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

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Abstract

Using an original administrative dataset in the context of a scarcity induced-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. (*JEL R28, I38, R20, H53, H75, D91, J22*)

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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 (Cassidy, 2019), these compound challenges nevertheless mark homeless families as a population especially deserving of attention.

Their numbers are not small. 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 shelter 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 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). However, this policy has never been systematically assessed.

In this study, I do exactly that and evaluate how families assigned to shelters in their

[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.

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.

I explain these findings with a "search effort model of family homelessness," in which sheltered families must choose how to allocate resources between housing search and other activities they value, such as labor and leisure. If, as revealed preferences suggest, families prefer to be placed in their pre-shelter neighborhoods, they will allocate less effort to finding permanent housing when assigned there, devoting correspondingly more energy to non-housing pursuits, including labor, public benefits, and their children's educations. Shelter stays will be longer, but paychecks will be larger and school attendance better. They may also require additional incentives—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.

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: families placed locally maintain better connections to existing or prospective employers, enabling them to move out more quickly. 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.

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—in 2016, the vacancy rate for traditional shelters was below 1 percent (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—the most important of which are 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

[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).

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 a local 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 shelter eligibility 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.

Policy discretion comes into play at shelter exit as well. During my study period, the City variously initiated and ended several rental assistance programs intended to shrink the shelter census. The availability of these subsidies similarly depends upon political priorities and budgetary constraints. My second instrument, which I refer to as the "aversion ratio," extends the first by giving the rate of shelter stays averted—through ineligible applications and subsidized exits—per new entrant. I use these instruments separately, each characterizing an experiment influencing the treatment statuses of shelter-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 serve as counterfactuals

[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.

for themselves.

Neighborhood placements have powerful impacts on families. 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-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. It turns out these subgroups—which include families who are large, young, disconnected from services, or from neighborhoods where homelessness is common—are 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.

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.

[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.

These findings complement those in Cassidy (2019), 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 Cassidy (2019), 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. In contrast, most previous economic studies of homelessness[6] have focused on one of five themes: macro issues[7], single adults[8], theory[9], description[10], prevention and prediction[11], or housing stability interventions[12].

My work also contributes to two other literatures. The first is neighborhood effects[13]. 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[14]. 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[15]. 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 (Nichols and Zeckhauser, 1982; Besley and Coate, 1992, 1995). My findings inform this trade-off. Interpreted abstractly, capacity-constrained neighborhood shelter placements constitute exogenous variation in a public benefit program.

[6]O'Flaherty (2019) and Evans, Philips and Ruffini (2019) provide two recent and comprehensive summaries of this literature from the perspective of economists.

[7]Cragg and O'Flaherty (1999); O'Flaherty and Wu (2006); Gould and Williams (2010); Corinth (2017).

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

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

[10]Shinn et al. (1998); Culhane et al. (2007); Ellen and O'Flaherty (2010).

[11]Goodman, Messeri and O'Flaherty (2014); Goodman, Messeri and O'Flaherty (2016); Evans, Sullivan and Wallskog (2016); O'Flaherty, Scutella and Tseng (2018*a*); O'Flaherty, Scutella and Tseng (2018*b*).

[12]Wood, Turnham and Mills (2008); Gubits et al. (2016).

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

[14]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).

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

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 bad) but they will be better connected to government services, jobs, and their children's schools (generally regarded as good). In other words, the two current pillars of New York City's family homeless policy—stays that are short and comfortable—are not complementary. 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 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)[16].

In contrast to the street homeless who tend to dominate public perceptions, this misbranded cohort of families—"unhoused" is more accurate—suffers not, primarily, from substance abuse and mental illness, but from poverty. Aside from bad luck–e.g., unexpected income loss, health crisis, or domestic strife—these families are indistinguishable from the marginally housed poor at large[17]. For them, homelessness is a temporary condition, not an immutable characteristic (Mullainathan and Shafir, 2013; Desmond, 2016).

