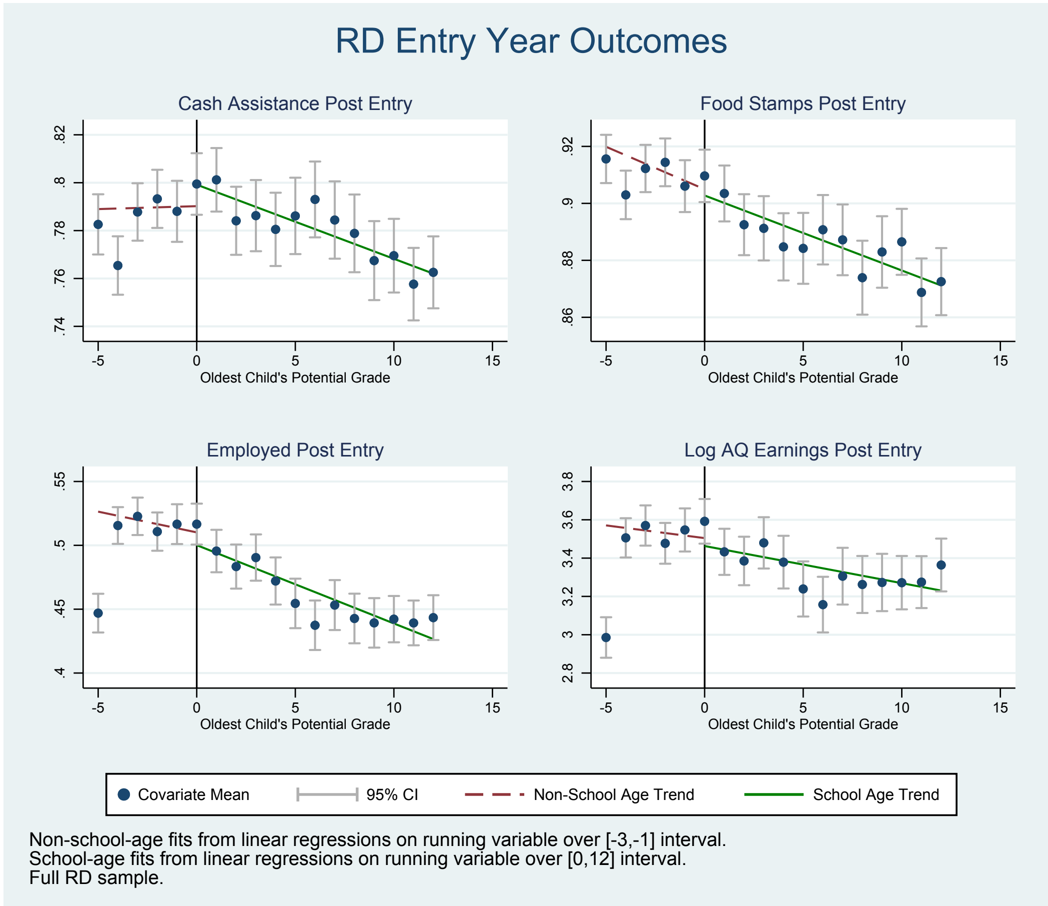


Figure 2.4: Regression Discontinuity Exit Year Outcomes

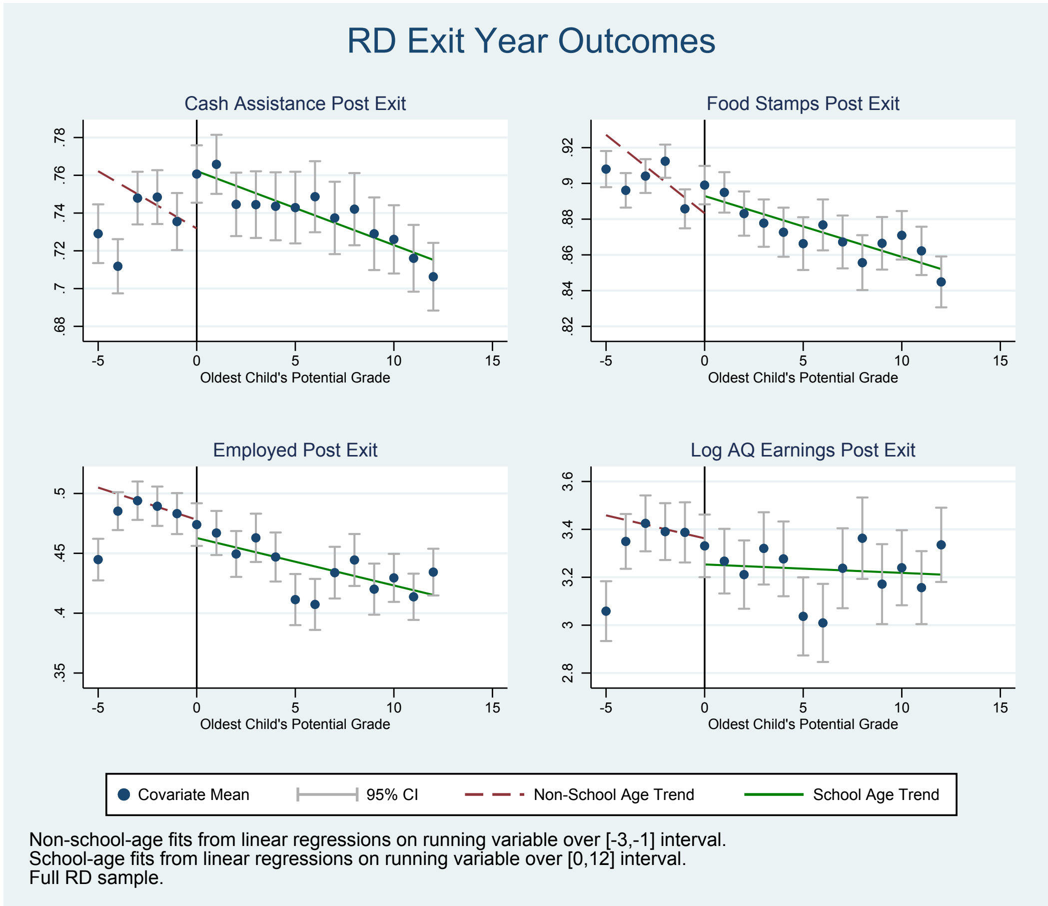


Figure 2.5: Density of Assignment Variable

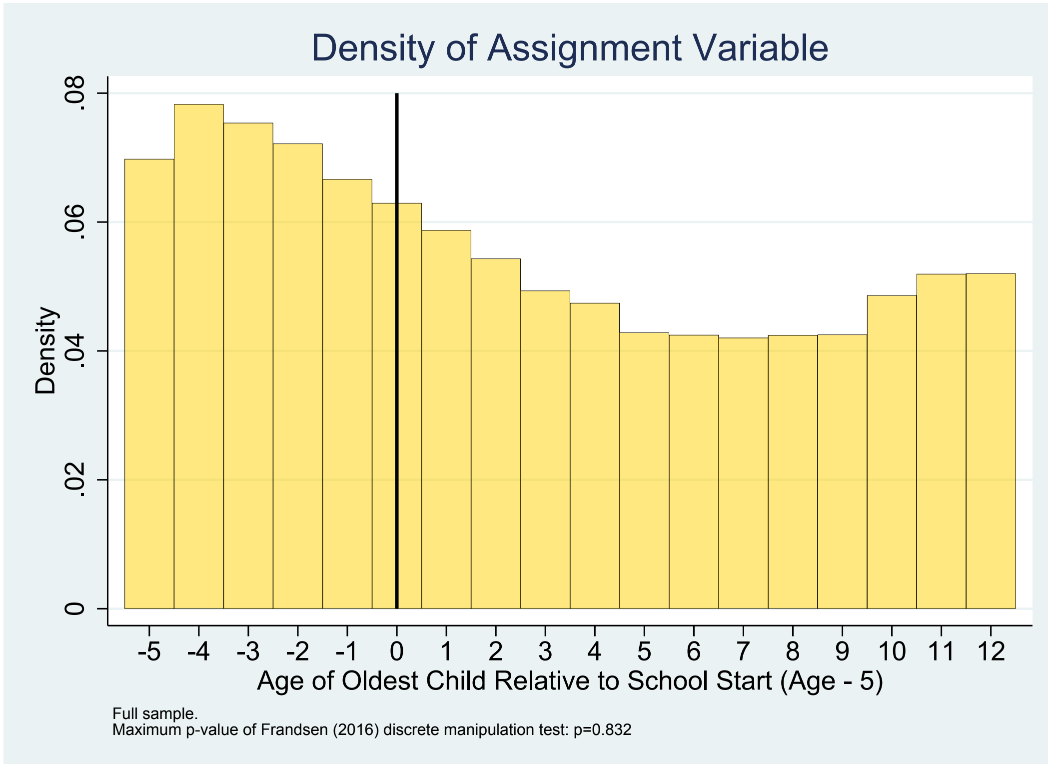
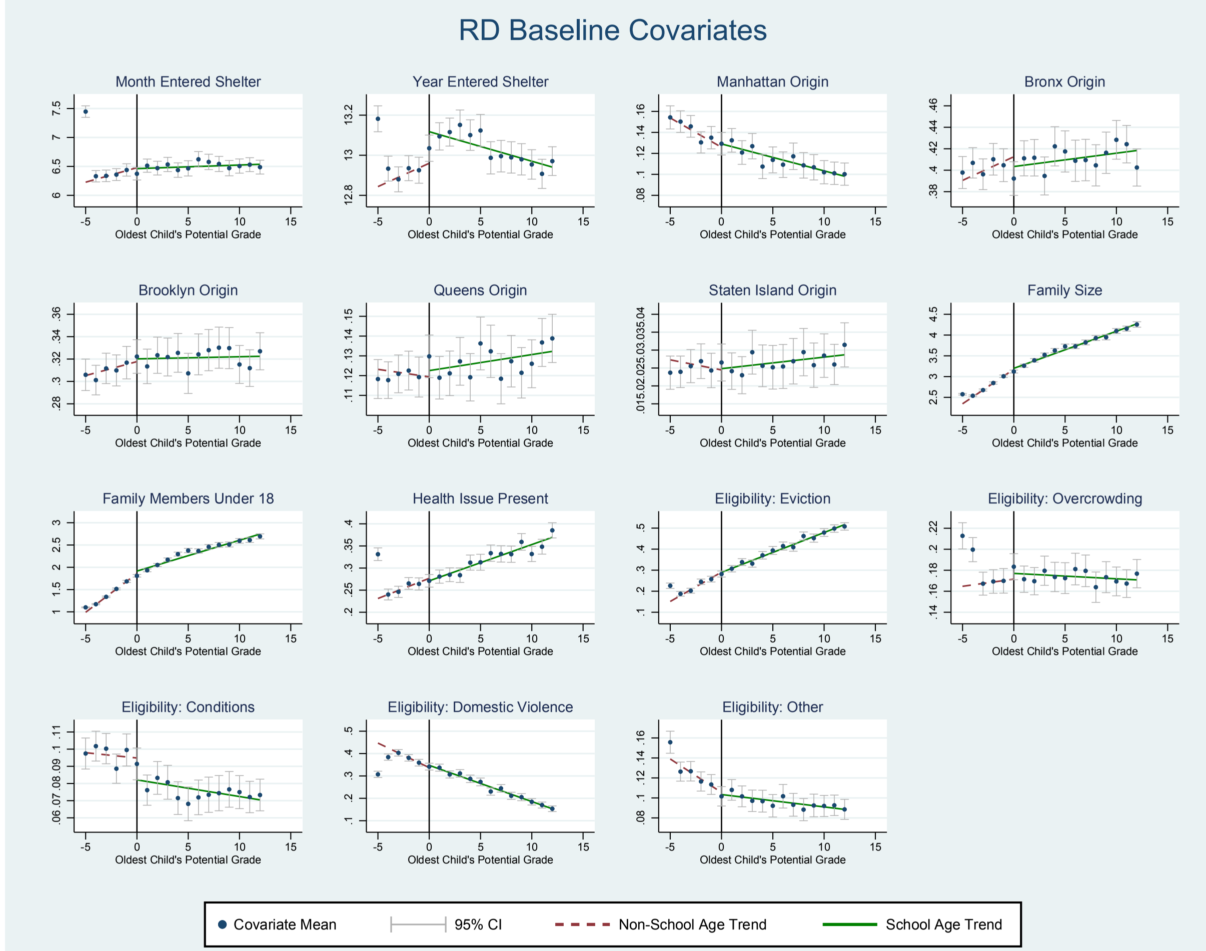
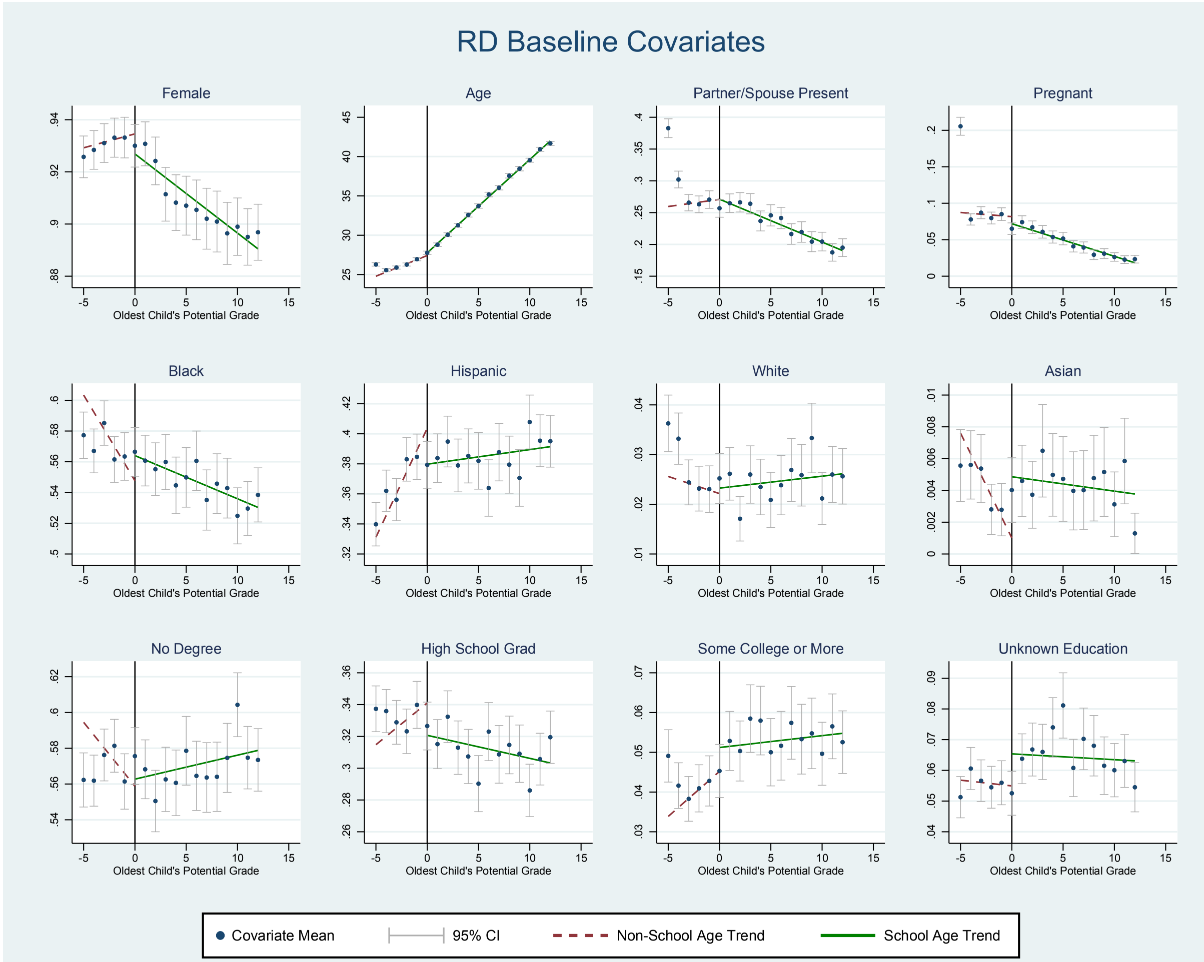


Figure 2.6: Regression Discontinuity Baseline Covariates



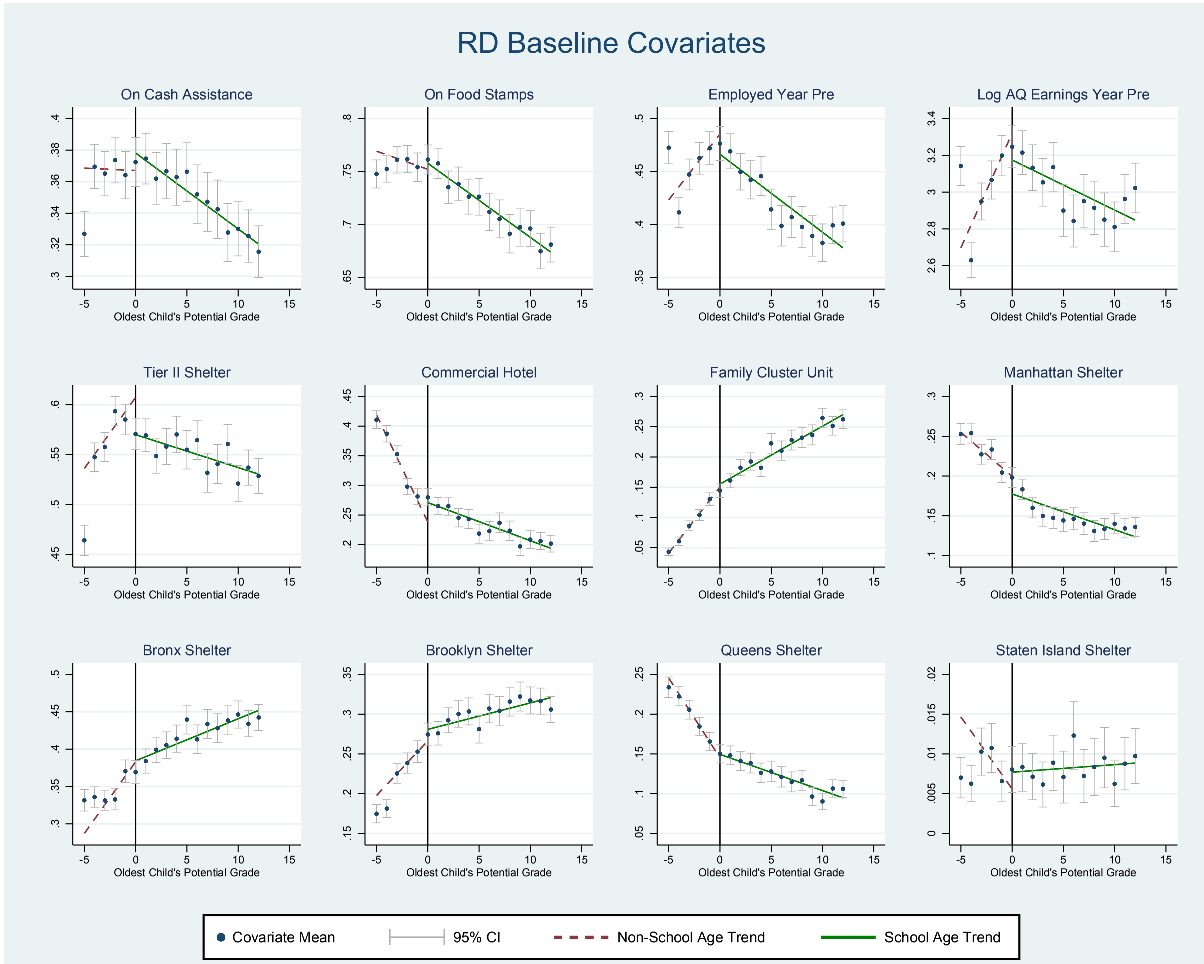
Non-school-age fits from linear regressions of detrended outcome on running variable over [-3,-1] interval.
School-age fits from linear regressions of detrended outcome on running variable over [0,12] interval.
Full RD sample.

Figure 2.7: Regression Discontinuity Baseline Covariates



Non-school-age fits from linear regressions of detrended outcome on running variable over [-3,-1] interval.
School-age fits from linear regressions of detrended outcome on running variable over [0,12] interval.
Full RD sample.

Figure 2.8: Regression Discontinuity Baseline Covariates



Non-school-age fits from linear regressions of detrended outcome on running variable over [-3,-1] interval.
School-age fits from linear regressions of detrended outcome on running variable over [0,12] interval.
Full RD sample.

Appendix B

Supplemental Appendices to "Short Moves and Long Stays: Homeless Family Responses to Exogenous Shelter Assignments in New York City"

B.1 Data Appendix

Unless otherwise noted, data management activities are carried out using `Stata 16`. For certain tasks where `R` has a comparative advantage, I use it instead and make note.

B.1.1 Data Sources

My data consist of administrative records matched across several City agencies. The core data source is the Department of Homeless Services' (DHS) Client Assistance and Rehousing Enterprise System (CARES), which is the City's management information system of record for homeless families. CARES is designed to accommodate all aspects of homeless services provision and program management. At the front-end, CARES consists of a graphical user interface software application that allows both City staff and contracted service providers to enter, update, and view client information in accordance with role-based access privileges. Behind the scenes is an elaborate relational database where records are stored. While the primary purpose of CARES is prosaic—to permit efficient administration of homeless services—the system also includes fairly robust (if sometime convoluted) reporting capabilities to facilitate program evaluation and statistical reporting.

My sample consists of all eligible family shelter applications from January 1, 2010 to December 31, 2016[1]. I focus on these years because this is the period in which shelter capacity constraints have been the most binding, and thus where the case for random neighborhood assignment is the strongest. In addition, the CARES system came online during 2012; prior to that, DHS relied on less robust information technologies.

CARES is a comprehensive system, encompassing virtually all aspects of family homelessness, from application through case management[2]. Data in CARES is collected from two main sources. The first is the Temporary Housing Assistance (THA) application, which all families requesting shelter are required to fill out at intake[3]. The THA consists of information pertinent to the eligibility determination and placement decisions made by DHS staff. In addition to basic identifying information for all family members at the time of application (e.g., name, date of birth, Social Security Number) and their relationships, it also contains demographic attributes (e.g., sex,

[1]Specifically, it consists of all families who both began their application and their shelter stay between 1/1/10 and 12/31/16.

[2]CARES is similarly used to manage single adult homelessness, but as that is not the focus of this study, I do not discuss it here.

[3]While NYC has a right to shelter, families must be deemed eligible, in the sense that they are bona-fide homeless with no other place to go.

race, ethnicity, pregnancy status) as well as the family's address of origin and reason for applying for homeless assistance. As might be expected, CARES also records information relevant to the application process itself, including application type, eligibility determination outcome and official eligibility reason, diversion efforts, and dates of application and adjudication.

The second main CARES data domain relevant to this paper is known as the Lodge History (Lodge), which, as the name suggests, tracks families' experiences in shelter. Unlike the THA, it is not a form, but rather a query culling key stay-related data from multiple tables (and which are collected at various points during a family's time in shelter). It records the facility, building, and unit into which a family is placed, and for what dates they resided there[4]. It is not uncommon for families to change facilities or units during a shelter stay; correspondingly, the system tracks all of the ins and outs. When families leave shelter, the Lodge component of CARES records the date, as well as the type of exit and destination address (if known)[5].

The distinction between the THA and Lodge is somewhat artificial, as CARES is an integrated application used across multiple DHS administrative units (including eligibility and placement staff) and providers. Thus, families' information can continually be updated or augmented; indeed, the source of a particular data field is sometimes categorized by the main data tables upon which a particular query relies, rather than the point at which the data was collected. Another distinguishing feature is who does the data entry: THA information is entered by frontline DHS staff, while Lodge data may be entered by DHS staff or providers.

One example illustrative of the complexities of data collection in CARES is families' health status. Medical and mental health information relevant to shelter place-

[4]Some facilities consist of multiple buildings. In the case of cluster units—apartments scattered across otherwise private residences—these buildings may not even be in the same borough. Thus, facility alone, which is more of a synonym for "provider contract," is not sufficient to identify shelter location.

[5]Families are not required to report their exit to DHS.

ments is collected via several standard assessments which may take place at various points during a family's shelter stay, beginning at intake. Consequently, DHS' health query comprises data from both the THA and the Lodge History.

To summarize, CARES client data may be categorized along several non-mutually exclusive dimensions: transactional source, point of collection, user role, topical content (as organized by relational database tables), or the query that extracts it. For purposes of this analysis, I typically classify CARES client data as coming from the THA or Lodge, depending on whether the data is primarily collected at intake (THA) or during a shelter stay (Lodge). Strictly speaking, this may be an oversimplification, but it is one that useful for organizing data concepts.

Though the focus of CARES, first and foremost, is on clients, DHS families need places to go. Consequently, CARES also functions as an inventory management system, allowing staff to track the capacity and occupancy of all homeless shelter units within DHS' purview. These include, in addition to traditional Tier II shelters (these are apartment buildings officially designated as shelters), "cluster" units scattered among private apartments, contracted hotels, and commercial hotels. While the City owns and operates some shelters directly, the majority are under contract with non-profit service providers. This facility management aspect of CARES is critical to the ability of staff to place clients in suitable situations[6].

Correspondingly, the third CARES-based data source for this paper is DHS' facilities query. It includes daily capacity and occupancy for each facility and building within DHS' portfolio, along with addresses and unique identifiers.

Client data from CARES constitutes the core data for this paper. However, it is hardly the case that all information relevant to assessing homeless services is maintained by DHS alone. Indeed, the vast majority of the City's social services and poverty alleviation programs are the domain of the Department of Social Services

[6]As the facilities management component of CARES is not as well developed as the client management part, DHS also relies on several other information systems to manage facilities.

(DSS). Also known as the Human Resources Administration (HRA), DSS is NYC's officially designated local social service agency[7]. It bears responsibility for administering virtually all of the programs associated with the social safety net, notably: Temporary Assistance for Needy Families (TANF) and its NYS counterpart for single adults, Safety Net Assistance (SNA); the Supplemental Nutrition Assistance Program (SNAP, formerly know as Food Stamps); and Medicaid[8].

Data on public benefit use is maintained in HRA's Welfare Management System (WMS), which is the NYS information system of record for cash assistance (TANF/SNA) and SNAP. Reporting from WMS is conducted through an analytically-oriented front-end application, the Electronic Data Warehouse (EDW). For this study, HRA provided data for all individuals who interacted with CA from 2001–2016 and SNAP from 2004 to 2016, as well as the type of assistance received and the associated dates of receipt[9]. Linking information on patterns public benefit use to family shelter stays is critical for understanding how shelter services impact other economic outcomes.

Of course, the ultimate ambition of most government-administered human service programs, from homeless services to poverty assistance, is employment and earned income. Accordingly, a rigorous evaluation of family homelessness policy must include an accounting of labor market outcomes. To that end, the New York State Department of Labor (DOL) has provided quarterly employment and earnings data for all DHS family shelter clients whose Social Security Numbers match DOL records. This labor data spans the first quarter of 2004 to the first quarter of 2017.

An earlier version of this paper, completed in November 2017, was based on DHS

[7]In this paper, I use "DSS" and "HRA" interchangeably when referring to the agency.

[8]In fact, the relationship between DHS and DSS is complicated and dynamic, largely for reasons having to do with the challenges of family homelessness. DHS was originally part of DSS, until it was spun off as an independent agency in 1993. However, in 2016, Mayor de Blasio again consolidated DHS under the DSS umbrella, managed by a single commissioner, Steve Banks. Nevertheless, it remains conventional to refer to the departments as distinct.

[9]Several demographic variables are present as well, including race and education. These fields can be used as a robustness check on CARES data (or as an IV for measurement error).

data through 2016. Subsequently, in early 2018, DHS data on stays of 2010–2016 family shelter entrants was provided and used to match the 2001–2016 CA and FS records. A second DHS data update, in May 2019, revised length of stay, exits, and returns data for the 2010–2016 DHS families cohort through May 2019. However, no additional match with CA, FS, or DOL records was made. Due to improved data quality, the May 2019 DHS data update also revised pre-2017 shelter exit dates for a small number of spells, with implications for pre-existing public benefit data matches. 79 spells previously marked as incomplete ended prior to 2017, and thus should have CA/FS data, while 7 spells erroneously marked as complete—and thus with CA/FS outcome data—are included in the sample. These observations have no meaningful impact on the results.

B.1.2 Querying

CARES is an ambitious and detailed information system, customized for DHS' unique needs with many features, user levels, and purposes. Although it was designed, in part, with reporting and analysis in mind, its underlying complexity—literally thousands of relational database tables—means that extracting information often requires a bit of programming gymnastics. In addition to user-entered data, CARES automatically generates several fields, including unique identifiers for individuals, families, and cases, as well as the dates on which transactions (e.g., application approval, moves, case closing) take place. Such automation simplifies data entry and facilitates reporting.

The majority of CARES statistical reporting is conducted by means of standard "stock" queries, including the THA and Lodge data discussed above. The underlying SQL code is written and maintained by staff in DHS' Management Information Systems (MIS) and Policy & Planning (PP) units, as well as by CIDI staff. A common extension is joining the results of several queries through unique identifiers. However, in the case of several fields crucial to this study—target schools, shelter building ID's and addresses, race, health status—DHS had to customize existing queries to include additional fields.

To be precise, the DHS data in this paper come from six separate CARES queries:

1. **Standard THA**: described above.

2. **Standard Lodge**: described above.

3. **THA supplemented with target school**: Given DHS' school-based placement policy, caseworkers collect information on youngest child's school. However, this field is sparsely and irregularly populated.

4. **Lodge supplemented with race and shelter building ID**: Standard queries lack building identifiers and the race category variable.

5. **Facilities**: provides daily shelter capacity and occupancy at the facility-building level, along with addresses.

6. **Health**: contains information on family members' medical and mental health (including substance abuse), which may pertain to shelter placement decisions. Health assessments may occur both at intake and during shelter stay.

B.1.3 Structure of the Data

The Core DHS Data

As described above, the foundational data for this paper consist of a joined standard THA-Lodge query encompassing all eligible families with children who applied for shelter and began their stays between 1/1/2010 and 12/31/2016. The raw data are at the individual-bed stay level: that is, there is one record corresponding to each shelter unit assignment for each individual—437,337 observations in all.

Key variables in the foundational data include: unique family and individual identifiers (including system generated ID's as well as name, date of birth, and SSN); application attributes (e.g., type of application, client-provided homelessness reason, officially determined eligibility reason, address of origin, and key dates in the application process); basic personal characteristics (e.g., sex, household relationships, ethnicity, a pregnancy indicator); and shelter stay characteristics (facility, facility type, dates of stay). The majority of these variables are self-reported by the (prospective) clients; exceptions are staff-designated fields, such as official eligibility reason. However, all information is entered into CARES by caseworkers, providing a measure of validation and error-checking. Of note, this data entry process also provides rationale for asserting that, to the extent errors occur in the data, mismeasurement is of the classical variety.

To this foundational data is appended THA-based target school information and Lodge-based building ID and race category. None of these variables are present in the standard queries. Target school gives the name and code (or sometimes the address) of the youngest child's school, which provides the target shelter neighborhood. Unfortunately, this variable is populated irregularly. Race is self-reported based on standard categories (e.g., White, Black, Asian); note that Hispanic/Latino identity is recorded by the separate ethnicity variable. Building ID gives the precise building where a family is placed within a facility. In CARES nomenclature, "facility" is a loose term, referring more to a distinct provider contract than to a particular location. For example, buildings within cluster facilities may be spread widely across neighborhoods—in some cases, even across different boroughs.

Once a building ID for each family is established, records are linked to the facilities query in order to append data on shelter address (as well as such things as facility and building name).

As a final preliminary step, records are matched to the standalone health query.

This provides information on all family members' physical and mental health, including such things a mobility limitations and medical device usage, which in part determine which shelters can suitably accommodate families with special needs.

These queries are linked together based on several identifier fields. Depending on the queries involved, uniquely identifying records may require using several ID fields simultaneously. Together, I refer to the aggregately joined DHS data as the "Core DHS" data.

DSS/HRA Data

On a parallel track, HRA benefits data are processed into a form suitable for linkage to the Core DHS data. Raw HRA data consists of individual-case status level records. There are separate files for each program (CA and SNAP) and each year (2001–2016 for CA and 2004–2016 for SNAP). That is, for each program and each year, a file consists of every case status (applying, active, single issue, sanctioned, closed, denied) each individual had during that year and the corresponding dates. These files also include personal identifiers (name, SSN, DOB, WMS ID, case number) as well as demographic information (e.g., sex, race, education level). Separate years are necessary as the files are very large, containing potentially millions of records.

Variable fields are first cleaned and standardized along the lines described for the DHS data below. Relevant analytical variables, such as length of benefit receipt and benefit indicators, are defined. At the same time, irrelevant variables are dropped, as are individuals too young to be heads of household.

The individual years of data are then appended together into a single file for each program (CA and SNAP) and collapsed to a single summary observation for each unique individual, as indicated by SSN[10]. This process reduces the resulting files—one for CA and one for SNAP—to manageable sizes for purposes of linking to the

[10]Neither WMS ID nor case number uniquely identify records; moreover, SSN provides a common link to DHS data.

DHS Core data. As described below, the actual linkage of HRA and Core DHS data occurs only after the Core DHS data is cleaned and collapsed. This sequencing is practical: the linking process relies on probabilistic matching, which can only be accomplished in reasonable time if the number of records is modest.

DOL Data

DOL data consists of quarterly earnings and industry[11] for each individual in the DHS Core data with a matching Social Security Number. That is, in contrast to the DHS–HRA data match, the DHS–DOL match, discussed below, is entirely deterministic, requiring exact SSN matches. Observations are at the individual-quarter level.

Processing the DOL data consists of several steps. First, nominal dollars are converted to real fourth quarter (Q4) 2016 dollars, using the Consumer Price Index (CPI) for All Urban Consumers. In addition, industry codes are summarized in terms of NAICS sectors[12]. Then (and in reference to DHS family-episodes), data are aggregated over the appropriate analytical time periods—the year prior to shelter entry, the year following shelter entry, and the year post-shelter exit. For each of these periods, I define an indicator for employment, a count of quarters worked, and a sum of earnings. Finally, I calculate average quarterly earnings (always dividing by the minimum of four quarters or the number of quarters maximally observed in the given period, regardless of whether an individual was employed). For analytical purposes I add one to this total and take the natural logarithm, thus arriving at measures of log average quarterly earnings for the three periods of interest, and without excluding individuals with zero earnings.

[11] Industry is described by standard North American Industry Classification System (NAICS) codes.

[12] However, I exclude sector covariates from earnings analysis due to the possible simultaneous determination of industry and wages.

B.1.4 Geocoding and Linking

Preprocessing

Having constructed the DHS portion of the Analytical dataset, two major data management steps remain: linking records across agencies and geocoding. Each is described in its own section below.

To carry out either task with maximal effectiveness, however, first requires cleaning and standardizing the variables implicated. This turns out to be a not inconsiderable challenge.

Geocoding software generally requires addresses to be inputed in standardized format—with, for example, street address, city, and zip codes in stored in separate fields—and largely error free (some software is better than others at discerning near matches). In other words, address data requires some of the highest accuracy of any field to be useful; if it contains errors, the software is unable to code addresses correctly. Ironically, addresses tend to be one of the most error-prone fields in DHS data. Common mistakes include misspelled street names, erroneous zip codes, addresses out of the valid range for a street, and boroughs inconsistent with street names. Particularly problematic are hyphenated addresses and prefixed street names (e.g., East or West). In addition, some entries erroneously merge separate fields (e.g., a street address containing an apartment number).

To address these address issues, I wrote a simple `R` script that corrects the most glaring mistakes. The program takes as its input the list of addresses from my Analytical Stata dataset. It parses addresses into conceptually distinct elements (address number, street, borough, city, state, and zip code). Then, using regular expressions and other string functions, it corrects the most common spelling, punctuation, grammatical, and notational mistakes, resulting in a list of mostly standardized addresses. Finally, using string distance algorithms, it compares street names to an official registry, replacing likely mistakes with their closest valid substitutes. These cleaned and standardized addresses are then inputed to geocoding software, with better success than the raw data.

The second place cleaning and standardization arises is with linking administrative records across agencies. The City does not, in general, have unique cross-agency identifiers for clients who interact with multiple departments. What's more, the standard individual identifier—Social Security Number—is error prone and often missing, either because clients' forget them or never had them. Thus, to achieve the highest possible matching rate between DHS and HRA data—the absence of evidence is not evidence of absence, after all—requires use of probabilistic linkage techniques.

Because probabilistic linkage typically relies on string comparison metrics, the success of the process will only be as good as the quality of the underlying data. Thus, I make simple alterations to improve the data quality of matching fields—first name, last name, date of birth, and SSN. Adjustments include: adding leading zeros to erroneously front-truncated SSN's, ensuring all names are fully uppercase, and arranging dates in standard formats.

Geocoding

Broadly, geocoding is the process of assigning standardized geographical coordinates or categories to addresses, areas, or other spatial positions—in essence, a systematic way of locating places on a map. In the case of administrative records, it entails iterating multiple rounds with specialized software packages.

The first step, as described in the previous section, is to clean and standardize the raw address data queried from CARES. This consists of parsing the data into its topically distinct subcomponents—address number, street name, city (borough), state, and zip—and making several simple cosmetic adjustments, such as removing extraneous punctuation and spaces and enforcing uniform capitalization. This is necessary because geocoding software can be quite literal its interpretation, demanding punctilious formatting and offering scant ability to make approximate matches.

The client address of origin variables from the Stata dataset are then exported to a Microsoft Excel file, which serves as the input to my geocoding software of choice, Geosupport Desktop Edition, version 17.1, which is a highly customized geocoding application for addresses in New York City published by the NYC Department of City Planning (DCP). Usually referred to by its acronym, GBAT, Geosupport Desktop Edition is a publicly available graphical front-end to the comprehensive Geosupport System mainframe application designed and maintained by DCP.

Taking as inputs address number, street name, and borough (or zip), GBAT can return a wide array of geographical classifiers. For purposes of this study, I emphasize several important neighborhood classifications: borough (boro), school district (SD), community district (CD), Census tract (CT), and neighborhood tabulation area (NTA).

I also output spatial X-Y coordinates for each address. GBAT uses the State Plane Coordinate (SPC) system, which approximates the Earth's surface as being flat within relatively confined geographic areas. According SPC, NYC falls in the New York-Long Island zone (NAD 83). With the origin of this zone set to the extreme Southwest, all NYC locations receive positive Cartesian coordinates, with X indicating East and Y indicating North. Units are in feet. Thus, SPC makes it simple to calculate the Cartesian distance between two addresses (NYC Department of City Planning, 2017).

GBAT returns an updated Excel file appended with the geocoded fields, which is straightforward to merge back into the original Stata dataset using unique record identifiers. (Recall there is one record per family-episode.)

Approximately 20 percent of addresses fail to geocode in the first round. For about half of these, this is appropriate: the addresses are outside NYC, as a nontrivial share of the family shelter population arrives from other cities and states (though some of these families may have prior ties to NYC).

The other half of geocoding failures are attributable to frequent errors in the raw DHS data. To remedy such mismeasurement, I import the list of failed addresses into `R` and implement the address cleaning program discussed in the previous section. This code corrects common data entry errors, such as misspellings and inconsistent use of directional prefixes. I then export the results to a second Excel file and repeat the GBAT geocoding process. This improves the success rate somewhat.

Overall, 57,500 of 70,000 client address observations code successfully. Of the remainder, 7,300 are out-of-towners. 5,200 fail to geocode. Future work will entail investigating the reasons for these failures and writing code to improve the success rate. In other words, the iterative data cleaning-geocoding process will repeat several more cycles.

Of course, addresses of origin are only half the story, and I repeat the geocoding process for shelter building addresses. As these addresses are maintained by DHS staff, the success rate is quite high.

Finally, with all geocoding data merged back into the Analytical dataset, I use the geocoded neighborhoods to classify families assigned to shelters in their neighborhoods of origin and those placed in distant neighborhoods. Given the fluid definition of neighborhood, I use the full set of potential categories: borough, SD, CD, CT, NTA, and zip. Spatial coordinates also permit a continuous proximity metric.

Future work may also involve geocoding exit addresses in those cases where these addresses are known.

Record Linkage

In the presence of common individual identifiers, linking records from disparate databases is simple and fast. Unfortunately, DHS family and individual ID's are not the same as those used by HRA in the administration of CA and SNAP, complicating the task of discerning patterns of public benefit use among homeless families.

In principle, Social Security numbers should serve as a cross-agency link, but in practice SSNs are frequently entered erroneously or missing. Thus, it is necessary to rely of probabilistic, or stochastic, linking methods. Also know as "fuzzy matching," there are several probabilistic linkage techniques common in the computer science and statistics literatures, most of which entail the use of string comparison metrics and are based on the pioneering work of Fellegi and Sunter (1969).

Though the mathematics can get complicated, the basic idea is to compare all possible pairs of records in each data set and assess their similarity—for instance, by counting the number of changes (insertions, deletions, and substitutions) to one string necessary to arrive at the other (the Levenshtein distance), or by considering the number of shared character sequences of a given length (q-grams). Patterns of matches among the compared fields are fed into an maximum likelihood type algorithm in order to categorize probable matches and non-matches, with probability thresholds set to distinguish true matches. Though sophisticated, these techniques also review considerable clerical review and judgment calls.

In this study, I primarily rely upon the user-written `reclink2` Stata command, which utilizes a bigram (two-character) string comparator and achieves success rates on the order of 97 percent(Wasi, Flaaen et al., 2015). In some cases, I also rely upon the `R` packages `RecordLinkage` and `stringdist` (Sariyar and Borg, 2010; Borg and Sariyar, 2016; van der Loo, 2014)[13]. I match on four variables: SSN, first name, last name, and date of birth (as a six-digit string with two-digit day, month, and year).

Besides distinguishing between true matches on the one hand and false positives and false negatives on the other, the other major challenge of probabilistic record linkage is computational efficiency. Comparing datasets of size m and n requires $m \times n$ computations, which become unmanageably slow on computers with conventional

[13]The help files and associated journal articles documenting these commands have also been invaluable resources in learning about the techniques, as described above.

memory capabilities, given the millions of records involved.

I employ several strategies to improve the speed of computation. First, I reduce the linking datasets to the minimal useful record sets. In the case of the core Analytical dataset, this means running the match after collapsing the data to one observation per family-episode (so that the match occurs based on household head only). For the HRA data, this entails dropping all observations with a date of birth such that they would not be 16 years of age by the end of the sample period (New York requires individuals to be 16 in order to be a CA or SNAP head of household), as well as collapsing to a unique observation for each SSN.

However, there are still in excess of 2 million CA observations and 3 million SNAP observations that must be matched with the 68,079 DHS family observations. Exact matches—where all four fields perfectly correspond—reduce the workload greatly. About 57,000 DHS observations are perfect matches, removing these from subsequent computation. In addition, as is conventional, I employ a "blocking" strategy on all four linking variables, which means than only pairs with an exact match on at least one of these fields is considered, significantly reducing the number of comparisons. Finally, I match on CA first and then take only the remaining non-matches to the larger SNAP data; this is possible because HRA maintains common identifiers across the programs it administers.

Erring modestly on the side of false positives, I successfully match about 67,600 of the 70,000 DHS families to HRA—in line with what would be expected about homeless family participation of public benefit programs.

Currently, I am able to identify whether a family received CA or SNAP, during which years, and their lifetime lengths of benefit receipt. The next steps in this process are to use the unique identifiers—which obviate the need for future fuzzy matching—to link DHS data to the uncollapsed HRA data sets, in order to distinguish between benefit receipt occurring before, during, and after shelter episodes. This is a data-intensive task, since it entails unique start and end dates for each family (rather than simple year indicators). However, since my DHS and HRA data are now deterministically linked, it should be computationally feasible.

The linking process for the DOL data is simplified by an administrative constraint: because DOL conducts strictly deterministic SSN matches with DHS data, my DOL data sample consists only of successfully matched SSN's present in the DHS Core data.

B.1.5 Defining Analytical Variables

Having pre-processed each data set—DHS, HRA, and DOL—and defined data linkage rules, what remains is to use the raw data to construct variables that are most appropriate for analytical purposes. These variables include both covariates to be used as controls (e.g., earnings and benefit use pre-shelter) as well as outcomes (e.g., earnings and benefit use post-shelter). Creating these variables is not a simple task, either conceptually or logistically.

The complexity arises from the flow nature of the data sample: I do not observe all families for the same length of time. This is true not only of the core DHS data in isolation—obviously families who enter shelter in 2016 have less potential observation time than those entering in 2010—but, in fact, it is doubly true of the matched HRA and DOL data: families who enter shelter earlier in my sample have less potential observation time pre-shelter and more potential observation time post-shelter. As a result, raw comparisons of earnings, employment, or benefit use can be misleading—biased as an artifact of the sampling scheme.

To best put families on an equal footing for purposes of benefit and employment analysis, I take the approach of focusing three one-year windows: the year (or, as necessary, four quarters) prior to shelter entry, the year following shelter entry, and the year following shelter exit. (When quarters are the unit of time, all such periods are defined as excluding the quarter of transition and inclusive of the following four quarters. When days are the time unit, periods begin on the day of transition and extend for the the next 365 days, inclusive.)

Because observations can still be censored within these year intervals, my second normalization is use indicator or rate variables. Specifically, for benefit use and employment, I prioritize binary indicators (e.g., a dummy for employment or CA receipt) or fractional responses, with denominators set to the minimum of a year or the length of observation before censoring (e.g., percent of quarters employed or percent of days active on CA). For earnings, I focus on average real quarterly earnings, where the denominator is the minimum of four quarters or the number of quarters before censoring. In addition, I count all quarters, whether or not employed, so this measure is not conditional upon working.

A second complexity is that some families are observed for more than one episode during the sample period, necessitating separate computation of these analytical variables for each episode, which, for technical reasons, requires considerable care, as well as iterating the variable definition code for each episode instance. For purposes of variable definition, my general approach is to treat each episode as independent. This means that certain components of the raw data can overlap episodes. For example, if a family reenters shelter within six months of exiting, the subsequent six months of earnings will count as post-exit earnings for the first episode and post-entry earnings for the second episode.

DHS Analytical Data: Reshaping and Conceptualizing

Returning to the Core DHS data, the centerpiece of the analysis, the first step in creating the final “Analytical” dataset is to organize and restructure the raw data. The raw individual-bed date file structure is too detailed to be analytically tractable, so the basic idea is to collapse records into a single observation for each family and shelter episode. As described below, this data management process consists of four key activities: reshaping, deduplicating, defining, and recoding.

To do so is not necessarily straightforward, as it requires defining the key concept of shelter "episode." Conceptually, an *episode* is a discrete stay in shelter. However, it is common in the family shelter system that families enter and exit multiple times in close proximity—a few days in and a few days out—as they shuttle between shelter apartments, family, and friends. Brief hiatuses are not true exits. Conventionally, DHS defines the true end of a shelter episode as one in which a family does not return for at least 30 days; thus, any return within 30 days is considered to be part of the same episode.

I adopt the same 30-day standard for defining episodes in this paper. However, this notion does not have an analogue in CARES; case numbers, which are probably the closest proxy, are not defined by gaps in stays but by applications and case composition.