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[18]. The City is legally obligated to provide emergency accommodations to any family able to demonstrate it has no suitable alternative. Second is NYC's relentless real estate market. New York is a city of leaseholders, 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 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).

Expensive housing and a legal escape valve, paired with poverty's vicissitudes, make NYC's steady rise in homelessness none too surprising. The City has had to expand services apace. 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)[19].

Families apply for shelter at a central intake center in the Bronx. The eligibility determination process is rigorous and requires families to prove they have no suitable housing alternative. State guidelines and court orders govern these determinations, but administrators—and in particular, policymakers—retain considerable discretion. Families deemed eligible are given formal shelter assignments by dedicated placement staff, who take into account such criteria as family size, health issues, safety, and proximity to children's schools[20].

The shelter system these families enter is vast and complex, consisting of traditional

[16] 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).

[17] See, e.g., O'Flaherty (2010); Culhane et al. (2007); Shinn et al. (1998); Curtis et al. (2013).

[18] 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.

[19] 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.

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

Tier II shelters[21], 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[22].

It 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[23].

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 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 child's school, but in practice DHS counts any placement within the youngest child's 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.

3 Theory

A formal theoretical model is not necessary to identify the reduced form impacts of neighborhoods on homeless families. But it is indispensable in organizing ideas. A parsimonious model of homeless family decision making, which I term the *search effort model of fam-*

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

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

[23]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).

ily homelessness, coherently characterizes my main results and offers generalizable insights. Homeless families' most pressing problem is to find permanent housing. Hence it is natural to adapt search theory to their context[24].

Agents are homeless families, characterized by their head (usually a single mother) and indexed by i. They inhabit the simplest possible setting: a static, one period environment. They start the period in shelter. Families value two goods, housing (H) and consumption (C), an aggregate good comprising everything besides housing, including leisure, 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 normalized 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 between housing search (e_S) and consumption ($e_C = 1 - e_S$). No effort is ever wasted[25]. Since e_S, a family's housing search effort, is measured on a 0–1 scale, it affords a further simplification: 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)[26]. In words, families value housing and consumption, there is diminishing marginal utility, and families are risk adverse. Also assume complementarity (or supermodularity), $u_{HC} > 0$. This says 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 $u_C(H, c) > u_C(S, c)$. The marginal utility of consumption is greater when families are housed than when they are in shelter; increments to consumption are more enjoyable when families like where they live.

Neighborhoods enter into preferences in a single place: they affect families' valuation of homeless shelter as a housing good. Being explicit about this dependence, the utility of families in shelter is $u(S(N), C)$, which makes clear that assessments of shelter depend on N, an indicator for local placement. I assume families prefer to be placed in their own 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

[24]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).

[25] "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.

[26]The subscript i, denoting individual families, is omitted for clarity.

expected within-period utility:

$$\max_{0 \leq e_S \leq 1} (1 - e_S) u(S(N), C) + e_S u(H, C)$$

$$\text{subject to}$$

$$C \leq w(1 - e_S)$$

where w denotes the "wage" or, more generally, the return to effort not expended on housing search, inclusive of opportunity costs. I assume that w is a function of the effort expended on housing search (analogous to a cost function), and denote it explicitly as $w(e_S)$.

Assuming that the consumption constraint binds with equality at an interior solution, optimal housing search effort, e_S^* is implicitly defined by the first-order condition:

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

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[27].

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

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 (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[28].

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

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

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.

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. I focus on these years because this is the period in which shelter capacity constraints have been the most binding, and thus when the case for random neighborhood assignment is the strongest footnoteIn addition, the CARES system came online during 2012; prior to that, DHS relied on less robust information technologies. These legacy records were imported to CARES when the system transitioned.. 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, using both probabilistic and deterministic techniques[29].

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[30]; 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[31], my complete sample consists of 68,584 family-spells. This is a near-census of family homelessness. As

[29] As described in the Appendix, these linkages, together with the fact that the foremost purposes of these data systems is program administration, not analysis, means my data management process is not inconsiderable. Major steps besides cross-agency matching include cleaning and standardization, geocoding addresses, defining analytical variables.

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

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

shown in Table 1, my analytical sample shrinks for three reasons. First, 7,178 families originate from outside NYC, leaving 61,406 family-spells relevant for assessing neighborhood effects. Another 286 spells lack data on borough of origin, leaving 61,120 spells for which treatment status can be inferred[32] Finally, I limit my analytical sample to those families whose oldest is under 18 years of age[33]. Henceforth I refer to these 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-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).

Variables are defined and measured at the time of shelter entry. For group characteristics shared by family members, like shelter assignment, I assign that value to the family. For characteristics that are aggregated among family members, like family size, I violate the "at-entry" rule and assign the family its maximum for the spell. Maximums better reflect families' true compositions. For individual characteristics that vary among members, such as age or sex, I assign a family value in terms of the (initial) head of family, on the basis that the family head exerts the greatest influence on outcomes.

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.

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[34]. As the most *immediate* shelter outcome, LOS 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. I take logs, for several reasons: (1) durations are non-negative, (2) it's more reasonable to assume proportional (multiplicative) effects of treatment on durations, (3) the log of length of stay is approximately normally distributed.

[32] 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.

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

[34] 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.

Shelter stays 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. The circumstances of exit also matter. Perhaps the most policy-relevant way to characterize shelter departures is by the presence or absence of rental subsidies. My third outcome is an indicator for subsidy receipt. 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, public benefits, derives from records maintained HRA, the City's designated Local Social Service Agency. These records span 2001–2016. HRA oversees virtually all aspects of the social safety net, including the two most important income supports for homeless families: Cash Assistance and Food Stamps.

Cash Assistance (CA) consists of Temporary Assistance for Needy Families (TANF), which, in New York, is referred to as Family Assistance (FA), and its State counterpart for single adults and TANF time-limited families, Safety Net Assistance (SNA). Sometimes described as "public assistance" or "welfare," CA 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 CA as of August 2019[35].

Food Stamps (FS), officially known as the Supplemental Nutrition Assistance Program (SNAP), provides low-income families with categorical monthly dollars that must be spent on food. Its eligibility standards are less strict than CA; 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[36].

My measures of public benefit use consist of indicators for families' receipt of Cash Assistance and Food Stamps. I focus on two periods: the year post-shelter entry and the year post-shelter exit. Receipt is defined as having active status at any time during the period of interest. As discussed below, the choice of one-year indicator variables for these outcome measures is partly designed to address censoring issues.

Of course, the ultimate ambition of most government-administered human service programs is employment and earned income. Accordingly, a complete evaluation of family homelessness policy must include an accounting of labor market outcomes. To that end, 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.

I construct indicators for positive earnings during any quarter in those years as my

[35]Cohen and Giannarelli (2016); New York State Office of Temporary and Disability Assistance (2016*b*, 2015*b*, 2017); NYC Human Resources Administration (2019).

[36]New York State Office of Temporary and Disability Assistance (2019); NYC Human Resources Administration (2019)

measure of employment, again focusing the year post-entry and the year post-exit[37]. 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, so as to avoid omitting families with zero earnings when taking logs[38].

Public benefit and labor market outcomes require cross-agency data matches. Because individual identifiers vary by program (and are subject to administrative error), I use probabilistic matching techniques to link DHS and HRA data[39]. Per DOL policy, the DHS-DOL link is deterministic based on Social Security Number (SSN).

4.2 Treatment

In my leading case, I define treatment as in-borough placement[40]. Origin address is defined as the family's "last known address" reported to DHS[41]. 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 and therefore endogenous, 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 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[42]. The average in-borough family is placed in a shelter 2.7 miles from their previous address, while the average out-of-borough one is placed 9.3 miles away. As a second check, I define 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.