Thus, it is necessary to define an episode "by hand." To do so, I order observations by family ID (which uniquely identify families)[14]. A further complexity in this regard is that, in the raw data, there are potentially multiple observations for each individual in each family and, moreover, family composition can change during the course of a stay as members enter and leave[15]. This creates complex patterns of overlapping and interweaving shelter unit stays for families; recall that each move within the shelter system—it is common for families to move to different units within a building or to different facilities altogether—triggers a new observation in the raw data. What's more, data for certain fields are occasionally missing, which complicates accurate ordering of the data.

To deal with these complications, I take the following approach in defining episodes.

[14]Note that an individual may be part of more than one family, e.g., in the case of child that has her own child and subsequently becomes a head of household.

[15]It is not uncommon, for instance, for older children to come and go during a parents' stay in shelter, spending the interludes with relatives.

First, I drop any observations with irredeemably missing data (e.g., lack all key identifiers), about 10,000 observations in all (a trivial fraction of the data). I then define the start date of an observation as the "bed start" date for that record (in DHS terminology, "bed start" means beginning of stay in a particular unit), or, if this is missing, as the application date. The corresponding observation end date is the "bed end" date for the record, or, if it is missing, the exit date. I then order the observations by date within each unique family ID. Note that in this setup, observations for each individual in the family are not sequential; the continuity of a family-episode is defined by the continued (without $>$ 30-day gaps) presence of *any* family member, not dependent upon particular family members. I then calculate the gap between the beginning of one observation and the end of its predecessor. Any gap greater than 30 days defines a new episode for that family. Episode start date is defined as the minimum (first) observed date for the family, while episode end date is defined as the maximum (most recent) observation date.

Corresponding to the concept of episode are measures of length of stay (LOS), the proximate outcome of utmost importance to City policymakers. While there is not official LOS metric (and specifically none recorded in the data), DHS maintains two standard concepts.

The most straightforward is *system* length of stay, which is simply defined as the difference, in days, between the family's episode end date and start date. It does not exclude any gaps in stay that might occur if a family leaves temporarily and returns within 30 days. A somewhat more refined concept is *shelter* length of stay, which does deduct shelter occupancy gaps from the total. In practice, the both concepts yield similar results, so for simplicity I favor the system LOS measure. Note that many episodes are censored in the sense of stays not completed during the sample period. Such observations are tracked with a censoring indicator and assigned a LOS based on the latest observed bed end date of 1/1/2017.

Having defined a coherent concept of episode, I collapse observations into the desired single observation per family-episode structure. From a data management perspective, this is classified as deduplication: creating unique records at the desired unit of analysis.

Other data management tasks are of the more routine variety, and include the following:

- **Converting variables to formats suitable for analysis**: Many variables are initially stored as strings and must be converted to factors or continuous variables. In addition, dates (also strings) must be converted to analytical date formats.

- **Recoding overly-detailed categorical variables**: Some fields, such as eligibility reason and exit reason, contain a multitude of nuanced codes that can more helpfully be classified in fewer broader categories.

- **Defining derivative variables**: Some variables must be transformed for purposes of analysis. For example, age is more useful than date of birth. Other examples include indicators for year of entry, quarter of entry, incomplete episodes, originating from outside NYC, and having a school age child.

When all is said and done, there is one unique record for each family-shelter episode (some families enter and leave shelter multiple times). The raw data consists of 70,632 family-episodes. 2,553 were dropped due to decisively missing data (e.g., family ID, entry dates, no children present), leaving 68,079 observations in my complete Analytical dataset. However, for two reasons my effective Analytical sample is smaller. 7,099 families originate from outside NYC, leaving 60,980 family-episodes relevant for assessing neighborhood effects (non-NYC families cannot be placed in their home neighborhood). However, 8,008 NYC family-episodes were unable to be geocoded, due to missing or erroneous origin or shelter address. Thus, what I refer to as my “full sample” consists of 52,972 family-episodes, which both originate in NYC and are not missing any defining data.

In addition to a family identifiers, key variables of DHS origin include household demographics (age, sex, race); household composition (household size, number of children, number of adults, ages, and relationship descriptors); address of origin; and homelessness episode attributes (reason found eligible (e.g., eviction, overcrowding, domestic violence), shelter ID, shelter address, shelter type (Tier II, cluster, contracted hotel, commercial hotel), shelter entry date, shelter exit date, exit type (subsidized, unsubsidized, type of subsidy), exit destination type and address).

DSS/HRA and DOL Analytical Data: Reshaping and Conceptualizing

For both the HRA and DOL data, I only retain analytical information only for family heads for computational simplicity. In practice, this is not likely to significantly impact the results, as most families are headed by a single adult, upon who the family depends for both employment and benefits access. Moreover, of necessity, many family covariates, such as race and age, are defined in terms of the household head, so this is consistent with my general approach to defining family attributes.

From a technical standpoint, constructing analytical variables from the HRA and DOL data require four steps. First, using only key individual identifiers (like SSN and name), I create the DHS-HRA and DHS-DOL linkage keys (as described above). Second, I use these keys to respectively merge DHS family-episodes and associate key attributes (like start and end dates) into each of the HRA and DOL datasets. Third, I create the pre/during/post-shelter analytical variables of interest in each dataset. If necessary, I collapse the data so as to maintain a unique observation for each family-episode. Finally, I merge the results back to the DHS Core data, such that my main dataset is neatly appended with the necessary HRA and DOL analytical variables.

In the following section, I outline the basic principles and assumptions used in constructing the key analytical variables. I then describe these variables, organized by source, beginning with those derived from DHS data, followed by HRA and DOL.

B.1.6 Basic Principles for Analytical Variables

From an econometric standpoint, my population of interest is the universe of potential entrants to NYC family shelter. Viewed from this perspective, my (raw) sample consists of all families who applied for and were found eligible for NYC family shelter from 2010 to 2016.

In the ideal world, I would fully observe all families in my sample, with complete, accurate data on all characteristics of interest, including uncensored lengths of stay and post-shelter outcomes.

In practice, of course, this is impossible. The recency of the data combined with flow sampling guarantees right-censoring; moreover, the censoring point will be variable, with families who entered shelter more recently more likely to be censored.

While I could focus on earlier entrants, there are several strong reasons for not doing so. DHS' information systems underwent a major overhaul in 2011–2012, and the more recent data is higher quality. What's more, shelter capacity has gotten tighter over time, which makes the natural experiment assumption more viable in recent years. Finally, recency means relevance, and all else equal it is of greatest policy interest to characterize the situation today.

But the data is imperfect in other ways, too. While administrative data carries with it the legitimacy of official records, errors remain. In particular, key variables, such as client addresses, can be missing or mistaken. Identifiers can be miscoded or absent as well, and match rates are not 100 percent.

Dealing with these inevitable imperfections means making assumptions. Most important are the following four.

First, I assume censoring is noninformative. That is, conditional on what I can observe, length of stay is independent of censoring time. This is plausible since censoring is an artifact of my flow sampling scheme. Of course, for any given shelter entry date, families that stay longer are more likely to be censored; for purposes of estimating the causal effect of local placement, independent censoring means I must be able to assume uncensored observations are representative of censored ones. In other words, there is no unobservable that is systematically related to both treatment status and censoring.

In some cases, I also make the related assumption that, "selected" observations—families for whom post-shelter outcomes are fully observed because their shelter stays ended early enough relative to the censoring date in my sample—are are representative of those for whom outcomes are unavailable. But I also pursue estimations strategies that allow me to weaken this assumption.

Second, I assume missing data is noninformative. Since missing data can arise in my sample either because a field is missing or because of a non-match, this assumption actually nested two subparts. On one hand, I assume that when fields are missing or miscoded, such errors happen at random—or at least for reasons unrelated to treatment status. On the other, I assume that a non-linkage between DHS and HRA/DOL data consists a true non-match: these families are truly not receiving benefits or not working. Or, at the least, if a false negative occurs (due to, for instance, erroneous SSN), it is at random conditional on observables and not systematically related to treatment status. This assumption is strengthened by the fact that the data is entered by case workers, who both serve as a quality control and a potential source of errors; in either case, the point is that the flawed data is not systematically attributable to family unobservables.

Third, and along related lines, to avoid incidentally truncating the analytical sample, where defensible I code potentially missing data as zero for binary indicators and continuous variables, and as an "unknown" category for categorical variables.

This arises in two types of cases. In the first type of case, as with the indicator for health issues, missing values are interpreted as indicative of true absences. Health is an important criterion in shelter placement decisions, and thus families not receiving such a screening are assumed not to have significant limitations. Similarly, a non-link to CA data is interpreted as truly not being on CA. While these assumptions are surely violated in some cases, it is reasonable that they hold on average—and average marginal effects is typically what I am interested in measuring.

The second type of case arises when I introduce covariates to control for potentially confounding influences—but not with the goal of interpreting these covariate coefficients causally. Prominent cases are race and education. Some families do not report their race or have missing education data. I wish to control for race and education when estimating treatment effects, but I do not want to exclude the (small) subsets of families from whom such information is unavailable. Group such families into an "unknown" category is a compromise. While this complicates interpretation of race and education coefficients due to the potential heterogeneity within these groups, these are not the coefficients I care about. What's more, if such data is missing at random, then these categories approximate a group with average characteristics (which is somewhat interpretable). At the other extreme, if data is not unknown at random, unknowingness can itself be informative. As a matter of practice, my results do not much change whether I omit missing data or code it as unknown.

My fourth and final data assumption is to treat family-episodes as independent events, with the exception of clustering standard errors at the family group level. While the data are clearly not completely independent and identically distributed (iid), as an approximation it is not so bad, and it simplifies the analysis. For one thing, over two-thirds of families in the data are present for only one episode. For another, prior research (O'Flaherty, 2010) has demonstrated family homelessness is largely a matter of bad luck—and so the event of becoming homeless, even among those with a history of homelessness, is driven in part by factors beyond a family's control. This, combined with adjusting standard errors appropriately, accounts for arbitrary within family-group correlation of unobservables.

I do, however, explore the robustness of this assumption using several strategies. First, I re-estimate important results keeping only the first episode for each family-group, which leaves the results unchanged. (On the other hand, doing this is undesirable as a control for prior shelter experience, as some families may have had shelter episodes before my sample period began.) Second, at the other extreme, I estimate a family fixed effects specification (which includes families with two or more episodes), and also find my main results to be unchanged.

Having made the necessary assumptions about the data generating process, I adhere to two general rules when defining analytical variables. Note that I use the term "analytical variable" to distinguish variables I create for purposes of analysis from "raw" variables present in the original administrative data. Unless otherwise noted, "variable" used without a qualifier refers to analytical variables, since almost all fields requiring some degree of editing to be suitable for econometric analysis.

The first rule is to define variables at the time of shelter entry. This is sensible because, at least for the DHS data, this is the point at which the data is actually collected. Further, it puts all families on equal footing in terms of their shelter experiences. Finally, for factors where endogeneity might be a concern, it is the point at which conditions are most plausibly exogenous. (For example, initial shelter placement is likely to be more exogenous than subsequent moves to other facilities.) Implicit in this setup is the assumption that variables are time-invariant. As a first approximation, this is probably sufficient. Although family circumstances change (e.g., the birth of a child), most shelter stays are less than two years long, a relatively brief window for evolution. As with most rules, there are a few exceptions to this edict, which I discuss below.

The second rule consists of a two-level hierarchy for assigning characteristics to families. For "compilable" characteristics which are shared by all family members, like shelter assignment or eligibility reason, I do the obvious thing and assign that value upon shelter entry to the family. For compilable characteristics which can be sensibly aggregated across family members (e.g., family size or number of children), I violate the "at-entry" rule and assign the family its maximum (or total, as the case may be) for the episode. For example, family size is defined as the total unique number of family members present during a shelter episode, whether or not initially present. It is relatively common for both children and adults to come and go during the course of a shelter stay (spending interims with relatives or friends). Thus, fully accounting for all family members, rather than just those present on day one, seems more sensible. Econometric considerations guide these choices. For example, *maximum* household size likely best reflects a family's true resource constraints and opportunities, while *initial* shelter assignment is more plausibly exogenous than subsequent moves, which a family may have a stronger role in directing

The second level of family characteristics consists of what I refer to as "uncompilable" characteristics. These are attributes that have no simple aggregate (at least insofar as econometric meaningfulness is concerned), such as age, sex, and race. Rather than try to create summary measures of questionable import (e.g., average age), I instead define these characteristics in terms of the (initial) head of family, on the basis the family head exerts the greater influence on outcomes—especially given that the typical homeless family is consists of a single mother with young children.

With these guiding principles in mind, I now turn to definitions of key concepts and variables. I highlight only the most important variables used in the analysis. For a complete listing of variables and descriptive statistics, refer to the tables at the end of the document. The following sections categorize variables based on their role in the analysis: outcomes, treatments, or explanatory covariates. In the presentation, I emphasize key assumptions, missing data issues, and resolving potential ambiguities.

Covariates

Most of my explanatory variables consist of family characteristics. Female is a dummy that is equal to one for female head of family and zero otherwise. Age is a continuous measure of the duration between the head's date of birth and shelter entry date. Race consists of six mutually exclusive categories: White, Black, Hispanic, Asian, Other, and Unknown (if race is refused or missing). Partner present is a dummy equal to one if the head's significant other is present in shelter, whether or not such a partner is a married spouse. Family size is a count of unique individuals present at any time during a shelter stay. Children (under 21 year of age) and dependents (which may include adults) are similarly defined. Pregnancy is a dummy equal to one if the family indicates a pregnant member at shelter entry, and zero otherwise. School age is a dummy equal to one if there is a family member present between the ages of five and 21 (inclusive) prior to 2014, and between four and 21 from 2014 on (the year universal pre-k (UPK) began in the City). Health issue is a dummy based on screenings performed by DHS and providers both at intake and during shelter stays. It equals one if any family member has a medical, mental health, or substance abuse issue (each consisting of multiple subcategories). Education consists of four mutually exclusive categories: no degree (less than high school), high school graduate, some college or more, and unknown. On Cash Assistance and On Food Stamps are dummies equal to one if a family has an active benefit case in the respective program at the time of shelter entry. Log average quarterly earnings in the year prior to shelter entry is exactly what it sounds like; it factors in all quarters, whether or not a family is working (and I add one to each family's earnings before taking the log, to avoid omitting these families).

The next important category of controls are shelter covariates: variables related to a family's shelter episode. These include categorical variables for primary (official) shelter eligibility reason (8 categories: eviction, overcrowding, conditions, domestic violence, child welfare, existing case, discharge, and other) and facility type (4 categories: Tier II shelter, commercial hotel, cluster unit, or other). I also include a dummy for whether a family receives diversion services designed to prevent homelessness.

All main regression specifications also include fixed effects (dummies) for year of shelter entry, quarter of shelter entry, borough of origin, and shelter borough. Some specifications also include borough-year fixed effects, which are interaction dummies for year and origin borough and year and shelter borough. To control for unobservable facility and provider quality, some specifications additionally feature facility fixed effects (264 dummies). In all cases with dummies, categorical variables, and fixed effects, a base category is dropped in estimation to avoid multicollinearity in the presence of a constant term.

Treatments

I use several definitions of treatment. Key to treatment definitions are the address data maintained by DHS. To be part of the analytical sample, families must have valid, non-missing, geocodable addresses, both of origin and of shelter. Origin address is defined as the family's "last known address" reported to DHS. Note that a small share of families (less than 4%) report other shelters as their prior address. In light of this, and given that unstably housed family may move frequently, it is best to interpret origin addresses as a place where families have some preexisting community ties.

In my main analysis, treatment is defined as a family being placed in its borough of origin. New York City consists of five boroughs, or counties, Manhattan, the Bronx, Brooklyn, Queens, and Staten Island, ranging in size from about half a million persons in Staten Island to 2.5 million in Brooklyn. Clearly, referring to geographies of such breadth does not quite comport with the conventional definition of a neighborhood. Nevertheless, as geographically contiguous entities with legally designated boundaries, distinct identities, and palpable intra-borough affinities, NYC's five counties do embody many of the characteristics associated with small communities. Boroughs are also appealing as a neighborhood definition from the standpoint of treatment balance: about half of homeless families in my sample are placed in shelters in their home boroughs and half in other boroughs.

Alternatively, I also define neighborhoods in terms of the City's 32 school districts, which are administrative boundaries for the public school system. These are the next largest geographies for which data is readily available; about 9% of my sample is placed in their school districts of origin. Smaller units of geography, such as Community Districts or Census Tracts, do not have sufficient local placements to permit precise analysis.

Finally, I consider a continuous measure of treatment that measures the distance, in miles, between a family's last known address and its shelter address. This is based on Cartesian geospatial coordinates produced by GBAT. It is straightforward to calculate the Euclidean straight line distance between pairs of addresses and convert the units to miles.

Outcomes

I consider a range of outcomes. The most salient one, and the one I feature most prominently, is length of stay (LOS). This is a measure, in days, of the elapsed time between a family's entry into shelter and its exit. In particular, I prioritize a "system" LOS concept, which counts gaps in stay towards the total, so long as these gaps are 30 days or fewer; it is not uncommon for families to leave shelter for a few days, then return. Out-of-shelter gaps longer than 30 days are considered true exits; subsequent returns are consider new episodes. Incidentally, this is how another outcome I consider, returns to shelter within a year of exit, is computed. Subsidized exits from shelter are those in which the family receives any form of rental assistance. This encompasses a variety of programs, which typically offer time-limited benefits that partially offset housing costs so long as the family meets eligibility criteria. An alternative duration measure, “shelter” LOS, excludes the interludes from the count. In practice, both measures produce similar results, so I use the shelter concept, because it is simpler.

I also consider two other primary categories of outcomes: public benefit use and labor market results. My public benefit use data comes from HRA and consists of indicators and durations of families’ receipt of Cash Assistance and Food Stamps. I focus on two period: the year post-shelter entry and the year post-shelter exit. While I have durations of active receipt, for simplicity I prioritize dummies indicating active program status at any time during these periods.

My labor market data derives from DOL. Again focusing the year post-entry and year post-exit, I construct indicators for positive earnings during any quarter in those years as my measure of employment. Correspondingly, my measure of earnings is log average quarterly earnings. Average quarterly earnings themselves are in real 2016 dollars, are inclusive of all quarters, whether working or not, and have one dollar added to them for each family, so as not to incidentally drop observations when taking logs.

B.2 Policy Background: Family Homelessness in NYC

Neither homelessness nor poverty are foreign to municipalities anywhere in the United States, but nowhere is the intersection of these issues thrown into starker resolution than it is in New York City. Complicating understanding of homelessness—and perhaps, in part, explaining its absence from economists' agenda—is a fundamental misconception about *who* the homeless really are. While disheveled shopping carts, cardboard tatters, and infelicitous hygiene pervade the popular consciousness, it is actually the case that some 200,000 of the 550,000 Americans who are homeless each day are *families with children* (National Alliance to End Homelessness, 2016; Khadduri and Culhane, 2016).

Unlike their single adult counterparts, this misbranded cohort— "unhoused" African-American and Hispanic mothers and young children is more accurate—suffers not, primarily, from substance abuse and mental illness, but from poverty. Aside from bad luck—often in form of unexpected income loss, health crisis, or domestic strife—these families are otherwise mostly indistinguishable from the marginally housed poor at large, not in the least in that the "shelters" in which they are placed frequently resemble the momentarily unaffordable apartments from whence they came (O'Flaherty, 2010; Culhane et al., 2007; Shinn et al., 1998; Curtis et al., 2013).

For them, homelessness is a temporary condition, not an immutable characteristic—a particularly acute form of poverty manifested in the deprivation of a fundamental element of the consumption bundle (Mullainathan and Shafir, 2013; Desmond, 2016). Getting these families back on their feet fast—or preventing their displacement in the first place—is thus an important policy goal.

The task in an exceedingly difficult one. Since 1994, New York's homeless census has nearly tripled, from 24,000 to 60,000 in 2017. More than two-thirds of these are people in families; fully 23,000 are children (NYC Department of Homeless Services, 2019*b*). Indeed, NYC accounts for about a fifth of all homeless families in the U.S (NYC Department of Homeless Services, 2019*e*; National Alliance to End Homelessness, 2016).

Family homelessness is particularly pronounced in New York City for two reasons. First, unique among municipalities in the U.S., NYC has a legal right to shelter, the consequence of a series of consent decrees in the 1980s (NYC Independent Budget Office, 2014). The City is legally obligated to provide emergency accommodations to any family able to demonstrate it has no suitable alternative.

This legal mandate has evolved over time as settlements worked their ways through the courts; the right originated from a class action, *McCain v. Koch*, brought by the Legal Aid Society in 1983, in which New York State Court held the City and State were required to provide homeless families with emergency housing under the State Constitution and Social Services Law (NYC Independent Budget Office, 2014; University of Michigan Law School, 2017; Kaufman and Chen, 2008) [16].

Derivative cases during the ensuing decades established standards for temporary shelter, as homeless services where governed by a mix of executive policymaking and judicial edict. A formal settlement was not reached until 2008, in the form of *Boston v. City of New York*, whereupon the Bloomberg administration, Legal Aid, and the courts came to agreement on appropriate eligibility determination and shelter management standards (NYC Independent Budget Office, 2014; University of Michigan Law School, 2017; Kaufman and Chen, 2008). These mandates mean that NYC faces a steady inflow of homeless families in ways that other cities do not; indeed, a tenth of family shelter entrants report most recent prior addresses that are outside of the City.

Further complicating matters is NYC's notoriously competitive real estate market. New York is a city of renters, with over two-thirds of households renting their residences, nearly double the national average. In the decade ending in 2015, median rent in NYC grew three times the pace of median incomes (18.3% versus 6.6%). Vacancy rates are consistently below 4% (NYU Furman Center, 2016). According the City, demand for affordable apartments exceeds supply by a factor of two; approximately half of renters in the City as rent-burdened, defined as allocating more

[16] A 1981 predecessor case, also brought by Legal Aid, *Callahan v. Cary*, introduced the right to shelter for single adults. (NYC Independent Budget Office, 2014)

than 30% of household income to rent (NYC Mayor's Office, 2017). The situation is especially severe for the lowest income families most at-risk for homelessness. Nine in ten households with income below 30% of the area median spent upwards of 30% of their income on rent (NYU Furman Center, 2016).

Expensive housing, paired with poverty's relentless vicissitudes and a legal escape valve, make NYC's steady rise in homelessness none too surprising. The City has had to expand shelter apace. In 2016, shelter vacancy rates were easily below 1%, even as commercial hotels were brought into the mix to fill gaps (NYC Mayor's Office, 2017). Yet adding capacity is a Sisyphean struggle of its own, with proposals for new shelters frequently greeted by virulent community opposition (Stewart, 2017). Homeless service provision is thus forced to strike a delicate compromise between policy ideals and political realities, an important constraint on optimal implementation.

Responsibility for managing shelters and supports for homeless families and individuals falls primarily to the Department of Homeless Services (DHS), a Mayoral agency under the purview of the larger City's Department of Social Services (DSS), which is the City's officially designated local social service agency. Families apply for shelter at at a central intake center in the Bronx, known as PATH (Prevention Assistance and Temporary Housing). There, they are screened by HRA caseworkers for prevention services, including eligibility for temporary rental assistance and anti-eviction legal services, as well as for domestic violence. If alternative housing remedies are unavailable, families apply for shelter apartments, which requires, among other things proper, identification and detailed housing histories. Families are given temporary (generally about 10-day) accommodations while DHS investigative staff assesses eligibility. Families deemed eligible are then given formal shelter assignments by dedicated placement staff, who consider such criteria as family size, health issues, safety, and proximity to children's schools. Often the preliminary and formal shelter assignments are the same[17]. It is this group of families (those deemed eligible) and this placement step (initial formal shelter assignment) that constitute my sample and treatment.

NYC's family shelter system is vast and complex. As of November 2016, the City's shelter portfolio consisted of 169 traditional Tier II shelters (housing 8,617 families and 26,225 individuals), 276 cluster apartments scattered in otherwise private buildings (3,045 familes ; 11,067 individuals), and 68 commercial hotels (2,057 families; 5,798 individuals) (NYC Mayor's Office, 2017).

It is also expensive. In 2017, the average cost of sheltering one family for one night (inclusive of rent and services) was $171. Overall, DHS' budget, inclusive of management operations, is $1.8 billion—and this does not include welfare benefits administered by other agencies (NYC Mayor's Office of Operations, 2017).

Also of note is how services are carried out. While DHS does operate some shelters directly, most homeless services provision is carried out through contracts with community-based non-profit organizations who operate shelters. A case in point: 82% of DHS' budget consists of such contracts. This service arrangement is not unique to homeless services; most social service programs in the City are administered this way (NYC Mayor's Office of Operations, 2017).

Given homelessness' stubborn rise, my sample period, 2010–2016 has been a time of flux for homeless policy in New York City. The sample begins in the aftermath of the Great Recession and concludes at a time when the economy had regained nearly full strength. Michael Bloomberg's mayoralty spanned the first four years, while Bill de Blasio's tenure began in 2014. Developments at the State and Federal levels—both critical funding sources—has also played a leading role.

Throughout this period, a pillar of the City's homelessness strategy has been community continuity. To the extent capacity allows, the City endeavors to place

[17] Details are based on NYC Department of Homeless Services (2019*c*); NYC Independent Budget Office (2014) as well as author's conversations with City officials.

families in their neighborhoods of origin. Predicated on the goal of keeping children in their home schools', the policy reflects a more general premise—that families are better positioned to expeditiously return to permanent housing when they remain connected to their support networks, including relatives, friends, and places of work and worship (NYC Mayor's Office, 2017). Since at least 1997, the city has monitored the share of families placed in shelters according to their youngest child's school as a DHS performance indicator. By this measure, 84 percent of families where successfully placed in their home neighborhoods as of 2010. However, capacity constraints have become increasingly binding as the shelter population has grown. By 2017, this share of families placed in proximity to their children's schools had dropped to 50 percent (NYC Mayor's Office of Operations, 2002, 2012, 2017).

While a full accounting homeless policy developments is beyond the scope of this paper, a brief discussion of its contours provide context. Core elements of the City's strategy to reduce homelessness include prevention, affordable housing, and rental assistance[18].

Homebase, the City's signature homeless prevention program, offers families at risk for homelessness a panoply of supports, ranging from case management and counseling to benefits assistance and referrals. Instituted in 2004, as of 2016 it serves 25,000 families a year. Academic research finds it to be effective in forestalling shelter entries. Further, as of 2017, the City spends upward of $62 million a year on anti-eviction legal services, which helped to avoid about 20,000 evictions per year. Similarly, emergency rental assistance, typically for families in arrears, stop temporary difficulties from ballooning. From 2014–2016, the City allocated $551 million to assisting 161,000 such households. In terms of affordable housing, the de Blasio administration has pledged to create of preserve 200,000 units, of which 62,000 were financed as of 2016.

[18]The following discussion of prevention, affordable housing, and rental assistance is primarily based NYC Mayor's Office (2017) and discussions with City officials. Additional details are provided by: NYC Department of Homeless Services (2019*d*); NYC Independent Budget Office (2011, 2014)

Strategies include zoning regulations, tax credits, and capital funding.

For families in shelter, rental assistance is frequently a catalyst for returns to permanent housing. Advantage, the most prominent Bloomberg-era program, provided some 25,000 formerly homeless families with two years of subsidized housing. At its peak it cost $207 million, but ended in controversial fashion in 2011 when the State withdrew funding. In its place has come Living in Communities (LINC), launched by the de Blasio administration in 2014. LINC, a collection of six programs targeting families meeting various criteria—involving such things as employment, age, or domestic violence status—offers time-limited (usually 2–5 years) rental assistance to families meeting income standards (usually below 200% of the federal poverty level) and minimum shelter stays (usually 90 days). Along related lines, CityFEPS provides families who have been evicted or are at-risk for losing their homeless with an "eviction prevention supplement." Both programs require families to contribute 30% of their income towards rent; the subsidy covers the remainder, up to a maximum of $1,515 for a family of three. From 2014 to 2016, the programs combined to serve more than 26,000 people. Subsequent to my study period, LINC and CityFEPS have been replaced by CityFHEPS (NYC Human Resources Administration, 2019*a*). Traditional federal programs, including Section 8 Housing Choice Vouchers and public housing also play a role, though both have been limited by funding constraints and long waiting lists in recent years. There are also some smaller programs.

Of course, homeless services is but one—albeit highly visible—component of NYC's safety net for low-income families. The City's Human Resources Administration (HRA)—which along with DHS comprises the the Department of Social Services (DSS)—oversees the nation's largest apparatus for administering poverty alleviation programs[19]. Notable in HRA's portfolio are the "big three" social benefit programs:

[19] In fact, the relationship between DHS and HRA is complicated and dynamic, largely for reasons having to do with the challenges of family homelessness. DHS was originally part of HRA, until it was spun off as an independent agency in 1993. However, in 2016, Mayor de Blasio again consolidated DHS and HRA under the DSS umbrella, managed by a single commissioner, Steve Banks.

Cash Assistance (CA), the Supplemental Nutrition Assistance Program (SNAP, formerly know as Food Stamps), and Medicaid. Because Cash Assistance and Food Stamps figure prominently in the analysis—and also because they help to characterize the poverty that homeless families face—a bit of background is helpful.

Cash Assistance (CA) consists of Temporary Assistance for Needy Families (TANF; which, in New York, is referred to as FA, or Family Assistance) and its State counterpart for single adults and time-limited families, Safety Net Assistance (SNA). Sometimes described as "public assistance" or "welfare," CA provides unrestricted monetary transfers to poor individuals and families. As such, CA can be thought of as the present-day version of the classic poverty alleviation program. Since welfare reform of the 1990s, work requirements have been the centerpiece of CA. In order to maintain eligibility, able recipients must be engaged in 30 hours of employment activities per week, which can include such things as training, education, and job search. (In practice, exemptions are common and sanctions may be unevenly enforced.) Eligibility for CA is limited to the very poorest. In New York, maximum monthly income at initial eligibility is $879 per month for a family of three. Benefits are similarly tight, topping out at $789 a month for a three-person family. Together, these strict requirements, and well as the need for periodic recertification, means benefits can frequently lapse as families are sanctioned. 358,000 New York City residents were actively receiving CA as of August 2017 (Cohen and Giannarelli, 2016; New York State Office of Temporary and Disability Assistance, 2016, 2015, 2017; NYC Human Resources Administration, 2019*b*).

Food Stamps (FS), officially known as SNAP, provides low-income families with categorical dollars each month that must be spent on food. Its eligibility standards are less strict than CA; correspondingly, its caseloads are much larger. Income is the primary criterion; as of 2017, a family of three with earned income could quality

Nevertheless, it remains conventional to refer to the departments as distinct. See NYC Department of Homeless Services (2019*a*) for more detail.

so long as household income was $30,636 or less ($2,500/month). (For such families without earnings, the eligibility standard was $26,556.) While some able-bodied adults without dependents may be required to work, such requirements are typically mild and unevenly enforced. Benefits, like eligibility, is based on a formula determined by family size. In 2017, a family of three receives $504 monthly. 1.7 million NYC residents received SNAP as of August 2017 (New York State Office of Temporary and Disability Assistance, 2019; NYC Human Resources Administration, 2019*b*).

B.3 Supplementary Analysis

B.3.1 Subsidies and Length of Stay

My main empirical result is that in-borough families remain in shelter significantly longer than those placed out-of-borough. They are also more likely to exit shelter with rental assistance. I interpret these facts through the lens of my search effort model. Families prefer local placements so they stay longer and either (a) need increased incentive to leave, or (b) are willing to tolerate longer stays to access subsidies[20].

One concern with this interpretation is endogenous subsidy allocation: City subsidy policy could be driving length of stay. If, for example, the City prioritized out-of-borough families for rental assistance, longer stays for in-borough families would be an artifact of subsidy queuing. In this section, I provide evidence that this is not the case.

The first observation is that, per my OLS results with subsidy as the dependent variable, in-borough families are about 5 percent more likely to exit with a subsidy. So, a stylized fact is that the City is more apt to allocate subsidies to in-borough families.

[20] I define subsidies broadly, as including Advantage, LINC, NYCHA public housing, Section 8, FEPS, rental assistance one-shots, and similar programs. The availability of these subsidies vary widely over time, given frequent policy changes.

Question two is the effect. Naively, the effect of subsidies on LOS is ambiguous. If subsides hasten exits, LOS is reduced; if families remain in shelter longer waiting for subsidies, LOS would increase. In the former case (subsides shorten stays), any subsidy-based endogeneity biases the LOS effect downward, since in-borough families are receiving more of them. In the latter case (subsides lengthen stays), the endogeneity concern would necessarily be that the City is reserving more subsidies for in-borough families, which may be the case, given in-borough families' greater likelihood of subsidy receipt. But this would seem unlikely, since out-of-borough families are theoretically facing a harder time in shelter.

A third—perhaps most realistic—possibility is that the City simply forces in-borough families to wait longer for subsidies than out-of-borough ones. But, under this hypothesis, it would seem odd that in-borough families are more likely to receive subsidies. If out-of-borough families get priority for subsides—and theoretically have greater incentive to use them—why are they less likely to exit shelter with subsidies?

Tables B.6 and B.7 provide evidence assessing these possibilities. The former is for OLS; the latter for IV. They follow the same setup. There are 6 columns. The first four consider the full 2010–2016 period; the last two are limited to the April 2011 to December 2013 period, when subsidies for homeless families were quite scarce (and thus serve as a test of length of stay in a "no subsidy" environment). Col 1 repeats my main analysis. Col 2 is limited to unsubsidized exits. Col 3 is limited to subsidized exits. Col 4 includes an interaction between subsidized exits and treatment (as well as the main effect). Col 5 is limited to families entering shelter between 4/2011 and 12/2013. Col 6 is limited to families both entering and exiting within that period.

In brief, the findings are that the effect of in-borough placement on length of stay is, if anything, strengthened when accounting for subsidies. The Col 5 results for both OLS and IV are larger than their Col 1 counterparts. The Col 6 result is smaller given the exclusion long stayers, but the LOS effect is still significant (in any case, selecting a sample in this fashion is problematic). Further, Cols 2–4 show that basically all of the effect of in-borough placement is on unsubsidized families. To be precise, unsubsidized in-borough families stay longer than unsubsidized out-of-borough ones, but subsidized in-borough families stay about the same as, or shorter than, subsidized out-of-borough ones. Put slightly differently, subsidy increases LOS, but less for in-borough families.

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B.5 Supplementary Tables

Table B.1: Summary of Key Variables by Shelter Entry Year

	Year of Shelter Entry							
	2010	2011	2012	2013	2014	2015	2016	Total
A. Shelter Entry Characteristics								
Families Entering	9,911	7,475	7,937	7,642	8,752	8,161	9,375	59,253
Individuals Entering	31,789	25,219	27,873	26,619	29,610	27,264	30,370	198,744
Borough Placement	0.66	0.59	0.51	0.51	0.44	0.46	0.38	0.51
Placement Distance (miles)	4.68	5.21	5.88	5.80	6.43	6.37	6.91	5.89
Ineligibility Rate	0.25	0.23	0.24	0.21	0.17	0.28	0.26	0.23
Aversion Ratio	1.53	1.17	1.09	1.05	0.75	1.58	1.32	1.22
Occupancy Rate	0.89	0.90	0.95	0.96	0.96	0.97	0.96	0.94
B. Stays and Returns								
Length of Stay	365.1	441.2	451.9	436.2	436.3	438.1	417.3	424.3
Subsidized Exit	0.34	0.14	0.26	0.39	0.50	0.56	0.53	0.39
Returned to Shelter	0.12	0.15	0.17	0.16	0.13	0.15	0.20	0.15
C. Year Post-Shelter Entry								
Cash Assistance	0.77	0.80	0.81	0.79	0.79	0.81	0.72	0.78
Food Stamps	0.91	0.90	0.91	0.91	0.91	0.89	0.85	0.90
Employed	0.47	0.44	0.44	0.46	0.52	0.53	0.48	0.48
Avg. Quarterly Earnings	1094.6	1015.1	958.1	1045.2	1232.8	1416.2	1500.6	1188.9
D. Year Post-Shelter Exit								
Cash Assistance	0.72	0.72	0.74	0.74	0.77	0.77	0.68	0.74
Food Stamps	0.89	0.88	0.89	0.89	0.89	0.88	0.85	0.88
Employed	0.44	0.43	0.44	0.47	0.49	0.49	0.40	0.45
Avg. Quarterly Earnings	1219.9	1169.8	1175.5	1322.3	1476.9	1550.0	1342.3	1306.2
E. Censoring								
Family Spell	0.00	0.00	0.00	0.01	0.02	0.04	0.07	0.02
Full Year Post-Spell	0.00	0.00	0.01	0.02	0.04	0.08	0.21	0.05
CA/FS Year Post-Entry	0.00	0.00	0.00	0.00	0.00	0.00	1.00	0.16
CA/FS Year Post-Exit	0.01	0.03	0.07	0.13	0.31	0.72	1.00	0.34
Labor Year Post-Exit	0.01	0.02	0.06	0.11	0.24	0.61	0.98	0.30

Includes only family shelter entrants originating from NYC. Unit of observation is family-spell. Families and individual entering are counts; all other statistics are family-spell means.

Table B.2: Families by Number of Spells

Homeless Spells	# of Families	Percent
1	37,587	78.3
2	8,015	16.7
3	1,831	3.8
4+	544	1.1
Total	47,977	100.0

Includes only family shelter entrants originating from NYC.

Table B.3A: Descriptives and Random Assignment

	Overall			Randomization Check		
Variable	N	Mean	SD	Out-of-Boro	In-Boro	Diff.
Year Entered Shelter	59,253	2013.01	2.07	2013.38	2012.65	-0.72**
Month Entered Shelter	59,253	6.52	3.40	6.78	6.28	-0.50**
Q1 Entry	59,253	0.25	0.43	0.22	0.27	0.05**
Q2 Entry	59,253	0.23	0.42	0.22	0.25	0.03**
Q3 Entry	59,253	0.28	0.45	0.31	0.26	-0.05**
Q4 Entry	59,253	0.24	0.42	0.25	0.22	-0.03**
Manhattan Origin	59,253	0.12	0.33	0.16	0.09	-0.07**
Bronx Origin	59,253	0.41	0.49	0.33	0.49	0.16**
Brooklyn Origin	59,253	0.32	0.47	0.31	0.32	0.01**
Queens Origin	59,253	0.12	0.33	0.15	0.10	-0.06**
Staten Island Origin	59,253	0.03	0.16	0.05	0.01	-0.04**
Family Size	59,253	3.35	1.39	3.34	3.36	0.02*
Family Members Under 18	59,253	1.97	1.19	1.95	1.99	0.04**
Oldest Child's Grade	59,253	2.57	5.32	1.95	3.18	1.23**
Health Issue Present	59,253	0.30	0.46	0.32	0.28	-0.04**
Eligibility: Eviction	59,253	0.33	0.47	0.28	0.39	0.10**
Eligibility: Overcrowding	59,253	0.18	0.38	0.17	0.19	0.02**
Eligibility: Conditions	59,253	0.08	0.28	0.08	0.09	0.01**
Eligibility: Domestic Violence	59,253	0.30	0.46	0.37	0.22	-0.15**
Eligibility: Other	59,253	0.11	0.31	0.10	0.11	0.01**
Eligibility: Unknown	59,253	0.00	0.01	0.00	0.00	0.00
Female	59,253	0.92	0.28	0.92	0.91	-0.01**
Age	59,253	31.54	8.86	30.94	32.13	1.20**
Partner/Spouse Present	59,253	0.26	0.44	0.27	0.24	-0.03**
Pregnant	59,253	0.07	0.25	0.07	0.06	-0.01**

Treatment defined as placed in-borough. Group contrasts obtained from separate bivariate OLS regressions of each characteristic 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. Unit of observation is family-spell. Full sample. * $p < 0.10$, ** $p < 0.05$

Table B.3B: Descriptives and Random Assignment

	Overall			Randomization Check		
Variable	N	Mean	SD	Out-of-Boro	In-Boro	Diff.
Black	59,253	0.56	0.50	0.57	0.55	-0.02**
White	59,253	0.03	0.16	0.03	0.02	-0.01**
Hispanic	59,253	0.38	0.48	0.36	0.39	0.03**
Asian	59,253	0.00	0.07	0.00	0.00	-0.00
Other Race	59,253	0.00	0.06	0.00	0.00	-0.00
Unknown Race	59,253	0.03	0.17	0.03	0.03	-0.00*
No Degree	59,253	0.57	0.50	0.56	0.58	0.01**
High School Grad	59,253	0.32	0.47	0.32	0.32	-0.01*
Some College or More	59,253	0.05	0.22	0.05	0.05	-0.00
Unknown Education	59,253	0.06	0.24	0.06	0.06	-0.00
On Cash Assistance	59,253	0.35	0.48	0.36	0.35	-0.01**
On Food Stamps	59,253	0.73	0.44	0.73	0.73	0.00
Employed Year Pre	59,253	0.43	0.50	0.44	0.43	-0.01**
Log AQ Earnings Year Pre	59,253	3.01	3.58	3.02	2.99	-0.03
Tier II Shelter	59,253	0.55	0.50	0.55	0.55	0.01**
Commercial Hotel	59,253	0.28	0.45	0.30	0.25	-0.05**
Family Cluster Unit	59,253	0.16	0.37	0.14	0.19	0.05**
Other Facility	59,253	0.01	0.10	0.01	0.01	-0.01**
Manhattan Shelter	59,253	0.18	0.39	0.27	0.09	-0.18**
Bronx Shelter	59,253	0.39	0.49	0.29	0.49	0.20**
Brooklyn Shelter	59,253	0.27	0.44	0.22	0.32	0.11**
Queens Shelter	59,253	0.15	0.36	0.21	0.10	-0.11**
Staten Island Shelter	59,253	0.01	0.09	0.01	0.01	-0.01**
School District Placement	54,306	0.10	0.30	0.00	0.19	0.19**
Placement Distance (miles)	54,306	5.89	4.65	9.27	2.66	-6.61**
Borough Placement	59,253	0.51	0.50	0.00	1.00	1.00

Treatment defined as placed in-borough. Group contrasts obtained from separate bivariate OLS regressions of each characteristic 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. Unit of observation is family-spell. Full sample. * $p < 0.10$, ** $p < 0.05$

Table B.4: Descriptives and Random Assignment

	Overall			Randomization Check		
Variable	N	Mean	SD	Out-of-Boro	In-Boro	Diff.
Jan Entry	59,253	0.09	0.29	0.08	0.10	0.01**
Feb Entry	59,253	0.08	0.26	0.07	0.08	0.02**
Mar Entry	59,253	0.08	0.27	0.07	0.09	0.02**
Apr Entry	59,253	0.08	0.27	0.07	0.08	0.02**
May Entry	59,253	0.08	0.27	0.07	0.08	0.01**
Jun Entry	59,253	0.08	0.27	0.08	0.08	0.01**
Jul Entry	59,253	0.09	0.28	0.10	0.08	-0.02**
Aug Entry	59,253	0.10	0.30	0.11	0.09	-0.02**
Sep Entry	59,253	0.10	0.30	0.10	0.09	-0.02**
Oct Entry	59,253	0.09	0.28	0.09	0.08	-0.01**
Nov Entry	59,253	0.08	0.27	0.08	0.07	-0.01**
Dec Entry	59,253	0.07	0.26	0.08	0.07	-0.01**
2010 Entry	59,253	0.17	0.37	0.12	0.22	0.10**
2011 Entry	59,253	0.13	0.33	0.10	0.15	0.04**
2012 Entry	59,253	0.13	0.34	0.13	0.14	0.00

Treatment defined as placed in-borough. Group contrasts obtained from separate bivariate OLS regressions of each characteristic 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. Unit of observation is family-spell. Sample is all NYC family shelter entrants from 2010–2016 with non-missing origin and shelter boroughs. * $p < 0.10$, ** $p < 0.05$

Table B.5: Descriptives and Random Assignment

	Overall			Randomization Check		
Variable	N	Mean	SD	Out-of-Boro	In-Boro	Diff.
Log Length of Stay	59,253	5.50	1.24	5.43	5.57	0.14**
Log Shelter LOS (Excl. Gaps)	59,253	5.50	1.24	5.42	5.57	0.14**
Length of Stay (Days)	59,253	424.33	406.67	410.96	437.35	26.40**
Log LOS (2017)	59,253	5.48	1.21	5.40	5.55	0.15**
Subsidized Exit	57,962	0.39	0.49	0.39	0.40	0.01*
Unsubsidized Exit	57,962	0.60	0.49	0.60	0.60	-0.00
Returned to Shelter (One Year)	52,274	0.15	0.36	0.16	0.14	-0.03**
Cash Assistance Post Entry	59,253	0.78	0.41	0.77	0.79	0.02**
CA Post Entry Percent	59,253	0.62	0.41	0.61	0.64	0.02**
Food Stamps Post Entry	59,253	0.90	0.31	0.89	0.90	0.01**
FS Post Entry Percent	59,253	0.82	0.34	0.80	0.83	0.03**
Employed Post Entry	59,253	0.48	0.50	0.48	0.48	0.01
Empl. Post Entry Percent	59,253	0.34	0.41	0.33	0.34	0.01**
Log AQ Earnings Post Entry	59,253	3.38	3.68	3.33	3.42	0.09**
AQ Earnings Post Entry	59,253	1188.87	2274.61	1153.03	1223.76	70.73**
Cash Assistance Post Exit	48,082	0.74	0.44	0.73	0.74	0.01**
CA Post Exit Percent	59,253	0.41	0.44	0.39	0.42	0.03**
Food Stamps Post Exit	48,082	0.88	0.32	0.88	0.89	0.01**
FS Post Exit Percent	59,253	0.60	0.45	0.57	0.62	0.05**
Employed Post Exit	48,082	0.45	0.50	0.45	0.46	0.01
Empl. Post Exit Percent	59,253	0.27	0.40	0.26	0.29	0.03**
Log AQ Earnings Post Exit	48,082	3.27	3.73	3.22	3.31	0.09**
AQ Earnings Post Exit	48,082	1306.24	2515.85	1247.82	1358.75	110.93**

Treatment defined as placed in-borough. Group contrasts obtained from separate bivariate OLS regressions of each characteristic 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. Unit of observation is family-spell. Sample is all NYC family shelter entrants from 2010–2016 with non-missing origin and shelter boroughs. * $p < 0.10$, ** $p < 0.05$

Table B.6: Subsidized Exits and Length of Stay: OLS Results

	Full Period (2010-2016)				Apr. 2011-Dec. 2013	
	All Families (1)	Unsubs-idized (2)	Subsid-ized (3)	Interac-tion (4)	Entry (5)	Entry & Exit (6)
Borough Placement	0.120**	0.152**	0.020*	0.218**	0.132**	0.066**
	(0.011)	(0.014)	(0.011)	(0.014)	(0.020)	(0.020)
Subsidized Exit				1.024**		
				(0.013)		
Borough Placement × Subsidized Exit				-0.282**		
				(0.017)		
Obs.	59,253	35,260	22,702	57,962	20,918	4,852

Each column is a separate regression of log length of stay on an indicator for in-borough placement, Main covariates, and addressing subsidized exits in the column-enumerated manner. Subsidized exits include any sort of rental assistance (e.g., Advantage, LINC, NYCHA, Section 8, FEPS, one-shots). Base sample is Full sample. Col 1 repeats results from main text. Col 2 is limited to families with unsubsidized exits only. Col 3 is limited to families with subsidized exits only. Col 4 includes an indicator for subsidized exit and its interaction with treatment. Col 5 is limited to families entering shelter 4/1/2011–12/31/2013.Col 6 is limited to families entering and exiting shelter 4/1/2011–12/31/2013. Standard errors clustered at family group level in parentheses. * $p < 0.10$, ** $p < 0.05$

Table B.7: Subsidized Exits and Length of Stay: Aversion Ratio IV Results

	Full Period (2010-2016)				Apr. 2011-Dec. 2013	
	All Families (1)	Unsubsidized (2)	Subsidized (3)	Interaction (4)	Entry (5)	Entry & Exit (6)
Borough Placement	0.95**	2.63**	0.59	6.02**	1.38**	-10.07
	(0.34)	(0.64)	(0.37)	(0.93)	(0.71)	(6.63)
Subsidized Exit				7.13**		
				(0.69)		
Borough Placement × Subsidized Exit				-12.32**		
				(1.35)		
Obs.	59,253	35,260	22,702	57,962	20,918	4,852

Each column is a separate regression of log length of stay on an indicator for in-borough placement, Main covariates, and addressing subsidized exits in the column-enumerated manner. Subsidized exits include any sort of rental assistance (e.g., Advantage, LINC, NYCHA, Section 8, FEPS, one-shots). Base sample is Full sample. Col 1 repeats results from main text. Col 2 is limited to families with unsubsidized exits only. Col 3 is limited to families with subsidized exits only. Col 4 includes an indicator for subsidized exit and its interaction with treatment. Col 5 is limited to families entering shelter 4/1/2011–12/31/2013.Col 6 is limited to families entering and exiting shelter 4/1/2011–12/31/2013. Standard errors clustered at family group level in parentheses. * $p < 0.10$, ** $p < 0.05$

Table B.8: OLS Outcome Robustness

	Full Sample					Non-DV	Pre-2015
Outcome	Outcome Mean (1)	Raw (2)	Placement (3)	Main (4)	Shelter (5)	Main (6)	Main (7)
A. Stays and Returns							
Log LOS (excl. gaps)	5.496**	0.141**	0.108**	0.120**	0.115**	0.086**	0.126**
	(1.243)	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)	(0.013)
	{59,253}	{59,253}	{59,253}	{59,253}	{59,247}	{41,744}	{41,717}
Length of Stay (days)	424.333**	26.397**	17.587**	23.090**	22.341**	19.767**	24.741**
	(406.668)	(3.334)	(3.417)	(3.544)	(3.541)	(4.430)	(4.446)
	{59,253}	{59,253}	{59,253}	{59,253}	{59,247}	{41,744}	{41,717}
Log LOS (as of 2017)	5.476**	0.146**	0.105**	0.117**	0.113**	0.083**	0.125**
	(1.215)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)	(0.013)
	{59,253}	{59,253}	{59,253}	{59,253}	{59,247}	{41,744}	{41,717}
Unsubsidized Exit	0.600**	-0.004	-0.020**	-0.017**	-0.016**	-0.016**	-0.015**
	(0.490)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
	{57,962}	{57,962}	{57,962}	{57,962}	{57,954}	{40,766}	{41,420}
B. Year Post-Shelter Entry							
CA Percent of Year	0.624**	0.024**	0.009**	0.004	0.004	0.006	0.006
	(0.410)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
	{59,253}	{59,253}	{59,253}	{59,253}	{59,247}	{41,744}	{41,717}
FS Percent of Year	0.815**	0.032**	0.004	-0.001	-0.000	-0.006**	0.004
	(0.342)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)
	{59,253}	{59,253}	{59,253}	{59,253}	{59,247}	{41,744}	{41,717}
Employed: Quarterly Proportion	0.337**	0.010**	0.014**	0.012**	0.011**	0.011**	0.011**
	(0.406)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
	{59,253}	{59,253}	{59,253}	{59,253}	{59,247}	{41,744}	{41,717}
Avg. Quarterly Earnings	1188.870**	70.734**	36.204*	27.902	23.739	29.523	22.168
	(2274.606)	(18.974)	(19.470)	(17.893)	(18.000)	(22.207)	(20.448)
	{59,253}	{59,253}	{59,253}	{59,253}	{59,247}	{41,744}	{41,717}
C. Year Post-Shelter Exit							
CA Percent of Year	0.407**	0.027**	-0.002	-0.006*	-0.005	-0.002	-0.001
	(0.435)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
	{59,253}	{59,253}	{59,253}	{59,253}	{59,247}	{41,744}	{41,717}
FS Percent of Year	0.596**	0.053**	-0.004	-0.010**	-0.008**	-0.013**	-0.003
	(0.451)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
	{59,253}	{59,253}	{59,253}	{59,253}	{59,247}	{41,744}	{41,717}
Employed: Quarterly Proportion	0.270**	0.031**	0.007**	0.004	0.004	0.006	0.005
	(0.397)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
	{59,253}	{59,253}	{59,253}	{59,253}	{59,247}	{41,744}	{41,717}
Avg. Quarterly Earnings	1306.237**	110.929**	48.035**	33.994	28.066	38.382	24.918
	(2515.846)	(23.163)	(24.293)	(23.227)	(23.470)	(29.069)	(25.269)
	{48,082}	{48,082}	{48,082}	{48,082}	{48,076}	{33,761}	{39,974}
Time Control		None	Year^3	Year^3	Year^3	Year^3	Year^3
Placement Controls		No	Yes	Yes	Yes	Yes	Yes
Family & Shelter Controls		No	No	Yes	Yes	Yes	Yes
Shelter FE		No	No	No	Yes	No	No

Each cell reports the coefficient on in-borough shelter placement from a separate OLS regression of the row-delineated outcome on the treatment indicator, controlling for the column-enumerated covariates. Supercolumns give samples. Standard errors clustered at family group level in parentheses. Number of observations given in braces. * $p < 0.10$, ** $p < 0.05$

Table B.9: Compliance Type Shares: Ineligibility Rate Instrument

	1%	1.5%	2%
Compliers	0.08	0.08	0.07
Always-Takers	0.64	0.64	0.64
Never-Takers	0.28	0.28	0.28

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 Chapter 1 for estimation method details.