[37]DOL data lacks information on work hours. All that is known is quarterly earnings; if this greater than zero, I consider an individual to be employed

[38]For censored spells, the earnings denominator is the minimum of four quarters or the number of quarters before censoring.

[39]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).

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

[41]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.

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

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*, as it is my preferred empirical specification.

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 includes non-relative 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. 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.

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 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. Pregnancy is a dummy equal to one if the 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. 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, both of which can exert influences on families' shelter experiences. In some specifications, I also include dummies for the 271 individual "facilities" into which families in my sample were placed. These dummies proxy for unobservable shelter and provider characteristics[43].

As is standard, I assume missing or erroneous data is noninformative. This is particularly noteworthy in the context of cross-agency matches. I assume that a non-linkage between a DHS family and HRA/DOL records is a true non-match: these families are truly not receiving benefits or not working. Similarly, for the DHS health issues indicator, I interpret missing values 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. In addition, to avoid incidentally truncating the sample, I create "unknown" categories for three categorical variables: eligibility reason, race, and education[44]

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 censored. For outcomes derived from DHS records (LOS, 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 A.1).

However, my HRA data only extends through 2016 and my DOL data through the first quarter of 2017. Thus, for these outcomes, as discussed above, 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[45]. 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

[43] 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.

[44] The issue is more pronounced for the latter two variables, which derive from HRA records. While some families do not report race or education, non-matches between DHS and HRA account for most of the unknown cases. Because these families are those who have never received public benefits, it can reasonably be inferred that they either draw from the higher end of the education/labor spectrum or are immigrants (whose eligibility for some programs is more limited).

[45] 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.

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 representative of longer-staying later-year ones. Nevertheless, for robustness, I replicate most of my main analyses for a sample of pre-2015 entrants[46].

5 Empirical Approach

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. For tractability, my primary outcome is log length of stay. 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:

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

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 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. 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.

[46] 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.

5.2 Instrumental Variables: Exogenous Policy Shocks

In Section 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 (Angrist and Pischke, 2008). 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. Broadly, these measures can be classified into two categories: front door and back door. Front-door policies, like those influencing eligibility determinations, affect the pace of shelter entry, while back-door approaches, like rental subsidies, impact exit rates[47]. 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 of quasi-random variation—to doubly justify the natural experiment assumption.

I consider two such instruments. The first, borrowed from Cassidy (2019), 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 enforced[48]. As can be seen in the top panel of Figure 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[49]. The figure also makes plain the

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

[48] 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).

[49] 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

strong relationship between eligibility policy and in-borough placement[50].

Specifically, my first instrument is the 15-day moving average of the initial ineligibility rate for rolling 30-day application periods[51]. For family i entering shelter on day $D = d$, my instrument Z_{id}^{IE} is defined as average ineligibles divided average applications:

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

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. As usual, the verdict turns on independence and exclusion.

Independence requires that the ineligibility rate not influence the mix of shelter entrants; because ineligibility policy plays a direct role in selecting the sample, this is a nontrivial concern. In Cassidy (2019), I present detailed evidence that this is not the case. 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 trends without overfitting. In addition, eligibility policy is the most direct front-door intervention, so correlated policy changes 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: an upper bound on true LATE's than may be partially inflated by related policy changes.

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

Assistance. (2015*a*); Fermino (2016*a*); Eide (2018); New York Daily News Editorial (2014); Fermino (2016*b*); Katz (2015); Routhier (2017*b*).

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

[51] 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.

strengthen stability, the City has implemented a variety of rental assistance programs over the years. Typically offering time-limited benefits (e.g., for two to five years) and requiring client contributions (usually 30 percent of income), these programs, which are often conditioned on criteria such as holding a job, 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 1, the aversion ratio has an even tighter correspondence with movements in the probability of in-borough placement; accounting for both front- and back-door policies makes the instrument stronger. The arguments required to justify independence and exclusion are similar as 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) 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 1 representing the second stage and first stages given by:

$$N_{ip} = \mathbf{X}_{ip}\boldsymbol{\pi}_0 + \pi_1 Z_{ip} + \nu_{ip} \tag{2}$$

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, necessarily 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.