Table B.10: Compliance Type Shares: Aversion Ratio

	1%	1.5%	2%
Compliers	0.10	0.10	0.09
Always-Takers	0.62	0.62	0.62
Never-Takers	0.28	0.28	0.28

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 Chapter 1 for estimation method details.

Table B.11: Complier Characteristics: Ineligibility Rate Instrument

	Compliers	Non-Compliers	Diff.
Manhattan Origin	0.00	0.14	-0.13
	(0.003)	(0.000)	[-2.56]
Bronx Origin	0.57	0.39	0.18
	(0.006)	(0.000)	[2.28]
Brooklyn Origin	0.25	0.32	-0.07
	(0.005)	(0.000)	[-0.99]
Queens Origin	0.10	0.13	-0.02
	(0.003)	(0.000)	[-0.45]
Staten Island Origin	0.02	0.03	-0.00
	(0.000)	(0.000)	[-0.33]
Health Issue Present	0.33	0.30	0.04
	(0.004)	(0.000)	[0.61]
Eligibility: Eviction	0.29	0.34	-0.05
	(0.005)	(0.000)	[-0.67]
Eligibility: Overcrowding	0.16	0.18	-0.02
	(0.003)	(0.000)	[-0.34]
Eligibility: Conditions	0.11	0.08	0.03
	(0.002)	(0.000)	[0.67]
Eligibility: Domestic Violence	0.30	0.30	0.00
	(0.004)	(0.000)	[0.01]
Eligibility: Other	0.08	0.11	-0.03
	(0.003)	(0.000)	[-0.67]
Female	0.97	0.91	0.06
	(0.002)	(0.000)	[1.26]
Partner/Spouse Present	0.31	0.25	0.06
	(0.004)	(0.000)	[0.99]
Pregnant	0.04	0.07	-0.03
	(0.001)	(0.000)	[-0.86]
Black	0.43	0.57	-0.14
	(0.006)	(0.000)	[-1.79]
Hispanic	0.46	0.37	0.09
	(0.006)	(0.000)	[1.18]
White	0.06	0.02	0.04
	(0.001)	(0.000)	[1.57]
No Degree	0.61	0.57	0.05
	(0.005)	(0.000)	[0.67]
High School Grad	0.30	0.32	-0.02
	(0.005)	(0.000)	[-0.29]
Some College or More	0.06	0.05	0.01
	(0.001)	(0.000)	[0.21]
Unknown Education	0.02	0.07	-0.05
	(0.001)	(0.000)	[-1.27]
On Cash Assistance	0.30	0.36	-0.06
	(0.005)	(0.000)	[-0.78]
On Food Stamps	0.75	0.73	0.02
	(0.006)	(0.000)	[0.29]
Employed Year Pre	0.39	0.44	-0.05
	(0.005)	(0.000)	[-0.67]
Tier II Shelter	0.63	0.54	0.08
	(0.004)	(0.000)	[1.27]
Commercial Hotel	0.08	0.29	-0.21
	(0.005)	(0.000)	[-3.08]
Family Cluster Unit	0.19	0.16	0.02
	(0.003)	(0.000)	[0.46]
Family Size 1–3	0.54	0.64	-0.11
	(0.006)	(0.000)	[-1.43]
Family Size 4–5	0.39	0.28	0.11
	(0.004)	(0.000)	[1.68]
Family Size 6+	0.07	0.08	-0.01
	(0.002)	(0.000)	[-0.27]
Age	31.69	31.53	0.16
	(1.350)	(0.013)	[0.14]
Log AQ Earnings Year Pre	2.73	3.03	-0.30
	(0.259)	(0.002)	[-0.60]

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 families 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 Chapter 1. Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family group.

Table B.12: Complier Characteristics: Aversion Ratio Instrument

	Compliers	Non-Compliers	Diff.
Manhattan Origin	0.09	0.13	-0.04
	(0.001)	(0.000)	[-1.18]
Bronx Origin	0.55	0.39	0.16
	(0.004)	(0.000)	[2.67]
Brooklyn Origin	0.20	0.33	-0.13
	(0.003)	(0.000)	[-2.29]
Queens Origin	0.12	0.13	-0.01
	(0.002)	(0.000)	[-0.17]
Staten Island Origin	0.03	0.03	0.01
	(0.000)	(0.000)	[0.51]
Health Issue Present	0.35	0.29	0.06
	(0.002)	(0.000)	[1.27]
Eligibility: Eviction	0.33	0.34	-0.01
	(0.003)	(0.000)	[-0.13]
Eligibility: Overcrowding	0.15	0.18	-0.03
	(0.002)	(0.000)	[-0.61]
Eligibility: Conditions	0.11	0.08	0.02
	(0.001)	(0.000)	[0.73]
Eligibility: Domestic Violence	0.27	0.30	-0.03
	(0.002)	(0.000)	[-0.59]
Eligibility: Other	0.10	0.11	-0.01
	(0.001)	(0.000)	[-0.28]
Female	0.94	0.91	0.03
	(0.001)	(0.000)	[0.82]
Partner/Spouse Present	0.28	0.25	0.03
	(0.002)	(0.000)	[0.61]
Pregnant	0.05	0.07	-0.02
	(0.001)	(0.000)	[-0.66]
Black	0.47	0.57	-0.09
	(0.004)	(0.000)	[-1.51]
Hispanic	0.43	0.37	0.06
	(0.003)	(0.000)	[0.98]
White	0.05	0.02	0.03
	(0.000)	(0.000)	[1.46]
No Degree	0.59	0.57	0.02
	(0.003)	(0.000)	[0.41]
High School Grad	0.31	0.32	-0.01
	(0.002)	(0.000)	[-0.21]
Some College or More	0.07	0.05	0.02
	(0.001)	(0.000)	[0.76]
Unknown Education	0.02	0.07	-0.04
	(0.001)	(0.000)	[-1.47]
On Cash Assistance	0.23	0.37	-0.14
	(0.003)	(0.000)	[-2.43]
On Food Stamps	0.70	0.74	-0.04
	(0.003)	(0.000)	[-0.63]
Employed Year Pre	0.39	0.44	-0.05
	(0.003)	(0.000)	[-0.91]
Tier II Shelter	0.59	0.55	0.04
	(0.003)	(0.000)	[0.82]
Commercial Hotel	0.14	0.29	-0.16
	(0.003)	(0.000)	[-2.83]
Family Cluster Unit	0.19	0.16	0.02
	(0.001)	(0.000)	[0.63]
Family Size 1–3	0.60	0.64	-0.04
	(0.003)	(0.000)	[-0.76]
Family Size 4–5	0.35	0.28	0.06
	(0.003)	(0.000)	[1.26]
Family Size 6+	0.05	0.08	-0.02
	(0.001)	(0.000)	[-0.82]
Age	32.20	31.47	0.73
	(0.949)	(0.014)	[0.74]
Log AQ Earnings Year Pre	2.76	3.03	-0.27
	(0.147)	(0.002)	[-0.71]

Main sample. Treatment is in-borough placement. Instrument is 15-day moving average of the aversion ratio. Compliers are families placed in-borough when the aversion ratio 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 Chapter 1. Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family group.

Table B.13: IV Robustness: Ineligibility Rate

	Borough			School District			Distance		
	Full (1)	Non-DV (2)	Pre-2015 (3)	Full (4)	Non-DV (5)	Pre-2015 (6)	Full (7)	Non-DV (8)	Pre-2015 (9)
A. Stays and Returns									
Log Length of Stay	1.367**	0.971*	5.083*	4.650**	2.205*	7.198**	-0.203**	-0.166*	-0.497**
	(0.527)	(0.573)	(2.820)	(2.084)	(1.201)	(3.135)	(0.078)	(0.097)	(0.213)
	[28.8]	[18.3]	[4.0]	[8.7]	[11.7]	[6.9]	[17.1]	[8.6]	[7.2]
Subsidized Exit	-0.789**	-1.145**	-2.513*	-2.070**	-1.998**	-2.244**	0.094**	0.161**	0.173**
	(0.257)	(0.380)	(1.489)	(0.885)	(0.721)	(1.017)	(0.037)	(0.071)	(0.084)
	[26.2]	[16.8]	[3.3]	[9.2]	[12.7]	[7.2]	[15.6]	[7.7]	[6.0]
Returned to Shelter	0.287*	0.362*	-0.301	1.287	1.039	-0.201	-0.039	-0.066	0.017
	(0.166)	(0.210)	(0.405)	(0.879)	(0.676)	(0.397)	(0.025)	(0.047)	(0.032)
	[25.2]	[15.3]	[4.3]	[4.3]	[5.3]	[9.2]	[13.4]	[4.7]	[7.8]
B. Year Post-Shelter Entry									
Cash Assistance	0.651**	0.699**	0.356	1.498**	1.051**	0.688	-0.085**	-0.107**	-0.048
	(0.183)	(0.238)	(0.415)	(0.654)	(0.457)	(0.519)	(0.027)	(0.045)	(0.036)
	[28.8]	[18.3]	[4.0]	[8.7]	[11.7]	[6.9]	[17.1]	[8.6]	[7.2]
Food Stamps	-0.142	-0.199*	-0.107	-0.546*	-0.456*	-0.065	0.017	0.029	0.005
	(0.093)	(0.120)	(0.247)	(0.323)	(0.251)	(0.279)	(0.013)	(0.020)	(0.020)
	[28.8]	[18.3]	[4.0]	[8.7]	[11.7]	[6.9]	[17.1]	[8.6]	[7.2]
Employed	-0.020	0.012	0.037	-0.161	-0.050	0.226	0.001	-0.001	-0.015
	(0.171)	(0.213)	(0.474)	(0.511)	(0.410)	(0.560)	(0.023)	(0.032)	(0.040)
	[28.8]	[18.3]	[4.0]	[8.7]	[11.7]	[6.9]	[17.1]	[8.6]	[7.2]
Log Avg. Quarterly Earnings	1.245	1.020	0.606	2.445	1.437	1.817	-0.155	-0.149	-0.118
	(1.243)	(1.553)	(3.354)	(3.747)	(2.997)	(3.973)	(0.169)	(0.240)	(0.280)
	[28.8]	[18.3]	[4.0]	[8.7]	[11.7]	[6.9]	[17.1]	[8.6]	[7.2]
C. Year Post-Shelter Exit									
Cash Assistance	0.428**	0.227	0.283	1.289*	0.487	0.598	-0.059**	-0.048	-0.045
	(0.210)	(0.236)	(0.405)	(0.763)	(0.529)	(0.482)	(0.030)	(0.047)	(0.036)
	[20.3]	[14.1]	[5.2]	[6.2]	[7.1]	[9.3]	[12.5]	[4.5]	[8.9]
Food Stamps	-0.064	-0.187	-0.197	-0.102	-0.416	-0.122	0.001	0.031	0.008
	(0.130)	(0.165)	(0.271)	(0.381)	(0.372)	(0.293)	(0.017)	(0.032)	(0.022)
	[20.3]	[14.1]	[5.2]	[6.2]	[7.1]	[9.3]	[12.5]	[4.5]	[8.9]
Employed	0.397*	0.405	0.448	1.422*	1.002	0.632	-0.066*	-0.091	-0.047
	(0.232)	(0.279)	(0.479)	(0.862)	(0.675)	(0.537)	(0.034)	(0.064)	(0.040)
	[20.3]	[14.1]	[5.2]	[6.2]	[7.1]	[9.3]	[12.5]	[4.5]	[8.9]
Log Avg. Quarterly Earnings	2.508	2.133	1.998	9.156	5.547	2.754	-0.419*	-0.509	-0.206
	(1.673)	(1.988)	(3.298)	(5.991)	(4.640)	(3.726)	(0.240)	(0.427)	(0.277)
	[20.3]	[14.1]	[5.2]	[6.2]	[7.1]	[9.3]	[12.5]	[4.5]	[8.9]
Time Control	Year^3	Year^3	Year^3	Year^3	Year^3	Year^3	Year^3	Year^3	Year^3
Placement Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family & Shelter Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shelter FE	No	No	No	No	No	No	No	No	No

Each cell reports the coefficient on local shelter placement from a separate 2SLS regression of the row-delineated outcome on the treatment using the ineligibility rate as the instrument and controlling for Main covariates. Columns give samples; supercolumns give treatment definitions. Standard errors clustered at family group level in parentheses. First-stage F-stats in brackets. * $p < 0.10$, ** $p < 0.05$

Table B.14: IV Robustness: Aversion Ratio

	Borough			School District			Distance		
	Full	Non-DV	Pre-2015	Full	Non-DV	Pre-2015	Full	Non-DV	Pre-2015
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Stays and Returns									
Log Length of Stay	0.946**	0.531	2.110**	2.930**	1.275	4.479**	-0.120**	-0.071	-0.205**
	(0.342)	(0.357)	(0.634)	(1.109)	(0.806)	(1.547)	(0.043)	(0.048)	(0.058)
	[60.8]	[42.9]	[27.5]	[20.3]	[21.4]	[14.6]	[45.2]	[26.7]	[38.1]
Subsidized Exit	-0.331**	-0.527**	-0.483**	-0.874**	-1.078**	-0.836*	0.034*	0.061**	0.035*
	(0.147)	(0.190)	(0.219)	(0.427)	(0.416)	(0.445)	(0.018)	(0.026)	(0.020)
	[55.8]	[38.8]	[26.3]	[20.3]	[22.1]	[14.7]	[41.8]	[23.8]	[36.4]
Returned to Shelter	0.088	0.098	-0.337**	0.276	0.240	-0.702**	-0.006	-0.009	0.038**
	(0.104)	(0.122)	(0.162)	(0.372)	(0.345)	(0.330)	(0.013)	(0.019)	(0.016)
	[55.7]	[37.7]	[27.1]	[12.2]	[11.7]	[16.2]	[37.2]	[17.4]	[37.8]
B. Year Post-Shelter Entry									
Cash Assistance	0.338**	0.318**	0.015	0.600**	0.384	0.123	-0.041**	-0.042**	-0.006
	(0.105)	(0.125)	(0.149)	(0.304)	(0.268)	(0.304)	(0.013)	(0.017)	(0.014)
	[60.8]	[42.9]	[27.5]	[20.3]	[21.4]	[14.6]	[45.2]	[26.7]	[38.1]
Food Stamps	-0.100	-0.133*	-0.028	-0.374*	-0.336*	0.071	0.009	0.015	-0.003
	(0.064)	(0.076)	(0.095)	(0.195)	(0.176)	(0.189)	(0.008)	(0.010)	(0.009)
	[60.8]	[42.9]	[27.5]	[20.3]	[21.4]	[14.6]	[45.2]	[26.7]	[38.1]
Employed	0.116	0.164	0.284	0.190	0.279	0.576	-0.013	-0.021	-0.027
	(0.118)	(0.141)	(0.190)	(0.334)	(0.308)	(0.396)	(0.014)	(0.019)	(0.018)
	[60.8]	[42.9]	[27.5]	[20.3]	[21.4]	[14.6]	[45.2]	[26.7]	[38.1]
Log Avg. Quarterly Earnings	1.085	1.258	1.101	2.085	2.354	2.541	-0.126	-0.172	-0.121
	(0.851)	(1.019)	(1.310)	(2.424)	(2.245)	(2.686)	(0.104)	(0.138)	(0.126)
	[60.8]	[42.9]	[27.5]	[20.3]	[21.4]	[14.6]	[45.2]	[26.7]	[38.1]
C. Year Post-Shelter Exit									
Cash Assistance	0.265**	0.087	0.102	0.789*	0.192	0.285	-0.033**	-0.018	-0.015
	(0.129)	(0.148)	(0.171)	(0.447)	(0.392)	(0.331)	(0.016)	(0.024)	(0.017)
	[46.4]	[34.2]	[27.2]	[12.5]	[11.4]	[16.2]	[33.5]	[15.1]	[37.1]
Food Stamps	0.023	-0.075	0.003	0.089	-0.214	0.018	-0.007	0.009	-0.004
	(0.086)	(0.103)	(0.114)	(0.267)	(0.275)	(0.214)	(0.011)	(0.016)	(0.011)
	[46.4]	[34.2]	[27.2]	[12.5]	[11.4]	[16.2]	[33.5]	[15.1]	[37.1]
Employed	0.338**	0.352**	0.268	1.138**	0.930*	0.585	-0.047**	-0.060**	-0.028
	(0.149)	(0.174)	(0.197)	(0.545)	(0.509)	(0.387)	(0.019)	(0.030)	(0.019)
	[46.4]	[34.2]	[27.2]	[12.5]	[11.4]	[16.2]	[33.5]	[15.1]	[37.1]
Log Avg. Quarterly Earnings	2.035*	1.975	1.221	7.314*	5.541	3.126	-0.295**	-0.358*	-0.143
	(1.078)	(1.261)	(1.406)	(3.850)	(3.590)	(2.739)	(0.138)	(0.215)	(0.137)
	[46.4]	[34.2]	[27.2]	[12.5]	[11.4]	[16.2]	[33.5]	[15.1]	[37.1]
Time Control	Year^3	Year^3	Year^3	Year^3	Year^3	Year^3	Year^3	Year^3	Year^3
Placement Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family & Shelter Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shelter FE	No	No	No	No	No	No	No	No	No

Each cell reports the coefficient on local shelter placement from a separate 2SLS regression of the row-delineated outcome on the treatment using the aversion ratio as the instrument and controlling for Main covariates. Columns give samples; supercolumns give treatment definitions. Standard errors clustered at family group level in parentheses. First-stage F-stats in brackets. * $p < 0.10$, ** $p < 0.05$

Table B.15: Time Trend Robustness

	OLS				Ineligibility Rate IV				Aversion Ratio IV			
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Stays and Returns												
Log Length of Stay	0.121**	0.119**	0.120**	0.118**	1.704**	3.941**	1.943**	0.093	1.298**	1.787**	1.284**	0.108
	(0.011)	(0.011)	(0.011)	(0.011)	(0.438)	(1.815)	(0.652)	(0.352)	(0.275)	(0.542)	(0.318)	(0.274)
	{59,253}	{59,253}	{59,253}	{59,253}	[47.0]	[6.5]	[23.2]	[52.8]	[106.2]	[32.3]	[76.8]	[85.8]
Subsidized Exit	0.024**	0.019**	0.021**	0.021**	0.110	-1.301*	-2.675**	0.338**	1.446**	-0.207	0.057	0.227*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.159)	(0.712)	(0.635)	(0.154)	(0.176)	(0.196)	(0.127)	(0.118)
	{57,962}	{57,962}	{57,962}	{57,962}	[44.7]	[5.5]	[20.8]	[48.9]	[101.7]	[28.3]	[70.5]	[79.3]
Returned to Shelter	-0.006*	-0.005	-0.006*	-0.005	0.347**	-0.047	0.772**	0.208*	-0.147*	-0.234	-0.064	0.156*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.141)	(0.294)	(0.258)	(0.120)	(0.083)	(0.143)	(0.093)	(0.091)
	{52,274}	{52,274}	{52,274}	{52,274}	[36.9]	[7.2]	[18.2]	[46.7]	[91.2]	[33.1]	[69.2]	[79.2]
B. Year Post-Shelter Entry												
Cash Assistance	0.011**	0.011**	0.012**	0.012**	-0.361**	-1.348**	0.954**	-0.387**	-0.346**	-0.572**	0.642**	-0.380**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.124)	(0.618)	(0.250)	(0.117)	(0.083)	(0.174)	(0.112)	(0.091)
	{59,253}	{59,253}	{59,253}	{59,253}	[47.0]	[6.5]	[23.2]	[52.8]	[106.2]	[32.3]	[76.8]	[85.8]
Food Stamps	0.003*	0.003	0.004*	0.004*	-0.257**	-1.015**	-0.048	-0.473**	-0.120**	-0.372**	0.101*	-0.352**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.080)	(0.452)	(0.099)	(0.095)	(0.048)	(0.115)	(0.056)	(0.066)
	{59,253}	{59,253}	{59,253}	{59,253}	[47.0]	[6.5]	[23.2]	[52.8]	[106.2]	[32.3]	[76.8]	[85.8]
Employed	0.010**	0.010**	0.011**	0.011**	-0.877**	-0.441	-0.087	-0.276**	-0.384**	0.168	0.439**	-0.207**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.187)	(0.407)	(0.193)	(0.132)	(0.099)	(0.167)	(0.115)	(0.101)
	{59,253}	{59,253}	{59,253}	{59,253}	[47.0]	[6.5]	[23.2]	[52.8]	[106.2]	[32.3]	[76.8]	[85.8]
Log Avg. Quarterly Earnings	0.095**	0.093**	0.100**	0.097**	-3.259**	-1.624	0.975	-1.473	-1.096*	0.343	3.084**	-1.412*
	(0.028)	(0.028)	(0.028)	(0.028)	(1.081)	(2.702)	(1.383)	(0.929)	(0.658)	(1.186)	(0.825)	(0.725)
	{59,253}	{59,253}	{59,253}	{59,253}	[47.0]	[6.5]	[23.2]	[52.8]	[106.2]	[32.3]	[76.8]	[85.8]
C. Year Post-Shelter Exit												
Cash Assistance	0.017**	0.017**	0.017**	0.017**	-0.441**	0.240	-0.098	-0.062	-0.193*	0.271	0.159	-0.075
	(0.004)	(0.004)	(0.004)	(0.004)	(0.200)	(0.406)	(0.235)	(0.160)	(0.109)	(0.182)	(0.111)	(0.117)
	{48,082}	{48,082}	{48,082}	{48,082}	[23.1]	[4.9]	[13.3]	[28.8]	[66.4]	[25.3]	[60.0]	[53.1]
Food Stamps	0.009**	0.009**	0.009**	0.009**	-0.131	-0.090	-0.188	-0.256**	0.021	0.139	0.035	-0.112
	(0.003)	(0.003)	(0.003)	(0.003)	(0.124)	(0.271)	(0.166)	(0.121)	(0.071)	(0.122)	(0.075)	(0.084)
	{48,082}	{48,082}	{48,082}	{48,082}	[23.1]	[4.9]	[13.3]	[28.8]	[66.4]	[25.3]	[60.0]	[53.1]
Employed	0.003	0.003	0.004	0.003	-1.171**	0.477	-0.018	0.322*	-0.716**	0.375*	0.326**	0.171
	(0.004)	(0.004)	(0.004)	(0.004)	(0.314)	(0.493)	(0.266)	(0.189)	(0.148)	(0.209)	(0.130)	(0.132)
	{48,082}	{48,082}	{48,082}	{48,082}	[23.1]	[4.9]	[13.3]	[28.8]	[66.4]	[25.3]	[60.0]	[53.1]
Log Avg. Quarterly Earnings	0.042	0.042	0.047	0.040	-7.755**	2.137	-0.792	2.054	-4.504**	1.855	1.978**	1.029
	(0.033)	(0.033)	(0.033)	(0.033)	(2.186)	(3.385)	(1.970)	(1.371)	(1.039)	(1.477)	(0.945)	(0.973)
	{48,082}	{48,082}	{48,082}	{48,082}	[23.1]	[4.9]	[13.3]	[28.8]	[66.4]	[25.3]	[60.0]	[53.1]
Time Control	Year Linear	Year Dummies	3-Knot Month^2 Spline	7-Knot Month^3 Spline	Year Linear	Year Dummies	3-Knot Month^2 Spline	7-Knot Month^3 Spline	Year Linear	Year Dummies	3-Knot Month^2 Spline	7-Knot Month^3 Spline
Main Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Each cell reports the coefficient on in-borough shelter placement from a separate regressions of the row-delineated outcome on the treatment indicator using the supercolumn-enumerated method, controlling for Main covariates. Columns give alternative time trend controls. Standard errors clustered at family group level in parentheses. Number of observations given in braces. First-stage F-stats in brackets. * $p < 0.10$, ** $p < 0.05$

Table B.16: Regression Discontinuity Main Results: Wald Estimates

	No Controls				Controls			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Stays and Returns								
Log Length of Stay	1.986**	1.619**	1.557**	1.612**	1.205**	0.732**	0.705*	0.467
	(0.705)	(0.311)	(0.442)	(0.271)	(0.569)	(0.299)	(0.388)	(0.284)
	{7,679}	{15,436}	{7,430}	{14,925}	{7,679}	{15,436}	{7,430}	{14,925}
Subsidized Exit	0.353*	0.473**	0.406**	0.661**	0.128	0.231**	0.170	0.363**
	(0.211)	(0.109)	(0.152)	(0.106)	(0.184)	(0.108)	(0.141)	(0.111)
	{7,548}	{15,156}	{7,299}	{14,642}	{7,548}	{15,156}	{7,299}	{14,642}
Returned to Shelter	-0.067	-0.199**	-0.167	-0.247**	-0.060	-0.167*	-0.172	-0.220**
	(0.153)	(0.083)	(0.117)	(0.075)	(0.152)	(0.095)	(0.122)	(0.094)
	{6,798}	{13,725}	{6,590}	{13,268}	{6,798}	{13,725}	{6,590}	{13,268}
B. Year Post-Shelter Entry								
Cash Assistance	0.223	0.126	0.172	0.025	0.131	0.146*	0.248**	0.162**
	(0.188)	(0.087)	(0.126)	(0.076)	(0.151)	(0.085)	(0.112)	(0.082)
	{7,679}	{15,436}	{7,430}	{14,925}	{7,679}	{15,436}	{7,430}	{14,925}
Food Stamps	0.070	-0.049	-0.034	-0.137**	0.012	-0.002	0.052	0.018
	(0.130)	(0.062)	(0.089)	(0.056)	(0.089)	(0.050)	(0.065)	(0.049)
	{7,679}	{15,436}	{7,430}	{14,925}	{7,679}	{15,436}	{7,430}	{14,925}
Employed	0.001	-0.094	-0.275*	-0.268**	0.000	-0.027	-0.108	-0.083
	(0.223)	(0.108)	(0.159)	(0.098)	(0.189)	(0.106)	(0.138)	(0.102)
	{7,679}	{15,436}	{7,430}	{14,925}	{7,679}	{15,436}	{7,430}	{14,925}
Log Avg. Quarterly Earnings	0.881	0.059	-1.491	-1.131	0.567	0.306	-0.560	-0.100
	(1.623)	(0.776)	(1.130)	(0.690)	(1.333)	(0.745)	(0.970)	(0.718)
	{7,679}	{15,436}	{7,430}	{14,925}	{7,679}	{15,436}	{7,430}	{14,925}
C. Year Post-Shelter Exit								
Cash Assistance	0.403**	0.250**	0.354**	0.138*	0.303*	0.255**	0.373**	0.231**
	(0.191)	(0.096)	(0.140)	(0.084)	(0.172)	(0.103)	(0.135)	(0.099)
	{6,295}	{12,675}	{6,092}	{12,246}	{6,295}	{12,675}	{6,092}	{12,246}
Food Stamps	0.212	-0.031	0.107	-0.107*	0.130	0.008	0.157*	0.021
	(0.130)	(0.065)	(0.094)	(0.059)	(0.106)	(0.062)	(0.082)	(0.061)
	{6,295}	{12,675}	{6,092}	{12,246}	{6,295}	{12,675}	{6,092}	{12,246}
Employed	-0.147	-0.189*	-0.189	-0.287**	-0.170	-0.088	-0.009	-0.092
	(0.203)	(0.109)	(0.153)	(0.099)	(0.190)	(0.114)	(0.143)	(0.110)
	{6,295}	{12,675}	{6,092}	{12,246}	{6,295}	{12,675}	{6,092}	{12,246}
Log Avg. Quarterly Earnings	-0.901	-1.063	-1.404	-1.533**	-1.241	-0.603	-0.305	-0.458
	(1.485)	(0.793)	(1.126)	(0.714)	(1.372)	(0.826)	(1.039)	(0.798)
	{6,295}	{12,675}	{6,092}	{12,246}	{6,295}	{12,675}	{6,092}	{12,246}
First Stage	0.051**	0.076**	0.077**	0.089**	0.053**	0.068**	0.075**	0.073**
	(0.011)	(0.008)	(0.012)	(0.008)	(0.010)	(0.007)	(0.011)	(0.008)
	[20.4]	[90.3]	[44.1]	[117.8]	[25.9]	[83.8]	[50.0]	[89.8]
Bandwidth	{-1,0}	[-2,1]	{-1,1}	{-2,-1,1,2}	{-1,0}	[-2,1]	{-1,1}	{-2,-1,1,2}
Covariates	No	No	No	No	Yes	Yes	Yes	Yes

This table presents a more comprehensive set of Wald fuzzy regression discontinuity estimates. Each cell reports the coefficient on in-borough shelter placement from a separate 2SLS regression of the row-delineated outcome on the treatment indicator, using as the instrument an indicator for whether a family's oldest child's potential grade (end-of-calendar-year age year minus five) is zero or greater. Wald estimates pool the running variable for the given bandwidth; coefficients are thus instrumented mean comparisons between families without and with school-aged children. The first four columns have no covariates. The last four control for RD Main covariates. Standard errors clustered at family group level in parentheses. Number of observations given in braces. First-stage given for in-borough placement indicator. First-stage F-stat, in brackets, given for log length of stay regressions. * $p < 0.10$, ** $p < 0.05$

Table B.17: Regression Discontinuity Main Results: Linear Estimates

	No Controls					Controls				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Stays and Returns										
Log Length of Stay	1.611	0.910	2.075	1.357**	1.281**	1.505*	0.917	1.885*	1.065**	0.918**
	(0.993)	(0.959)	(1.340)	(0.436)	(0.354)	(0.827)	(0.855)	(1.042)	(0.331)	(0.285)
	{26,046}	{22,316}	{19,641}	{50,480}	{55,118}	{26,046}	{22,316}	{19,641}	{50,480}	{55,118}
Subsidized Exit	0.247	0.121	0.261	0.622**	0.479**	0.160	0.092	0.159	0.370**	0.256**
	(0.299)	(0.333)	(0.365)	(0.171)	(0.131)	(0.255)	(0.302)	(0.297)	(0.126)	(0.104)
	{25,543}	{21,886}	{19,284}	{49,334}	{53,907}	{25,543}	{21,886}	{19,284}	{49,334}	{53,907}
Returned to Shelter	0.226	0.204	0.212	0.013	-0.084	0.187	0.131	0.193	-0.042	-0.107
	(0.230)	(0.265)	(0.277)	(0.120)	(0.097)	(0.212)	(0.252)	(0.246)	(0.101)	(0.089)
	{23,141}	{19,860}	{17,508}	{44,574}	{48,712}	{23,141}	{19,860}	{17,508}	{44,574}	{48,712}
B. Year Post-Shelter Entry										
Cash Assistance	0.361	0.363	0.400	0.170	0.059	0.242	0.333	0.215	0.183**	0.104
	(0.301)	(0.318)	(0.381)	(0.128)	(0.107)	(0.216)	(0.253)	(0.254)	(0.092)	(0.079)
	{26,046}	{22,316}	{19,641}	{50,480}	{55,118}	{26,046}	{22,316}	{19,641}	{50,480}	{55,118}
Food Stamps	0.157	0.086	0.228	-0.037	-0.095	0.033	0.061	0.029	0.009	-0.018
	(0.203)	(0.214)	(0.261)	(0.090)	(0.077)	(0.125)	(0.143)	(0.146)	(0.055)	(0.048)
	{26,046}	{22,316}	{19,641}	{50,480}	{55,118}	{26,046}	{22,316}	{19,641}	{50,480}	{55,118}
Employed	0.090	-0.293	0.403	-0.123	-0.145	-0.019	-0.341	0.238	-0.081	-0.070
	(0.340)	(0.380)	(0.455)	(0.156)	(0.131)	(0.265)	(0.312)	(0.319)	(0.114)	(0.101)
	{26,046}	{22,316}	{19,641}	{50,480}	{55,118}	{26,046}	{22,316}	{19,641}	{50,480}	{55,118}
Log Avg. Quarterly Earnings	1.169	-2.488	4.022	-0.568	-0.747	0.374	-2.688	2.673	-0.277	-0.213
	(2.476)	(2.784)	(3.483)	(1.124)	(0.940)	(1.868)	(2.244)	(2.335)	(0.815)	(0.723)
	{26,046}	{22,316}	{19,641}	{50,480}	{55,118}	{26,046}	{22,316}	{19,641}	{50,480}	{55,118}
C. Year Post-Shelter Exit										
Cash Assistance	0.650**	0.672*	0.670*	0.398**	0.173	0.557**	0.666**	0.505*	0.347**	0.206**
	(0.301)	(0.349)	(0.378)	(0.152)	(0.119)	(0.253)	(0.312)	(0.294)	(0.120)	(0.099)
	{21,348}	{18,327}	{16,182}	{41,110}	{44,941}	{21,348}	{18,327}	{16,182}	{41,110}	{44,941}
Food Stamps	0.322*	0.299	0.346	0.091	-0.016	0.183	0.264	0.130	0.071	0.029
	(0.193)	(0.218)	(0.242)	(0.100)	(0.081)	(0.144)	(0.177)	(0.166)	(0.073)	(0.062)
	{21,348}	{18,327}	{16,182}	{41,110}	{44,941}	{21,348}	{18,327}	{16,182}	{41,110}	{44,941}
Employed	-0.132	-0.222	-0.017	-0.219	-0.268**	-0.178	-0.236	-0.056	-0.135	-0.134
	(0.283)	(0.322)	(0.349)	(0.162)	(0.135)	(0.255)	(0.292)	(0.298)	(0.128)	(0.113)
	{21,348}	{18,327}	{16,182}	{41,110}	{44,941}	{21,348}	{18,327}	{16,182}	{41,110}	{44,941}
Log Avg. Quarterly Earnings	-1.315	-2.477	-0.269	-1.606	-1.898*	-1.561	-2.406	-0.547	-0.909	-0.866
	(2.095)	(2.444)	(2.566)	(1.192)	(0.992)	(1.873)	(2.171)	(2.162)	(0.935)	(0.826)
	{21,348}	{18,327}	{16,182}	{41,110}	{44,941}	{21,348}	{18,327}	{16,182}	{41,110}	{44,941}
First Stage	0.033**	0.045**	0.032**	0.051**	0.057**	0.039**	0.050**	0.040**	0.058**	0.063**
	(0.014)	(0.018)	(0.014)	(0.013)	(0.011)	(0.013)	(0.016)	(0.013)	(0.012)	(0.010)
	[7.4]	[4.9]	[4.3]	[89.6]	[109.1]	[8.6]	[5.6]	[6.8]	[104.1]	[120.7]
Bandwidth	[-3,3]	[-3,3]	[-3,2]	[-3,12]	[-4,12]	[-3,3]	[-3,3]	[-3,2]	[-3,12]	[-4,12]
Includes Threshold	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes

This table presents a more comprehensive set of linear fuzzy regression discontinuity estimates. Each cell reports the coefficient on in-borough shelter placement from a separate 2SLS regression of the row-delineated outcome on the running variable (oldest child's potential grade; i.e., end-of-calendar-year age year minus five), the treatment indicator, and treatment interacted with the running variable, so as to allow for different slopes on either side of the threshold (school starting; i.e., potential grade zero). The instrument an indicator for whether a family's oldest child's potential grade is zero or greater; the interaction term is also instrumented. Reported coefficients are thus the difference in intercepts at the threshold. The first four columns have no covariates. The last four control for RD Main covariates. Standard errors clustered at family group level in parentheses. Number of observations given in braces. First-stage given for in-borough placement indicator. First-stage F-stat, in brackets, given for log length of stay regressions. * $p < 0.10$, ** $p < 0.05$

Table B.18: Regression Discontinuity Robustness: Alternative Samples for Borough Treatment

	Non-DV Sample				Pre-2015 Sample				One School Age Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Stays and Returns												
Log Length of Stay	1.070*	0.968**	0.107	0.590**	1.788**	1.426**	0.197	0.675**	1.588**	1.331**	0.645**	1.172**
	(0.632)	(0.265)	(0.267)	(0.192)	(0.771)	(0.284)	(0.322)	(0.319)	(0.620)	(0.264)	(0.277)	(0.395)
	{4,986}	{9,718}	{9,718}	{36,023}	{5,435}	{10,541}	{10,541}	{35,758}	{7,441}	{13,212}	{13,212}	{32,006}
Subsidized Exit	0.203	0.561**	0.263**	0.252**	0.236	0.513**	0.310**	0.255**	0.255	0.471**	0.290**	0.451**
	(0.246)	(0.123)	(0.119)	(0.085)	(0.229)	(0.103)	(0.121)	(0.115)	(0.196)	(0.097)	(0.104)	(0.148)
	{4,889}	{9,508}	{9,508}	{35,155}	{5,411}	{10,479}	{10,479}	{35,498}	{7,321}	{12,969}	{12,969}	{31,310}
Returned to Shelter	0.027	-0.150*	-0.118	-0.059	-0.124	-0.165**	-0.174*	-0.037	-0.070	-0.239**	-0.177**	0.024
	(0.173)	(0.081)	(0.092)	(0.070)	(0.182)	(0.077)	(0.101)	(0.097)	(0.148)	(0.073)	(0.088)	(0.124)
	{4,419}	{8,595}	{8,595}	{31,764}	{5,305}	{10,260}	{10,260}	{34,744}	{6,607}	{11,787}	{11,787}	{28,404}
B. Year Post-Shelter Entry												
Cash Assistance	0.437*	0.031	0.114	0.178**	0.230	0.055	0.145	0.102	0.213	0.071	0.155**	0.259**
	(0.234)	(0.089)	(0.086)	(0.062)	(0.208)	(0.080)	(0.092)	(0.088)	(0.177)	(0.077)	(0.079)	(0.110)
	{4,986}	{9,718}	{9,718}	{36,023}	{5,435}	{10,541}	{10,541}	{35,758}	{7,441}	{13,212}	{13,212}	{32,006}
Food Stamps	0.119	-0.152**	-0.033	-0.019	0.110	-0.100*	-0.026	-0.041	0.069	-0.064	0.053	0.038
	(0.143)	(0.064)	(0.051)	(0.037)	(0.141)	(0.056)	(0.054)	(0.053)	(0.122)	(0.055)	(0.048)	(0.064)
	{4,986}	{9,718}	{9,718}	{36,023}	{5,435}	{10,541}	{10,541}	{35,758}	{7,441}	{13,212}	{13,212}	{32,006}
Employed	0.127	-0.221*	-0.026	-0.090	-0.150	-0.342**	-0.118	-0.149	0.051	-0.008	-0.007	0.083
	(0.262)	(0.113)	(0.107)	(0.077)	(0.252)	(0.105)	(0.116)	(0.113)	(0.210)	(0.095)	(0.098)	(0.133)
	{4,986}	{9,718}	{9,718}	{36,023}	{5,435}	{10,541}	{10,541}	{35,758}	{7,441}	{13,212}	{13,212}	{32,006}
Log Avg. Quarterly Earnings	1.806	-1.032	0.081	-0.619	-0.116	-2.008**	-0.616	-0.745	1.257	0.697	0.265	0.858
	(1.957)	(0.811)	(0.760)	(0.555)	(1.774)	(0.735)	(0.808)	(0.802)	(1.539)	(0.693)	(0.694)	(0.949)
	{4,986}	{9,718}	{9,718}	{36,023}	{5,435}	{10,541}	{10,541}	{35,758}	{7,441}	{13,212}	{13,212}	{32,006}
C. Year Post-Shelter Exit												
Cash Assistance	0.561**	0.141	0.179*	0.260**	0.364*	0.093	0.157	0.226**	0.385**	0.173**	0.210**	0.478**
	(0.230)	(0.096)	(0.102)	(0.077)	(0.218)	(0.087)	(0.105)	(0.105)	(0.185)	(0.084)	(0.096)	(0.154)
	{4,091}	{7,912}	{7,912}	{29,238}	{5,234}	{10,112}	{10,112}	{34,229}	{6,144}	{10,882}	{10,882}	{26,170}
Food Stamps	0.225	-0.088	0.016	0.021	0.115	-0.130**	-0.058	-0.029	0.209*	-0.030	0.066	0.126
	(0.145)	(0.065)	(0.063)	(0.047)	(0.146)	(0.062)	(0.065)	(0.065)	(0.127)	(0.058)	(0.059)	(0.090)
	{4,091}	{7,912}	{7,912}	{29,238}	{5,234}	{10,112}	{10,112}	{34,229}	{6,144}	{10,882}	{10,882}	{26,170}
Employed	-0.020	-0.287**	-0.075	-0.168**	-0.275	-0.281**	-0.074	-0.172	-0.097	-0.095	-0.048	0.028
	(0.230)	(0.113)	(0.113)	(0.083)	(0.243)	(0.104)	(0.118)	(0.117)	(0.197)	(0.095)	(0.107)	(0.156)
	{4,091}	{7,912}	{7,912}	{29,238}	{5,234}	{10,112}	{10,112}	{34,229}	{6,144}	{10,882}	{10,882}	{26,170}
Log Avg. Quarterly Earnings	0.159	-1.582*	-0.271	-1.215**	-1.450	-1.694**	-0.518	-1.211	-0.616	-0.309	-0.345	0.056
	(1.698)	(0.825)	(0.824)	(0.607)	(1.744)	(0.747)	(0.852)	(0.858)	(1.444)	(0.699)	(0.777)	(1.140)
	{4,091}	{7,912}	{7,912}	{29,238}	{5,234}	{10,112}	{10,112}	{34,229}	{6,144}	{10,882}	{10,882}	{26,170}
First Stage	0.054**	0.093**	0.084**	0.064**	0.055**	0.101**	0.078**	0.069**	0.055**	0.096**	0.081**	0.061**
	(0.014)	(0.010)	(0.009)	(0.015)	(0.013)	(0.010)	(0.009)	(0.014)	(0.012)	(0.009)	(0.008)	(0.012)
	[15.3]	[85.4]	[79.8]	[94.6]	[16.6]	[106.1]	[73.4]	[86.1]	[23.0]	[115.9]	[97.7]	[76.3]
Order	Wald	Wald	Wald	Linear	Wald	Wald	Wald	Linear	Wald	Wald	Wald	Linear
Bandwidth	{-1,0}	{-2,-1,1,2}	{-2,-1,1,2}	[-3,12]	{-1,0}	{-2,-1,1,2}	{-2,-1,1,2}	[-3,12]	{-1,0}	{-2,-1,1,2}	{-2,-1,1,2}	[-3,12]
Threshold	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes
Covariates	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes

This table extends the fuzzy regression discontinuity analysis for three alternative samples, given in supercolumns. The Non-DV sample consists of families eligible for shelter for reasons other than domestic violence. The Pre-2015 sample consists of families entering shelter before 2014. The One School Age Sample consists of families with a single school-aged child. Each cell reports the coefficient on in-borough shelter placement from a separate 2SLS regression of the row-delineated outcome on the treatment indicator, using as the instrument an indicator for whether a family's oldest child's potential grade (end-of-calendar-year age year minus five) is zero (i.e., in-school) or greater. The first three columns for each sample present Wald estimates for varying bandwidths, while the fourth fits a linear regression on the running variable for the given bandwidths, allowing for different slopes on either side of the threshold. The first two columns for each sample have no covariates; the last two control for Main RD covariates. Standard errors clustered at family group level in parentheses. Number of observations given in braces. First-stage given for in-borough placement indicator. First-stage F-stat, in brackets, given for log length of stay regressions. * $p < 0.10$, ** $p < 0.05$

Table B.19: Regression Discontinuity Robustness: Distance Treatment

	Full Sample				Non-DV Sample				Pre-2015 Sample				One School Age Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
A. Stays and Returns																
Log Length of Stay	-0.266**	-0.199**	-0.054	-0.114**	-0.141	-0.121**	-0.014	-0.075**	-0.270*	-0.176**	-0.025	-0.082**	-0.195**	-0.160**	-0.081**	-0.138**
	(0.118)	(0.037)	(0.033)	(0.032)	(0.101)	(0.035)	(0.032)	(0.022)	(0.151)	(0.039)	(0.038)	(0.040)	(0.092)	(0.033)	(0.033)	(0.055)
	{7,028}	{13,639}	{13,639}	{46,282}	{4,611}	{8,928}	{8,928}	{33,311}	{4,938}	{9,577}	{9,577}	{32,635}	{6,807}	{12,073}	{12,073}	{29,280}
Subsidized Exit	-0.057	-0.084**	-0.043**	-0.041**	-0.025	-0.072**	-0.035**	-0.030**	-0.039	-0.065**	-0.039**	-0.034**	-0.041	-0.058**	-0.038**	-0.064**
	(0.036)	(0.015)	(0.013)	(0.013)	(0.039)	(0.017)	(0.014)	(0.010)	(0.042)	(0.014)	(0.015)	(0.015)	(0.030)	(0.013)	(0.013)	(0.022)
	{6,910}	{13,377}	{13,377}	{45,228}	{4,523}	{8,735}	{8,735}	{32,511}	{4,918}	{9,524}	{9,524}	{32,405}	{6,699}	{11,846}	{11,846}	{28,644}
Returned to Shelter	0.002	0.034**	0.026**	0.011	-0.020	0.019*	0.012	0.009	0.005	0.022**	0.021*	0.008	0.004	0.031**	0.021**	-0.006
	(0.027)	(0.010)	(0.011)	(0.011)	(0.032)	(0.011)	(0.011)	(0.009)	(0.034)	(0.010)	(0.012)	(0.012)	(0.024)	(0.010)	(0.011)	(0.019)
	{6,211}	{12,114}	{12,114}	{40,829}	{4,078}	{7,892}	{7,892}	{29,354}	{4,819}	{9,319}	{9,319}	{31,707}	{6,034}	{10,755}	{10,755}	{25,947}
B. Year Post-Shelter Entry																
Cash Assistance	-0.035	-0.003	-0.021**	-0.019**	-0.067*	-0.007	-0.017*	-0.022**	-0.041	-0.006	-0.018*	-0.012	-0.033	-0.008	-0.020**	-0.035**
	(0.030)	(0.010)	(0.010)	(0.009)	(0.040)	(0.012)	(0.010)	(0.007)	(0.038)	(0.011)	(0.011)	(0.011)	(0.026)	(0.009)	(0.009)	(0.016)
	{7,028}	{13,639}	{13,639}	{46,282}	{4,611}	{8,928}	{8,928}	{33,311}	{4,938}	{9,577}	{9,577}	{32,635}	{6,807}	{12,073}	{12,073}	{29,280}
Food Stamps	-0.009	0.017**	-0.002	0.002	-0.014	0.018**	0.004	0.002	-0.017	0.014*	0.003	0.008	-0.009	0.008	-0.006	-0.006
	(0.020)	(0.008)	(0.006)	(0.005)	(0.022)	(0.008)	(0.006)	(0.004)	(0.025)	(0.008)	(0.006)	(0.007)	(0.017)	(0.007)	(0.006)	(0.009)
	{7,028}	{13,639}	{13,639}	{46,282}	{4,611}	{8,928}	{8,928}	{33,311}	{4,938}	{9,577}	{9,577}	{32,635}	{6,807}	{12,073}	{12,073}	{29,280}
Employed	0.002	0.032**	0.008	0.015	-0.023	0.028*	0.007	0.016*	0.032	0.040**	0.011	0.022	-0.006	0.000	-0.000	-0.010
	(0.034)	(0.013)	(0.012)	(0.011)	(0.041)	(0.015)	(0.013)	(0.009)	(0.045)	(0.014)	(0.014)	(0.014)	(0.030)	(0.012)	(0.012)	(0.018)
	{7,028}	{13,639}	{13,639}	{46,282}	{4,611}	{8,928}	{8,928}	{33,311}	{4,938}	{9,577}	{9,577}	{32,635}	{6,807}	{12,073}	{12,073}	{29,280}
Log Avg. Quarterly Earnings	-0.142	0.127	-0.002	0.070	-0.338	0.124	0.008	0.107*	0.048	0.223**	0.040	0.107	-0.186	-0.094	-0.042	-0.126
	(0.250)	(0.091)	(0.084)	(0.081)	(0.317)	(0.106)	(0.091)	(0.063)	(0.310)	(0.097)	(0.096)	(0.101)	(0.224)	(0.086)	(0.082)	(0.132)
	{7,028}	{13,639}	{13,639}	{46,282}	{4,611}	{8,928}	{8,928}	{33,311}	{4,938}	{9,577}	{9,577}	{32,635}	{6,807}	{12,073}	{12,073}	{29,280}
C. Year Post-Shelter Exit																
Cash Assistance	-0.073*	-0.015	-0.026**	-0.039**	-0.091**	-0.017	-0.020*	-0.027**	-0.069	-0.009	-0.017	-0.026*	-0.065**	-0.019*	-0.025**	-0.074**
	(0.038)	(0.011)	(0.012)	(0.014)	(0.046)	(0.013)	(0.012)	(0.009)	(0.046)	(0.012)	(0.013)	(0.013)	(0.033)	(0.011)	(0.012)	(0.028)
	{5,743}	{11,166}	{11,166}	{37,616}	{3,774}	{7,254}	{7,254}	{26,999}	{4,751}	{9,184}	{9,184}	{31,229}	{5,603}	{9,913}	{9,913}	{23,882}
Food Stamps	-0.034	0.015*	-0.003	-0.006	-0.036	0.011	-0.003	-0.002	-0.016	0.019**	0.007	0.007	-0.032	0.004	-0.008	-0.022
	(0.024)	(0.008)	(0.007)	(0.009)	(0.026)	(0.009)	(0.008)	(0.006)	(0.027)	(0.008)	(0.008)	(0.008)	(0.021)	(0.007)	(0.007)	(0.015)
	{5,743}	{11,166}	{11,166}	{37,616}	{3,774}	{7,254}	{7,254}	{26,999}	{4,751}	{9,184}	{9,184}	{31,229}	{5,603}	{9,913}	{9,913}	{23,882}
Employed	0.019	0.033**	0.006	0.019	-0.006	0.035**	0.009	0.020**	0.048	0.032**	0.003	0.023	0.011	0.007	0.001	-0.011
	(0.035)	(0.014)	(0.013)	(0.015)	(0.039)	(0.015)	(0.014)	(0.010)	(0.047)	(0.014)	(0.014)	(0.015)	(0.031)	(0.012)	(0.013)	(0.026)
	{5,743}	{11,166}	{11,166}	{37,616}	{3,774}	{7,254}	{7,254}	{26,999}	{4,751}	{9,184}	{9,184}	{31,229}	{5,603}	{9,913}	{9,913}	{23,882}
Log Avg. Quarterly Earnings	0.102	0.163*	0.021	0.124	-0.091	0.185*	0.033	0.152**	0.233	0.176*	0.013	0.159	0.058	-0.000	0.004	-0.075
	(0.255)	(0.098)	(0.097)	(0.110)	(0.291)	(0.110)	(0.100)	(0.073)	(0.329)	(0.101)	(0.104)	(0.109)	(0.231)	(0.090)	(0.095)	(0.186)
	{5,743}	{11,166}	{11,166}	{37,616}	{3,774}	{7,254}	{7,254}	{26,999}	{4,751}	{9,184}	{9,184}	{31,229}	{5,603}	{9,913}	{9,913}	{23,882}
First Stage	-0.350**	-0.706**	-0.645**	-0.520**	-0.368**	-0.745**	-0.734**	-0.545**	-0.329**	-0.796**	-0.680**	-0.553**	-0.402**	-0.813**	-0.718**	-0.504**
	(0.110)	(0.080)	(0.074)	(0.114)	(0.134)	(0.095)	(0.089)	(0.141)	(0.129)	(0.092)	(0.086)	(0.132)	(0.112)	(0.086)	(0.079)	(0.117)
	[10.1]	[78.7]	[75.5]	[80.2]	[7.5]	[61.0]	[67.7]	[74.1]	[6.5]	[74.4]	[63.2]	[56.9]	[12.9]	[89.7]	[82.3]	[49.1]
Order	Wald	Wald	Wald	Linear	Wald	Wald	Wald	Linear	Wald	Wald	Wald	Linear	Wald	Wald	Wald	Linear
Bandwidth	{-1,0}	{-2,-1,1,2}	{-2,-1,1,2}	[-3,12]	{-1,0}	{-2,-1,1,2}	{-2,-1,1,2}	[-3,12]	{-1,0}	{-2,-1,1,2}	{-2,-1,1,2}	[-3,12]	{-1,0}	{-2,-1,1,2}	{-2,-1,1,2}	[-3,12]
Threshold	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes
Covariates	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes

This table extends the fuzzy regression discontinuity analysis for distance treatment, measured in miles between origin and shelter address. Supercolumns give samples. Each cell reports the coefficient on placement distance from a separate 2SLS regression of the row-delineated outcome on distance treatment, using as the instrument an indicator for whether a family's oldest child's potential grade (end-of-calendar-year age year minus five) is zero (i.e., in-school) or greater. The first three columns for each sample present Wald estimates for varying bandwidths, while the fourth fits a linear regression on the running variable for the given bandwidths, allowing for different slopes on either side of the threshold. The first two columns for each sample have no covariates; the last two control for Main RD covariates. Standard errors clustered at family group level in parentheses. Number of observations given in braces. First-stage given for distance treatment. First-stage F-stat, in brackets, given for log length of stay regressions.
* $p < 0.10$, ** $p < 0.05$

Table B.20: Regression Discontinuity Robustness: School Year Running Variable Definition

	No Controls				Controls			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Stays and Returns								
Log Length of Stay	1.998**	1.419**	0.979	0.874**	0.975	-0.061	0.675	0.670**
	(0.900)	(0.323)	(1.395)	(0.324)	(0.784)	(0.363)	(1.289)	(0.321)
	{7,384}	{14,306}	{25,160}	{48,230}	{7,384}	{14,306}	{25,160}	{48,230}
Subsidized Exit	0.462	0.666**	-0.188	0.521**	0.183	0.254*	-0.319	0.323**
	(0.282)	(0.134)	(0.450)	(0.138)	(0.272)	(0.141)	(0.456)	(0.129)
	{7,240}	{14,027}	{24,651}	{47,108}	{7,240}	{14,027}	{24,651}	{47,108}
Returned to Shelter	-0.303	-0.240**	0.102	-0.112	-0.285	-0.217*	0.011	-0.147
	(0.208)	(0.095)	(0.340)	(0.100)	(0.214)	(0.123)	(0.321)	(0.100)
	{6,487}	{12,676}	{22,300}	{42,506}	{6,487}	{12,676}	{22,300}	{42,506}
B. Year Post-Shelter Entry Outcomes								
Cash Assistance	0.235	0.057	0.587	0.212**	0.210	0.250**	0.375	0.207**
	(0.242)	(0.094)	(0.509)	(0.106)	(0.223)	(0.109)	(0.395)	(0.096)
	{7,384}	{14,306}	{25,160}	{48,230}	{7,384}	{14,306}	{25,160}	{48,230}
Food Stamps	0.032	-0.138*	0.325	-0.007	0.085	0.055	0.159	0.048
	(0.169)	(0.070)	(0.338)	(0.075)	(0.131)	(0.064)	(0.228)	(0.060)
Employed	-0.264	-0.278**	0.099	-0.129	-0.149	0.027	-0.064	-0.118
	(0.301)	(0.121)	(0.525)	(0.127)	(0.278)	(0.131)	(0.463)	(0.117)
Log Avg. Quarterly Earnings	-1.614	-1.113	-0.358	-1.039	-1.217	0.663	-1.465	-0.909
	(2.155)	(0.855)	(3.783)	(0.924)	(1.972)	(0.931)	(3.344)	(0.824)
C. Year Post-Shelter Exit Outcomes								
Cash Assistance	0.164	0.180*	0.688	0.333**	0.161	0.322**	0.591	0.298**
	(0.233)	(0.106)	(0.486)	(0.137)	(0.232)	(0.132)	(0.421)	(0.125)
	{5,986}	{11,666}	{20,531}	{39,145}	{5,986}	{11,666}	{20,531}	{39,145}
Food Stamps	-0.166	-0.126*	0.127	-0.031	-0.128	0.060	0.006	0.002
	(0.170)	(0.076)	(0.280)	(0.089)	(0.148)	(0.080)	(0.224)	(0.073)
Employed	-0.356	-0.293**	-0.232	-0.271*	-0.207	0.029	-0.244	-0.191
	(0.283)	(0.125)	(0.464)	(0.149)	(0.270)	(0.142)	(0.428)	(0.133)
Log Avg. Quarterly Earnings	-1.962	-1.439	-2.227	-1.787	-1.129	0.468	-2.179	-1.087
	(2.023)	(0.898)	(3.475)	(1.088)	(1.934)	(1.030)	(3.152)	(0.964)
	{5,986}	{11,666}	{20,531}	{39,145}	{5,986}	{11,666}	{20,531}	{39,145}
First Stage	0.040**	0.074**	0.013	0.031**	0.037**	0.058**	0.013	0.029**
	(0.012)	(0.008)	(0.014)	(0.013)	(0.011)	(0.008)	(0.013)	(0.012)
	[12.0]	[76.7]	[8.3]	[88.3]	[12.3]	[54.1]	[9.2]	[95.8]
Order	Wald	Wald	Linear	Linear	Wald	Wald	Linear	Linear
Bandwidth	{-1,0}	{-2,-1,1,2}	[-3,3]	[-3,12]	{-1,0}	{-2,-1,1,2}	[-3,3]	[-3,12]
Threshold	Yes	No	Yes	Yes	Yes	No	Yes	Yes
Covariates	No	No	No	No	Yes	Yes	Yes	Yes

The table presents fuzzy regression discontinuity analysis using families' oldest children's potential grade levels, adjusted for timing of shelter entry relative to the school year as the running variable (i.e., end-of-calendar-year age year minus five for July-December shelter entrants and end-of-calendar-year age year minus six for January-June shelter entrants). Each cell reports the coefficient on in-borough shelter placement from a separate 2SLS regression of the row-delineated outcome on the treatment indicator, using as the instrument an indicator for whether a family's oldest child's potential grade is zero or greater. Columns 1, 2, 5, and 6 give Wald estimates pooling the running variable for the given bandwidth; coefficients are thus instrumented mean comparisons between families without and with school-aged children. Columns 3, 4, 7, and 8 fit linear regressions on the running variable for the given bandwidths, allowing for different slopes on either side of the threshold; the coefficients are the difference in intercepts at the threshold. Columns 1, 3, 5, and 7 include the threshold in the analysis; Columns 2, 4, 6, and 8 exclude it. The last four columns control for Main RD covariates. Standard errors clustered at family group level in parentheses. Number of observations given in braces below first outcome in each panel, as well as for any subsequent outcome where the sample size differs due to censoring. First-stage given for in-borough placement indicator. First-stage F-stat, in brackets, given for log length of stay regressions. * $p < 0.10$, ** $p < 0.05$

Table B.21A: Regression Discontinuity Baseline Covariates

	Wald				Linear	
	(1)	(2)	(3)	(4)	(5)	(6)
Month Entered Shelter	-1.284	0.965	0.656	1.068*	-1.870	0.188
	(1.506)	(1.049)	(0.628)	(0.642)	(2.321)	(1.040)
Year Entered Shelter	2.147*	2.206**	1.862**	1.950**	2.003	2.778**
	(1.108)	(0.770)	(0.462)	(0.470)	(1.643)	(0.837)
Manhattan Origin	-0.112	-0.033	-0.062	-0.065	0.071	0.018
	(0.152)	(0.103)	(0.063)	(0.063)	(0.233)	(0.106)
Bronx Origin	-0.243	0.084	-0.038	0.043	-0.283	-0.107
	(0.230)	(0.148)	(0.092)	(0.092)	(0.352)	(0.155)
Brooklyn Origin	0.107	-0.044	0.081	0.056	0.056	0.060
	(0.208)	(0.141)	(0.087)	(0.087)	(0.313)	(0.144)
Queens Origin	0.205	-0.004	0.031	-0.011	0.159	0.032
	(0.156)	(0.098)	(0.061)	(0.061)	(0.231)	(0.101)
Staten Island Origin	0.044	-0.002	-0.013	-0.023	-0.004	-0.003
	(0.073)	(0.046)	(0.029)	(0.029)	(0.107)	(0.048)
Family Size	2.227**	3.311**	4.096**	4.487**	-0.949	1.140**
	(0.685)	(0.599)	(0.425)	(0.463)	(0.759)	(0.402)
Family Members Under 18	2.441**	3.240**	4.105**	4.412**	-0.672	1.901**
	(0.660)	(0.556)	(0.408)	(0.439)	(0.597)	(0.434)
Health Issue Present	0.135	0.216	0.173**	0.202**	-0.033	-0.078
	(0.200)	(0.140)	(0.085)	(0.087)	(0.299)	(0.136)
Eligibility: Eviction	0.474**	0.647**	0.709**	0.798**	0.010	0.074
	(0.216)	(0.161)	(0.100)	(0.106)	(0.297)	(0.136)
Eligibility: Overcrowding	0.263	0.021	0.069	0.011	0.219	0.103
	(0.178)	(0.115)	(0.070)	(0.070)	(0.264)	(0.116)
Eligibility: Conditions	-0.158	-0.305**	-0.126**	-0.160**	-0.184	-0.190**
	(0.137)	(0.098)	(0.055)	(0.055)	(0.208)	(0.095)
Eligibility: Domestic Violence	-0.348	-0.292**	-0.515**	-0.539**	0.025	0.098
	(0.216)	(0.145)	(0.094)	(0.094)	(0.325)	(0.151)
Eligibility: Other	-0.231	-0.071	-0.141**	-0.113*	-0.074	-0.086
	(0.150)	(0.096)	(0.060)	(0.059)	(0.215)	(0.099)
Female	-0.061	-0.031	-0.058	-0.062	0.047	-0.076
	(0.113)	(0.076)	(0.048)	(0.048)	(0.171)	(0.081)
Age	16.141**	24.170**	27.804**	31.521**	0.138	-1.438
	(4.616)	(4.190)	(2.839)	(3.173)	(4.779)	(2.547)
Partner/Spouse Present	-0.266	-0.073	-0.053	-0.012	-0.307	0.094
	(0.205)	(0.135)	(0.082)	(0.082)	(0.313)	(0.138)
Pregnant	-0.390**	-0.143*	-0.169**	-0.130**	-0.314	-0.114
	(0.144)	(0.084)	(0.052)	(0.051)	(0.205)	(0.086)
Obs.	7,679	7,430	18,655	14,925	26,046	50,480
Order	Wald	Wald	Wald	Wald	Linear	Linear
Bandwidth	{-1,0}	{-1,1}	[-2,2]	{-2,-1,1,2}	[-3,3]	[-3,12]
Threshold	Yes	No	Yes	No	Yes	Yes
Covariates	No	No	No	No	No	No

This table assesses the plausibility of the fuzzy regression discontinuity design by checking whether baseline covariates are similar on both sides of the treatment threshold (oldest child of school-starting age). Each cell reports the coefficient on in-borough shelter placement from a separate 2SLS regression of the row-delineated characteristic on the treatment indicator, using as the instrument an indicator for whether a family's oldest child's potential grade (end-of-calendar-year age year minus five) is zero or greater. The first four columns present Wald estimates (pooled instrumented mean comparisons), while the last two present linear estimates, allowing for different slopes on either side of the threshold. Within these groups, columns vary by bandwidth and whether the threshold itself is included. Standard errors clustered at family group level in parentheses. * $p < 0.10$, ** $p < 0.05$

Table B.21B: Regression Discontinuity Baseline Covariates

	Wald				Linear	
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.060	-0.035	-0.017	-0.049	0.344	0.165
	(0.219)	(0.150)	(0.093)	(0.093)	(0.347)	(0.157)
Hispanic	-0.104	-0.011	0.022	0.059	-0.405	-0.228
	(0.215)	(0.147)	(0.090)	(0.091)	(0.348)	(0.159)
White	0.042	0.040	-0.001	-0.015	0.071	0.007
	(0.068)	(0.047)	(0.028)	(0.027)	(0.104)	(0.046)
Asian	0.024	0.024	0.017	0.016	0.050	0.046**
	(0.026)	(0.020)	(0.012)	(0.012)	(0.044)	(0.020)
No Degree	0.278	0.089	-0.080	-0.136	0.365	-0.025
	(0.226)	(0.150)	(0.093)	(0.094)	(0.350)	(0.154)
High School Grad	-0.260	-0.322**	-0.083	-0.087	-0.331	-0.243
	(0.215)	(0.150)	(0.088)	(0.089)	(0.329)	(0.151)
Some College or More	0.049	0.131*	0.094**	0.110**	0.010	0.111*
	(0.093)	(0.068)	(0.040)	(0.042)	(0.140)	(0.067)
Unknown Education	-0.067	0.102	0.069	0.112**	-0.043	0.157**
	(0.102)	(0.073)	(0.043)	(0.045)	(0.156)	(0.075)
On Cash Assistance	0.161	0.137	0.010	-0.007	0.199	0.215
	(0.221)	(0.150)	(0.090)	(0.091)	(0.337)	(0.154)
On Food Stamps	0.142	0.048	-0.074	-0.124	0.331	0.123
	(0.193)	(0.131)	(0.080)	(0.082)	(0.311)	(0.134)
Employed Year Pre	0.086	-0.038	-0.017	-0.081	0.087	-0.092
	(0.225)	(0.153)	(0.094)	(0.095)	(0.351)	(0.164)
Log AQ Earnings Year Pre	0.931	0.215	0.887	0.513	0.662	-0.689
	(1.597)	(1.090)	(0.670)	(0.674)	(2.487)	(1.167)
Tier II Shelter	-0.282	-0.207	-0.326**	-0.338**	-0.720*	-0.392**
	(0.229)	(0.154)	(0.096)	(0.097)	(0.401)	(0.172)
Commercial Hotel	-0.028	-0.211	-0.245**	-0.279**	0.767**	0.199
	(0.201)	(0.137)	(0.085)	(0.085)	(0.390)	(0.161)
Family Cluster Unit	0.275*	0.405**	0.563**	0.614**	-0.085	0.195*
	(0.162)	(0.120)	(0.079)	(0.084)	(0.233)	(0.110)
Mahattan Shelter	-0.122	-0.275**	-0.475**	-0.531**	0.031	-0.404**
	(0.174)	(0.117)	(0.076)	(0.077)	(0.275)	(0.123)
Bronx Shelter	-0.026	0.178	0.406**	0.454**	-0.248	0.156
	(0.216)	(0.143)	(0.089)	(0.090)	(0.348)	(0.147)
Brooklyn Shelter	0.421**	0.302**	0.438**	0.431**	0.196	0.324**
	(0.206)	(0.136)	(0.085)	(0.086)	(0.295)	(0.137)
Queens Shelter	-0.303*	-0.227**	-0.358**	-0.342**	-0.040	-0.089
	(0.167)	(0.110)	(0.071)	(0.070)	(0.249)	(0.116)
Staten Island Shelter	0.029	0.023	-0.011	-0.011	0.060	0.013
	(0.039)	(0.027)	(0.017)	(0.017)	(0.063)	(0.027)
Obs.	7,679	7,430	18,655	14,925	26,046	50,480
Order	Wald	Wald	Wald	Wald	Linear	Linear
Bandwidth	{-1,0}	{-1,1}	[-2,2]	{-2,-1,1,2}	[-3,3]	[-3,12]
Threshold	Yes	No	Yes	No	Yes	Yes
Covariates	No	No	No	No	No	No

This table assesses the plausibility of the fuzzy regression discontinuity design by checking whether baseline covariates are similar on both sides of the treatment threshold (oldest child of school-starting age). Each cell reports the coefficient on in-borough shelter placement from a separate 2SLS regression of the row-delineated characteristic on the treatment indicator, using as the instrument an indicator for whether a family's oldest child's potential grade (end-of-calendar-year age year minus five) is zero or greater. The first four columns present Wald estimates (pooled instrumented mean comparisons), while the last two present linear estimates, allowing for different slopes on either side of the threshold. Within these groups, columns vary by bandwidth and whether the threshold itself is included. Standard errors clustered at family group level in parentheses. * $p < 0.10$, ** $p < 0.05$

Table B.22: Complier Characteristics: Ineligibility Rate Instrument

	Compliers	Non-Compliers	Diff.
Manhattan Origin	0.00	0.14	-0.13
	(0.003)	(0.000)	[-2.56]
Bronx Origin	0.57	0.39	0.18
	(0.006)	(0.000)	[2.28]
Brooklyn Origin	0.25	0.32	-0.07
	(0.005)	(0.000)	[-0.99]
Queens Origin	0.10	0.13	-0.02
	(0.003)	(0.000)	[-0.45]
Staten Island Origin	0.02	0.03	-0.00
	(0.000)	(0.000)	[-0.33]
Health Issue Present	0.33	0.30	0.04
	(0.004)	(0.000)	[0.61]
Eligibility: Eviction	0.29	0.34	-0.05
	(0.005)	(0.000)	[-0.67]
Eligibility: Overcrowding	0.16	0.18	-0.02
	(0.003)	(0.000)	[-0.34]
Eligibility: Conditions	0.11	0.08	0.03
	(0.002)	(0.000)	[0.67]
Eligibility: Domestic Violence	0.30	0.30	0.00
	(0.004)	(0.000)	[0.01]
Eligibility: Other	0.08	0.11	-0.03
	(0.003)	(0.000)	[-0.67]
Female	0.97	0.91	0.06
	(0.002)	(0.000)	[1.26]
Partner/Spouse Present	0.31	0.25	0.06
	(0.004)	(0.000)	[0.99]
Pregnant	0.04	0.07	-0.03
	(0.001)	(0.000)	[-0.86]
Black	0.43	0.57	-0.14
	(0.006)	(0.000)	[-1.79]
Hispanic	0.46	0.37	0.09
	(0.006)	(0.000)	[1.18]
White	0.06	0.02	0.04
	(0.001)	(0.000)	[1.57]
No Degree	0.61	0.57	0.05
	(0.005)	(0.000)	[0.67]
High School Grad	0.30	0.32	-0.02
	(0.005)	(0.000)	[-0.29]
Some College or More	0.06	0.05	0.01
	(0.001)	(0.000)	[0.21]
Unknown Education	0.02	0.07	-0.05
	(0.001)	(0.000)	[-1.27]
On Cash Assistance	0.30	0.36	-0.06
	(0.005)	(0.000)	[-0.78]
On Food Stamps	0.75	0.73	0.02
	(0.006)	(0.000)	[0.29]
Employed Year Pre	0.39	0.44	-0.05
	(0.005)	(0.000)	[-0.67]
Tier II Shelter	0.63	0.54	0.08
	(0.004)	(0.000)	[1.27]
Commercial Hotel	0.08	0.29	-0.21
	(0.005)	(0.000)	[-3.08]
Family Cluster Unit	0.19	0.16	0.02
	(0.003)	(0.000)	[0.46]
Family Size 1–3	0.54	0.64	-0.11
	(0.006)	(0.000)	[-1.43]
Family Size 4–5	0.39	0.28	0.11
	(0.004)	(0.000)	[1.68]
Family Size 6+	0.07	0.08	-0.01
	(0.002)	(0.000)	[-0.27]
Age	31.69	31.53	0.16
	(1.350)	(0.013)	[0.14]
Log AQ Earnings Year Pre	2.73	3.03	-0.30
	(0.259)	(0.002)	[-0.60]

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 families 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 Chapter 1. Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family group.

Table B.23: Complier Characteristics: Aversion Ratio Instrument

	Compliers	Non-Compliers	Diff.
Manhattan Origin	0.09	0.13	-0.04
	(0.001)	(0.000)	[-1.18]
Bronx Origin	0.55	0.39	0.16
	(0.004)	(0.000)	[2.67]
Brooklyn Origin	0.20	0.33	-0.13
	(0.003)	(0.000)	[-2.29]
Queens Origin	0.12	0.13	-0.01
	(0.002)	(0.000)	[-0.17]
Staten Island Origin	0.03	0.03	0.01
	(0.000)	(0.000)	[0.51]
Health Issue Present	0.35	0.29	0.06
	(0.002)	(0.000)	[1.27]
Eligibility: Eviction	0.33	0.34	-0.01
	(0.003)	(0.000)	[-0.13]
Eligibility: Overcrowding	0.15	0.18	-0.03
	(0.002)	(0.000)	[-0.61]
Eligibility: Conditions	0.11	0.08	0.02
	(0.001)	(0.000)	[0.73]
Eligibility: Domestic Violence	0.27	0.30	-0.03
	(0.002)	(0.000)	[-0.59]
Eligibility: Other	0.10	0.11	-0.01
	(0.001)	(0.000)	[-0.28]
Female	0.94	0.91	0.03
	(0.001)	(0.000)	[0.82]
Partner/Spouse Present	0.28	0.25	0.03
	(0.002)	(0.000)	[0.61]
Pregnant	0.05	0.07	-0.02
	(0.001)	(0.000)	[-0.66]
Black	0.47	0.57	-0.09
	(0.004)	(0.000)	[-1.51]
Hispanic	0.43	0.37	0.06
	(0.003)	(0.000)	[0.98]
White	0.05	0.02	0.03
	(0.000)	(0.000)	[1.46]
No Degree	0.59	0.57	0.02
	(0.003)	(0.000)	[0.41]
High School Grad	0.31	0.32	-0.01
	(0.002)	(0.000)	[-0.21]
Some College or More	0.07	0.05	0.02
	(0.001)	(0.000)	[0.76]
Unknown Education	0.02	0.07	-0.04
	(0.001)	(0.000)	[-1.47]
On Cash Assistance	0.23	0.37	-0.14
	(0.003)	(0.000)	[-2.43]
On Food Stamps	0.70	0.74	-0.04
	(0.003)	(0.000)	[-0.63]
Employed Year Pre	0.39	0.44	-0.05
	(0.003)	(0.000)	[-0.91]
Tier II Shelter	0.59	0.55	0.04
	(0.003)	(0.000)	[0.82]
Commercial Hotel	0.14	0.29	-0.16
	(0.003)	(0.000)	[-2.83]
Family Cluster Unit	0.19	0.16	0.02
	(0.001)	(0.000)	[0.63]
Family Size 1–3	0.60	0.64	-0.04
	(0.003)	(0.000)	[-0.76]
Family Size 4–5	0.35	0.28	0.06
	(0.003)	(0.000)	[1.26]
Family Size 6+	0.05	0.08	-0.02
	(0.001)	(0.000)	[-0.82]
Age	32.20	31.47	0.73
	(0.949)	(0.014)	[0.74]
Log AQ Earnings Year Pre	2.76	3.03	-0.27
	(0.147)	(0.002)	[-0.71]

Main sample. Treatment is in-borough placement. Instrument is 15-day moving average of the aversion ratio. Compliers are families placed in-borough when the aversion ratio 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 Chapter 1. Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family group.

Table B.24: Compliance Type Shares: Regression Discontinuity

	1%	1.5%	2%
Compliers	0.01	0.01	0.01
Always-Takers	0.67	0.67	0.67
Never-Takers	0.33	0.33	0.33

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 Cassidy (2019) for estimation method details.

Table B.25: Complier Characteristics: Regression Discontinuity

	Compliers	Non-Compliers	Diff.
Manhattan Origin	-0.03	0.13	-0.16
	(0.000)	(0.000)	[-13.42]
Bronx Origin	0.56	0.41	0.16
	(0.000)	(0.000)	[8.37]
Brooklyn Origin	0.50	0.31	0.19
	(0.000)	(0.000)	[9.94]
Queens Origin	-0.07	0.13	-0.20
	(0.000)	(0.000)	[-14.23]
Staten Island Origin	0.01	0.03	-0.02
	(0.000)	(0.000)	[-4.54]
Health Issue Present	0.28	0.30	-0.02
	(0.000)	(0.000)	[-1.69]
Eligibility: Eviction	0.26	0.34	-0.08
	(0.000)	(0.000)	[-4.89]
Eligibility: Overcrowding	0.16	0.18	-0.02
	(0.000)	(0.000)	[-1.38]
Eligibility: Conditions	0.07	0.08	-0.01
	(0.000)	(0.000)	[-1.66]
Eligibility: Domestic Violence	0.20	0.30	-0.10
	(0.000)	(0.000)	[-6.34]
Eligibility: Other	0.09	0.11	-0.02
	(0.000)	(0.000)	[-1.85]
Female	0.91	0.92	-0.01
	(0.000)	(0.000)	[-0.98]
Partner/Spouse Present	0.27	0.26	0.01
	(0.000)	(0.000)	[0.86]
Pregnant	0.08	0.07	0.01
	(0.000)	(0.000)	[0.94]
Black	0.58	0.56	0.02
	(0.000)	(0.000)	[1.27]
Hispanic	0.36	0.38	-0.02
	(0.000)	(0.000)	[-1.08]
White	0.02	0.03	-0.00
	(0.000)	(0.000)	[-0.83]
No Degree	0.59	0.57	0.02
	(0.000)	(0.000)	[0.88]
High School Grad	0.32	0.32	-0.00
	(0.000)	(0.000)	[-0.22]
Some College or More	0.06	0.05	0.01
	(0.000)	(0.000)	[1.14]
Unknown Education	0.04	0.06	-0.02
	(0.000)	(0.000)	[-2.09]
On Cash Assistance	0.39	0.35	0.03
	(0.000)	(0.000)	[1.90]
On Food Stamps	0.77	0.73	0.04
	(0.000)	(0.000)	[2.39]
Employed Year Pre	0.46	0.43	0.02
	(0.000)	(0.000)	[1.16]
Tier II Shelter	0.64	0.55	0.09
	(0.000)	(0.000)	[4.69]
Commercial Hotel	0.16	0.28	-0.11
	(0.000)	(0.000)	[-7.49]
Family Cluster Unit	0.08	0.17	-0.09
	(0.000)	(0.000)	[-5.68]
Family Size 1–3	0.86	0.63	0.23
	(0.000)	(0.000)	[10.71]
Family Size 4–5	0.20	0.29	-0.09
	(0.000)	(0.000)	[-5.05]
Family Size 6+	-0.00	0.08	-0.08
	(0.000)	(0.000)	[-5.23]
Age	27.87	32.38	-4.51
	(0.118)	(0.008)	[-12.74]
Log AQ Earnings Year Pre	3.06	3.00	0.06
	(0.017)	(0.001)	[0.45]

Main sample. Treatment is in-borough placement. Instrument is an indicator for whether a family's oldest child's potential grade is zero (kindergarten) or greater. Compliers are families placed in-borough when they have school-aged children, 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 Cassidy (2019). Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family group.

B.6 Supplementary Figures

Figure B.1: Policy Instrument Time Series: Seasonally Detrended

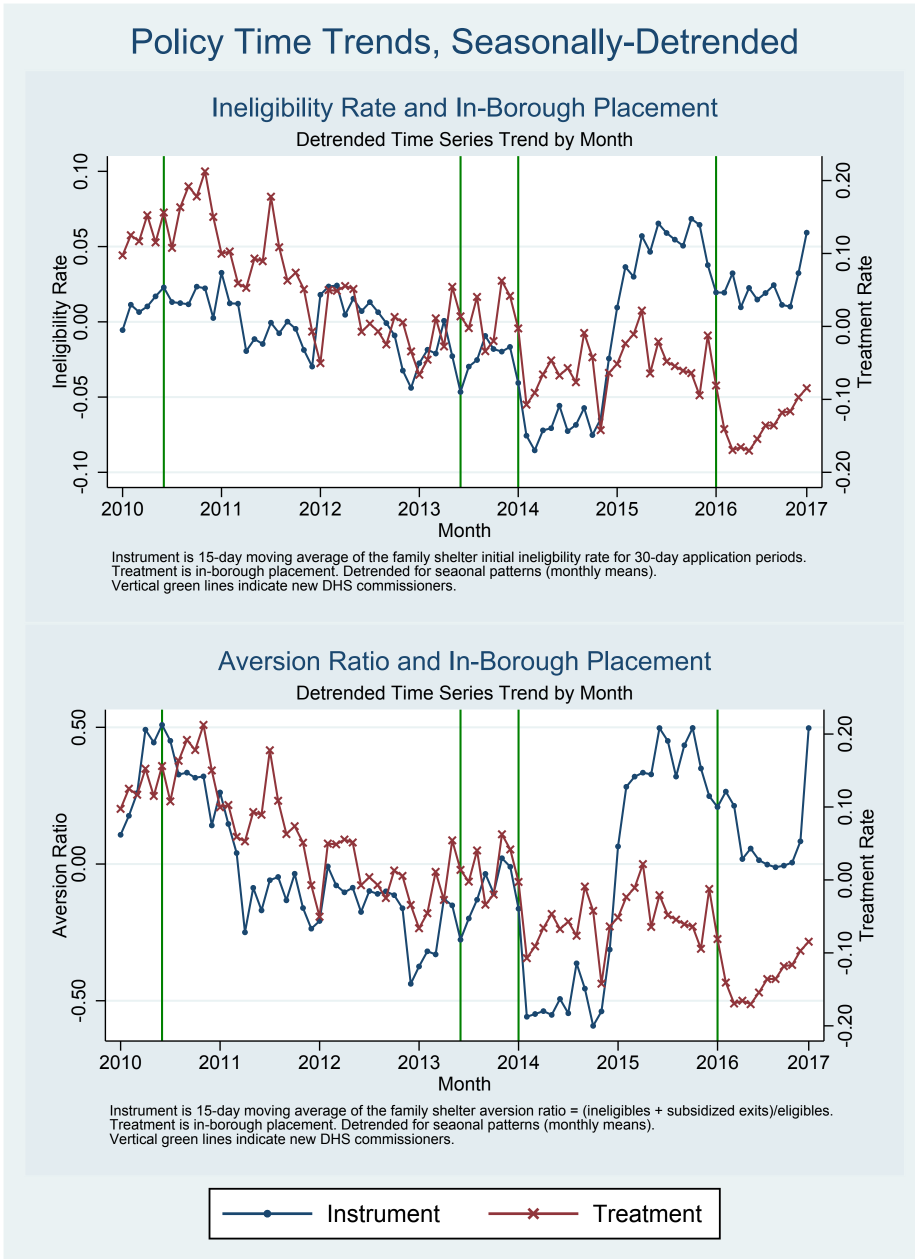
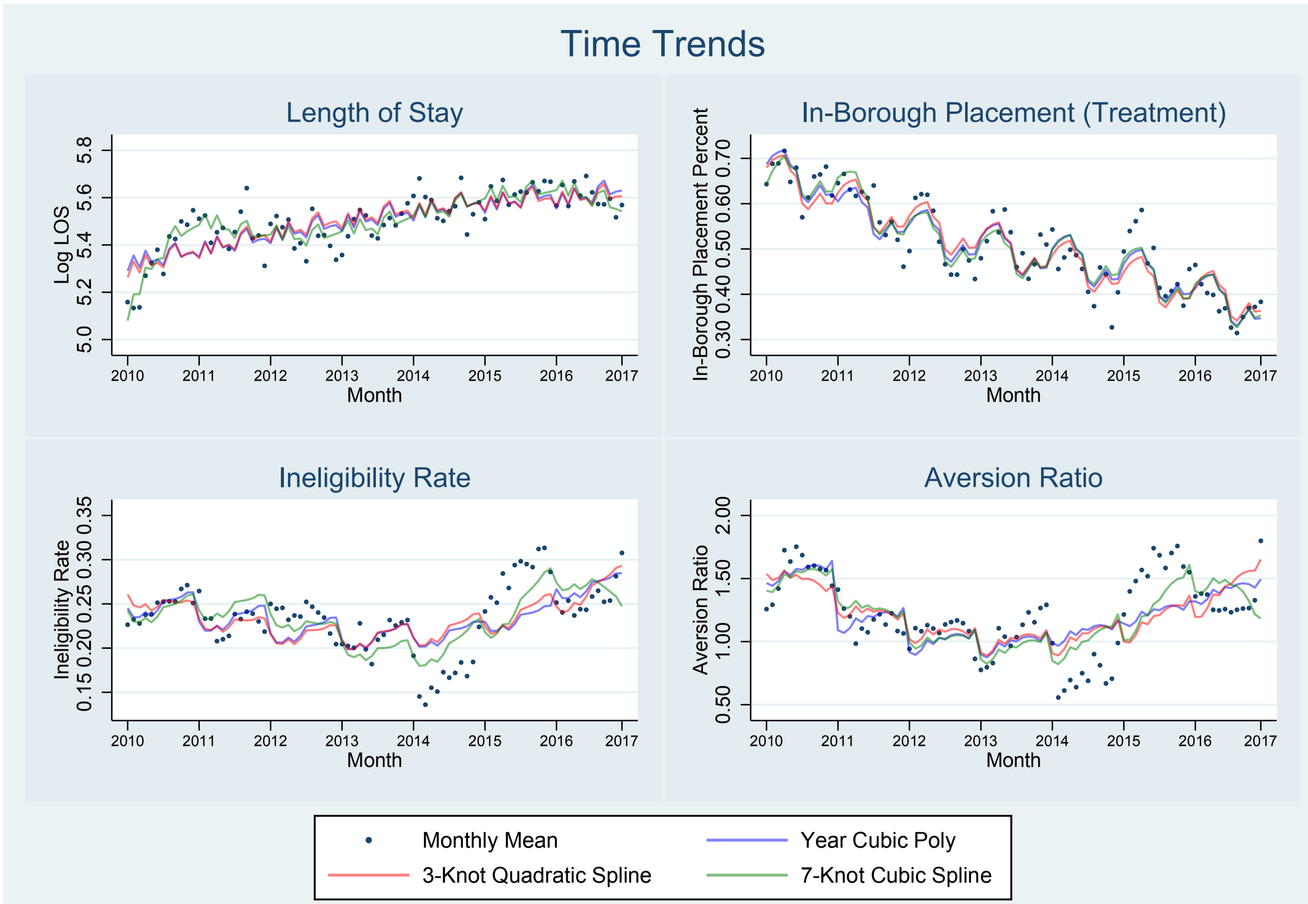
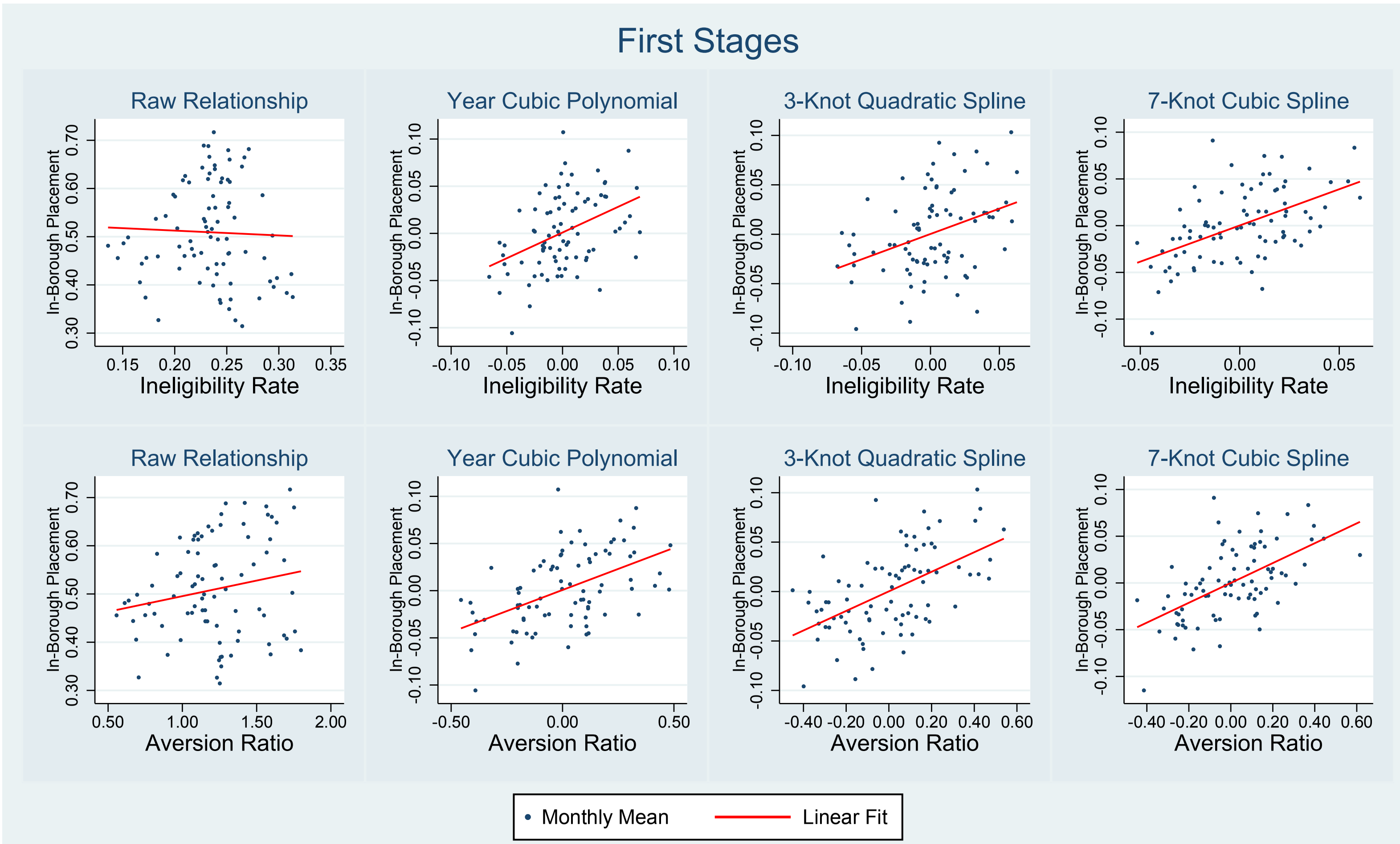


Figure B.2: Treatment, Length of Stay, and Policy Time Trends



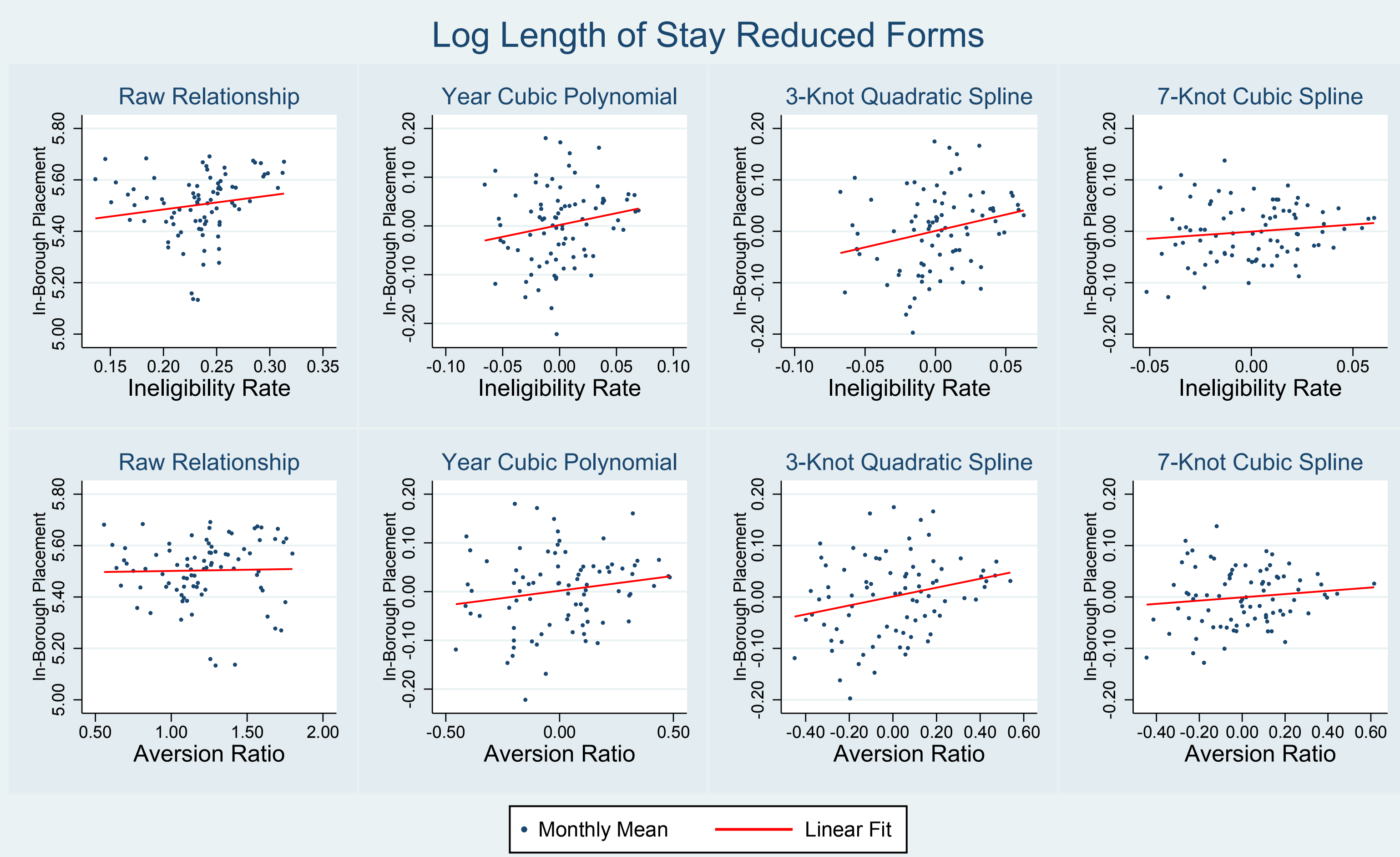
Fitted curves from linear regression on indicated time trend and month dummies.

Figure B.3: Instrument First Stages and Time Trends



Panels are detrended by title time trend and month.

Figure B.4: Instrument Length of Stay Reduced Form and Time Trends



Panels are detrended by title time trend and month.

Figure B.5: Randomization Check

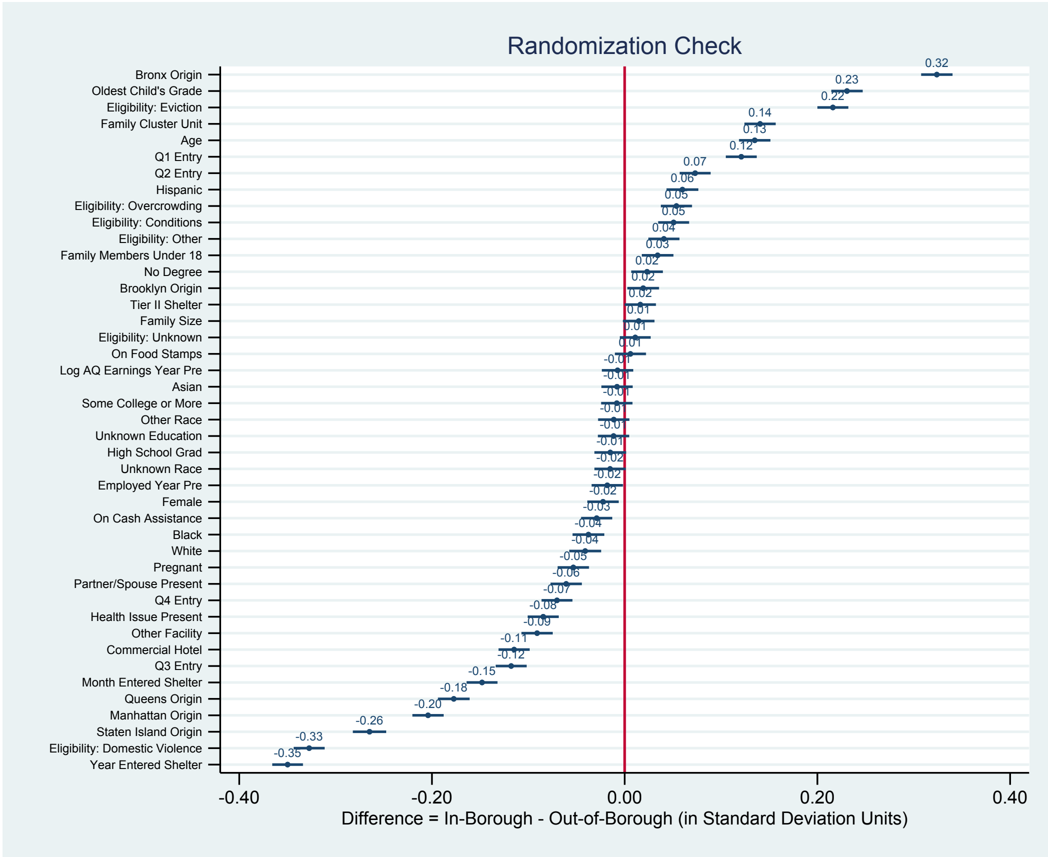


Figure B.6: Length of Stay Density

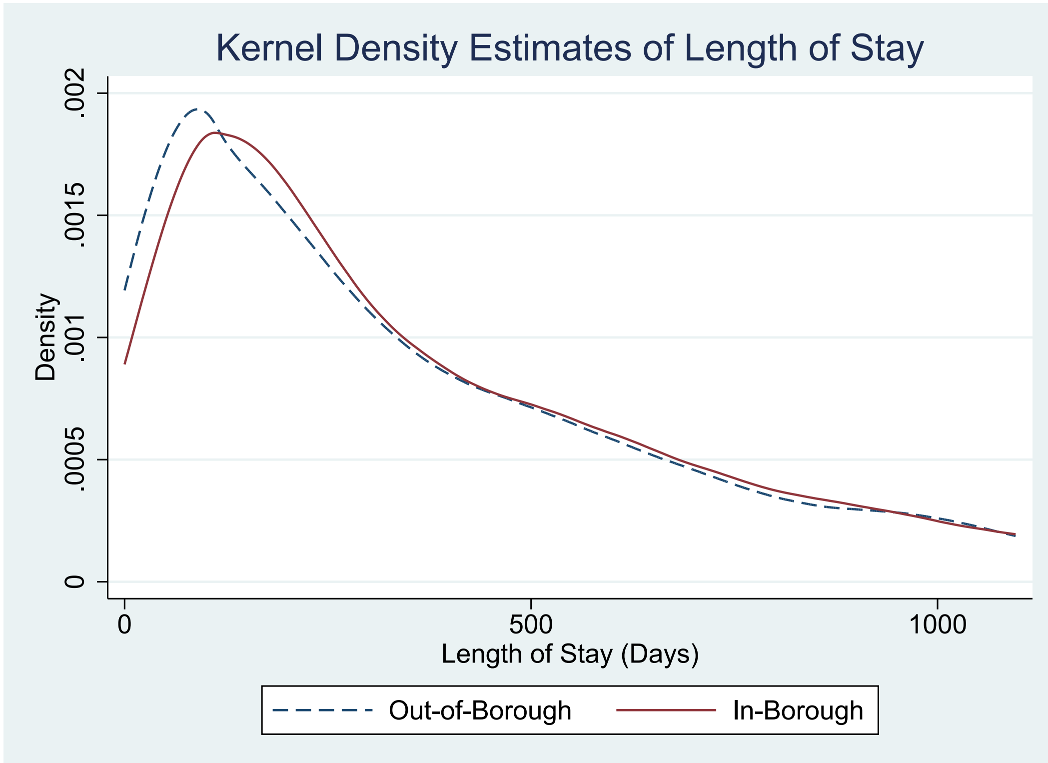


Figure B.7: Log Length of Stay Density

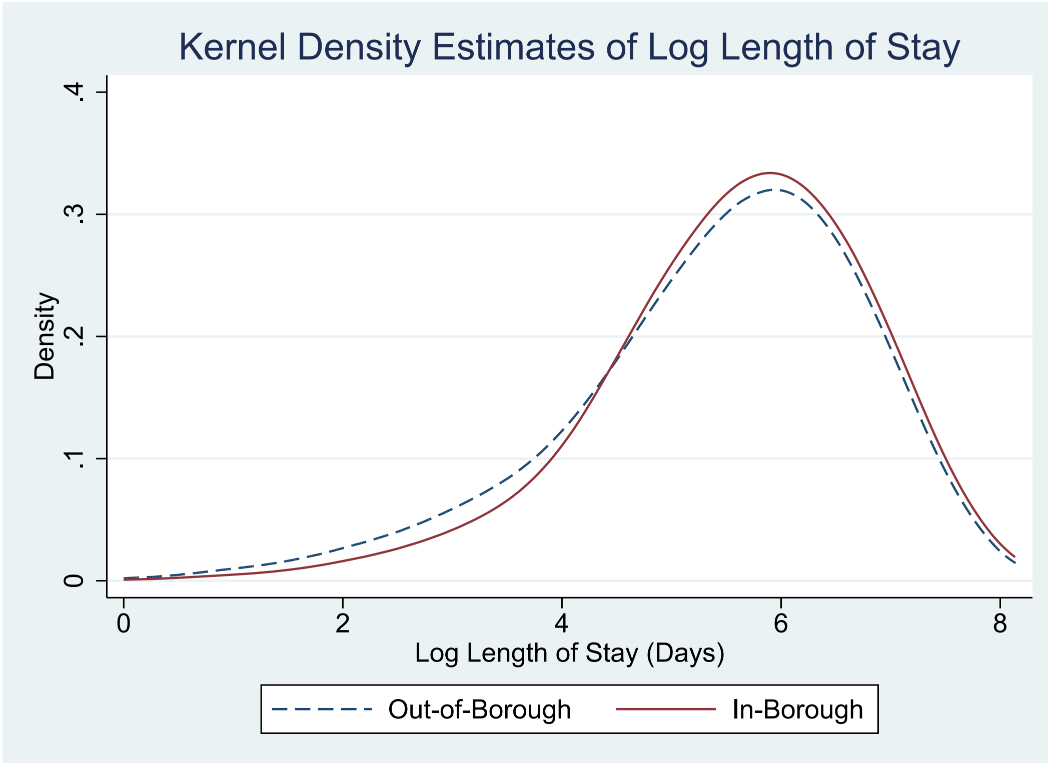


Figure B.8: Regression Discontinuity First Stages

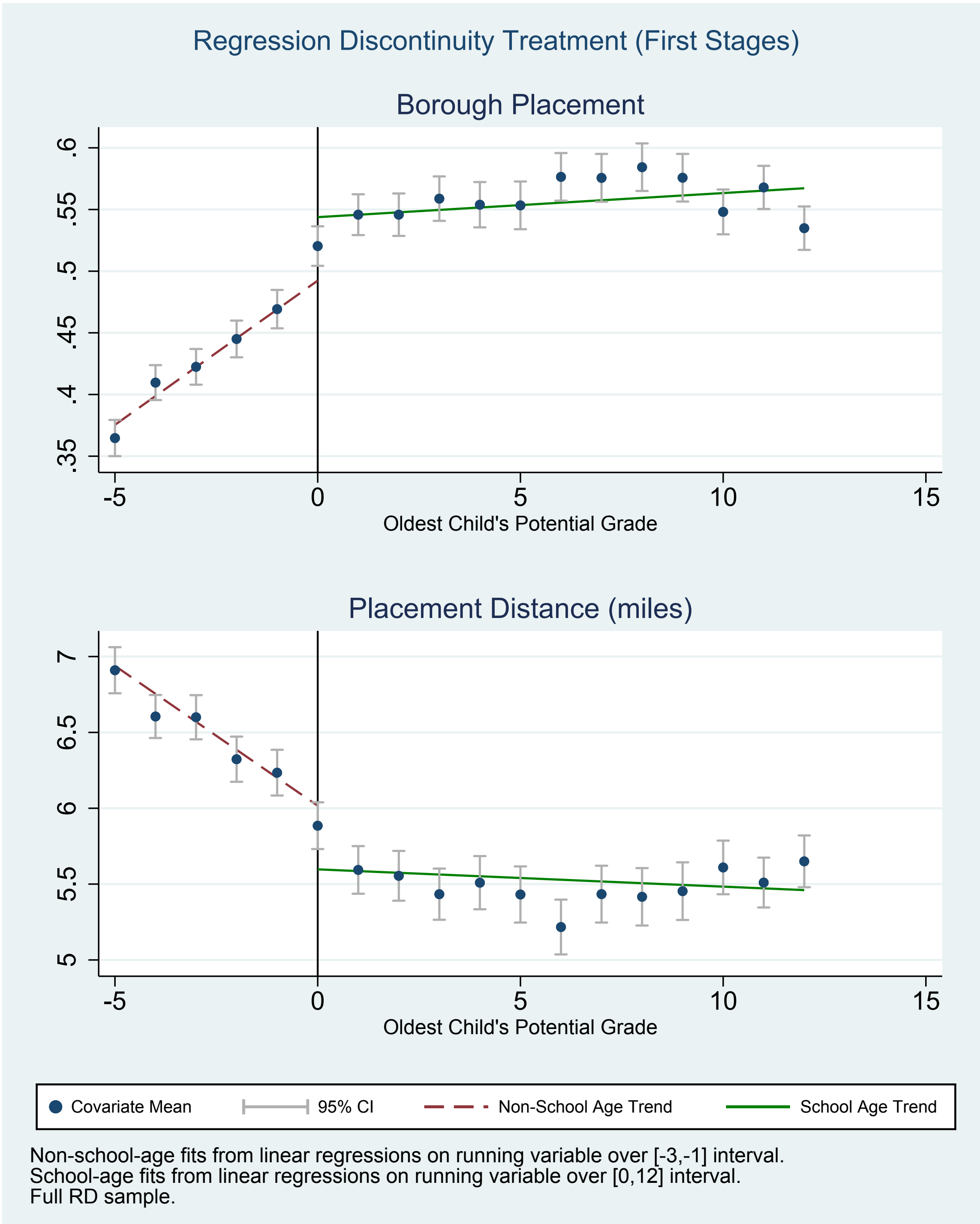
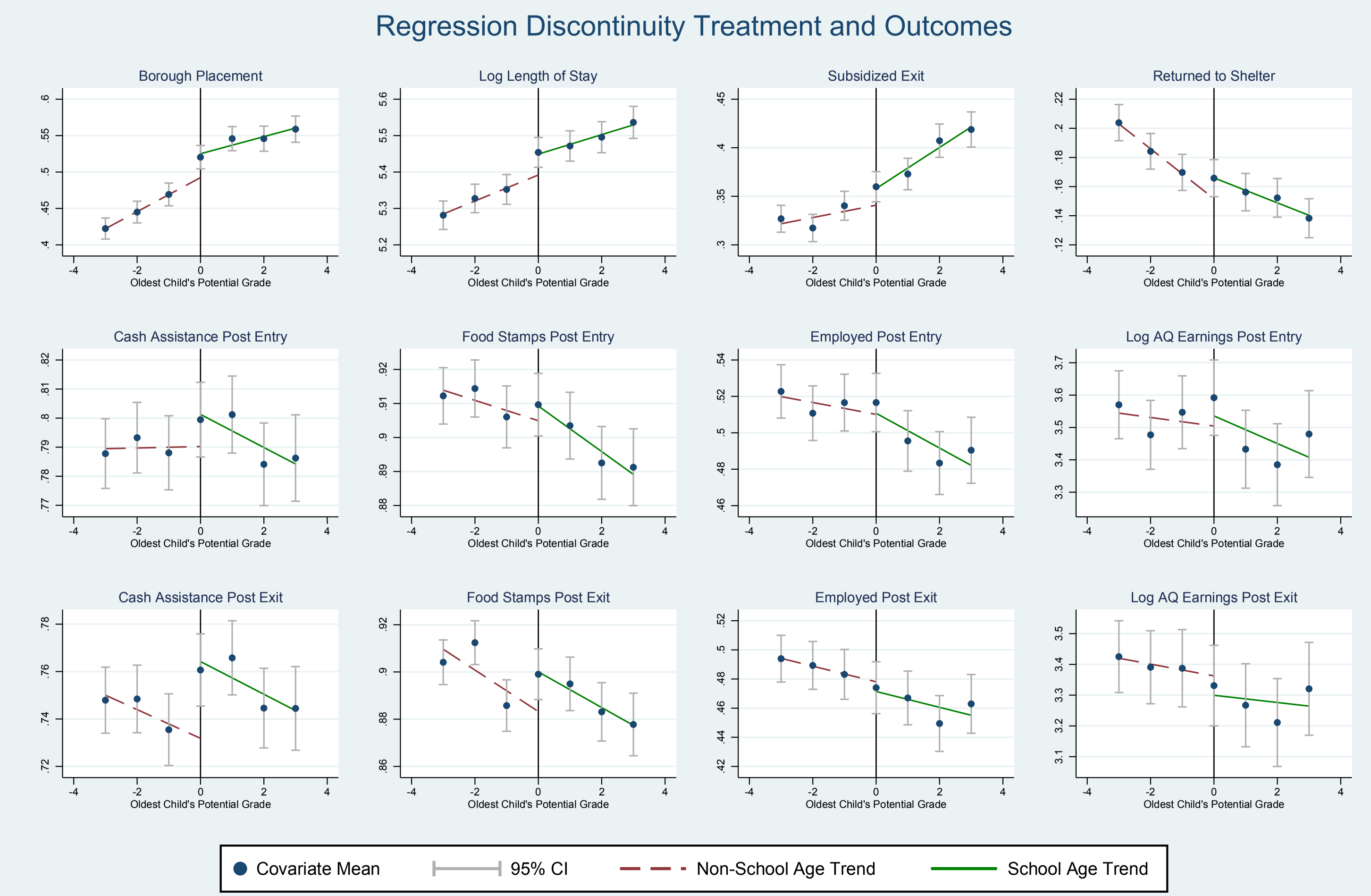
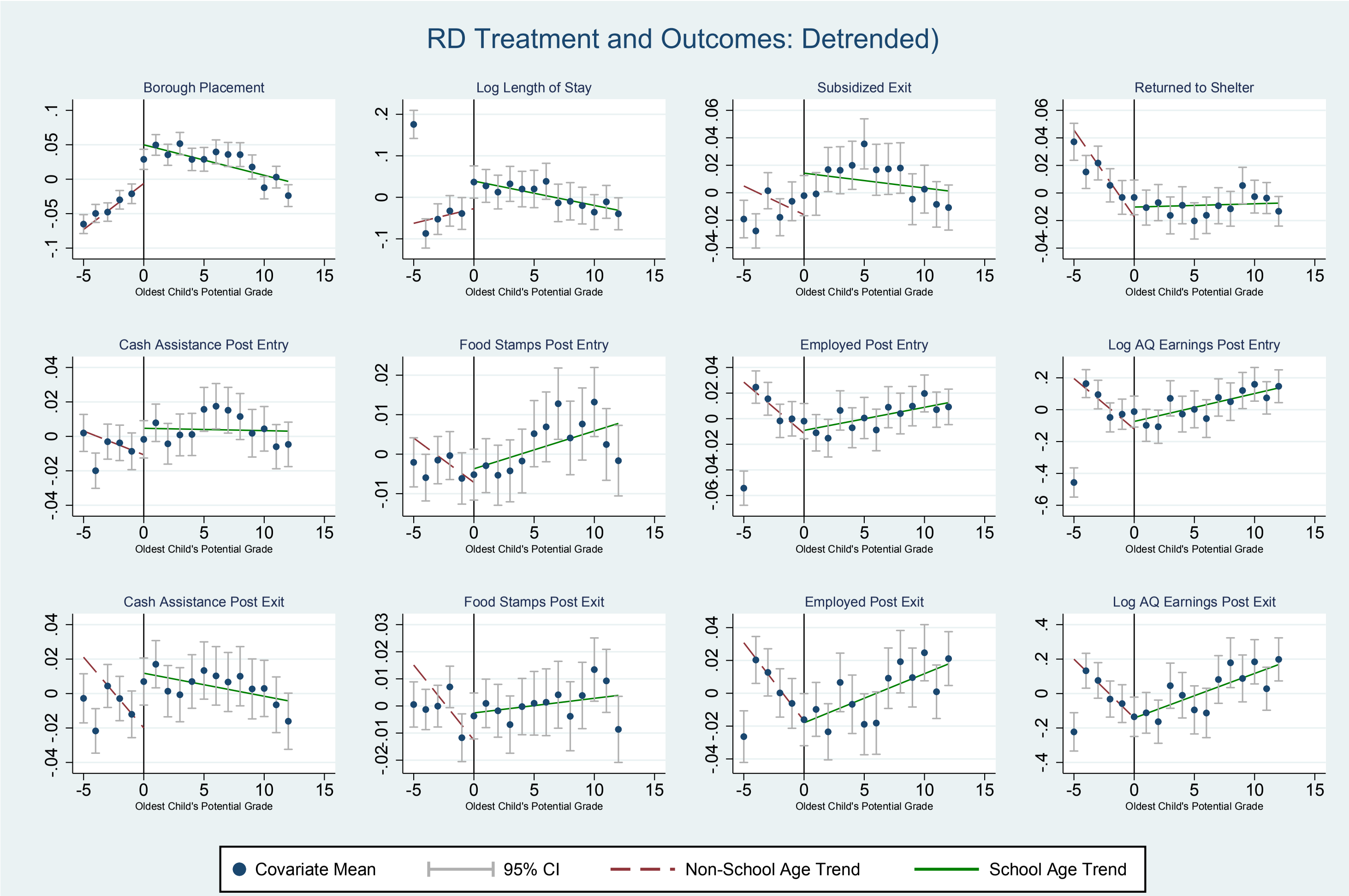


Figure B.9: Regression Discontinuity Treatment and Outcomes



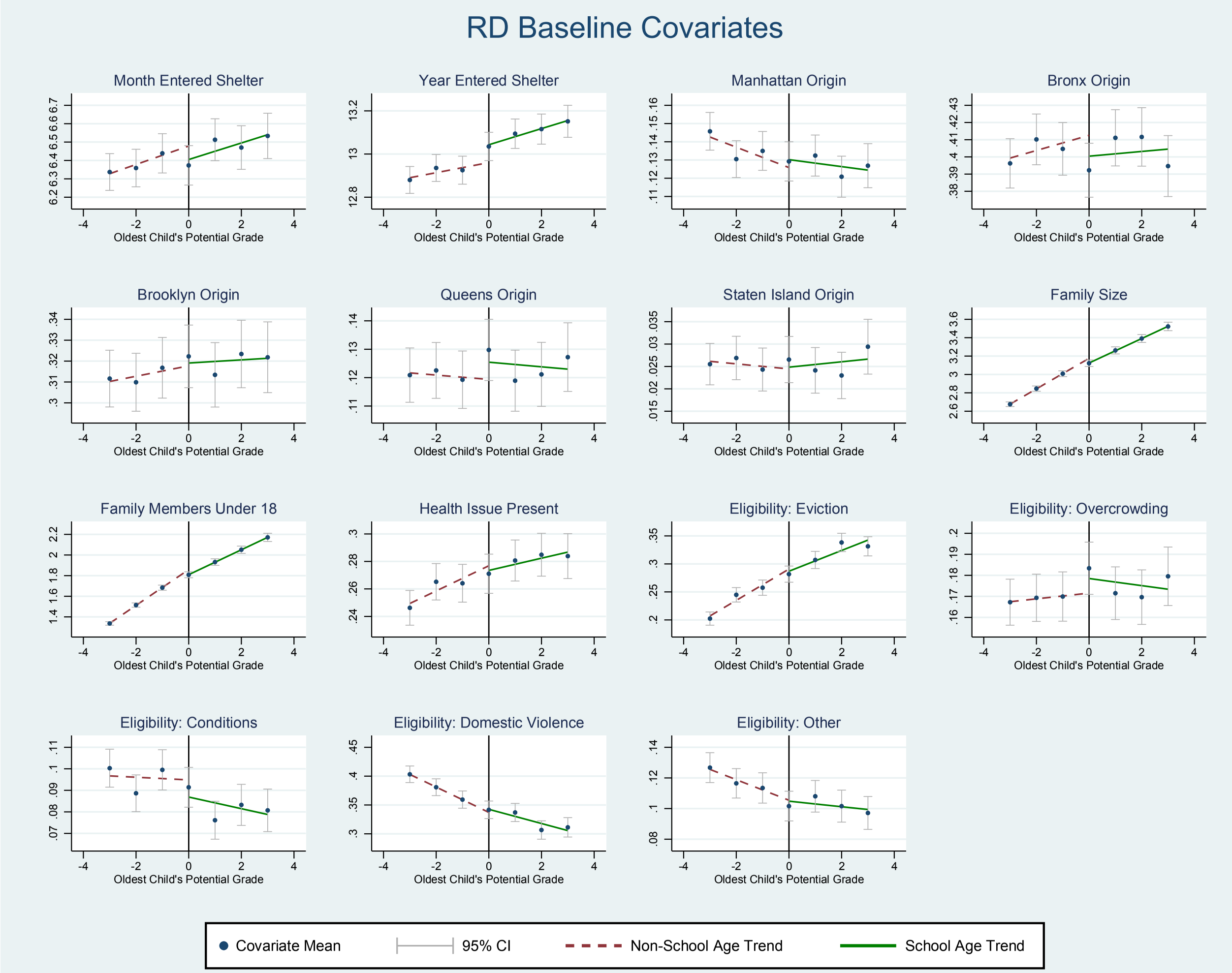
Non-school-age fits from linear regressions on running variable over [-3,-1] interval.
School-age fits from linear regressions on running variable over [0,3] interval.
Full RD sample.

Figure B.10: Regression Discontinuity Treatment and Outcomes: Detrended



Non-school-age fits from linear regressions of detrended outcome on running variable over [-3,-1] interval.
School-age fits from linear regressions of detrended outcome on running variable over [0,12] interval.
Detrended outcomes are residuals from a linear regression on Main RD covariates.
Full RD sample.

Figure B.11: Regression Discontinuity Baseline Covariates

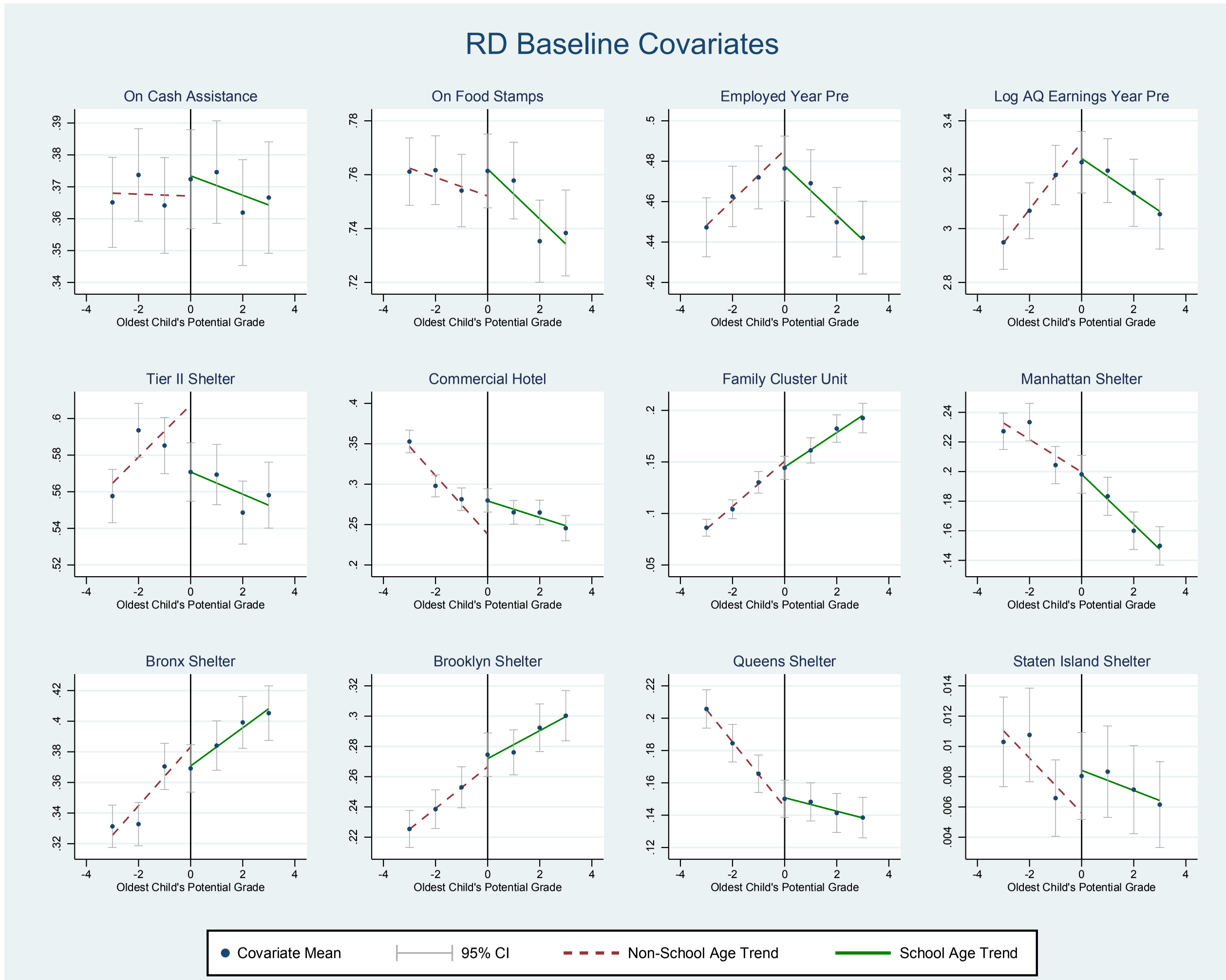


Non-school-age fits from linear regressions of detrended outcome on running variable over [-3,-1] interval.
School-age fits from linear regressions of detrended outcome on running variable over [0,3] interval.
Full RD sample.

Figure B.12: Regression Discontinuity Baseline Covariates

Non-school-age fits from linear regressions of detrended outcome on running variable over [-3,-1] interval.
School-age fits from linear regressions of detrended outcome on running variable over [0,3] interval.
Full RD sample.

Figure B.13: Regression Discontinuity Baseline Covariates



Non-school-age fits from linear regressions of detrended outcome on running variable over [-3,-1] interval.
School-age fits from linear regressions of detrended outcome on running variable over [0,3] interval.
Full RD sample.

Chapter 3

Breastfeed, If You Choose: Parental Context and the Long-Term Legacy of Lactation

3.1 Introduction

Breastfeeding is nature's way of nourishing infants, so it is hardly surprising that it is associated with an array of benefits. The medical literature is replete with studies finding breastfed babies are healthier—quicker to acquire immunity and less susceptible to the infectious respiratory and gastrointestinal maladies of infancy. As children and young adults, they exhibit augmented intelligence, lower likelihoods of being overweight, and attenuated incidences of cardiovascular and metabolic diseases. Their mothers experience improved birth spacing and reduced propensities for reproductive cancers[1]. The evidence is seen as sufficiently strong that, since 1997, the American Academy of Pediatrics has decreed six months of exclusive breastfeeding

[1]There is an extensive medical literature studying breastfeeding. Horta et al. (2007); Ip et al. (2007); Salone, Vann Jr and Dee (2013); Horta et al. (2013); Dieterich et al. (2013); Horta, Loret De Mola and Victora (2015); Victora et al. (2016) offer perhaps the best systematic reviews to date; the many studies cited therein comprise the evidence discussed in these first three paragraphs.

as a public health goal—a sentiment echoed by the World Health Organization and U.S. Department of Health and Human Services[2].

Despite this consensus, the causal evidence to support these claims is surprisingly weak. Virtually the entire literature is observational, and many studies fail to convincingly address confounding factors. A fundamental challenge complicates evaluation: breastfeeding is the outcome of a parental optimization problem and not randomly assigned[3]. Consequently, favorable correlations between breastfeeding choices and wellbeing may be the artifact of unobserved heterogeneity. Several recent systematic meta-analyses underscore this concern: as a rule, the more rigorous the study, the more modest, if any, are breastfeeding's salubrious associations (see, e.g., Ip et al. (2007); Horta et al. (2013); Victora et al. (2016)). Beyond health advantages during infancy—for which both empirical documentation and biological plausibility is strong—the claims on breastfeeding's behalf consist mostly of informed speculation. Further complicating matters is generalizability: breastfeeding is studied in settings rich and poor, with scopes micro and macro, and across margins intensive and extensive, raising oft-neglected issues of external validity.

Work by economists and other social scientists placing greater priority on causal inference emphasizes the theme of apparent advantage obscured by endogeneity. With cognitive performance the preferred topic of inquiry[4], multivariate regression and propensity score matching approaches almost always document favorable associations between breastfeeding various measures of achievement, from childhood intelligence

[2]See Eidelman and Schanler (2012); US Department of Health and Human Services et al. (2011); U.S. Department of Health and Human Services, Office of Disease Prevention and Health Promotion (2020); World Health Organization (2018).

[3]Their is a rich literature on the determinants of breastfeeding. See, e.g., Rollins et al. (2016); Dennis (2002); Dieterich et al. (2013).

[4]For a helpful summary of research in economics on breastfeeding and cognitive performance through 2013, see Rothstein (2013).

tests[5] to educational attainment[6] and earnings[7]. Nearly as often, these studies find advantages attenuate in the face of mother fixed effects, or simply adjusting for correlated parenting behaviors[8]. The same is true of health and noncognitive outcomes evaluated by any method: there appear few impacts enduring beyond infancy[9]. These findings are in keeping with the one randomized control trial of breastfeeding, outside of economics, which documents small cognitive gains but few health advantages beyond infancy (Kramer et al., 2001, 2007, 2008). While breastfeeding's purported benefits have deemed additional RCT's unethical, economists have contributed quasi-experimental evidence in the form of instrumental variable approaches exploiting such things as breastfeeding promotion programs, non-elective C-sections, and hospital staffing patterns[10]. While raising questions of validity and power, these results generally tend towards null effects.

As a theoretical matter, it is not obvious breastfeeding would be unambiguously preferred. On one hand, the medical literature posits several hypotheses as to why breastfeeding may be advantageous. One is the biochemical properties of breastmilk. In addition to its macronutrient composition—evolutionarily attuned to adapt to infants' ever-evolving nutritional needs—human milk is abundant in immune-boosting antibodies, awash in essential minerals, vitamins, hormones, and enzymes, and rich in long-chain fatty acids, which are thought to promote neural development[11]. But more important than covalent bonding may be that between mother and child: intimate

[5] Evenhouse and Reilly (2005); Der, Batty and Deary (2006); Denny and Doyle (2010); Jiang, Foster and Gibson-Davis (2011); Belfield and Kelly (2012); Del Bono and Rabe (2012); Borra, Iacovou and Sevilla (2012); Rothstein (2013); Fitzsimons and Vera-Hernández (2013); Onda et al. (2016).

[6] Rees and Sabia (2009).

[7] Cesur et al. (2017).

[8] Evenhouse and Reilly (2005); Der, Batty and Deary (2006); Gibson-Davis and Brooks-Gunn (2006); Rees and Sabia (2009); Rothstein (2013); Gibbs and Forste (2014); Colen and Ramey (2014); Cesur et al. (2017).

[9] Baker and Milligan (2008); Del Bono and Rabe (2012); Fitzsimons and Vera-Hernández (2013).

[10] Denny and Doyle (2010); Del Bono and Rabe (2012); Fitzsimons and Vera-Hernández (2013).

[11] For details, see Lessen and Kavanagh (2015); Martin, Ling and Blackburn (2016); Victora et al. (2016).

time together may catalyze the gains for which milk is credited. Indeed, the most important changes may occur in the mother, with breastfeeding stimulating nurturing instincts[12].

On the other hand, the scientifically-engineered alternative—infant formula—has progressed to the point where, at least from a nutritional standpoint, it is virtually equivalent to mother's milk[13]. Likewise, lactation is neither a necessary nor a sufficient condition for conscientious parenting or quality time together. In addition, breastfeeding imposes non-trivial physical and time costs on women, potentially compromising other elements of child wellbeing[14].

In this paper, I bring the causal question to the fore. Using a expansive set of empirical approaches in the context of rich longitudinal data, I investigate a comprehensive collection of educational, health, labor, and behavioral outcomes from childhood through young adulthood, offering a perspective novel in its scope and temporal integrity. My contribution is twofold. First, where prior research has settled for snapshots—infants, children, adolescents, *or* young adults—of disjoint domains—cognition, education, labor, *or* health—I provide a synthesis, evaluating a broad spectrum of subjects among the same individuals from ages 5 through 25 years, imparting continuity and coherence that has been lacking. Second, I harmonize previously contradictory results by introducing a new identification strategy to the study of breastfeeding while characterizing the extant biases of observational econometric estimates.

Good data makes this possible. I use the 1979 National Longitudinal Survey of Youth Child and Young Adult cohort (NLSY-CYA), which, through the 2016 cycle, follows 11,530 individuals from birth through adulthood in the United States. Featuring contemporaneous queries on infant feeding and extraordinary detail de-

[12]World Health Organization (2009); Rothstein (2013).
[13]See Stevens, Patrick and Pickler (2009); Martin, Ling and Blackburn (2016).
[14]Rollins et al. (2016).

scribing children and their mothers, accumulated through biennial surveys spanning five decades, the NLSY-CYA is especially well-suited to the study of breastfeeding in the rich country context.

Given this extensive detail and following convention, I begin with OLS linear regression. I augment standard demographic and socioeconomic controls with covariates encompassing maternal intelligence, employment, health, perinatal behaviors, and home environments, as well as children's birth circumstances—a collection rarely, if ever, matched in the breastfeeding literature. This helps lay claim to more a credible selection-on-observables story than is usually possible. Per OLS, breastfeeding is associated with early gains in cognitive achievement—by about 0.1 of a standard deviation (SD) on math, reading, and vocabulary intelligence tests—that translate into 2–3 percentage point (pp) gains in the probabilities of completing high school and attending college by age 25.

These results, which are in line with prior estimates by economists, pertain to breastfeeding initiation, which is the simplest treatment definition and the measure most commonly used in the literature. Other gains appear on the intensive margin. Individuals breastfed at least three months are 3 pp less likely to receive public benefits (welfare, Food Stamps, or medical insurance) at age 21 and are 6 pp more likely to be employed at 25—advantages not apparent on the extensive margin. They are also 4 pp less likely to have a child before marriage. Early cognitive gains exhibit dose-response as well, peaking near 0.2 SD's among those breastfeed between six and twelve months.

In contrast, there is little evidence for enduring health benefits, and, indeed, a positive association between having been breastfed and reporting health problems in childhood and young adulthood. Given breastfeeding's known benefits during infancy, this is puzzling.

These results assume no omitted variables. My second identification strategy, mother fixed effects, is also common in the breastfeeding literature. By narrowing the unit of comparison to siblings with varied breastfeeding experiences, this approach implicitly differences out family-level unobservables invariant among offspring. In keeping with the existing literature, breastfeeding appears to matter little among siblings, with precise nulls estimated for nearly all outcomes.

As with OLS, mother fixed effects relies on strong assumptions: mothers who, by choice or necessity, breastfeed one child and not another might compensate in other ways, potentially creating biases worse than the omissions of OLS. To address this concern, I introduce a new identification approach to the study of breastfeeding: extended family fixed effects. Made possible because the NLSY includes linkages between mothers and their sisters, this strategy includes cousins as well as siblings in the within-family comparison set, thus rendering more explicable contrasting breastfeeding choices while retaining some of the shared genetic and parental background features that make family fixed effects attractive[15]. It confirms the nuclear family findings: among kin, breastfeeding appears to make little difference in cognitive or other outcomes.

It is tempting to view fixed effects results as a corrective of OLS. I provide evidence—previously lacking in the breastfeeding literature (see, e.g., (Colen and Ramey, 2014; Rees and Sabia, 2009; Rothstein, 2013))—that this is a mistake. The subset of families contributing to fixed effects identification is significantly negatively selected. Among other things, mothers from inconsistently-breastfeeding families have markedly lower educational attainment, income, and intelligence scores than their counterparts from families with uniform feeding behaviors. Their children have worse outcomes by almost all measures; for example, they score about 0.2 SD's worse on cognitive assessments and are 5 pp less likely to have college experience by age 25. In addition, when these mothers breastfeed, they do so 30 percent shorter, on

[15]While new to breastfeeding, the so-called "cousin" fixed effects approach has been used in other settings (e.g., (Barclay, Lyngstad and Conley, 2018; Duncan et al., 2018*a*)).

average, than their breastfeeding-consistent peers. Excluding these incongruously fed kin from the OLS analysis boosts many coefficients by between a fifth and a half, rendering some newly statistically significant.

Put differently, null fixed effects findings are, at least in part, the consequence non-intensive breastfeeding in disadvantaged environments. This turns the conventional wisdom on its head: it would appear downward bias in fixed effects is at least as large a concern as the inverse bias in OLS.

My preferred interpretation is that breastfeeding is, on average, associated with modest, and persistent, intellectual advantages. Later labor and fertility benefits behaviorally derivative of cognitive antecedents arise among those breastfed longer. At the same time, there are reasons to believe at least a portion of these gains are attributable to parenting quality in general, rather than breastfeeding per se. Breastfeeding's abundance of auspicious associations makes it difficult to ensure all else is equal; to this point, dose-response relationships may reflect augmented selectivity among more intensively-breastfeeding mothers (e.g., those with access to generous parental leave). Even more plainly, the fixed effects analysis demonstrates that parents and their circumstances can compensate for infant feeding in other ways. Among the NLSY cohort, this recompense appears generally disadvantageous, but there of course remains scope for salutary redress as well. To the extent there is a true effect of breastfeeding, it likely lies somewhere in the middle: smaller than covariate-adjusted linear associations but greater than fixed effect nulls.

The takeaway for policymakers, physicians, and public health professionals is that breastfeeding, at least in the rich country context, is desirable, but not dispositive. For most children, it is likely to be an advantage, but for others, accumulating these assets may be achievable via alternative routes—or stymied by roadblocks. Encouraging breastfeeding among families least likely to pursue (or sustain) it own their own may be especially important, as these are likely the children with the most to gain.

However, in so doing, it is essential to acknowledge the ex ante costs of breastfeeding for these families may outweigh its benefits, potentially implicating deleterious trade-offs (e.g., employment) without adequate supports (e.g., paid family leave). Policies to promote breastfeeding must explicitly address opportunity costs and constraints, putting in place necessary foundations to ensure that, especially for disadvantaged populations, breastfeeding does not compromise other aspects of child or parent well-being.

3.2 Data

My data consists of the 1979 National Longitudinal Survey of Youth Child and Young Adult cohort (NLSY-CYA), which studies the 11,530 biological children of the original female members of the NLSY79. Sponsored by the U.S. Bureau of Labor Statistics (BLS), the NLSY79 thoroughly chronicles the characteristics and experiences of Americans born between 1957 and 1964 (and aged 14–22 in 1979). The NLSY-CYA charts the analogous attributes of their children, detailing their cognitive, physical, social, and behavioral development, as well as their educational, health, labor market, and familial experiences[16]. Born between 1970 and 2014, these children (and their mothers) have been interviewed biennially since 1986 and most recently in 2016[17]. Data is collected through four survey instruments: a child assessment administered to those 14 years of age and younger, a young adult questionnaire for those 15 and older, and a mother supplement augmenting the NLSY proper.

I transform the raw NLSY-CYA data, for which the unit of observation is the child[18], by age-aggregating survey round responses into pooled cross sections corre-

[16]The Center for Human Resource Research (CHRR) at The Ohio State University manages the NLSY79 and NLSY-CYA and the National Opinion Research Center (NORC) at the University of Chicago performs the actual interviews. The National Institute for Child Health and Human Development co-directs the NLSY-CYA along with BLS. For more, see Bureau of Labor Statistics, U.S. Department of Labor, and National Institute for Child Health and Human Development (2019).

[17]Interview rounds occur in even-numbered years.

[18]For simplicity, I frequently refer to respondents as "children" even subsequent to attaining

sponding to the five "outcome age" groups I study: ages 5, 10, 13, 21, and 25. Child i's age a outcome Y_{ia} comes from the earliest survey round r for which the child's age in years satisfies the relation $0 \leq age_{ir} - a \leq 2$ and the child was successfully interviewed. That is, outcomes are derived from the interviews most proximate to the pivotal age without going younger, given crossing thresholds such as 5 or 21 years can confer discontinuous social and legal changes. Correspondingly, time-varying covariates are culled from the interviews most proximate to, or immediately preceding, a child's birth, in order to avoid endogenous controls.

Table 3.1 illustrates the scope of the NLSY-CYA data. Each row describes the universe of individuals relevant to identification for an estimation strategy (to be described below). Cells give sample sizes, with all children in Column 2, those with breastfeeding data in Col 3, and interview participation at outcome ages enumerated in the ensuing columns. Response rates at my outcome ages of interest start at about 70 percent at age five and decline to about half by age 25 (with some of the reduction attributable individuals too young to have yet responded). Only 37 percent of children participate across all five age checkpoints. The numbers of individuals contributing identifying variation to breastfeeding in the fixed effects models (i.e., siblings or cousins with different breastfeeding statuses) are about a third of the sample.

In the following subsections, I focus on the aspects of the data most important to studying the long-term effects of breastfeeding[19].

3.2.1 Breastfeeding

The NLSY is especially well-suited to answer questions about the long-term effects of breastfeeding. Mothers who have given birth since the date of the last interview are

"young adult" status.

[19]For a thorough discussion of the data, including much more detail about variable definitions, see the extensive documentation BLS makes available at `https://www.nlsinfo.org/`, as well as the the Stata code for this paper.

asked two questions. The first is binary—“Was child breastfed?”—and the second records breastfeeding duration in weeks. Temporal proximity enhances recall and is a feature not always present in other datasets commonly used to study breastfeeding[20].

3.2.2 Outcomes

The first three age-outcome groups concern children; the latter two correspond to young adults. Table C.1 details the distribution of age-outcome ages. About 95 percent of age-outcome ages are within one year of the pivotal age, and split evenly between them (e.g., for outcome-age 25, 50.3 percent of respondents are 25 at the time of their outcome measurement, 46.2 percent are 26 years, and 3.5 percent are 27 years). I include indicators for these ages in all regressions to control for micro-age discrepancies[21].

The following subsections provide an overview of the outcomes I assess in the main text. Detailed definitions of these outcomes, as well as a discussion and analysis of several alternative outcome measures, are available in Appendices C.2 and C.4.

Child Outcomes

During childhood, I focus on three domains: cognitive performance, behavior, and health. Cognitive outcomes consist of four assessments: Peabody Individual Achievement Tests (PIAT) for math, reading recognition, and reading comprehension, as well as the Peabody Picture Vocabulary Test (PPVT). These tests are among the most common measures intelligence and are known to have high reliability and validity. I used normed scores of mean zero and standard deviation one, so that regression

[20]For example, the National Longitudinal Survey of Adolescent Health, in contrast, asks breastfeeding questions retrospectively of adolescents. Nevertheless, the NLSY-CYA is not exclusively contemporaneous. Births prior to 1979 (11 percent), when the mother interviews began, and prior to 1986 (51 percent), when child assessments commenced, rely partly on recall and the associated loss in precision.

[21]I analogously assign “age-outcome years” to the interview round corresponding with the “age-outcome” age, and include it as a covariate to control for year-of-interview effects.

results can be interpreted in standard deviation units, with the caveat that these norms are in reference to external norming samples with somewhat lower average performance than the NLSY-CYA cohort. I evaluate behavioral outcomes using the Behavioral Problems Index (BPI), which is also standard in the NLSY-CYA literature and analogously scaled as a standard normal. Higher scores indicate worse behaviors; the within-NLSY mean is somewhat above zero. I use two measures of health. The first is broad a binary indicator for health problem, which include school-limiting conditions, illnesses requiring medical attention, and subjective fair or poor health. The second is an binary indicator for overweight, defined as a body mass index (BMI) of 21 or greater.

Young Adult Outcomes

The young adult outcomes span an analogous four domains: educational attainment, labor market experiences, health, and behavior. For education, I consider binary indicators for high school graduation and college experience, as well as a count of years of education. For labor, I assess indicators for employed, either in school or working, and public benefit receipt (public assistance, Food Stamps, and/or public health insurance), as well as log earned income (with earnings in 2019 dollars). For health, I define health problem and overweight indicators symmetric with the child versions. For behavior, I consider an indicator for premarital childbearing.

3.2.3 Covariates

Throughout the analysis, I control for a rich set of factors jointly relevant to breastfeeding decisions and child outcomes. With the exception of home environment scores measured at ages 0–2, all covariates are defined at the time of, or in the year prior to, a child's birth. Given it is common for individuals to have missing data on at least one control, I append each covariate with a dummy reflecting unknown values.

These covariates cover three broad subjects, and are comprised as follows (with mutually exclusive indicators in parentheses and respective "unknown" categories left implicit):

- **Mother Characteristics**: education (less than high school, high school graduate, some college or more), race (Hispanic, White, Black), age (under 21, 21–24, 25–29, 30–50), year of birth $(1957, 1958, \ldots, 1965)$, region (Northeast, North Central, South, West), marital status (never married, married, other), nativity (U.S. native, foreign-born), Armed Forces Qualifying Test (AFQT) quartile, employment (no, yes; during 12 months prior to birth), family income quartile (12 months prior to birth)), age 21 BMI quartile, and having a sister in the NLSY (yes, no).

- **Mother Pregnancy Behaviors**: prenatal doctor's visit (no, yes), prenatal vitamin consumption (no, yes), smoking (no, yes; during 12 months prior to birth), and alcohol consumption (no, yes; during 12 months prior to birth).

- **Child Characteristics and Birth Circumstance**s: birth order (1, 2, 3+), female sex (no, yes), birth weight quartile, cesarean delivery (no, yes), preterm birth (no, yes; defined as < 37 weeks gestation), postnatal hospital stay longer than mother (no, yes), and Home Observation Measurement of the Environment (HOME) quality quartile (measured at age 0–2), as well as a quadratic in child's birth year and indicators for birth month.

I also define indicators for children's ages, in years, during the survey rounds their responses for particular outcome ages occur (to control for short-term age effects), as well as indicators for the interview years themselves (to control for response-year effects). Henceforth I refer to this collection of controls as "full" covariates.

3.3 Empirical Approach

The central causal challenge is that women who choose to breastfeed—or who choose to breastfeed certain children and not others—may be different than those who do not. I pursue two identification strategies[22].

3.3.1 OLS

My baseline estimation approach is multivariate linear regression estimated via ordinary least squares (OLS). For individual i, outcome Y is estimated as:

$$Y_i = \mathbf{X}_i\boldsymbol{\beta} + \tau^{OLS}B_i + \boldsymbol{\varepsilon}_i \tag{3.1}$$

where $\mathbf{X}$ denotes the full set of mother, pregnancy behavior, and child covariates described in Section 3.2, $\boldsymbol{\varepsilon}$ subsumes unobservables, and B represents breastfeeding status[23]. For reasons of simplicity and accuracy of recall, I focus on the $B_i \in \{0, 1\}$, an indicator equal to one if child i is ever breastfed and zero otherwise (i.e., breastfeeding initiation). However, for robustness and to investigate dose-response effects, I additionally consider indicators for breastfeeding durations of 3+, 6+, and 12+ months. I cluster standard errors at the mother level to allow for correlation in unobservables among siblings. All regressions are weighted using NLSY custom longitudinal weights for the outcome age sample in question. To avoid incidentally truncating the sample while guarding against confounding, each covariate contains a dummy reflecting unknown values; as shown in Section 3.5, this choice does not affect my conclusions.

My parameter of interest is τ^{OLS}, which approximates the causal effect of breastfeeding if outcomes are unconfounded by unobservables.

[22] In Appendix C.1, I sketch a theoretical model to inform the empirical design.

[23] Implicitly, outcomes Y_i are indexed to specific ages, $a \in \{5, 10, 13, 21, 25\}$, obviating the need for explicit subscripts.

3.3.2 Fixed Effects

The NLSY's abundant detail lays claim to plausible independence of breastfeeding and potential outcomes. Nevertheless, omitted factors—notably paternal characteristics—remain. To remove sibling-invariant family-level unobservables which may be correlated with B, I add mother fixed effects[24], ϕ_f, to Equation 3.1:

$$Y_{if} = \mathbf{X}_{if}\boldsymbol{\beta} + \tau^{FE}B_{if} + \phi_f + \boldsymbol{\varepsilon}_{if} \tag{3.2}$$

with Equation 3.1 augmented to include indicators, ϕ_f, for nuclear family membership.

But siblings with differential breastfeeding experiences begs an equally obvious criticism: why would a mother choose to breastfeed one child and not another? If breastfeeding statuses change, there may be other child-varying unobservables as well—and this sort of granular heterogeneity is exactly the blind spot of mother fixed effects. From this standpoint, sibling comparisons could be biased worse than OLS (e.g., the breastfed sibling may have benefited from a stay-at-home mother, while formula feeding may be indicative of financial hardships necessitating a quicker return to work). One's stance on this issue depends upon whether one views between- or within-family unobservables as the greater threat[25].

Given these downsides, I introduce an extension: extended family fixed effects. I do so by exploiting sister linkages in the NLSY79. Included in the NLSY proper are sisters who were both aged 14–21 as of December 31, 1978 and resident in the same household[26]. 3,279 children in the NLSY-CYA have an NLSY79 aunt.

The idea is that grouping women with their sisters retains the advantages of con-

[24] Also popular in the breastfeedng literature, this is sometimes confusingly referred to as "sibling" fixed effects. I prefer the "mother" label, as it makes the level of clustering clear.

[25] For a recent perspective, see: Miller, Shenhav and Grosz (2019).

[26] The NLSY79's defines "sister" based on respondents' self-reported relationship statuses.

trolling for family backgrounds (including genetics) while expanding the comparison pool in a manner that makes the exogeneity of time-varying breastfeeding decisions more plausible. Cousins are more comparable than randomly-selected children; at the same time, mother-varying observables are plentiful in the data. In the interest of retaining as many children contributing to identification as possible, I define the extended family for children without cousins to be the nuclear family. Although so-called "cousin" fixed effects have been used in other settings[27], this approach is novel in the breastfeeding literature[28].

In the estimation, I modify Equation 3.2 such that ϕ_f refers to mothers and their sisters. As usual, consistency relies upon the assumption that cousin-varying (where, here, the term "cousin" encompasses siblings) unobservables are unrelated to breastfeeding status and outcomes.

3.4 Results

3.4.1 Descriptive Statistics

Tables 3.2 and 3.3 provide descriptive statistics for covariates and outcomes, respectively. The first three columns of Table 3.2 provide mean contrasts by breastfeeding status among those successfully interviewed at age five; the succeeding columns give means among respondents at each outcome age[29].

Most striking is the considerable positive selection among breastfed children. By virtually every measure, breastfed children have advantages compared with their formula-fed peers[30]. The mothers of breastfed children are 29 percentage points (pp)

[27] Duncan et al. (2018*b*); Barclay, Lyngstad and Conley (2018).

[28] I prefer "extended family" to "cousin" as the label because the family is the child-invariant "within" level of clustering. Another way of thinking about this grouping is as maternal grandparent fixed effects.

[29] Since covariates are measured at, or prior to, birth, differences at different ages reflect the changing composition of respondents, not changes for any single respondent.

[30] One exception is alcohol consumption, which is 8 pp more common among breastfeeding mothers,

more likely to be college educated, 20 pp more likely to be White, 2.6 years older, and 18 pp more likely to be married. They have higher rates of employment (7 pp), more income ($38k), and lower BMI (1.3 points). Perhaps most notably, they score 21 percentiles higher on the AFQT. Pregnancy behaviors follow suit. Breastfeeding moms have a higher probability of taking prenatal vitamins (4 pp), but lower probabilities of smoking (15 pp), preterm births (4 pp), and having children with long neonatal hospital stays (5 pp). Breastfed children weigh a third of a pound more at birth. They are also 5 pp more likely to be first-born and have better home environments following birth (by a third of a standard deviation). Figure 3.1 visually emphasizes this selection bias theme: there are pronounced gaps in breastfeeding by race, education, and cognitive test scores. At the same time, breastfeeding rates among the NLSY cohort evolved markedly during their childbearing epoch, as it did for the U.S. generally[31].

Table 3.3 gives a corresponding overview of outcomes. The basic point is clear: having been breastfed is associated with demonstrably better outcomes in childhood and young adulthood.

3.4.2 Main Results

Breastfeeding's unequivocal affiliation with *both* propitious circumstances and auspicious outcomes makes plain the difficulty isolating the effects of infant feeding. In this section, I present my main results for the relationship between ever having been breastfed and summary measures of intellect, health, and behavior—after adjusting for a wide array of confounding forces.

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a fact likely explicable by education and income.

[31] Table C.2 gives birth counts and breastfeeding rates by year.

Child Outcomes

Table 3.4 analyzes childhood outcomes. Rows index outcomes. Supercolumns denote outcome ages (5, 10, and 13), with one column within each group for each of my three identification strategies: OLS, mother fixed effects, and extended family fixed effects. Each cell gives the treatment effect coefficient from a separate (weighted) regression of the row-enumerated outcome on an indicator for ever having been breastfed and full covariates. Mother-clustered standard errors are in parentheses. Numbers of observations are in braces[32].

OLS results (Cols 1, 4, and 7) demonstrate that the strong association between breastfeeding and cognitive performance persists even after controlling for maternal characteristics, pregnancy behaviors, and children's birth circumstances. At age 5, breastfed children perform 0.1 standard deviations (SD's) better in math, 0.07 SD's better in reading recognition, and 0.13 SD's better in vocabulary. These age 5 findings are in line with the previous literature on cognitive effects. What is notable, however, is that these advantages persist—and even grow stronger—during childhood. Breastfeeding-related gains at age 10 are 0.11 SD's for math, 0.07 SD's for reading recognition, 0.11 SD's for reading comprehension, and 0.16 SD's for vocabulary. At age 13, all three PIAT scores remain 0.11–0.12 SD's higher for breastfed children (the PPVT is not common at that age).

A tenth of a standard deviation is economically meaningful. Across all the cognitive assessments, the ranges between the 25th and 75th percentiles are 1–1.5 standard deviations, so the breastfeeding-related boost is worth several percentiles at the least—and as much as a decile in the middles of the distributions.

While a rich conditioning set cannot guarantee the absence of omitted variable bias warping these associations, the more detailed the controls, the more plausible

[32]For fixed effects, I give the *effective* numbers of observations contributing to identification—that is, the counts of siblings/cousins with different breastfeeding statuses. The models, nevertheless, are estimated on the full sample.

the argument. Especially valuable in this regard are typically elusive traits, such as maternal intelligence. Excluding AFQT performance from the age 5 PIAT Math regression, for example, raises the coefficient on breastfeeding to to 0.131—an increase of 34 percent.

OLS results for health and behavior are more ambiguous. Breastfed children are no more likely to report health or behavioral issues at ages 5 or 10, though they are somewhat less prone to be overweight (by 0.7 pp at age 5 and 1.7 pp at age 10). By age 13, the weight difference disappears, but gaps emerge for health issues and behavior—in the opposite directions as expected. As early teens, breastfeed children are 2.7 pp more likely to have a notable health issue and are rated 0.06 SD's worse behaviorally. Whether these gaps reflect true disadvantages or more conscientious reporting by their mothers is unclear.

In contrast to OLS, mother (Cols 2, 5, 8) and extended family (Cols 3, 6, 9) fixed effects models suggest the cognitive benefits of breastfeeding may be smaller and transitory. While sibling comparisons at age 5 yield similar estimates to OLS (0.12 SD's for math and 0.28 SD's for reading comprehension), including cousins in the comparison attenuates these coefficients and renders them statistically insignificant. What's more, there is little evidence of continued gains in either model at ages 10 and 13; coefficients are uniformly smaller than 0.07 in magnitude and statistically insignificant, with standard errors that are relatively tight. Nevertheless, the samples of children contributing to fixed effects identification—which include only those families whose offspring have differential feeding experiences—are small, generally on the order of 1,000–2,000 observations, which may preclude the necessary power to detect effects. Health and behavioral effects in both FE models are also precise zeros, with two exceptions. Compared with siblings and cousins who are not breastfed, breastfed kin have 0.12 fewer behavioral problems at age 5 (per extended families), but are 6 pp more likely to have health issues at age 13 (per both FE models).

The many outcomes I assess are instructive in their breadth but may yield RCT-style concerns about multiple hypothesis testing[33]. There are well-known methods of adjusting significance levels to account for multiple comparisons (Romano and Wolf, 2005; Romano, Shaikh and Wolf, 2010). However, I do not pursue them here, as my objective is not to stake my claims on any one significant result, but instead to infer conclusions from the themes presented by the preponderance of the evidence, whether or not this evidence falls precisely on one side or the other of conventional significance levels.

By this standard, a reasonable interpretation of the foregoing analysis is that breastfeeding—and the interactions that go along with it—is indicative of modest, but persistent, cognitive gains throughout childhood, on average, but such advantages can also be conferred by other means, as the largely null fixed effects results suggest. On the other hand, there is little evidence for noncognitive benefits, and indeed some to the contrary: increased prevalence of health issues at age 13 is the lone result significant across all three models.

3.4.3 Young Adult Outcomes

An important question is whether these patterns persist into the young adult years. Table 3.5 provides some answers.

Similar in setup to Tables 3.4, Table 3.5 is split into two horizontal panels, with the first three columns describing age 21 outcomes and the latter trio studying age 25. Based on the OLS results for childhood cognition, one would expect to see better educational attainment among breastfed young adults, and indeed this is what OLS finds. At age 25 (Col 4), breastfed YA's are 2.7 pp more likely to have graduated high school (Row 1) and 3.1 pp more likely to have attended at least some college (Row 2).

[33] As outcomes proliferate, the probability of a Type I error (erroneous rejection of a true null hypothesis) becomes increasingly likely for conventional hypothesis tests and confidence intervals. With 20 independent outcomes at the standard $\alpha = 0.05$ level, the probability of at least one false positive is 0.64.

Much of these gains come early: by age 21 (Col 1), they have 0.14 additional years of education (Row 3), are 2.5 pp more likely to have completed high school, and have 4.6 pp greater probability of some college experience. The fixed effects educational results (Cols 2–3 and 5–6) are also in keeping with childhood: precise zeros for all outcomes.

The labor outcomes assessed in the next four rows reinforce this theme of punctilious nulls. In this case, even OLS finds breastfed young adults are no more likely to be employed or in school at ages 21 or 25; nor do they earn more or receive fewer public benefits. The same is generally true of health and behavior (bottom three rows). With the exception of an OLS-estimated 4.6 pp reduction in the probability of being overweight at 25, there are no significant results; indeed, the point estimates for the likelihood of health issues remain positive, as is true at age 13, though imprecisely so.

To summarize, OLS suggests the modest childhood gains in cognitive performance associated with breastfeeding continue through the young adult years, but fail to translate into labor market advantages. As in childhood, there is no evidence of noncognitive benefits, as well as reason—thoroughly null fixed effects results—to believe family context looms large.

3.5 Discussion, Robustness, and Extensions

The analysis thus far begs questions of sensitivity and substance. In this section, I demonstrate the robustness of my main findings while explaining how treatment intensity and quantifiable biases render comprehensible the (potentially) discordant implications of discernible cognitive benefits estimated via OLS and the null findings across other domains and fixed effects models.

3.5.1 Robustness: Alternative Outcomes, Covariates, and Weights

The outcomes I assess are comprehensive and conventional, but each is, nevertheless, subject to a series of subjective definitional decisions. In Tables C.3 (children) and C.4A–C.4B (young adults), I test the sensitivity of my results to alternative outcome measures (with associated descriptive statistics given in Tables C.5 and C.6). These measures are variants or constituents of my primary outcomes; full definitions are available in the Appendix. My main results are confirmed.

Tables 3.6 (children) and 3.7 (young adults) repeat the main analysis using conventional covariates. That is, individuals with missing data on any control variable are omitted. I also use continuous (rather than quartile) versions of covariates that are natively so (BMI, birthweight, income, AFQT), and exclude (potentially endogenous) age 0–2 HOME scores. The main results are again confirmed, albeit with somewhat less precision due to smaller sample sizes.

Tables C.7 (children) and C.8 (young adults) repeat the main analysis without using the NLSY-CYA longitudinal custom weights to weight the regressions. The main results are again largely confirmed, with the exception that somewhat more precise young adult OLS estimates detect statistically significant higher probabilities of health issues (by 2.2 pp at 21 and 2.7 pp at 25) and lower probabilities of premartial childbearing (by 2.3 pp at 21 and 2.8 pp at 25).

3.5.2 Survey Nonresponse

Another concern is bias from differential survey nonresponse. In Appendix C.3 (and Tables C.9–C.13), I investigate this issue. I distill two facts: (1) breastfed individuals are about 4 percent more likely to respond, and (2) those who consistently respond to surveys have better outcomes. Together, these patterns impart mild accentuation bias in age 5–21 outcomes, as well as mild attenuation bias in age 25 outcomes, helping explain the observed temporal diminution of breastfeeding associations. Nevertheless, my main conclusions remain unchanged.

3.5.3 Treatment Intensity: Breastfeeding Duration

Prior research has frequently found breastfeeding and its benefits to exhibit a dose-response relationship: the longer breastfeeding continues, the larger the advantages, at least up until a point. In this section, I refine my main results by investigating breastfeeding duration.

Table 3.8 presents my childhood duration OLS results. I assess three (not mutually exclusive) treatment definitions: indicators for breastfeeding durations of at least 3, 6, and 12 months, given in columns. Extended breastfeeding is also somewhat rare: while 53 percent of NLSY-CYA respondents are ever breastfed, this rate drops to 28 percent at three months, 17 percent at six months, and 6 percent at twelve months. Among those breastfed, the mean duration is 23 weeks. As in the main analysis, supercolumns group results by outcome ages and rows index outcomes, with each cell giving estimated average treatment effects from a separate regression.

The big picture point of cognitive gains and noncognitive nulls is reinforced. In the case of the latter, the dose-response question is moot: an intensely null treatment remains null. For cognitive outcomes, the refinement is illuminating. At age 5, I find little evidence of a dose-response relationship. With the exception of vocabulary scores, which peak among those breastfed at least six months, OLS estimates indicate little changes beyond three months. Ages 10 and 13, however, suggest 6 months may be a sweet spot. There are pronounced 6-month peaks in almost all test scores; continuing beyond 12 months does not appear to offer additional gains, and may implicate modest losses.

Table 3.9 gives analogous childhood duration results for extended family fixed effects (I omit mother fixed effects for parsimony). The findings are simple to summarize: uniformly precise nulls, though cognitive point estimates are in keeping with a six-month sweet spot.

The childhood duration analysis mostly reinforces the main results, while suggesting a Goldilocks rule of sorts for maximizing breastfeeding's benefits. By contrast, the young adult OLS duration results in Table 3.10 are as much corrective as they are confirmatory. The education estimates in the first three rows are consistent with the six-month childhood peak in cognition. Those breastfed at least six months are 2.1 pp more likely to graduate high school, 6.2 pp more likely to go to college, and attain 0.28 more years of education by age 25 (Col 5). The effects are somewhat less when including those breastfeed between three and six months (Col 4) and disappear altogether when limited to those breastfed more than a year. The gap is less pronounced, but still apparent, at 21, when individuals breastfed 6+ months are 4.5 pp more likely to have attended college.

The big differences from the main results on breastfeeding initiation are labor and behavior. At age 21, those breastfed at least a year are 8 pp more likely to be employed and earn 173 percent more (Col 3); the earnings (but not employment) bump also remains visible in consideration of those breastfed at least six months (Col 2). In addition, the probability of receiving public benefits at 21 is significantly reduced with as little three months of breastfeeding (-2.7 pp), with the effect strengthening at six (-3.2 pp) and twelve (-5.6 pp) months. By age 25, however, there is a bit of a reversal. While three months of breastfeeding is associated with boosts of 6.1 pp in employment and 3.4 pp in the probability of being engaged in work or school (Col 4), those breastfed longer show no such gains. In addition, there is no evidence for improvements in earnings or reductions in public benefit use. Somewhat unexpectedly, one outcome that is consistent across both ages is behavioral: premarital childbearing. With the exception of 12+ months of breastfeeding at age 25, all durations are associated with reductions of 2.6–4.5 pp in the probability of early fertility. There is also some, albeit uneven, evidence of health impacts, with a 4.9 pp increase in the probability of age 21 health issues and a 4 pp reduction in the probability of being overweight at age 25, both among those breastfed six months or more.

The extended family fixed effects duration analysis for young adults (Table 3.11) provides no evidence for dose-response, or, for that matter, benefits of any sort. There is one exception: a positive, and statistically significant, relation between breastfeeding duration and the probability of overweight—by 7 pp at age 21 and 10–12 pp at age 25 among those breastfed at least six months.

Taken together, the dose-response results indicate young adult outcomes are more sensitive to duration that are those in childhood. Effects on labor and fertility behavior that are absent from treatment defined as breastfeeding initiation emerge among individuals breastfed at least three months. Though employment impacts attenuate somewhat by age 25, educational effects strengthen, and more so for those breastfed at least six months. At the same time, while any amount of breastfeeding has positive associations for child cognition, gains are stronger among those breastfed for prolonged periods.

These relationships should be construed cautiously: it is not clear whether they reflect true intensity effects or, rather, accentuated selectivity among families able to breastfeed longer (e.g., due to higher incomes or more generous family leave). As with the main analysis, the fixed effects results make plain that breastfeeding should not be interpreted outside the overall parental context. And, once again, there is little evidence of enduring health impacts.

3.5.4 Fixed Effects Selection

The null fixed effects results in the main analysis could reflect heterogeneous circumstances (genuinely different breastfeeding impacts for some families) or they could be seen as evidence of omitted variables biasing OLS. My preferred interpretation is the former, and here I provide two pieces of evidence in its favor.

The first is breastfeeding intensity. Extended families with diverse breastfeeding experiences also breastfeed shorter. Mean breastfeeding duration among families with unitary breastfeeding outcomes is 25.3 weeks. Among the subsample contributing to extended family identification, it drops to 17.7 weeks—a 30 percent reduction (see Table 3.12). Given the evidence for dose-response, this diminished duration is one explanation for smaller effects.

The second piece of evidence is sample composition. The extended family (identification) subsample is negatively selected. Tables 3.12 (covariates) and 3.13 (outcomes) provide descriptive statistics comparing these families with the larger sample of families with uniform infant feeding experiences. In comparison to those from feeding-consistent families, children from breastfeeding-variant ones have consistently worse cognitive outcomes throughout childhood (by 0.1–0.2 SD's), though noncognitive contrasts are minor and gaps appear to close by young adulthood. What's more, the feeding-variant subsample is generally disadvantaged, with mothers, who, among other things, have lower AFQT scores, income, HOME scores, and probabilities of attending college. In other words, breastfeeding may have less impact for these families because it is fighting stronger headwinds.

These facts carry an important implication countercurrent to the conventional wisdom: the assumed upward bias of OLS is arguably less important than the definite downward bias of fixed effects. Moreover, it appears not that breastfeeding-variant mothers "make up" for not breastfeeding some children, but instead the reverse: the circumstances that make it less likely to breastfeed result in disadvantages that dominate any beneficial effects of breastfeeding.

Tables C.14 (children) and C.15 (young adults) confirm these intuitions: excluding children from breastfeeding-variant families from the OLS analysis consistently bolsters breastfeeding coefficients by between about a fifth to a half, and renders some newly significant.

Whether this negative selection among inconsistently breastfeeding families is a general phenomenon or specific to the NLSY-CYA (whose mothers were co-resident 14–22-year-old sisters in 1979) is an open question.

3.5.5 External Validity

The majority of this discussion has addressed questions of internal validity. But there is also a point of caution regarding generalizability. The NLSY-CYA's greatest strength—its extraordinary longitudinality—is also its greatest weakness: it follows the offspring of a specific cohort of women (those born between 1957 and 1964) during a particular period in American history (that closing decades of the 20th century and the early years of the 21st)—a period not only of rapidly evolving breastfeeding practices, but also one of unprecedented prosperity and many changes of other sorts. While the NLSY-CYA is representative of this generation of children, it is not assured that results apply with equal force to children situated in other settings—though the timeliness of the data bolsters the case for relevance to affluent countries today. On the other hand, my results should not be extrapolated to developing countries, where concerns about sanitation, malnutrition, and infectious disease burden are paramount and make breastfeeding correspondingly more essential.

3.6 Conclusion

The current consensus among public health authorities is six months of exclusive breastfeeding for all children. My results suggest that this edict is reasonable for most families.

Across a broader set of outcomes spanning a longer trajectory of ages and controlling for more characteristics than previously assessed, I find that, on average (per OLS), breastfeeding is associated with modest but persistent cognitive benefits, ranging from higher test scores during childhood to greater educational attainment by young adulthood. Nevertheless, I cannot claim causality. While the detail of the NLSY-CYA renders the scope for unobserved influences comparatively small, omitted variables remain (notably paternal characteristics and maternal intangibles). Similarly, gains in young adult labor outcomes and fertility that appear conditional upon sustained breastfeeding durations of at least three months, as well as childhood cognitive outcomes that are strongest among those breastfed 6–12 months, may reflect genuine dose-response—or, instead, more pronounced sample selection among families able to breastfeed longer.

In contrast, I find little evidence of health benefits (and some results to the contrary). This is surprising given the well-established linkages between breastfeeding and infant health—and especially in light of the economics literature on the enduring importance of early-life experiences[34]. One intriguing possibility is that the most important consequence of infant health is cognitive development.

At the same time, not everyone is average, and all else is not always equal. Family fixed effects results indicate breastfeeding is less impactful among kin with differential breastfeeding experiences: within-family comparisons yield consistently null contrasts. I find that part of the explanation is context. Families with diverse breastfeeding are worse off than average and breastfeed less intensively.

My preferred interpretation is that the OLS and fixed effects findings are not mutually exclusive. Allowing for the effects of breastfeeding to be heterogeneous (which any reasonable understanding of parenting choices of any sort must permit), responses will vary. It is entirely possible OLS accurately describes an average treatment effect of breastfeeding that truly confers modest intellectual advantages among the popu-

[34]Almond and Currie (2011*a*,*b*); Currie and Rossin-Slater (2015) and Almond, Currie and Duque (2018) summarize this work.

lation at large, while, simultaneously, fixed effects characterizes an equally genuine observation that women who, by choice or circumstance, breastfeed some children and not others can (dis)compensate human capital investment in other ways. Breastfeeding can be an advantage without being a panacea.

Nor need breastfeeding's impacts be strictly literal. Breastfeeding entails a variety of interactions, time allocations, and related behaviors beyond the physical contents of mother's milk; precisely deciphering the relative contributions of each component is less important than recognizing the effects of the complete package.

The lesson for policymakers and medical professionals is that breastfeeding should not be one-size-fits all. Exclusive breastfeeding for six months appears optimal for many—and perhaps most—families. However, as the fixed effects results suggest, the overall environment into which breastfeeding is introduced cannot be ignored. Formula should not be a four-letter-word; the focus instead ought be on formulating strategies that replicate breastfeeding's most essential elements—nutrients, nurturing, and a general predisposition for conscientious parenting—in manners tailored to families' preferences, constraints, and resource endowments.

More research needed in this regard: little is known about the heterogeneity of breastfeeding itself. Variates of interest, in addition to supplementation and longevity, include frequency and quantity of feeds, manner of administration (e.g., breast or bottle), identity of feeder (e.g., mother or other), introduction of solid foods, and individual-level variation in the composition of breastmilk. Perhaps more important, it is essential to understand what extant omitted variables, if any, may account for breastfeeding's positive associations—and what may be done to promote such behaviors. The reverse question is also valid: to what extent does breastfeeding encourage other favorable changes in parents? What seems clear, however, is that breastfeeding is but one element of propitious parenting—an element whose importance ought not be overstated and must be interpreted in the context of the full body of work.

Internal validity aside, there is a point of caution to bear in mind when generalizing these findings. The NLSY follows a very specific cohort of American women (those born 1957–1964) during a period of time that not only witnessed widespread peace and prosperity, but also in which breastfeeding practices in the U.S. were evolving rapidly[35]—conditions that may not be replicable. In particular, my results have limited relevance for low- and middle-income countries, where sanitary deficiencies, infectious disease burden, and risk of malnutrition make breastfeeding considerably more essential[36].

Breastfeeding does not occur in a vacuum. Context matters. Being the type of mother who would breastfeed—or who conditionally would not—is probably more important than its realization. This does not mean breastfeeding does not have a true effect. It only suggests there are multiple means to achieving identical ends, and that impacts depend on environment. Far more important than the what of infant nutrition is the who and how of parenting—the constellation of behaviors, interactions, and values that go into raising a child.

[35]See Baker (2016) for details.
[36]Victora et al. (2016).

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3.8 Tables

Table 3.1: National Longitudinal Survey of Youth Child and Young Adults, 1986-2016, Sample Overview

			Interviews Among Breastfed					
	All	BF Sample	Age 5	Age 10	Age 13	Age 21	Age 25	All
Ordinary Least Squares (OLS)	11,530	10,842	7,790	7,874	7,490	6,354	5,587	4,020
Mother Fixed Effects	3,543	3,066	2,245	2,281	2,206	1,923	1,716	1,260
Extended Family Fixed Effects	4,116	3,684	2,743	2,763	2,657	2,306	2,029	1,503

This table describes age-interview responses for the National Longitudinal Survey of Youth: Child and Young Adult 1986-2016 dataset. Rows enumerate identification strategies. Column 1 includes all observations. Column 2 includes observations with breastfeeding data. Columns 3–8 indicate age-interview responses among those with breastfeeding data. Each cell gives a count of observations. Unit of observation is individual child.

Table 3.2: Descriptive Statistics, NLSY-CYA 1986-2016 Breastfeeding Sample

	Age 5 Mean Contrast			Means by Age				
Variable	BF	Not BF	Diff	Age 5	Age 10	Age 13	Age 21	Age 25
Mom's Birth Year	1961.4	1961.5	-0.0	1961.4	1961.2	1961.2	1961.0	1960.8
Mom's Education: Less than HS	0.09	0.24	-0.15**	0.16	0.19	0.19	0.21	0.24
Mom's Education: High School	0.36	0.50	-0.13**	0.42	0.42	0.42	0.44	0.45
Mom's Education: Some College+	0.54	0.25	0.29**	0.41	0.38	0.37	0.33	0.28
Mom's Age	28.55	25.98	2.57**	27.45	26.48	26.33	24.97	23.70
Mom's Race: Hispanic	0.07	0.09	-0.01**	0.08	0.08	0.08	0.08	0.08
Mom's Race: White	0.86	0.66	0.20**	0.77	0.76	0.77	0.76	0.75
Mom's Race: Black	0.07	0.26	-0.19**	0.15	0.16	0.16	0.16	0.17
Mom Foreign-Born	0.05	0.04	0.01	0.05	0.05	0.04	0.04	0.05
Mom's Age 21 BMI	21.93	23.20	-1.27**	22.47	22.58	22.63	22.71	22.82
Mom AFQT Quartile	54.94	33.64	21.31**	45.83	44.87	44.91	43.36	41.29
Mom Employed	0.80	0.73	0.07**	0.77	0.77	0.77	0.79	0.79
Mom's Income	97901	59778	38123**	82194	79964	79433	68891	63005
Mom Region: Northeast	0.16	0.18	-0.02	0.17	0.16	0.15	0.15	0.13
Mom Region: North Central	0.28	0.26	0.02	0.27	0.25	0.25	0.27	0.26
Mom Region: South	0.25	0.36	-0.10**	0.30	0.27	0.27	0.28	0.27
Mom Region: West	0.21	0.11	0.10**	0.17	0.15	0.15	0.15	0.15
Mom Marital Status: Never Married	0.15	0.30	-0.15**	0.21	0.21	0.20	0.22	0.24
Mom Marital Status: Married	0.69	0.51	0.18**	0.61	0.56	0.55	0.56	0.51
Mom Marital Status: Other	0.07	0.10	-0.02**	0.08	0.08	0.07	0.08	0.07
Mom Prenatal Visit	0.99	0.99	0.01**	0.99	0.99	0.99	0.99	0.99
Mom Drink	0.53	0.45	0.08**	0.49	0.49	0.50	0.50	0.49
Mom Smoke	0.25	0.40	-0.15**	0.31	0.32	0.32	0.35	0.37
Mom Prenatal Vitamins	0.98	0.94	0.04**	0.96	0.96	0.96	0.95	0.95
C-Section	0.23	0.24	-0.01	0.24	0.22	0.22	0.21	0.21
Preterm Birth	0.11	0.15	-0.04**	0.12	0.12	0.12	0.12	0.12
Long Hospital Stay	0.06	0.11	-0.05**	0.08	0.08	0.08	0.08	0.08
Female	0.49	0.48	0.01	0.49	0.49	0.49	0.49	0.48
Birth Parity: 1	0.41	0.37	0.05**	0.39	0.43	0.43	0.45	0.49
Birth Parity: 2	0.34	0.37	-0.02**	0.36	0.34	0.34	0.34	0.33
Birth Parity: 3+	0.24	0.26	-0.02*	0.25	0.23	0.23	0.21	0.18
Birthweight	7.57	7.24	0.33**	7.43	7.41	7.41	7.41	7.37
Birth Month	6.54	6.56	-0.02	6.55	6.58	6.60	6.66	6.66
Birth Year	1989.7	1987.2	2.5**	1988.6	1987.4	1987.2	1985.7	1984.2
HOME Score Age 0-2	0.13	-0.17	0.30**	0.01	0.01	0.01	0.03	0.01
Has Aunt	0.32	0.33	-0.01	0.33	0.31	0.31	0.30	0.28
Obs.	3,784	4,006	7,790	7,790	7,874	7,490	6,354	5,587

This table summarizes covariates for the 1986–2016 NLSY-CYA. Columns 1–3 describe the differences in means by breastfeeding status for the age 5 interview sample, obtained from separate bivariate OLS regressions of each characteristic on the treatment indicator. Columns 4–8 give overall means for each age-interview sample. Unit of observation is individual child. All statistics are weighted using NLSY-CYA longitudinal custom weights for the corresponding age-interview sample. Categorical indicators do not sum to one due to missing values. See Appendix for additional covariate detail. * $p < 0.10$, ** $p < 0.05$

Table 3.3: Descriptive Outcome Statistics, NLSY-CYA 1986-2016 Breastfeeding Sample

	Overall			Breastfeeding Comparison				
	Mean	SD	Obs	Yes	No	Diff	SE	T-Stat
A. Age 5 Outcomes								
PIAT Math (standard deviation units)	0.15	0.94	5,710	0.32	-0.07	0.39**	0.03	11.88
PIAT Reading Recognition (std. dev. units)	0.54	0.92	5,600	0.69	0.33	0.36**	0.03	11.35
PIAT Reading Comprehension (std. dev. units)	0.82	0.81	1,964	0.90	0.72	0.19**	0.04	4.16
PPVT Vocabulary (std. dev. units)	-0.39	1.37	4,237	-0.14	-0.74	0.60**	0.05	11.14
Health Problem	0.43	0.50	7,759	0.46	0.39	0.07**	0.02	4.63
Behavior Problems Index (std. dev. units)	0.18	0.99	7,347	0.08	0.30	-0.21**	0.03	-6.31
Overweight	0.01	0.12	7,790	0.01	0.02	-0.01**	0.00	-2.66
B. Age 10 Outcomes								
PIAT Math (standard deviation units)	0.32	0.99	7,105	0.55	0.03	0.53**	0.03	16.88
PIAT Reading Recognition (std. dev. units)	0.42	1.01	7,101	0.62	0.18	0.43**	0.03	12.94
PIAT Reading Comprehension (std. dev. units)	0.16	0.92	6,989	0.34	-0.07	0.40**	0.03	13.52
PPVT Vocabulary (std. dev. units)	-0.12	1.29	6,024	0.20	-0.49	0.69**	0.04	15.92
Health Problem	0.33	0.47	7,806	0.34	0.31	0.03**	0.01	2.30
Behavior Problems Index (std. dev. units)	0.31	0.99	7,376	0.22	0.42	-0.20**	0.03	-6.26
Overweight	0.09	0.29	7,874	0.07	0.11	-0.04**	0.01	-4.78
C. Age 13 Outcomes								
PIAT Math (standard deviation units)	0.22	0.98	6,315	0.45	-0.06	0.51**	0.03	15.47
PIAT Reading Recognition (std. dev. units)	0.39	1.06	6,323	0.61	0.13	0.48**	0.04	13.33
PIAT Reading Comprehension (std. dev. units)	-0.06	0.88	6,269	0.14	-0.29	0.42**	0.03	14.29
PPVT Vocabulary (std. dev. units)	-0.42	1.16	857	-0.21	-0.57	0.36**	0.11	3.27
Health Problem	0.30	0.46	7,123	0.32	0.28	0.04**	0.01	2.81
Behavior Problems Index (std. dev. units)	0.36	0.99	6,764	0.30	0.42	-0.12**	0.03	-3.59
Overweight	0.16	0.37	7,490	0.14	0.19	-0.05**	0.01	-5.02
D. Age 21 Outcomes								
High School Grad+	0.87	0.34	5,751	0.92	0.81	0.10**	0.01	9.25
Some College+	0.54	0.50	5,751	0.64	0.43	0.21**	0.02	11.89
Years of Education	12.95	1.75	5,751	13.30	12.55	0.75**	0.06	11.81
Employed	0.65	0.48	5,927	0.68	0.63	0.04**	0.02	2.80
In School or Working	0.80	0.40	6,140	0.85	0.75	0.11**	0.01	8.34
Log Earnings (2019 Dollars)	7.94	3.34	5,294	8.10	7.76	0.34**	0.11	3.04
Public Benefits	0.16	0.37	6,349	0.12	0.21	-0.09**	0.01	-7.95
Health Problem	0.27	0.44	6,337	0.29	0.24	0.04**	0.01	2.96
Overweight	0.36	0.48	6,353	0.32	0.41	-0.09**	0.02	-6.03
Premarital Child	0.17	0.37	6,354	0.11	0.23	-0.13**	0.01	-10.95
E. Age 25 Outcomes								
High School Grad+	0.88	0.32	5,267	0.92	0.84	0.08**	0.01	7.25
Some College+	0.56	0.50	5,267	0.65	0.47	0.17**	0.02	9.48
Years of Education	13.49	2.29	5,267	13.94	13.04	0.90**	0.09	10.03
Employed	0.74	0.44	4,922	0.78	0.71	0.06**	0.02	4.13
In School or Working	0.79	0.41	5,014	0.83	0.76	0.07**	0.01	4.76
Log Earnings (2019 Dollars)	8.86	3.43	4,946	9.08	8.65	0.43**	0.12	3.56
Public Benefits	0.21	0.41	5,579	0.17	0.26	-0.09**	0.01	-6.96
Health Problem	0.32	0.47	5,583	0.34	0.31	0.03*	0.02	1.86
Overweight	0.41	0.49	5,587	0.35	0.46	-0.12**	0.02	-6.99
Premarital Child	0.28	0.45	5,587	0.20	0.36	-0.16**	0.02	-10.62

This table summarizes outcomes for the 1986–2016 NLSY-CYA sample with reported breastfeeding statuses. Columns 1–3 give overall means, standard deviations, and sample sizes. Cols 4–8 give mean comparisons by breastfeeding status, with respective group means for breastfed and not breastfed in Cols 4 and 5, point estimates for the mean differences in Col 6, and associated standard errors and test statistics in Cols 7 and 8. Results are obtained from separate bivariate OLS regressions of each outcome on the breastfeeding treatment indicator. Unit of observation is individual child. All statistics are weighted using NLSY-CYA longitudinal custom weights for the corresponding age-outcome sample. * $p < 0.10$, ** $p < 0.05$

Table 3.4: Child Outcomes

	Age 5			Age 10			Age 13		
	(1) OLS	(2) MOM	(3) EXT	(4) OLS	(5) MOM	(6) EXT	(7) OLS	(8) MOM	(9) EXT
Math	0.098**	0.121*	0.082	0.114**	-0.040	-0.028	0.116**	-0.004	0.019
	(0.033)	(0.071)	(0.072)	(0.031)	(0.055)	(0.059)	(0.032)	(0.058)	(0.062)
	{5,710}	{906}	{1,319}	{7,105}	{1,448}	{1,903}	{6,315}	{1,256}	{1,679}
Reading Recog.	0.070**	0.040	-0.029	0.073**	0.065	0.042	0.108**	0.026	0.009
	(0.031)	(0.065)	(0.066)	(0.034)	(0.056)	(0.061)	(0.037)	(0.061)	(0.067)
	{5,600}	{881}	{1,293}	{7,101}	{1,442}	{1,896}	{6,323}	{1,255}	{1,675}
Reading Comp.	0.020	0.284**	0.122	0.107**	-0.009	-0.002	0.111**	-0.039	-0.039
	(0.043)	(0.130)	(0.158)	(0.030)	(0.051)	(0.054)	(0.030)	(0.052)	(0.056)
	{1,964}	{120}	{184}	{6,989}	{1,409}	{1,857}	{6,269}	{1,227}	{1,636}
Vocabulary	0.129**	0.008	0.072	0.157**	0.056	0.012	-0.129	-0.347	-0.553
	(0.050)	(0.120)	(0.113)	(0.041)	(0.075)	(0.081)	(0.094)	(0.252)	(0.406)
	{4,237}	{518}	{801}	{6,024}	{1,076}	{1,444}	{857}	{40}	{45}
Health Problem	0.013	-0.029	-0.003	-0.005	0.005	0.012	0.027*	0.060*	0.055*
	(0.017)	(0.031)	(0.032)	(0.016)	(0.029)	(0.031)	(0.016)	(0.031)	(0.032)
	{7,759}	{1,505}	{2,114}	{7,806}	{1,629}	{2,164}	{7,123}	{1,458}	{1,953}
Behavior (BPI)	-0.037	-0.077	-0.117**	-0.018	0.056	0.008	0.064*	0.014	0.001
	(0.034)	(0.051)	(0.055)	(0.034)	(0.049)	(0.053)	(0.035)	(0.054)	(0.059)
	{7,347}	{1,337}	{1,910}	{7,376}	{1,475}	{1,988}	{6,764}	{1,337}	{1,793}
Overweight	-0.007*	-0.004	-0.005	-0.017*	0.006	0.001	-0.011	0.013	0.009
	(0.004)	(0.006)	(0.007)	(0.009)	(0.018)	(0.019)	(0.012)	(0.023)	(0.023)
	{7,790}	{1,513}	{2,130}	{7,874}	{1,659}	{2,204}	{7,490}	{1,601}	{2,144}

Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for maternal characteristics (categorical indicators for mother's education, race, age, year of birth, region, marital status, AFTQ quartile, family income quartile, age 21 BMI quartile, foreign nativity, and having a sister), maternal pregnancy behaviors (categorical indicators for prenatal visits, prenatal vitamins, smoking, and alcohol consumption), and child's birth circumstances (categorical indicators for child's birth order, sex, birthweight quartile, C-section delivery, preterm birth, hospital stay longer than mother's, and age 0–2 HOME quartile), as well as a quadratic in child's birth year and indicators for birth month. All covariates include a category for missing values, and are defined at the time of child's birth or during preceding year, unless otherwise noted. In subsequent tables, this collection of controls is referred to as covariates. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status and outcomes. All regressions are weighted using NLSY-CYA longitudinal custom weights for the age-interview sample. Standard errors clustered by mother in parentheses. Number of observations in braces; for fixed effects specifications, counts refer to observations contributing to breastfeeding identification.
* $p < 0.10$, ** $p < 0.05$

Table 3.5: Young Adult Outcomes

	Age 21			Age 25		
	(1) OLS	(2) MOM	(3) EXT	(4) OLS	(5) MOM	(6) EXT
High School Grad+	0.025**	0.008	0.001	0.027**	-0.010	0.000
	(0.012)	(0.024)	(0.025)	(0.012)	(0.025)	(0.026)
	{5,751}	{1,069}	{1,489}	{5,267}	{965}	{1,291}
Some College+	0.046**	-0.006	0.002	0.031*	0.012	0.021
	(0.017)	(0.030)	(0.032)	(0.018)	(0.032)	(0.034)
	{5,751}	{1,069}	{1,489}	{5,267}	{965}	{1,291}
Years of Education	0.141**	0.068	0.062	0.102	0.074	0.095
	(0.062)	(0.132)	(0.131)	(0.082)	(0.138)	(0.149)
	{5,751}	{1,072}	{1,492}	{5,267}	{965}	{1,291}
Employed	0.010	-0.031	-0.016	0.007	-0.044	-0.001
	(0.017)	(0.035)	(0.038)	(0.017)	(0.031)	(0.034)
	{5,927}	{1,169}	{1,569}	{4,922}	{892}	{1,174}
In School or Working	0.019	-0.026	-0.018	0.004	-0.040	-0.005
	(0.013)	(0.029)	(0.031)	(0.015)	(0.028)	(0.031)
	{6,140}	{1,210}	{1,631}	{5,014}	{917}	{1,203}
Log Earned Income	-0.034	0.122	0.119	-0.118	-0.000	0.001
	(0.125)	(0.269)	(0.292)	(0.130)	(0.263)	(0.285)
	{5,294}	{934}	{1,266}	{4,946}	{902}	{1,174}
Public Benefits	-0.011	-0.016	-0.008	-0.001	-0.028	-0.028
	(0.012)	(0.026)	(0.027)	(0.014)	(0.030)	(0.032)
	{6,349}	{1,275}	{1,723}	{5,579}	{1,087}	{1,422}
Health Problem	0.021	-0.006	-0.026	0.027	0.057	0.037
	(0.016)	(0.034)	(0.035)	(0.018)	(0.037)	(0.040)
	{6,337}	{1,271}	{1,718}	{5,583}	{1,088}	{1,423}
Overweight	-0.022	0.033	0.024	-0.046**	0.027	0.016
	(0.016)	(0.032)	(0.034)	(0.016)	(0.031)	(0.035)
	{6,353}	{1,277}	{1,726}	{5,587}	{1,088}	{1,424}
Premarital Child	-0.015	-0.006	0.003	-0.019	0.010	0.002
	(0.012)	(0.024)	(0.025)	(0.016)	(0.031)	(0.035)
	{6,354}	{1,277}	{1,726}	{5,587}	{1,088}	{1,424}

Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for full covariates, as described in Table 3.4. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status and outcomes. All regressions are weighted using NLSY-CYA longitudinal custom weights for the age-interview sample. Standard errors clustered by mother in parentheses. Number of observations in braces; for fixed effects specifications, counts refer to observations contributing to breastfeeding identification. * $p < 0.10$, ** $p < 0.05$

Table 3.6: Child Outcomes: Conventional Covariates

	Age 5			Age 10			Age 13		
	(1) OLS	(2) MOM	(3) EXT	(4) OLS	(5) MOM	(6) EXT	(7) OLS	(8) MOM	(9) EXT
Math	0.098**	0.084	0.076	0.081**	-0.073	-0.101	0.111**	0.118	0.091
	(0.041)	(0.112)	(0.113)	(0.041)	(0.093)	(0.096)	(0.043)	(0.108)	(0.111)
	{3,267}	{321}	{507}	{3,566}	{421}	{639}	{3,287}	{387}	{571}
Reading Recog.	0.071*	0.008	-0.029	0.058	0.084	0.043	0.075	0.091	0.033
	(0.040)	(0.094)	(0.097)	(0.044)	(0.092)	(0.103)	(0.049)	(0.108)	(0.115)
	{3,196}	{314}	{493}	{3,567}	{422}	{640}	{3,295}	{388}	{575}
Reading Comp.	0.048	-0.233**	-0.092	0.080**	-0.089	-0.036	0.084**	-0.023	-0.110
	(0.055)	(0.105)	(0.134)	(0.039)	(0.088)	(0.097)	(0.039)	(0.078)	(0.089)
	{1,030}	{39}	{69}	{3,518}	{419}	{624}	{3,264}	{382}	{563}
Vocabulary	0.052	0.045	0.131	0.123**	0.060	-0.101	0.399**	0.000	0.000
	(0.065)	(0.213)	(0.211)	(0.052)	(0.138)	(0.145)	(0.157)	(.)	(.)
	{2,201}	{158}	{281}	{3,035}	{305}	{471}	{199}		
Health Problem	0.015	0.001	0.008	-0.006	-0.009	-0.001	-0.000	0.056	0.054
	(0.021)	(0.051)	(0.053)	(0.022)	(0.055)	(0.057)	(0.022)	(0.056)	(0.058)
	{4,386}	{532}	{808}	{3,862}	{471}	{720}	{3,600}	{432}	{650}
Behavior (BPI)	-0.052	-0.112	-0.128	-0.039	-0.051	-0.046	0.070	-0.037	-0.057
	(0.044)	(0.078)	(0.092)	(0.045)	(0.098)	(0.103)	(0.047)	(0.087)	(0.091)
	{4,175}	{483}	{742}	{3,641}	{407}	{642}	{3,436}	{395}	{588}
Overweight	-0.005	0.000	-0.002	-0.004	-0.014	-0.016	-0.019	-0.008	-0.047
	(0.004)	(0.011)	(0.011)	(0.012)	(0.032)	(0.033)	(0.016)	(0.041)	(0.043)
	{4,395}	{537}	{813}	{3,874}	{477}	{725}	{3,788}	{472}	{706}

This table repeats the main analysis using conventional covariates. Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for full covariates; the difference from the main text is that individuals with missing data for any covariate are dropped and HOME scores are omitted. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status, outcomes, and covariates. All regressions are weighted using NLSY-CYA longitudinal custom weights for the age-interview sample. Standard errors clustered by mother in parentheses. Number of observations in braces; for fixed effects specifications, counts refer to observations contributing to breastfeeding identification. * $p < 0.10$, ** $p < 0.05$

Table 3.7: Young Adult Outcomes: Conventional Covariates

	Age 21			Age 25		
	(1) OLS	(2) MOM	(3) EXT	(4) OLS	(5) MOM	(6) EXT
High School Grad+	0.033**	-0.026	-0.015	0.027*	0.002	0.006
	(0.016)	(0.040)	(0.041)	(0.015)	(0.039)	(0.039)
	{3,262}	{365}	{559}	{3,051}	{353}	{512}
Some College+	0.050**	-0.032	-0.037	0.030	0.046	0.025
	(0.022)	(0.042)	(0.044)	(0.023)	(0.057)	(0.059)
	{3,262}	{365}	{559}	{3,051}	{353}	{512}
Years of Education	0.165**	0.234	0.142	0.111	0.352	0.248
	(0.083)	(0.263)	(0.247)	(0.106)	(0.235)	(0.244)
	{3,262}	{368}	{562}	{3,051}	{353}	{512}
Employed	-0.001	-0.058	-0.033	0.022	-0.064	0.004
	(0.022)	(0.059)	(0.062)	(0.022)	(0.060)	(0.063)
	{3,280}	{380}	{561}	{2,588}	{265}	{390}
In School or Working	0.020	-0.023	-0.020	0.011	-0.084	-0.034
	(0.017)	(0.046)	(0.049)	(0.021)	(0.056)	(0.060)
	{3,371}	{393}	{581}	{2,644}	{279}	{407}
Log Earned Income	-0.013	0.098	-0.098	-0.037	0.101	0.170
	(0.158)	(0.461)	(0.483)	(0.169)	(0.508)	(0.532)
	{2,873}	{292}	{443}	{2,736}	{299}	{428}
Public Benefits	-0.008	-0.022	-0.006	-0.012	-0.048	-0.051
	(0.016)	(0.045)	(0.047)	(0.019)	(0.054)	(0.057)
	{3,430}	{409}	{608}	{3,048}	{353}	{512}
Health Problem	0.023	-0.021	-0.052	0.042*	-0.035	-0.028
	(0.021)	(0.062)	(0.061)	(0.023)	(0.061)	(0.067)
	{3,430}	{409}	{607}	{3,050}	{353}	{512}
Overweight	-0.032	0.046	0.032	-0.025	0.092**	0.091*
	(0.022)	(0.057)	(0.060)	(0.020)	(0.045)	(0.048)
	{3,431}	{409}	{608}	{3,051}	{353}	{512}
Premarital Child	-0.014	0.017	0.013	-0.008	0.038	0.015
	(0.015)	(0.042)	(0.045)	(0.020)	(0.053)	(0.057)
	{3,431}	{409}	{608}	{3,051}	{353}	{512}

This table repeats the main analysis using conventional covariates. Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for full covariates; the difference from the main text is that individuals with missing data for any covariate are dropped and HOME scores are omitted. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status, outcomes, and covariates. All regressions are weighted using NLSY-CYA longitudinal custom weights for the age-interview sample. Standard errors clustered by mother in parentheses. Number of observations in braces; for fixed effects specifications, counts refer to observations contributing to breastfeeding identification. * $p < 0.10$, ** $p < 0.05$

Table 3.8: Child Outcomes: Breastfeeding Duration, OLS Estimates

	Age 5			Age 10			Age 13		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	3m+	6m+	12m+	3m+	6m+	12m+	3m+	6m+	12m+
Math	0.078**	0.060	-0.020	0.133**	0.170**	0.136**	0.150**	0.215**	0.196**
	(0.037)	(0.045)	(0.071)	(0.034)	(0.039)	(0.061)	(0.036)	(0.043)	(0.066)
	{5,560}	{5,560}	{5,560}	{6,946}	{6,946}	{6,946}	{6,172}	{6,172}	{6,172}
Reading Recog.	0.096**	0.060	0.032	0.047	0.119**	0.065	0.077**	0.115**	0.078
	(0.034)	(0.042)	(0.068)	(0.036)	(0.040)	(0.061)	(0.039)	(0.043)	(0.062)
	{5,457}	{5,457}	{5,457}	{6,942}	{6,942}	{6,942}	{6,180}	{6,180}	{6,180}
Reading Comp.	-0.033	-0.023	-0.144	0.125**	0.216**	0.236**	0.122**	0.179**	0.167**
	(0.049)	(0.064)	(0.142)	(0.034)	(0.039)	(0.058)	(0.033)	(0.038)	(0.056)
	{1,909}	{1,909}	{1,909}	{6,832}	{6,832}	{6,832}	{6,128}	{6,128}	{6,128}
Vocabulary	0.147**	0.194**	0.051	0.179**	0.211**	0.190**	-0.100	0.012	0.213
	(0.059)	(0.075)	(0.121)	(0.048)	(0.060)	(0.094)	(0.106)	(0.134)	(0.270)
	{4,131}	{4,131}	{4,131}	{5,885}	{5,885}	{5,885}	{855}	{855}	{855}
Health Problem	-0.005	0.002	-0.034	-0.021	-0.004	-0.028	0.009	0.005	-0.004
	(0.019)	(0.021)	(0.033)	(0.018)	(0.021)	(0.030)	(0.018)	(0.021)	(0.032)
	{7,565}	{7,565}	{7,565}	{7,634}	{7,634}	{7,634}	{6,963}	{6,963}	{6,963}
Behavior (BPI)	-0.063	-0.049	0.011	-0.037	-0.010	0.077	0.023	-0.005	0.051
	(0.039)	(0.043)	(0.064)	(0.039)	(0.045)	(0.070)	(0.040)	(0.044)	(0.065)
	{7,162}	{7,162}	{7,162}	{7,214}	{7,214}	{7,214}	{6,609}	{6,609}	{6,609}
Overweight	0.002	0.003	0.002	-0.011	0.000	-0.012	-0.011	0.000	-0.003
	(0.004)	(0.004)	(0.007)	(0.009)	(0.011)	(0.016)	(0.012)	(0.014)	(0.020)
	{7,596}	{7,596}	{7,596}	{7,702}	{7,702}	{7,702}	{7,321}	{7,321}	{7,321}

Rows enumerate outcomes. Supercolumns denote outcome ages. Columns correspond binary indicators for breastfeeding duration: 3 months or more (Cols 1, 4, 7), 6 months or more (Cols 2, 5, 8), and 12 months or more (Cols 3, 6, 9). Each cell reports the coefficient on column-enunumerated breastfeeding treatment indicator from a separate OLS regression. All regressions control for full covariates. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status and outcomes. Standard errors clustered by mother in parentheses. Number of observations in braces. * $p < 0.10$, ** $p < 0.05$

Table 3.9: Child Outcomes: Breastfeeding Duration, Extended Family Fixed Effects Estimates

	Age 5			Age 10			Age 13		
	(1) 3m+	(2) 6m+	(3) 12m+	(4) 3m+	(5) 6m+	(6) 12m+	(7) 3m+	(8) 6m+	(9) 12m+
Math	0.039	-0.024	-0.068	-0.036	0.010	0.052	0.014	0.060	-0.019
	(0.079)	(0.081)	(0.113)	(0.056)	(0.062)	(0.093)	(0.062)	(0.072)	(0.109)
	{1,214}	{1,214}	{1,214}	{1,784}	{1,784}	{1,784}	{1,577}	{1,577}	{1,577}
Reading Recog.	0.094	-0.054	-0.009	-0.006	-0.002	-0.091	0.033	0.050	-0.075
	(0.072)	(0.080)	(0.114)	(0.060)	(0.070)	(0.101)	(0.065)	(0.080)	(0.100)
	{1,194}	{1,194}	{1,194}	{1,778}	{1,778}	{1,778}	{1,574}	{1,574}	{1,574}
Reading Comp.	0.007	0.198	0.102	0.039	0.141**	0.044	-0.003	0.045	-0.057
	(0.164)	(0.157)	(0.343)	(0.053)	(0.064)	(0.083)	(0.059)	(0.068)	(0.094)
	{171}	{171}	{171}	{1,739}	{1,739}	{1,739}	{1,535}	{1,535}	{1,535}
Vocabulary	-0.089	0.001	-0.448*	-0.018	0.139	0.021	0.012	-0.020	-1.048
	(0.104)	(0.126)	(0.244)	(0.083)	(0.099)	(0.131)	(0.369)	(0.483)	(2.044)
	{753}	{753}	{753}	{1,345}	{1,345}	{1,345}	{45}	{45}	{45}
Health Problem	-0.012	-0.012	-0.029	0.003	0.040	0.043	0.011	0.035	0.017
	(0.033)	(0.036)	(0.050)	(0.031)	(0.038)	(0.053)	(0.033)	(0.037)	(0.054)
	{1,976}	{1,976}	{1,976}	{2,029}	{2,029}	{2,029}	{1,839}	{1,839}	{1,839}
Behavior (BPI)	-0.074	-0.050	-0.002	0.004	-0.020	-0.016	0.009	0.003	0.031
	(0.056)	(0.063)	(0.087)	(0.062)	(0.065)	(0.095)	(0.065)	(0.072)	(0.100)
	{1,780}	{1,780}	{1,780}	{1,869}	{1,869}	{1,869}	{1,689}	{1,689}	{1,689}
Overweight	0.004	0.000	-0.006	-0.018	-0.002	0.004	-0.019	0.018	0.024
	(0.006)	(0.007)	(0.012)	(0.016)	(0.018)	(0.025)	(0.023)	(0.027)	(0.035)
	{1,991}	{1,991}	{1,991}	{2,069}	{2,069}	{2,069}	{2,022}	{2,022}	{2,022}

Rows enumerate outcomes. Supercolumns denote outcome ages. Columns correspond binary indicators for breastfeeding duration: 3 months or more (Cols 1, 4, 7), 6 months or more (Cols 2, 5, 8), and 12 months or more (Cols 3, 6, 9). Each cell reports the coefficient on column-enunumerated breastfeeding treatment indicator from a separate extended family fixed effects regression. All regressions control for full covariates. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status and outcomes. Standard errors clustered by mother in parentheses. Number of observations contributing to breastfeeding identification in braces. * $p < 0.10$, ** $p < 0.05$

Table 3.10: Young Adult Outcomes: Breastfeeding Duration, OLS Estimates

	Age 21			Age 25		
	(1) 3m+	(2) 6m+	(3) 12+	(4) 3m+	(5) 6m+	(6) 12+
High School Grad+	-0.005	-0.008	-0.027	0.019	0.021*	-0.013
	(0.011)	(0.013)	(0.021)	(0.012)	(0.012)	(0.022)
	{5,576}	{5,576}	{5,576}	{5,116}	{5,116}	{5,116}
Some College+	0.024	0.045**	0.011	0.035*	0.062**	0.006
	(0.019)	(0.022)	(0.038)	(0.021)	(0.023)	(0.044)
	{5,576}	{5,576}	{5,576}	{5,116}	{5,116}	{5,116}
Years of Education	0.065	0.120	-0.029	0.188**	0.277**	-0.063
	(0.068)	(0.078)	(0.125)	(0.092)	(0.107)	(0.196)
	{5,576}	{5,576}	{5,576}	{5,116}	{5,116}	{5,116}
Employed	-0.012	0.003	0.080**	0.061**	0.035	0.022
	(0.020)	(0.024)	(0.039)	(0.019)	(0.024)	(0.043)
	{5,761}	{5,761}	{5,761}	{4,784}	{4,784}	{4,784}
In School or Working	-0.001	0.011	0.028	0.034*	0.015	0.001
	(0.014)	(0.016)	(0.025)	(0.017)	(0.020)	(0.036)
	{5,970}	{5,970}	{5,970}	{4,873}	{4,873}	{4,873}
Log Earned Income	0.023	0.272*	0.548**	-0.102	-0.216	-0.283
	(0.143)	(0.152)	(0.218)	(0.147)	(0.183)	(0.330)
	{5,151}	{5,151}	{5,151}	{4,809}	{4,809}	{4,809}
Public Benefits	-0.027**	-0.032**	-0.056**	-0.017	-0.015	0.003
	(0.013)	(0.014)	(0.018)	(0.016)	(0.017)	(0.029)
	{6,174}	{6,174}	{6,174}	{5,428}	{5,428}	{5,428}
Health Problem	0.026	0.049**	0.017	-0.001	0.014	-0.014
	(0.018)	(0.021)	(0.036)	(0.021)	(0.027)	(0.047)
	{6,162}	{6,162}	{6,162}	{5,432}	{5,432}	{5,432}
Overweight	-0.020	0.004	-0.018	-0.030	-0.040*	-0.026
	(0.019)	(0.021)	(0.032)	(0.020)	(0.024)	(0.037)
	{6,178}	{6,178}	{6,178}	{5,436}	{5,436}	{5,436}
Premarital Child	-0.026**	-0.029**	-0.045**	-0.042**	-0.045**	-0.036
	(0.012)	(0.013)	(0.016)	(0.016)	(0.018)	(0.029)
	{6,179}	{6,179}	{6,179}	{5,436}	{5,436}	{5,436}

Rows enumerate outcomes. Supercolumns denote outcome ages. Columns correspond binary indicators for breastfeeding duration: 3 months or more (Cols 1 and 4), 6 months or more (Cols 2 and 5), and 12 months or more (Cols 3 and 6). Each cell reports the coefficient on column-enunumerated breastfeeding treatment indicator from a separate OLS regression. All regressions control for full covariates. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status and outcomes. Standard errors clustered by mother in parentheses. Number of observations in braces. * $p < 0.10$, ** $p < 0.05$

Table 3.11: Young Adult Outcomes: Breastfeeding Duration, Extended Family Fixed Effects Estimates

	Age 21			Age 25		
	(1) 3m+	(2) 6m+	(3) 12+	(4) 3m+	(5) 6m+	(6) 12+
High School Grad+	-0.038*	-0.029	-0.049	-0.016	-0.015	-0.049
	(0.021)	(0.025)	(0.035)	(0.021)	(0.025)	(0.041)
	{1,361}	{1,361}	{1,361}	{1,174}	{1,174}	{1,174}
Some College+	-0.004	0.019	-0.004	-0.041	-0.045	-0.038
	(0.036)	(0.042)	(0.065)	(0.036)	(0.045)	(0.071)
	{1,361}	{1,361}	{1,361}	{1,174}	{1,174}	{1,174}
Years of Education	-0.027	0.064	-0.180	0.013	-0.104	-0.218
	(0.113)	(0.128)	(0.177)	(0.147)	(0.187)	(0.285)
	{1,364}	{1,364}	{1,364}	{1,174}	{1,174}	{1,174}
Employed	-0.050	-0.007	0.095	0.047	0.046	0.082
	(0.043)	(0.049)	(0.085)	(0.047)	(0.054)	(0.068)
	{1,442}	{1,442}	{1,442}	{1,073}	{1,073}	{1,073}
In School or Working	-0.059*	-0.034	-0.040	0.012	0.016	0.048
	(0.032)	(0.036)	(0.048)	(0.041)	(0.046)	(0.070)
	{1,501}	{1,501}	{1,501}	{1,098}	{1,098}	{1,098}
Log Earned Income	0.055	0.312	0.817*	-0.234	-0.018	-0.146
	(0.285)	(0.320)	(0.432)	(0.353)	(0.385)	(0.735)
	{1,172}	{1,172}	{1,172}	{1,072}	{1,072}	{1,072}
Public Benefits	-0.028	-0.022	-0.054	-0.007	0.047	0.034
	(0.030)	(0.032)	(0.040)	(0.036)	(0.038)	(0.056)
	{1,590}	{1,590}	{1,590}	{1,301}	{1,301}	{1,301}
Health Problem	-0.004	0.018	-0.049	-0.021	0.047	0.088
	(0.035)	(0.044)	(0.066)	(0.044)	(0.058)	(0.091)
	{1,585}	{1,585}	{1,585}	{1,302}	{1,302}	{1,302}
Overweight	0.029	0.068*	0.073	0.064	0.095**	0.118*
	(0.036)	(0.039)	(0.056)	(0.042)	(0.047)	(0.068)
	{1,593}	{1,593}	{1,593}	{1,303}	{1,303}	{1,303}
Premarital Child	-0.007	0.010	0.001	-0.010	0.042	0.048
	(0.023)	(0.025)	(0.027)	(0.034)	(0.040)	(0.052)
	{1,593}	{1,593}	{1,593}	{1,303}	{1,303}	{1,303}

Rows enumerate outcomes. Supercolumns denote outcome ages. Columns correspond binary indicators for breastfeeding duration: 3 months or more (Cols 1 and 4), 6 months or more (Cols 2 and 5), and 12 months or more (Cols 3 and 6). Each cell reports the coefficient on column-enunumerated breastfeeding treatment indicator from a separate extended family fixed effects regression. All regressions control for full covariates. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status and outcomes. Standard errors clustered by mother in parentheses. Number of observations contributing to breastfeeding identification in braces. * $p < 0.10$, ** $p < 0.05$

Table 3.12: Descriptive Covariate Statistics, Extended Family Fixed Effects Comparison

	Families		Mean Comparison			
	Non-FE	FE	Diff	SE	T-Stat	Obs
Mom's Birth Year	1961.2	1961.4	0.2	0.1	1.52	10,842
Mom's Education: Less than HS	0.18	0.22	0.04**	0.02	2.45	10,842
Mom's Education: High School	0.38	0.44	0.06**	0.02	2.86	10,842
Mom's Education: Some College+	0.42	0.32	-0.10**	0.02	-4.61	10,842
Mom's Age	27.19	26.52	-0.67**	0.23	-2.93	10,842
Mom's Race: Hispanic	0.07	0.13	0.06**	0.01	5.90	10,842
Mom's Race: White	0.75	0.67	-0.08**	0.02	-4.68	10,842
Mom's Race: Black	0.17	0.20	0.02	0.01	1.53	10,842
Mom Foreign-Born	0.04	0.07	0.03**	0.01	3.30	10,842
Mom's Age 21 BMI	22.34	22.90	0.56**	0.19	2.97	10,427
Mom AFQT	45.44	39.59	-5.85**	1.33	-4.39	10,301
Mom Employed	0.77	0.73	-0.04**	0.02	-2.65	8,581
Mom's Income	89851	67631	-22221**	5576	-3.99	7,002
Mom Region: Northeast	0.15	0.16	0.01	0.02	0.79	10,842
Mom Region: North Central	0.24	0.24	0.00	0.02	0.25	10,842
Mom Region: South	0.29	0.24	-0.05**	0.02	-3.26	10,842
Mom Region: West	0.15	0.17	0.02*	0.01	1.65	10,842
Mom Marital Status: Never Married	0.19	0.22	0.03**	0.01	2.84	10,842
Mom Marital Status: Married	0.57	0.50	-0.06**	0.02	-3.95	10,842
Mom Marital Status: Other	0.07	0.09	0.02**	0.01	2.42	10,842
Mom Prenatal Visit	0.99	0.99	-0.00	0.00	-0.65	9,919
Mom Drink	0.49	0.42	-0.07**	0.02	-3.94	9,898
Mom Smoke	0.31	0.33	0.01	0.02	0.61	9,888
Mom Prenatal Vitamins	0.95	0.96	0.01	0.01	0.84	8,774
C-Section	0.24	0.22	-0.02	0.02	-1.37	9,824
Preterm Birth	0.12	0.15	0.03**	0.01	2.33	9,730
Long Hospital Stay	0.08	0.09	0.01	0.01	1.07	9,444
Female	0.48	0.50	0.01	0.01	1.01	10,841
Birth Parity: 1	0.44	0.34	-0.10**	0.01	-12.54	10,842
Birth Parity: 2	0.34	0.33	-0.01	0.01	-1.57	10,842
Birth Parity: 3+	0.22	0.33	0.11**	0.01	8.98	10,842
Birthweight	7.38	7.31	-0.07	0.05	-1.45	10,117
Birth Month	6.57	6.54	-0.03	0.09	-0.32	10,842
Birth Year	1988.1	1987.6	-0.5*	0.3	-1.94	10,842
HOME Score Age 0-2	0.03	-0.11	-0.14**	0.04	-3.38	5,251
Has Aunt	0.24	0.47	0.23**	0.02	10.63	10,842
Breastfed	0.56	0.49	-0.07**	0.02	-4.12	10,842
Weeks Breastfed	25.28	17.72	-7.57**	1.51	-5.01	4,653

This table compares covariates between families contributing to extended family fixed effects breastfeeding identification (families with siblings or cousins with diverse breastfeeding experiences; FE) with children from families with unitary breastfeeding experiences (non-fixed effects families; Non-FE). Columns 1 and 2 give overall means. Cols 3–6 give mean comparisons, with point estimates for the mean differences in Col 3 (), and associated standard errors, test statistics, and sample sizes in Cols 4–6. Results are obtained from separate bivariate OLS regressions of each outcome on an indicator for fixed effects family membership. Unit of observation is individual child. All statistics are weighted using NLSY-CYA longitudinal custom weights for the corresponding age-outcome sample. * $p < 0.10$, ** $p < 0.05$

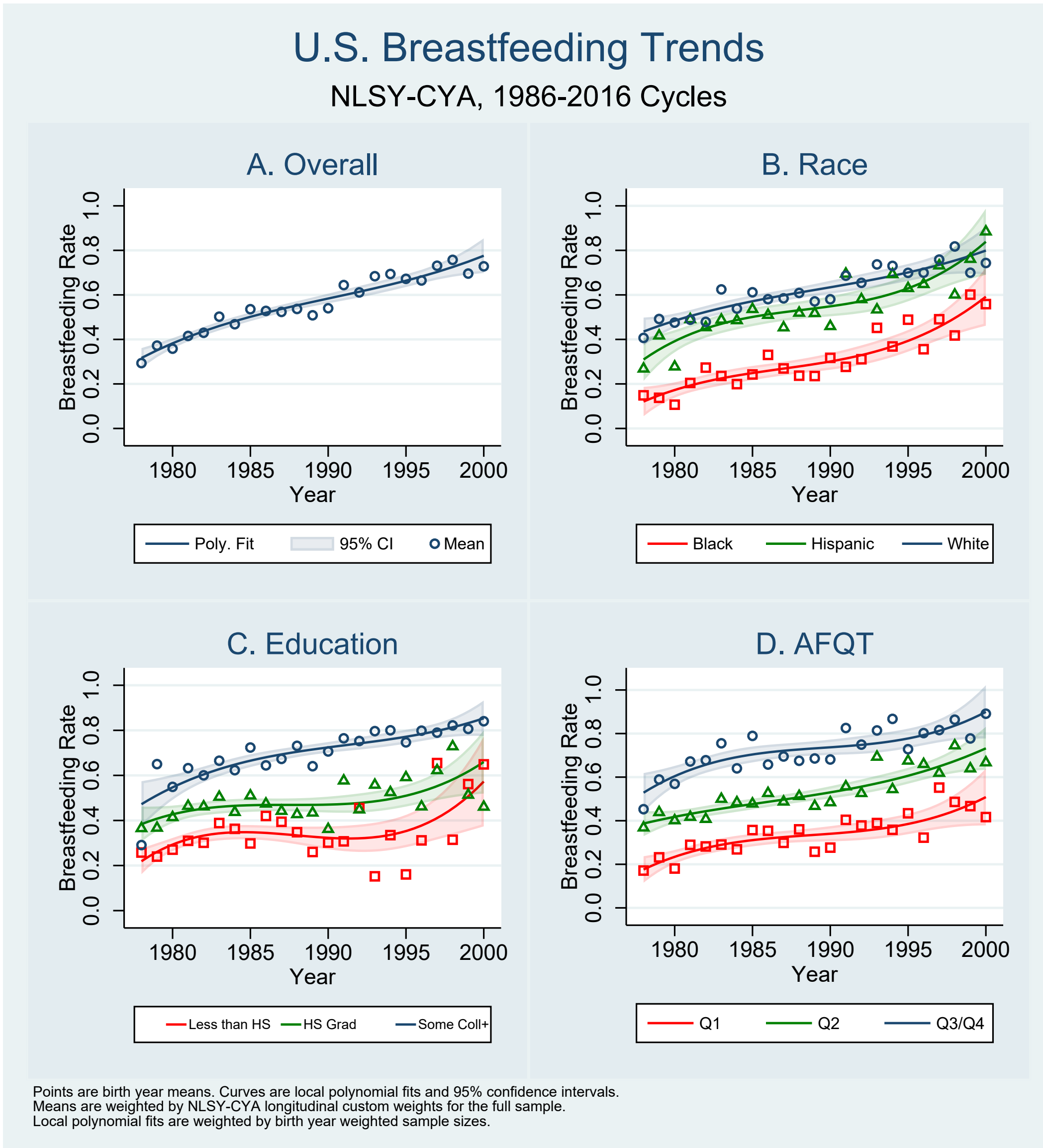
Table 3.13: Descriptive Outcome Statistics, Extended Family Fixed Effects Comparison

	Families		Mean Comparison			
	Non-FE	FE	Diff	SE	T-Stat	Obs
A. Age 5 Outcomes						
Math	0.20	0.04	-0.17**	0.04	-4.45	5,710
Reading Recog.	0.58	0.43	-0.15**	0.04	-3.94	5,600
Reading Comp.	0.84	0.78	-0.06	0.05	-1.23	1,964
Vocabulary	-0.32	-0.57	-0.26**	0.06	-4.02	4,237
Health Problem	0.44	0.41	-0.03	0.02	-1.48	7,759
Behavior (BPI)	0.17	0.20	0.03	0.04	0.72	7,347
Overweight	0.01	0.02	0.00	0.00	1.03	7,790
B. Age 10 Outcomes						
Math	0.38	0.17	-0.21**	0.04	-5.37	7,105
Reading Recog.	0.46	0.31	-0.15**	0.04	-3.77	7,101
Reading Comp.	0.19	0.07	-0.12**	0.04	-3.49	6,989
Vocabulary	-0.04	-0.30	-0.25**	0.05	-4.92	6,024
Health Problem	0.33	0.32	-0.01	0.02	-0.91	7,806
Behavior (BPI)	0.30	0.34	0.05	0.04	1.14	7,376
Overweight	0.08	0.11	0.03**	0.01	2.67	7,874
C. Age 13 Outcomes						
Math	0.27	0.09	-0.18**	0.04	-4.60	6,315
Reading Recog.	0.43	0.29	-0.15**	0.04	-3.34	6,323
Reading Comp.	-0.01	-0.18	-0.17**	0.03	-4.93	6,269
Vocabulary	-0.36	-0.57	-0.21*	0.11	-1.86	857
Health Problem	0.31	0.28	-0.03	0.02	-1.64	7,123
Behavior (BPI)	0.35	0.38	0.03	0.04	0.76	6,764
Overweight	0.15	0.19	0.04**	0.01	2.99	7,490
D. Age 21 Outcomes						
High School Grad+	0.88	0.85	-0.03**	0.01	-2.34	5,751
Some College+	0.56	0.49	-0.07**	0.02	-3.24	5,751
Years of Education	13.05	12.71	-0.33**	0.08	-4.31	5,751
Employed	0.65	0.66	0.01	0.02	0.34	5,927
In School or Working	0.81	0.78	-0.03*	0.02	-1.84	6,140
Log Earnings (2019 Dollars)	7.99	7.81	-0.18	0.14	-1.33	5,294
Public Benefits	0.15	0.19	0.05**	0.01	3.45	6,349
Health Problem	0.26	0.27	0.00	0.01	0.31	6,337
Overweight	0.36	0.39	0.03*	0.02	1.72	6,353
Premarital Child	0.16	0.20	0.04**	0.02	2.61	6,354
E. Age 25 Outcomes						
High School Grad+	0.88	0.87	-0.01	0.01	-0.69	5,267
Some College+	0.58	0.53	-0.05**	0.02	-2.34	5,267
Years of Education	13.58	13.29	-0.28**	0.10	-2.70	5,267
Employed	0.75	0.72	-0.03*	0.02	-1.65	4,922
In School or Working	0.80	0.78	-0.02	0.02	-1.41	5,014
Log Earnings (2019 Dollars)	8.90	8.76	-0.15	0.14	-1.06	4,946
Public Benefits	0.20	0.25	0.05**	0.02	2.96	5,579
Health Problem	0.31	0.36	0.05**	0.02	2.90	5,583
Overweight	0.40	0.43	0.03	0.02	1.63	5,587
Premarital Child	0.27	0.31	0.04**	0.02	2.37	5,587

This table compares outcomes between families contributing to extended family fixed effects breastfeeding identification (families with siblings or cousins with diverse breastfeeding experiences; FE) with children from families with unitary breastfeeding experiences (non-fixed effects families; Non-FE). Columns 1 and 2 give overall means. Cols 3–6 give mean comparisons, with point estimates for the mean differences in Col 3 (), and associated standard errors, test statistics, and sample sizes in Cols 4–6. Results are obtained from separate bivariate OLS regressions of each outcome on an indicator for fixed effects family membership. Unit of observation is individual child. All statistics are weighted using NLSY-CYA longitudinal custom weights for the corresponding age-outcome sample. * $p < 0.10$, ** $p < 0.05$

3.9 Figures

Figure 3.1: Breastfeeding Trends



Appendix C

Supplemental Appendices to "Breastfeed, If You Choose: Parental Context and the Long-Term Legacy of Lactation"

C.1 Theory

Breastfeeding has costs and benefits. Having peers who breastfeed can alter this calculus. In this section, I sketch a model of breastfeeding choices and consequences, with an emphasis on peer effects. The presentation is deliberately informal, as its purpose is to structure the empirical analysis to follow, not to provide detailed proofs. My theoretical approach to breastfeeding is inspired by Rothstein (2013), which itself builds on the more detailed models of Cunha and Heckman (2007).

Mothers face a scarcity of time. During the single period comprising their children's infancy, they may apportion their limited time among labor, L, leisure (R; "recreation"), and direct investments in their children, B, which, to fix ideas, consists exclusively of time spent breastfeeding. These time inputs produce things that mothers' value: present consumption (C), present leisure (R), and their children's outcomes (Y), which are realized in a future period. Preferences depend upon mother observables, $\mathbf{Z}$, and unobservables ν.

Mother's maximize utility

$$U(C, R, Y, \mathbf{Z}, \nu)$$

subject to a time constraint, normalized to sum to one,

$$L + p_R R + p_B B = 1$$

and a resource constraint

$$C \leq w_0 + wL$$

p_R is the price (opportunity cost) of leisure and p_B is the price of breastfeeding, both in units of the price of labor, which bears an inverse relationship to a mother's market wage ($w \propto 1/p_L$). These prices reflect a mother's comparative productivity at performing these activities (greater efficiency means lower price). w_0 is an exogenous resource endowment.

Children's outcomes, Y, are a function of breastfeeding and labor inputs, as well as child birth period observables, $\mathbf{X}$, and unobservables, ε, realized in expectation:

$$Y = E(f_Y(B, \mathbf{X}, \varepsilon)|I(\mathbf{Z}, \nu))$$

with the information set, I, determined by a mother's characteristics. Mother ($\mathbf{Z}$) and child ($\mathbf{X}$) observables overlap; the "at-birth" nature of the latter means maternal traits and circumstances bear greater weight; in particular, $\mathbf{X}$ includes both maternal traits (e.g., education, race, and age), *as well as* realized labor L and leisure R choices. Symmetrically, ε may include unobserved mother characteristics.

Assuming the usual regularity conditions, there exists a solution such that choices can be written in terms of the exogenous primitives of the model. In particular, the reduced-form breastfeeding decision is given by:

$$B = f_B(\mathbf{Z}, \varepsilon, \nu, p_R, p_B, w, w_0)$$

Women breastfeed up until the point the marginal benefits of breastfeeding equal its marginal costs, relative to competing uses of time. The benefits of breastfeeding include documented improvements infant and mother health, speculative gains in longer-term cognitive outcomes, and the enjoyment of time spent together. The costs involve physical hardships and competing uses of time, like returns to work. Especially relevant are corner solutions: no breastfeeding will occur unless the value of its first unit exceeds its opportunity cost at that point: $\partial U/\partial Y \times \partial Y/\partial B(B = 0) > p_B(B = 0)$, where, for simplicity, the relative (to consumption and leisure) nature of the inequality is left implicit. In the absence of breastfeeding, the child is fed with formula, which is captured through consumption, C. There are three basic trade-offs. The first is that, while breastfeeding directly improves children's outcomes, it necessitates a reduction labor and its attendant human-capital augmenting consumption benefits for mother and child. Second, breastfeeding reduces mothers' leisure opportunities. And third, costs are immediate and certain, while benefits are stochastic and asynchronous.

A point bearing emphasis is that breastfeeding is not unambiguously beneficial. All else equal, human milk may be superior to formula, but when time costs are factored the solution is not straightforward. Put differently, breastfeeding is a local optimum where the goal is a global solution. While there are many ways heterogeneity can enter the model, a particularly tractable place is through p_B, the opportunity cost of breastfeeding. To fix ideas, let there be two types of mothers, highly educated (H) and lowly educated (L). H types enjoy white-collar jobs with paid family leave and extensive lactation support, while L types lack these benefits and effectively must not work if breastfeeding; that is, $p_B(H) < p_B(L)$. When a type L mother breastfeeds, more is lost. This basic point extends to many other characteristics.

Related to this insight is a second reminder: mothers are not optimizing child outcomes, they are maximizing their own utility, of which child outcomes, which are influenced by breastfeeding, are one component. A woman who chooses to give up working to breastfeed—due, in part, to a intrinsic or exogenously-influenced preference for breastfeeding—may compromise rather than bolster long-term child outcomes. In other words, breastfeeding may be net beneficial for some children and net detrimental for others.

Correspondingly, the realized (to the econometrician) child outcomes can be written:

$$Y = f_Y(B, \mathbf{X}, \varepsilon)$$

That breastfeeding is the outcome of an maternal optimization problem is, of course, the central challenging in identifying its causal impact on child outcomes. One hope is that the observed data ($\mathbf{X}$) is sufficiently rich and the error (ε) sufficiently small that conditioning on observables renders a valid contrast, $B \perp \varepsilon | \mathbf{X}$. A second possibility is to limit the comparison to children with plausibly similar ε—for example, siblings or cousins. The downside is that such narrowed comparisons may not generalize—or, more problematic still, the refinement may increase the confounding influence of ν in the choice equation.

A more promising option is to find an instrument $Z \in \mathbf{Z}$ that exogenously shifts the relative price of breastfeeding without directly influencing outcomes ($Z \notin \mathbf{X}$, $Z \perp \varepsilon | \mathbf{X}$). One way to think about the effect of instruments is in changing relative prices (i.e., $\partial p_B / \partial Z$, $\partial p_L / \partial Z$, and/or $\partial p_R / \partial Z$). Finding convincing instruments is difficult and is a topic for future research.

C.2 Data

The raw NLSY-CYA data contains one variable for each question response in each survey round, in addition to "created" variables derived from responses or aggregated across answers—tens of thousands in total[1]. As packaged, variables are attached to survey rounds, not respondent ages. My central data task is to "age normalize" outcomes across survey rounds. The result is five "age-outcome" groups: ages 5, 10, 13, 21, and 25. That is,

The NLSY-CYA measures children's ages in months and years. I define a child's i's age in survey round r as $age_{ir} = year_r - birthyear_i$, irrespective of whether the interview took place before a child's birthday. I transform round-responses to age-responses by assigning age-outcome Y_{ia} at age a for child i the value of the child's response, Y_{ir} in the earliest survey round for which $0 \leq age_{ir} - age_a \leq 2$ and the child was successfully interviewed (given this definition, there are a maximum of two possible interview rounds for each age). The idea is to maximize the sample size of successful responses, while using the outcomes from the interview most proximate to the pivotal age without going younger, given that aging above specific thresholds, like 21 years, can come with important social and legal changes.

[1] As is standard in high-quality surveys, responses undergo extensive validation checks within and between survey rounds, and raw responses may be edited or imputed for quality assurance.

Child Outcomes

The NLSY-CYA encompasses an extensive ensemble of enquiries evaluating respondents' early-life experiences. In my main results, I focus on a handful of summary educational and health measures, selected so as to balance breadth with parsimony and familiarity. Alternative outcome metrics, some of which are constituent to the summary outcomes considered here, are available in the Appendix.

I assess the following child outcomes at ages 5, 10, and 13 years:

- **Peabody Individual Achievement Test (PIAT)**: The PIAT is a common cognitive assessment, popular because it is brief, but know to have high reliability and validity. While the full battery spans five subjects, the NLSY-CYA includes three subtests listed below, administered to children from ages 5–14 years. Scores are normed by age from a 1968 reference sample with mean 100 and standard deviation 15. I standardize these scores to have mean 0 and standard deviation 1 by subtracting 100 and dividing by 15. Consequently, regression results are measured in standard deviation units. Note that these statistics refer to the norming sample; due, presumably, to educational advancements since the 1960's, the weighted NLSY-CYA has PIAT means somewhat higher than 0.

  - **PIAT Math**: The Math subtest is comprised of 84 increasingly difficult multiple choice questions, ranging from number recognition to trigonometry. The lower of five consecutive correct answers or one constitutes a child's "basal"; the child's ceiling is reached when five of seven questions are answered incorrectly. The child's raw score is calculated as $raw\ score = ceiling - postbasal\ incorrects$.

  - **PIAT Reading Recognition**: Also consisting of 84 ascending multiple-choice questions, the Recognition subtest requires children to pronounce written words. Scoring is the same as Math, with the starting point determined by the Math basal.

- **PIAT Reading Comprehension**: The Comprehension subtest, which consists of 66 ascending-difficulty questions, requires children to read sentences silently and select pictures that best describe the meaning. Scoring is similar to Math, except that children scoring less than 19 on Reading Recognition also receive a 19 for Reading Comprehension; those scoring above 19 have their Recognition score serve as their Comprehension basal.

- **Peabody Picture Vocabulary Test (Revised Edition) (PPVT)**: The PPVT measures children's hearing vocabularies, assessed through 175 questions of increasing difficulty. The interviewer reads words aloud and children select the picture from a set of four that best describes the word's meaning. As with the PIAT, raw scores are computed as $raw\ score = ceiling - postbasal\ incorrects$. The "basal," or baseline, is the highest question number among eight straight correct responses; the "ceiling" is the highest question number attained when a child answers six of eight questions incorrectly. The PPVT is appropriate for children three years and above. However, over the years, the NLSY-CYA has varied as to which children are administered the test; frequently, testing has been limited to younger children or those without a prior test result. The PPVT was normed in 1979, to have age-graded standard scores with mean 100 and standard deviation 15. As with the PIAT, I transform these scores to be mean 0, standard deviation 1. Unlike the PIAT, NLSY-CYA means coincide closely with the normed sample. Like the PIAT, the PPVT is knwon to have high reliability and validity.

- **Health Problem**: The NLSY-CYA has questions spanning many aspects of health. To create a reasonably comprehensive marker of health challenges, I define an indicator equal to one if a child's mother reported any of: (a) a school attendance limiting health condition, (b) a school work limiting health condition, (c) any illness requiring medical attention, or (d) fair or poor heath (with good and excellent the omitted categories). If none of these apply, the health issue indicator is equal to zero.

- **Behavioral Problems Index (BPI)**; BPI measures behavioral problems in children four years and older through a series of 28 questions administered to their mothers. It covers six topics: (1) antisocial behavior, (2) anxiousness and depression, (3) headstrongness, (4) hyperactivity, (5) immature dependency, and (6) peer conflict and social withdrawal. Single-year age norms were developed through the 1981 National Health Interview Survey (NHIS) Child Supplement, with national means of 100 and standard deviation 15. I transform these scores to have national mean 0 and standard deviation 1, though as with the PIAT, the within-NLSY-CYA mean is somewhat higher than zero.

- **Overweight**: An indicator equal to one if a child has a Body Mass Index (BMI) of 25 or greater and zero otherwise. BMI is calculated as weight (in kilograms) divided by squared height (in meters).

  In the Appendix, I consider several alternative outcome measures for robustness. These are percentiles for cognitive assessments and BPI, as well as disaggregated health issue indicators (school-limiting conditions, medical illness, and fair or poor health), BMI, and an indicator for obesity.

Young Adult Outcomes

I evaluate the following young adult outcomes at ages 21 and 25 years:

- **High School Graduate+**: An indicator equal to one if a young adult has completed 12th grade or higher, or reports having graduated high school, and zero otherwise.

- **Some College+**: An indicator equal to one if a young adult has completed at least one year of college and zero otherwise.

- **Years of Education**: The precise manner in which the NLSY-CYA has asked respondents about years of completed education has evolved over time. For the 1994–2012 rounds, it is equal to highest grade completed. For the 2014–2016 rounds, I impute from educational attainment categories (e.g., 8th grade or less = 8 years; some high school = 10; high school grad = 12; post-high-school training = 13; some college or associate degree = 14; bachelor's degree = 16; some grad school = 17; master's degree = 18;; some post-master's = 19; doctoral or professional degree = 20).

- **Employed**: An indicator equal to one if a young adult reports usually working at a job at least one hour per week and zero otherwise.

- **In School or Working**: An indicator equal to one if a young adult is attending or enrolled in school, or employed.

- **Log Earned Income**: The natural logarithm of total income from wages, salary, commission, or tips in 2019 dollars, plus one.

- **Public Benefits**: An indicator equal to one if a young adult reports receiving any of public assistance/welfare, Food Stamps, or Medicaid/publicly-assisted health insurance, and zero otherwise.

- **Health Problem**: Analogous to the child health issue outcome, I define an indicator equal to one if a young adult reports any of: (a) a school or work limiting condition, (b) a condition requiring medical attention, regular medication, or special equipment, (c) any illness requiring medical attention, or (d) fair or poor heath (with good, very good, and excellent the omitted categories). If none of these apply, the health issue indicator is equal to zero.

- **Overweight**: An indicator equal to one if a young adult has a BMI of 25 or greater and zero otherwise. BMI is calculated as weight (in kilograms) divided by squared height (in meters).

- **Premarital Childbearing**: An indicator equal to one if a young adult had a child before marriage and the outcome age in question, and zero otherwise.

These outcomes collectively encompass a broad range of educational, labor market, health, and family formation behaviors. In the Appendix, I investigate additional outcomes—entailing either disaggregations of compound measures or alternative definitions—for robustness and clarity. By domain, they are: **educational attainment** (mutually exclusive indicators for less than HS, HS graduate, some college, college 4+ years, as well as an indicator for college graduation)[2]; **labor market** (an indicator for employment based on reported work status, an indicator for employment based on positive income, and earned income in 2019 dollars); **public benefits** (indicators for receipt of public assistance, Food Stamps, and Medicaid (or other public health insurance)); **health** (indicators for school limiting health condition, medical condition, medical illness, and fair or poor health; raw BMI; and an indicator for obesity); and **family formation** (indicators for living in parents' household, having children, ever marrying, and ever cohabiting).

Much more detail about these outcomes is available in the NLSY documentation.

C.3 Sample Nonresponse

Beyond definitions, another concern is the self-reported nature of survey data. In particular, not every individual responds to every question in every survey round—if

[2]Prior to 2014, the NLSY-CYA data for college graduation was somewhat sparse.

they respond at all. If the nature of the missing data is not random, it may bias the results.

Table C.9 investigates sample selection in the context of survey nonresponse. (Table C.10 repeats the analysis without using survey weights[3].) It follows a similar layout as earlier tables, except with interview responses at outcome ages of interest as the dependent variables (indexed by rows). Column 1 gives the overall response rate, while the cells in Cols 2–4 give the coefficients on breastfeeding from separate regressions using the column-enumerated estimation method (as in the main analysis, all regressions control for full covariates and are weighted using custom longitudinal weights). Response rates are around 80 percent during childhood and drop to about 75 percent during the young adult years, in part because not all NLSY-CYA are 21 or 25 years old by 2016. Only about half of respondents participate at all ages I consider. Beyond the prevalence of nonresponse, what is especially notable is that breastfed individuals are significantly more likely to participate, by 3–4 pp at all ages except 25 (Col 2). This is also true within the extended family fixed effects sample, particularly at early ages.

Table C.11 further underscores the non-random nature of survey response. Among both breastfed and non-breastfed groups, outcomes are notably better among those who respond at all five outcome ages I study. While this does not say anything about the outcomes of those not responding at particular ages, it is strongly indicative of positive selection: nonresponders have, on average, worse outcomes.

Together, these facts— higher response rates among the breastfed and worse outcomes for nonresponders—suggests the main results could be understated, since the breastfeeding sample is more complete and presumably includes more individuals with worse outcomes. On the other hand, if nonresponding breastfeeders perform significantly worse than nonresponding non-breastfeeders, the bias could operate in

[3] I do not investigate item nonresponse separately.

the opposite direction (e.g., breastfeeding is associated with positive traits, so to be breastfed and still not respond implies greater disadvantage).

Tables C.12 (children) and C.13 (young adults) study this question, repeating the OLS specifications from the main analysis for the sample limited to those who responding to all five interviews. An instructive pattern emerges. While qualitatively consistent with the main analysis, results for ages 5–21 are somewhat muted, while age 25 results are mildly accentuated. This is in keeping with a modest upward bias imparted by differential nonresponse: positive selection is stronger among breastfed individuals, but, as Table C.9 indicates, appears to disappear by age 25. In other words, limiting the sample to consistent-responders is an additional control, implicitly holding constant traits, like diligence, associated with responsiveness. If this story is true, it would help explain the observed diminution of breastfeeding effects with time: perhaps the early effects are exaggerated to begin with.

C.4 Supplementary Tables

Table C.1: NLSY-CYA (1986-2016 Cycles)
Ages at Interview

Age at Interview	Count	Pct.
A. Age 5 Interview		
5	3,837	46.6
6	3,812	46.3
7	581	7.1
Total	8,230	100.0
B. Age 10 Outcomes		
10	3,951	47.6
11	4,041	48.7
12	308	3.7
Total	8,300	100.0
C. Age 13 Outcomes		
13	3,869	48.9
14	3,720	47.1
15	317	4.0
Total	7,906	100.0
D. Age 21 Outcomes		
21	3,410	50.4
22	3,119	46.1
23	238	3.5
Total	6,767	100.0
E. Age 25 Outcomes		
25	2,901	50.3
26	2,667	46.2
27	204	3.5
Total	5,772	100.0

Each panel gives the age distribution of respondents at the given age-interview survey. Ages are computed as calendar interview year minus calendar birth year.

Table C.2: NLSY-CYA Births and Breastfeeding Rates by Birth Year

	Births	% Breastfed
1970	1	0.0
1971	3	33.3
1972	10	0.0
1973	39	15.4
1974	77	7.8
1975	156	12.8
1976	234	19.7
1977	292	20.2
1978	374	26.2
1979	513	34.3
1980	599	31.9
1981	700	40.0
1982	696	40.7
1983	708	47.6
1984	635	42.4
1985	650	47.8
1986	540	50.4
1987	553	48.1
1988	538	50.4
1989	606	46.0
1990	477	49.3
1991	403	56.3
1992	257	55.6
1993	206	61.2
1994	297	64.0
1995	242	62.4
1996	229	61.1
1997	212	67.9
1998	154	70.1
1999	127	69.3
2000	98	71.4
2001	75	68.0
2002	45	77.8
2003	33	78.8
2004	23	82.6
2005	15	60.0
2006	10	30.0
2007	4	75.0
2008	3	33.3
2009	3	100.0
2010	2	100.0
2011	2	0.0
2014	1	100.0
Total	10,842	45.6

NLSY-CYA births and (unweighted) breastfeeding rates by year of birth (1986-2016 cycles).

Table C.3: Child Alternative Outcomes

	Age 5			Age 10			Age 13		
	(1) OLS	(2) MOM	(3) EXT	(4) OLS	(5) MOM	(6) EXT	(7) OLS	(8) MOM	(9) EXT
Math Pct.	2.430**	3.748*	2.405	3.209**	-1.276	-0.754	3.334**	0.371	0.909
	(0.975)	(2.068)	(2.121)	(0.909)	(1.543)	(1.639)	(0.938)	(1.631)	(1.729)
	{5,710}	{906}	{1,319}	{7,105}	{1,448}	{1,903}	{6,315}	{1,256}	{1,679}
Read Recog. Pct.	2.026**	0.922	-1.001	2.247**	1.661	1.301	3.116**	0.410	-0.073
	(0.859)	(1.735)	(1.759)	(0.960)	(1.559)	(1.687)	(1.018)	(1.671)	(1.833)
	{5,600}	{881}	{1,293}	{7,101}	{1,442}	{1,896}	{6,323}	{1,255}	{1,675}
Read Comp. Pct.	0.717	6.183**	3.764	3.251**	0.075	-0.023	3.425**	-0.621	-0.541
	(0.742)	(2.737)	(3.130)	(0.909)	(1.557)	(1.645)	(0.933)	(1.557)	(1.701)
	{1,957}	{118}	{182}	{6,989}	{1,409}	{1,857}	{6,269}	{1,227}	{1,636}
Vocab. Pct.	2.573**	-2.694	-0.472	2.936**	0.236	-0.811	-3.112	-10.686	-16.313
	(1.163)	(2.593)	(2.529)	(1.038)	(1.657)	(1.845)	(2.616)	(6.672)	(11.105)
	{4,237}	{518}	{801}	{6,024}	{1,076}	{1,444}	{857}	{40}	{45}
Limiting Condition	0.002	-0.015	-0.015	0.003	-0.006	-0.007	0.008	-0.001	-0.005
	(0.006)	(0.015)	(0.015)	(0.007)	(0.016)	(0.017)	(0.007)	(0.016)	(0.017)
	{6,514}	{1,061}	{1,561}	{7,730}	{1,594}	{2,125}	{7,011}	{1,432}	{1,926}
Medical Illness	0.014	-0.025	0.001	-0.002	0.011	0.024	0.026*	0.048*	0.046
	(0.017)	(0.030)	(0.031)	(0.015)	(0.028)	(0.030)	(0.016)	(0.029)	(0.030)
	{7,744}	{1,497}	{2,104}	{7,766}	{1,611}	{2,143}	{7,097}	{1,450}	{1,945}
Fair-Poor Health	-0.010	-0.009	-0.025	-0.010	-0.004	-0.003	-0.015**	0.007	0.011
	(0.011)	(0.024)	(0.029)	(0.006)	(0.017)	(0.017)	(0.007)	(0.018)	(0.018)
	{1,631}	{94}	{165}	{4,676}	{659}	{980}	{4,933}	{840}	{1,145}
BPI Pct.	-0.013	-0.027*	-0.037**	-0.009	0.009	-0.001	0.018*	-0.002	0.000
	(0.010)	(0.015)	(0.016)	(0.010)	(0.014)	(0.015)	(0.010)	(0.015)	(0.016)
	{7,347}	{1,337}	{1,910}	{7,376}	{1,475}	{1,988}	{6,764}	{1,337}	{1,793}
BMI	0.046	0.037	0.037	-0.355**	-0.000	-0.083	-0.252	-0.201	-0.185
	(0.437)	(0.988)	(0.953)	(0.156)	(0.253)	(0.279)	(0.162)	(0.300)	(0.324)
	{7,138}	{1,308}	{1,862}	{7,225}	{1,436}	{1,917}	{6,721}	{1,332}	{1,810}
Obese	0.002	0.001	0.001	-0.003	-0.000	0.000	-0.019**	-0.022	-0.024
	(0.002)	(0.002)	(0.003)	(0.005)	(0.011)	(0.011)	(0.007)	(0.015)	(0.016)
	{7,790}	{1,513}	{2,130}	{7,874}	{1,659}	{2,204}	{7,490}	{1,601}	{2,144}

Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for full covariates. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status and outcomes. All regressions are weighted using NLSY-CYA longitudinal custom weights for the age-interview sample. Standard errors clustered by mother in parentheses. Number of observations in braces; for fixed effects specifications, counts refer to observations contributing to breastfeeding identification. * $p < 0.10$, ** $p < 0.05$

Table C.4A: Young Adult: Alternative Outcomes

	Age 21			Age 25		
	(1) OLS	(2) MOM	(3) EXT	(4) OLS	(5) MOM	(6) EXT
Less than HS	-0.025**	-0.008	-0.001	-0.027**	0.010	-0.000
	(0.012)	(0.024)	(0.025)	(0.012)	(0.025)	(0.026)
	{5,751}	{1,069}	{1,489}	{5,267}	{965}	{1,291}
High School Grad	-0.020	0.014	-0.001	-0.004	-0.021	-0.021
	(0.017)	(0.035)	(0.037)	(0.018)	(0.037)	(0.040)
	{5,751}	{1,069}	{1,489}	{5,267}	{965}	{1,291}
Some College	0.047**	-0.003	0.004	0.027	-0.027	-0.013
	(0.018)	(0.031)	(0.033)	(0.018)	(0.034)	(0.037)
	{5,751}	{1,069}	{1,489}	{5,267}	{965}	{1,291}
College 4+ Years	-0.014	-0.022	-0.019	-0.004	0.004	0.002
	(0.009)	(0.017)	(0.017)	(0.017)	(0.027)	(0.030)
	{5,751}	{1,069}	{1,489}	{5,267}	{965}	{1,291}
College Grad	-0.004	-0.009	-0.007	0.004	0.017	0.020
	(0.007)	(0.013)	(0.013)	(0.016)	(0.028)	(0.030)
	{5,751}	{1,069}	{1,489}	{5,278}	{969}	{1,295}
Employed (Status)	0.008	-0.034	-0.021	0.013	-0.045	-0.007
	(0.017)	(0.036)	(0.039)	(0.017)	(0.030)	(0.034)
	{5,835}	{1,146}	{1,541}	{4,862}	{875}	{1,152}
In School	0.045**	0.007	0.010	0.011	0.007	-0.008
	(0.017)	(0.029)	(0.031)	(0.013)	(0.027)	(0.030)
	{6,351}	{1,277}	{1,726}	{5,585}	{1,088}	{1,424}
Earnings (2019$)	134.295	-816.631	-569.452	-651.200	-624.538	-75.553
	(561.643)	(1085.803)	(1176.002)	(1016.843)	(1765.590)	(1910.398)
	{5,294}	{934}	{1,266}	{4,946}	{902}	{1,174}
Employed (Income)	-0.005	0.017	0.015	-0.006	0.006	0.005
	(0.013)	(0.029)	(0.031)	(0.012)	(0.025)	(0.028)
	{5,294}	{934}	{1,266}	{4,946}	{902}	{1,174}
Public Assistance	0.004	0.009	0.008	-0.005	-0.008	-0.011
	(0.005)	(0.011)	(0.011)	(0.006)	(0.014)	(0.015)
	{6,333}	{1,269}	{1,714}	{5,568}	{1,084}	{1,419}
Food Stamps	-0.011	-0.003	0.008	0.005	0.005	-0.002
	(0.009)	(0.022)	(0.023)	(0.012)	(0.023)	(0.026)
	{6,336}	{1,274}	{1,720}	{5,571}	{1,085}	{1,420}
Medicaid	-0.011	-0.045	-0.036	-0.008	-0.027	-0.023
	(0.012)	(0.028)	(0.029)	(0.012)	(0.026)	(0.028)
	{5,189}	{917}	{1,280}	{5,515}	{1,065}	{1,399}

Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for full covariates. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status and outcomes. All regressions are weighted using NLSY-CYA longitudinal custom weights for the age-interview sample. Standard errors clustered by mother in parentheses. Number of observations in braces.
* $p < 0.10$, ** $p < 0.05$

Table C.4B: Young Adult: Alternative Outcomes

	Age 21			Age 25		
	(1) OLS	(2) MOM	(3) EXT	(4) OLS	(5) MOM	(6) EXT
Limiting Condition	0.003	-0.006	-0.001	0.004	0.009	0.007
	(0.005)	(0.011)	(0.011)	(0.005)	(0.013)	(0.014)
	{6,007}	{1,166}	{1,584}	{5,202}	{953}	{1,266}
Medical Condition	-0.001	-0.016	-0.025	-0.007	-0.001	-0.005
	(0.009)	(0.022)	(0.022)	(0.010)	(0.021)	(0.022)
	{5,897}	{1,121}	{1,528}	{5,114}	{917}	{1,222}
Medical Illness	0.008	0.021	0.002	0.027	0.062	0.053
	(0.017)	(0.040)	(0.042)	(0.017)	(0.039)	(0.043)
	{4,415}	{674}	{952}	{4,930}	{897}	{1,196}
Fair-Poor Health	0.002	0.023	0.010	-0.004	-0.005	-0.015
	(0.010)	(0.025)	(0.026)	(0.012)	(0.023)	(0.025)
	{6,090}	{1,176}	{1,616}	{5,579}	{1,088}	{1,423}
BMI	-0.155	0.390	0.260	-0.486**	-0.285	-0.356
	(0.182)	(0.387)	(0.404)	(0.244)	(0.480)	(0.532)
	{6,281}	{1,253}	{1,691}	{5,523}	{1,063}	{1,396}
Obese	-0.015	-0.016	-0.006	-0.014	-0.017	-0.017
	(0.012)	(0.024)	(0.026)	(0.013)	(0.027)	(0.029)
	{6,353}	{1,277}	{1,726}	{5,587}	{1,088}	{1,424}
In Parents' Household	-0.033*	0.052	0.027	-0.026	0.003	-0.021
	(0.017)	(0.035)	(0.037)	(0.016)	(0.032)	(0.036)
	{6,352}	{1,276}	{1,725}	{5,585}	{1,087}	{1,423}
Has Child	-0.014	-0.000	0.014	-0.011	-0.007	0.006
	(0.013)	(0.023)	(0.025)	(0.017)	(0.032)	(0.036)
	{6,354}	{1,277}	{1,726}	{5,587}	{1,088}	{1,424}
Ever Married	-0.003	-0.022	-0.003	0.017	-0.020	-0.000
	(0.012)	(0.020)	(0.021)	(0.017)	(0.032)	(0.035)
	{6,354}	{1,277}	{1,726}	{5,587}	{1,088}	{1,424}
Ever Cohabitated	0.015	0.015	0.031	-0.022	-0.031	-0.017
	(0.017)	(0.032)	(0.033)	(0.018)	(0.035)	(0.039)
	{6,354}	{1,277}	{1,726}	{5,587}	{1,088}	{1,424}

Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for full covariates. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status and outcomes. All regressions are weighted using NLSY-CYA longitudinal custom weights for the age-interview sample. Standard errors clustered by mother in parentheses. Number of observations in braces; for fixed effects specifications, counts refer to observations contributing to breastfeeding identification.
* $p < 0.10$, ** $p < 0.05$

Table C.5: Descriptive Alternative Child Outcome Statistics, NLSY-CYA 1986-2016 Breastfeeding Sample

	Overall			Breastfeeding Comparison				
	Mean	SD	Obs	Yes	No	Diff	SE	T-Stat
A. Age 5 Outcomes								
Math Pct.	54.90	27.88	5,710	59.70	48.58	11.12**	0.97	11.51
Read Recog. Pct.	65.24	25.17	5,600	69.48	59.64	9.84**	0.87	11.28
Read Comp. Pct.	75.81	16.75	1,957	78.12	72.73	5.38**	0.91	5.92
Vocab. Pct.	42.32	30.54	4,237	48.53	33.90	14.63**	1.23	11.90
Limiting Condition	0.03	0.17	6,514	0.03	0.03	-0.00	0.01	-0.77
Medical Illness	0.42	0.49	7,744	0.45	0.38	0.07**	0.02	4.81
Fair-Poor Health	0.02	0.14	1,631	0.02	0.03	-0.01	0.01	-1.20
BPI Pct.	0.55	0.28	7,347	0.52	0.58	-0.06**	0.01	-6.16
BMI	14.26	18.19	7,138	14.78	13.59	1.19**	0.32	3.69
Obese	0.00	0.06	7,790	0.00	0.00	0.00	0.00	0.31
B. Age 10 Outcomes								
Math Pct.	59.54	28.39	7,105	66.26	51.33	14.93**	0.89	16.81
Read Recog. Pct.	62.30	28.45	7,101	67.86	55.51	12.35**	0.93	13.21
Read Comp. Pct.	55.54	27.30	6,989	61.01	48.84	12.17**	0.89	13.73
Vocab. Pct.	47.75	30.36	6,024	55.15	38.89	16.26**	1.06	15.39
Limiting Condition	0.05	0.22	7,730	0.05	0.05	-0.01	0.01	-0.97
Medical Illness	0.30	0.46	7,766	0.31	0.27	0.04**	0.01	2.84
Fair-Poor Health	0.03	0.16	4,676	0.02	0.04	-0.02**	0.01	-3.23
BPI Pct.	0.58	0.28	7,376	0.56	0.62	-0.06**	0.01	-6.03
BMI	18.62	5.74	7,225	18.64	18.60	0.04	0.16	0.26
Obese	0.02	0.15	7,874	0.02	0.03	-0.01**	0.00	-2.92
C. Age 13 Outcomes								
Math Pct.	56.28	27.91	6,315	62.93	48.28	14.65**	0.94	15.64
Read Recog. Pct.	61.15	28.94	6,323	67.41	53.63	13.78**	0.99	13.86
Read Comp. Pct.	48.75	27.02	6,269	54.71	41.59	13.12**	0.91	14.35
Vocab. Pct.	38.42	30.90	857	44.03	34.25	9.78**	2.95	3.31
Limiting Condition	0.05	0.21	7,011	0.05	0.05	-0.00	0.01	-0.30
Medical Illness	0.27	0.44	7,097	0.29	0.24	0.05**	0.01	3.68
Fair-Poor Health	0.04	0.19	4,933	0.03	0.05	-0.03**	0.01	-3.78
BPI Pct.	0.60	0.28	6,764	0.58	0.61	-0.03**	0.01	-3.01
BMI	21.38	5.42	6,721	21.04	21.80	-0.75**	0.17	-4.40
Obese	0.05	0.23	7,490	0.04	0.08	-0.04**	0.01	-6.31

This table summarizes outcomes for the 1986–2016 NLSY-CYA sample with reported breastfeeding statuses. Columns 1–3 give overall means, standard deviations, and sample sizes. Cols 4–8 give mean comparisons by breastfeeding status, with respective group means for breastfed and not breastfed in Cols 4 and 5, point estimates for the mean differences in Col 6, and associated standard errors and test statistics in Cols 7 and 8. Results are obtained from separate bivariate OLS regressions of each outcome on the breastfeeding treatment indicator. Unit of observation is individual child. All statistics are weighted using NLSY-CYA longitudinal custom weights for the corresponding age-outcome sample. * $p < 0.10$, ** $p < 0.05$

Table C.6: Descriptive Alternative Young Adult Outcome Statistics, NLSY-CYA 1986-2016 Breastfeeding Sample

	Overall			Breastfeeding Comparison				
	Mean	SD	Obs	Yes	No	Diff	SE	T-Stat
A. Age 21 Outcomes								
Less than HS	0.13	0.34	5,751	0.08	0.19	-0.10**	0.01	-9.25
High School Grad	0.33	0.47	5,751	0.28	0.38	-0.10**	0.02	-6.62
Some College	0.51	0.50	5,751	0.60	0.41	0.18**	0.02	10.50
College 4+ Years	0.07	0.25	5,751	0.08	0.05	0.03**	0.01	3.39
College Grad	0.04	0.19	5,751	0.05	0.02	0.03**	0.01	3.78
Employed (Status)	0.67	0.47	5,835	0.69	0.65	0.04**	0.02	2.68
In School	0.42	0.49	6,351	0.50	0.33	0.17**	0.02	10.37
Earnings (2019$)	13620	14220	5,294	13571	13676	-105	504	-0.21
Employed (Income)	0.86	0.34	5,294	0.88	0.84	0.04**	0.01	3.86
Public Assistance	0.02	0.15	6,333	0.02	0.03	-0.01**	0.00	-2.14
Food Stamps	0.09	0.28	6,336	0.06	0.12	-0.07**	0.01	-7.69
Medicaid	0.13	0.34	5,189	0.10	0.17	-0.07**	0.01	-6.54
Limiting Condition	0.02	0.14	6,007	0.02	0.02	0.00	0.00	0.23
Medical Condition	0.06	0.24	5,897	0.07	0.05	0.02**	0.01	2.27
Medical Illness	0.22	0.41	4,415	0.23	0.20	0.03**	0.02	2.19
Fair-Poor Health	0.10	0.30	6,090	0.09	0.10	-0.01	0.01	-1.56
BMI	21.98	9.74	6,281	20.63	23.44	-2.81**	0.32	-8.89
Obese	0.14	0.34	6,353	0.11	0.17	-0.06**	0.01	-5.47
In Parents' Household	0.54	0.50	6,352	0.54	0.53	0.01	0.02	0.58
Has Child	0.21	0.41	6,354	0.14	0.28	-0.14**	0.01	-10.74
Ever Married	0.12	0.32	6,354	0.10	0.13	-0.02**	0.01	-2.26
Ever Cohabitated	0.38	0.48	6,354	0.34	0.41	-0.07**	0.02	-4.22
B. Age 25 Outcomes								
Less than HS	0.12	0.32	5,267	0.08	0.16	-0.08**	0.01	-7.25
High School Grad	0.32	0.47	5,267	0.27	0.36	-0.09**	0.02	-5.47
Some College	0.45	0.50	5,267	0.49	0.40	0.09**	0.02	5.04
College 4+ Years	0.27	0.44	5,267	0.34	0.20	0.14**	0.02	7.99
College Grad	0.25	0.43	5,278	0.32	0.18	0.14**	0.02	8.18
Employed (Status)	0.75	0.43	4,862	0.79	0.72	0.07**	0.02	4.42
In School	0.16	0.36	5,585	0.18	0.14	0.04**	0.01	3.23
Earnings (2019$)	29292	25628	4,946	31383	27274	4110**	989	4.15
Employed (Income)	0.88	0.33	4,946	0.89	0.86	0.03**	0.01	3.06
Public Assistance	0.03	0.17	5,568	0.02	0.04	-0.02**	0.01	-3.70
Food Stamps	0.15	0.35	5,571	0.11	0.18	-0.07**	0.01	-6.53
Medicaid	0.14	0.35	5,515	0.11	0.17	-0.06**	0.01	-5.61
Limiting Condition	0.02	0.14	5,202	0.02	0.02	0.00	0.01	0.50
Medical Condition	0.06	0.24	5,114	0.07	0.06	0.01	0.01	0.81
Medical Illness	0.23	0.42	4,930	0.24	0.21	0.03**	0.01	2.18
Fair-Poor Health	0.12	0.32	5,579	0.10	0.13	-0.03**	0.01	-3.05
BMI	20.07	12.59	5,523	18.45	21.60	-3.16**	0.43	-7.29
Obese	0.17	0.37	5,587	0.14	0.20	-0.06**	0.01	-4.92
In Parents' Household	0.26	0.44	5,585	0.23	0.28	-0.05**	0.02	-3.30
Has Child	0.40	0.49	5,587	0.32	0.48	-0.16**	0.02	-9.31
Ever Married	0.32	0.47	5,587	0.34	0.31	0.03*	0.02	1.80
Ever Cohabitated	0.61	0.49	5,587	0.57	0.65	-0.08**	0.02	-4.36

This table summarizes outcomes for the 1986–2016 NLSY-CYA sample with reported breastfeeding statuses. Columns 1–3 give overall means, standard deviations, and sample sizes. Cols 4–8 give mean comparisons by breastfeeding status, with respective group means for breastfed and not breastfed in Cols 4 and 5, point estimates for the mean differences in Col 6, and associated standard errors and test statistics in Cols 7 and 8. Results are obtained from separate bivariate OLS regressions of each outcome on the breastfeeding treatment indicator. Unit of observation is individual child. All statistics are weighted using NLSY-CYA longitudinal custom weights for the corresponding age-outcome sample. * $p < 0.10$, ** $p < 0.05$

Table C.7: Child Outcomes: Unweighted

	Age 5			Age 10			Age 13		
	(1) OLS	(2) MOM	(3) EXT	(4) OLS	(5) MOM	(6) EXT	(7) OLS	(8) MOM	(9) EXT
Math	0.085**	0.030	0.005	0.086**	-0.049	-0.033	0.104**	-0.017	0.002
	(0.028)	(0.055)	(0.058)	(0.026)	(0.045)	(0.049)	(0.027)	(0.049)	(0.054)
	{5,710}	{906}	{1,319}	{7,105}	{1,448}	{1,903}	{6,315}	{1,256}	{1,679}
Reading Recog.	0.071**	0.029	-0.057	0.075**	0.059	0.043	0.084**	-0.032	-0.054
	(0.026)	(0.054)	(0.058)	(0.029)	(0.048)	(0.053)	(0.032)	(0.052)	(0.057)
	{5,600}	{881}	{1,293}	{7,101}	{1,442}	{1,896}	{6,323}	{1,255}	{1,675}
Reading Comp.	0.012	0.166	0.061	0.104**	0.021	0.020	0.117**	-0.032	-0.018
	(0.033)	(0.113)	(0.137)	(0.026)	(0.043)	(0.048)	(0.025)	(0.043)	(0.047)
	{1,964}	{120}	{184}	{6,989}	{1,409}	{1,857}	{6,269}	{1,227}	{1,636}
Vocabulary	0.108**	-0.101	-0.028	0.144**	0.036	0.018	-0.029	-0.230	-0.534
	(0.043)	(0.092)	(0.098)	(0.037)	(0.068)	(0.074)	(0.086)	(0.303)	(0.457)
	{4,237}	{518}	{801}	{6,024}	{1,076}	{1,444}	{857}	{40}	{45}
Health Problem	0.027**	-0.046*	-0.027	0.012	0.013	0.014	0.038**	0.030	0.041*
	(0.013)	(0.024)	(0.025)	(0.013)	(0.021)	(0.023)	(0.013)	(0.024)	(0.025)
	{7,759}	{1,505}	{2,114}	{7,806}	{1,629}	{2,164}	{7,123}	{1,458}	{1,953}
Behavior (BPI)	-0.033	-0.108**	-0.113**	0.008	0.055	0.040	0.041	-0.011	-0.011
	(0.029)	(0.046)	(0.049)	(0.029)	(0.041)	(0.045)	(0.030)	(0.046)	(0.050)
	{7,347}	{1,337}	{1,910}	{7,376}	{1,475}	{1,988}	{6,764}	{1,337}	{1,793}
Overweight	-0.005	-0.002	-0.001	-0.007	0.023	0.018	-0.000	0.027	0.022
	(0.004)	(0.008)	(0.008)	(0.008)	(0.015)	(0.016)	(0.010)	(0.019)	(0.021)
	{7,790}	{1,513}	{2,130}	{7,874}	{1,659}	{2,204}	{7,490}	{1,601}	{2,144}

This table repeats Table 3.4 without sample weights. Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for full covariates; the difference from the main text is that the regressions are not weighted. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status, outcomes, and covariates. Standard errors clustered by mother in parentheses. Number of observations in braces; for fixed effects specifications, counts refer to observations contributing to breastfeeding identification. * $p < 0.10$, ** $p < 0.05$

Table C.8: Young Adult Outcomes: Unweighted

	Age 21			Age 25		
	(1) OLS	(2) MOM	(3) EXT	(4) OLS	(5) MOM	(6) EXT
High School Grad+	0.026**	0.006	0.005	0.027**	-0.025	-0.009
	(0.011)	(0.021)	(0.023)	(0.011)	(0.023)	(0.024)
	{5,751}	{1,069}	{1,489}	{5,267}	{965}	{1,291}
Some College+	0.042**	-0.025	-0.010	0.042**	0.000	0.016
	(0.014)	(0.025)	(0.027)	(0.015)	(0.028)	(0.031)
	{5,751}	{1,069}	{1,489}	{5,267}	{965}	{1,291}
Years of Education	0.122**	-0.056	-0.015	0.114*	-0.071	-0.013
	(0.049)	(0.090)	(0.093)	(0.066)	(0.110)	(0.120)
	{5,751}	{1,072}	{1,492}	{5,267}	{965}	{1,291}
Employed	0.009	-0.013	0.005	-0.002	-0.027	-0.001
	(0.015)	(0.030)	(0.032)	(0.015)	(0.028)	(0.032)
	{5,927}	{1,169}	{1,569}	{4,922}	{892}	{1,174}
In School or Working	0.017	-0.027	-0.012	0.001	-0.013	0.003
	(0.012)	(0.026)	(0.027)	(0.014)	(0.027)	(0.030)
	{6,140}	{1,210}	{1,631}	{5,014}	{917}	{1,203}
Log Earned Income	0.056	0.197	0.268	-0.092	-0.118	-0.046
	(0.111)	(0.238)	(0.259)	(0.122)	(0.249)	(0.272)
	{5,294}	{934}	{1,266}	{4,946}	{902}	{1,174}
Public Benefits	-0.004	-0.007	-0.002	0.000	-0.014	-0.022
	(0.011)	(0.022)	(0.023)	(0.013)	(0.027)	(0.029)
	{6,349}	{1,275}	{1,723}	{5,579}	{1,087}	{1,422}
Health Problem	0.022*	0.012	-0.000	0.027*	0.043	0.030
	(0.013)	(0.027)	(0.028)	(0.015)	(0.029)	(0.031)
	{6,337}	{1,271}	{1,718}	{5,583}	{1,088}	{1,423}
Overweight	-0.016	0.007	-0.001	-0.015	0.036	0.018
	(0.014)	(0.026)	(0.028)	(0.014)	(0.027)	(0.029)
	{6,353}	{1,277}	{1,726}	{5,587}	{1,088}	{1,424}
Premarital Child	-0.023*	-0.019	-0.009	-0.028*	-0.016	-0.022
	(0.012)	(0.024)	(0.025)	(0.014)	(0.029)	(0.031)
	{6,354}	{1,277}	{1,726}	{5,587}	{1,088}	{1,424}

This table repeats Table 3.5 without sample weights. Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for full covariates; the difference from the main text is that the regressions are not weighted. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status, outcomes, and covariates. Standard errors clustered by mother in parentheses. Number of observations in braces; for fixed effects specifications, counts refer to observations contributing to breastfeeding identification. * $p < 0.10$, ** $p < 0.05$

Table C.9: Interview Responses

	(1) Mean	(2) OLS	(3) MOM	(4) EXT
Interview Response Age 5	0.811**	0.034**	0.048**	0.038**
	(0.572)	(0.010)	(0.016)	(0.017)
	{10,841}	{10,841}	{2,501}	{3,292}
Interview Response Age 10	0.821**	0.035**	0.035**	0.030*
	(0.599)	(0.012)	(0.016)	(0.017)
	{10,827}	{10,827}	{2,499}	{3,283}
Interview Response Age 13	0.795**	0.037**	0.021	0.026
	(0.656)	(0.013)	(0.014)	(0.017)
	{10,779}	{10,779}	{2,476}	{3,256}
Interview Response Age 21	0.748**	0.029**	0.012	0.013
	(0.664)	(0.014)	(0.019)	(0.020)
	{9,806}	{9,806}	{2,168}	{2,813}
Interview Response Age 25	0.738**	0.008	-0.006	-0.017
	(0.656)	(0.014)	(0.019)	(0.021)
	{8,804}	{8,804}	{1,895}	{2,415}
Responded to All Interviews	0.533**	0.022	0.010	0.015
	(0.751)	(0.015)	(0.020)	(0.022)
	{8,804}	{8,804}	{1,895}	{2,415}

Rows enumerate outcomes and columns indicate estimation methods. Outcomes are binary indicators for having responded to the age-interview in question. Column 1 gives outcome means with standard deviations in parentheses. Columns 2–4 give main results. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for full covariates. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status and outcomes. All regressions are weighted using NLSY-CYA longitudinal custom weights for the full NLSY-CYA sample through 2016. Standard errors clustered by mother in parentheses. Number of observations in braces; for fixed effects specifications, counts refer to observations contributing to breastfeeding identification. * $p < 0.10$, ** $p < 0.05$

Table C.10: Interview Responses: Unweighted

	(1) Mean	(2) OLS	(3) MOM	(4) EXT
Interview Response Age 5	0.719**	0.025**	0.031**	0.025
	(0.562)	(0.010)	(0.015)	(0.016)
	{10,841}	{10,841}	{2,501}	{3,292}
Interview Response Age 10	0.727**	0.024**	0.026*	0.016
	(0.608)	(0.011)	(0.014)	(0.015)
	{10,827}	{10,827}	{2,499}	{3,283}
Interview Response Age 13	0.695**	0.027**	0.006	0.006
	(0.651)	(0.011)	(0.013)	(0.014)
	{10,779}	{10,779}	{2,476}	{3,256}
Interview Response Age 21	0.648**	0.030**	0.017	0.013
	(0.658)	(0.012)	(0.014)	(0.016)
	{9,806}	{9,806}	{2,168}	{2,813}
Interview Response Age 25	0.635**	0.017	-0.006	-0.016
	(0.663)	(0.013)	(0.015)	(0.017)
	{8,804}	{8,804}	{1,895}	{2,415}
Responded to All Interviews	0.444**	0.020	-0.002	0.004
	(0.639)	(0.013)	(0.017)	(0.020)
	{8,804}	{8,804}	{1,895}	{2,415}

Rows enumerate outcomes and columns indicate estimation methods. Outcomes are binary indicators for having responded to the age-interview in question. Column 1 gives outcome means with standard deviations in parentheses. Columns 2–4 give main results. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for full covariates. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes all births with nonmissing breastfeeding status and outcomes. All regressions are unweighted. Standard errors clustered by mother in parentheses. Number of observations in braces; for fixed effects specifications, counts refer to observations contributing to breastfeeding identification. * $p < 0.10$, ** $p < 0.05$

Table C.11: Descriptive Outcome Statistics: Breastfeeding and Survey Responses

	Not Breastfed			Breastfed		
Survey Responses:	All	Not	Diff	All	Not	Diff
A. Age 5 Outcomes						
PIAT Math (standard deviation units)	-0.12	-0.11	-0.00	0.24	0.22	0.03
PIAT Reading Recognition (std. dev. units)	0.26	0.27	-0.02	0.58	0.53	0.05
PIAT Reading Comprehension (std. dev. units)	0.68	0.70	-0.02	0.85	0.87	-0.02
PPVT Vocabulary (std. dev. units)	-0.84	-0.72	-0.12	-0.19	-0.18	-0.01
Health Problem	0.40	0.40	-0.00	0.46	0.48	-0.01
Behavior Problems Index (std. dev. units)	0.37	0.32	0.04	0.22	0.14	0.09
Overweight	0.01	0.01	0.00	0.01	0.01	-0.00
B. Age 10 Outcomes						
PIAT Math (standard deviation units)	0.01	-0.11	0.12	0.44	0.36	0.08
PIAT Reading Recognition (std. dev. units)	0.15	0.12	0.04	0.54	0.52	0.02
PIAT Reading Comprehension (std. dev. units)	-0.08	-0.10	0.02	0.33	0.20	0.12
PPVT Vocabulary (std. dev. units)	-0.49	-0.59	0.10	0.10	0.04	0.07
Health Problem	0.32	0.28	0.04	0.34	0.32	0.02
Behavior Problems Index (std. dev. units)	0.44	0.53	-0.08	0.30	0.33	-0.03
Overweight	0.11	0.07	0.05	0.08	0.06	0.02
C. Age 13 Outcomes						
PIAT Math (standard deviation units)	-0.08	-0.15	0.07	0.34	0.27	0.06
PIAT Reading Recognition (std. dev. units)	0.13	0.00	0.12	0.52	0.56	-0.04
PIAT Reading Comprehension (std. dev. units)	-0.28	-0.30	0.03	0.09	0.11	-0.01
PPVT Vocabulary (std. dev. units)	-0.51	-0.63	0.12	-0.19	-0.27	0.09
Health Problem	0.30	0.24	0.06	0.34	0.32	0.01
Behavior Problems Index (std. dev. units)	0.46	0.52	-0.06	0.40	0.44	-0.04
Overweight	0.21	0.15	0.06	0.16	0.13	0.02
D. Age 21 Outcomes						
High School Grad+	0.81	0.78	0.03	0.91	0.89	0.02
Some College+	0.43	0.36	0.07	0.62	0.58	0.03
Years of Education	12.52	12.33	0.19	13.17	13.11	0.05
Employed	0.65	0.59	0.05	0.66	0.69	-0.02
In School or Working	0.76	0.70	0.06	0.83	0.83	-0.00
Log Earnings (2019 Dollars)	7.84	7.59	0.26	8.13	7.89	0.24
Public Benefits	0.21	0.19	0.02	0.11	0.15	-0.04
Health Problem	0.24	0.22	0.02	0.29	0.27	0.02
Overweight	0.45	0.46	-0.00	0.38	0.38	0.01
Premarital Child	0.23	0.27	-0.04	0.12	0.14	-0.03
E. Age 25 Outcomes						
High School Grad+	0.85	0.82	0.03	0.93	0.89	0.04
Some College+	0.49	0.45	0.04	0.67	0.56	0.11
Years of Education	13.10	12.91	0.19	14.09	13.38	0.71
Employed	0.72	0.69	0.03	0.79	0.74	0.04
In School or Working	0.78	0.73	0.05	0.84	0.79	0.05
Log Earnings (2019 Dollars)	8.70	8.56	0.14	9.12	8.93	0.19
Public Benefits	0.27	0.23	0.04	0.16	0.19	-0.04
Health Problem	0.31	0.30	0.02	0.33	0.35	-0.02
Overweight	0.44	0.51	-0.06	0.35	0.35	0.00
Premarital Child	0.34	0.39	-0.05	0.18	0.26	-0.08

This table compares outcomes between 1986–2016 NLSY-CYA interviewees who responded to all outcome age surveys (ages 5, 10, 13, 21, 25) and those who did not, separately for breastfed and not breastfed individuals. indicates an individual responded to all surveys and refers to those who missed at least one survey. Results are obtained from separate OLS regressions of each outcome on indicators for breastfeeding, reponding to all surveys, and their interaction. Unit of observation is individual child. All statistics are weighted using NLSY-CYA longitudinal custom weights for the corresponding age-outcome sample.

Table C.12: Child Outcomes: Responded to All Surveys

	Age 5	Age 10	Age 13
	(1) OLS	(2) OLS	(3) OLS
Math	0.102**	0.075*	0.077*
	(0.043)	(0.041)	(0.040)
	{2,934}	{3,676}	{3,466}
Reading Recog.	0.083**	0.059	0.063
	(0.040)	(0.044)	(0.048)
	{2,873}	{3,666}	{3,466}
Reading Comp.	0.028	0.124**	0.055
	(0.038)	(0.041)	(0.041)
	{982}	{3,618}	{3,432}
Vocabulary	0.085	0.100**	-0.045
	(0.063)	(0.051)	(0.130)
	{2,051}	{3,186}	{432}
Health Problem	0.009	-0.018	0.024
	(0.022)	(0.021)	(0.022)
	{3,900}	{3,906}	{3,751}
Behavior (BPI)	-0.014	0.020	0.071
	(0.043)	(0.044)	(0.044)
	{3,702}	{3,662}	{3,575}
Overweight	-0.005	-0.008	-0.014
	(0.005)	(0.012)	(0.016)
	{3,908}	{3,908}	{3,908}

Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression. All regressions control for full covariates. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes only those individuals who responded to surveys at all outcome ages (5, 10, 13, 21, and 25 years). All regressions are weighted using NLSY-CYA longitudinal custom weights for the age-interview sample. Standard errors clustered by mother in parentheses. Number of observations in braces. * $p < 0.10$, ** $p < 0.05$

Table C.13: Young Adult Outcomes: Responded to All Surveys

	Age 21	Age 25
	(1)	(2)
	OLS	OLS
High School Grad+	0.024	0.026*
	(0.016)	(0.014)
	{3,507}	{3,908}
Some College+	0.031	0.035*
	(0.021)	(0.020)
	{3,507}	{3,908}
Years of Education	0.098	0.135
	(0.080)	(0.095)
	{3,507}	{3,908}
Employed	-0.016	-0.001
	(0.020)	(0.019)
	{3,908}	{3,385}
In School or Working	-0.000	-0.003
	(0.017)	(0.018)
	{3,907}	{3,454}
Log Earned Income	0.021	-0.065
	(0.149)	(0.149)
	{3,255}	{3,489}
Public Benefits	-0.012	-0.009
	(0.014)	(0.016)
	{3,905}	{3,904}
Health Problem	0.034*	0.022
	(0.020)	(0.021)
	{3,906}	{3,906}
Overweight	-0.019	-0.026
	(0.021)	(0.019)
	{3,908}	{3,908}
Premarital Child	-0.018	-0.035**
	(0.015)	(0.018)
	{3,908}	{3,908}

Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression. All regressions control for full covariates. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample includes only those individuals who responded to surveys at all outcome ages (5, 10, 13, 21, and 25 years). All regressions are weighted using NLSY-CYA longitudinal custom weights for the age-interview sample. Standard errors clustered by mother in parentheses. Number of observations in braces. * $p < 0.10$, ** $p < 0.05$

Table C.14: Child Outcomes: Excluding Fixed Effects Sample

	Age 5	Age 10	Age 13
	(1) OLS	(2) OLS	(3) OLS
Math	0.097**	0.136**	0.126**
	(0.042)	(0.041)	(0.042)
	{3,917}	{4,893}	{4,322}
Reading Recog.	0.078*	0.065	0.108**
	(0.041)	(0.046)	(0.050)
	{3,837}	{4,893}	{4,328}
Reading Comp.	0.004	0.132**	0.158**
	(0.045)	(0.040)	(0.040)
	{1,368}	{4,815}	{4,297}
Vocabulary	0.171**	0.204**	-0.126
	(0.065)	(0.055)	(0.116)
	{2,896}	{4,157}	{583}
Health Problem	0.007	-0.027	0.003
	(0.022)	(0.021)	(0.021)
	{5,327}	{5,378}	{4,899}
Behavior (BPI)	-0.004	-0.031	0.076*
	(0.046)	(0.045)	(0.046)
	{5,052}	{5,092}	{4,650}
Overweight	-0.007	-0.032**	-0.019
	(0.005)	(0.011)	(0.015)
	{5,349}	{5,418}	{5,118}

Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for full covariates. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample excludes individuals from families with non-uniform breastfeeding experiences. All regressions are weighted using NLSY-CYA longitudinal custom weights for the age-interview sample. Standard errors clustered by mother in parentheses. Number of observations in braces. * $p < 0.10$, ** $p < 0.05$

Table C.15: Young Adult Outcomes: Excluding Fixed Effects Sample

	Age 21	Age 25
	(1)	(2)
	OLS	OLS
High School Grad+	0.045**	0.041**
	(0.016)	(0.016)
	{3,889}	{3,559}
Some College+	0.066**	0.041*
	(0.024)	(0.024)
	{3,889}	{3,559}
Years of Education	0.170**	0.122
	(0.080)	(0.113)
	{3,888}	{3,559}
Employed	0.032	0.012
	(0.022)	(0.022)
	{4,022}	{3,308}
In School or Working	0.047**	-0.002
	(0.017)	(0.020)
	{4,178}	{3,372}
Log Earned Income	0.039	-0.076
	(0.160)	(0.168)
	{3,608}	{3,356}
Public Benefits	-0.012	-0.007
	(0.015)	(0.018)
	{4,314}	{3,775}
Health Problem	0.032	0.014
	(0.020)	(0.023)
	{4,306}	{3,778}
Overweight	-0.037*	-0.077**
	(0.022)	(0.021)
	{4,315}	{3,781}
Premarital Child	-0.030*	-0.029
	(0.016)	(0.021)
	{4,316}	{3,781}

Rows enumerate outcomes, supercolumns denote outcome ages, and columns indicate estimation methods. Each cell reports the coefficient on an indicator for breastfeeding from a separate regression, with columns, respectively, for OLS, mother fixed effects, and extended family fixed effects. All regressions control for full covariates. Data source is NLSY-CYA, 1986–2016. Unit of observation is individual child. Sample excludes individuals from families with non-uniform breastfeeding experiences. All regressions are weighted using NLSY-CYA longitudinal custom weights for the age-interview sample. Standard errors clustered by mother in parentheses. Number of observations in braces. * $p < 0.10$, ** $p < 0.05$

C.5 References

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