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 Cassidy (2019), I extend this work to incorporate explicit hypothesis tests and continuous characteristics. I follow the same procedure here[52].

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[53]. This lends itself neatly to a regression discontinuity design[54]. 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[55]. 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 just 16 support points, $A_{ip} \in \{-3, -2, \ldots, 11, 12\}$[56].

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 fuzzy RD is IV, with school-age threshold crossing, $T_{ip} = \mathbf{1}\{A_{ip} \geq 0\}$, as the instrument (Angrist and Pischke, 2008).

My RD is also fuzzy in a less standard way. School-starting age is blurry[57]. Although most children begin kindergarten in their age-five year, parents retain the option of deferring

[52] 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 Cassidy (2019).

[53] 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.

[54] 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)

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

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

[57] In principle, this issue could be addressed with data on children's actual enrollment statuses, which I lack due to confidentiality restrictions. However, this limitation may be an advantage: focusing on conventional school starting (age five) avoids endogeneity issues with parental school enrollment choices.

enrollment to first grade during their children's age-six years. In addition, families may enter shelter at any time during their child's school-starting years; those that enter shelter prior school enrollment are less likely to receive local placement priority at the time of application[58]. School-starting blurriness will tend to attenuate my results towards zero due to noise.

Discrete fuzziness dictates my RD analysis reduces to standard IV. Traditional RD concerns—local polynomial choice and bandwidth 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

$$
\begin{aligned}
N_{ip} &= \mathbf{X}_{ip}\boldsymbol{\pi}_0 + \pi_1 T_{ip} + \nu_{ip} \implies \widehat{N}_{ip} && \text{(first stage)} \\
Y_{ip} &= \mathbf{X}_{ip}\boldsymbol{\pi}_1 + \tau^{RDW}\widehat{N}_{ip} + \varepsilon_{ip} && \text{(second stage)}
\end{aligned} \tag{3}
$$

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 treatment blurriness, 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 commonly, 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, attributing any discontinuity in extrapolated intercepts to the effect of threshold-crossing (Cattaneo, Idrobo and Titiunik, 2017). My "Linear" models are rooted in this framework. The linear label emphasizes my choice functional form: given the limited number of support points and IV setup, there is little to be gained from greater flexibility, but much risk of overfitting. The graphical analysis in Section 6 supports this choice. I allow the slopes to differ on either side of the threshold, estimating the following set of equations

[58]The City's introduction of universal pre-K in 2015 also guarantees children pre-kindergarten spots during the year they turn four. As an empirical matter, however, pre-K is not accorded the same treatment priority as core grades.

by 2SLS:

$$
\begin{aligned}
N_{ip} &= \mathbf{X}_{ip}\boldsymbol{\pi}_{10} + \pi_{11}T_{ip} + \pi_{12}A_{ip} + \pi_{13}(A_{ip} \times T_{ip}) + \nu_{1ip} \implies \widehat{N}_{ip} && \text{(first stage)} \\
N_{ip} \times A_{ip} &= \mathbf{X}_{ip}\boldsymbol{\pi}_{20} + \pi_{21}T_{ip} + \pi_{22}A_{ip} + \pi_{23}(A_{ip} \times T_{ip}) + \nu_{2i} \implies \widehat{N_{ip} \times A}_{ip} && \text{(first stage)} \\
Y_{ip} &= \mathbf{X}_{ip}\boldsymbol{\pi}_{30} + \tau^{RDL}\widehat{N}_{ip} + \pi_{32}A_{ip} + \pi_{33}(\widehat{A_{ip} \times N}_{ip}) + \varepsilon_{ip} && \text{(second stage)}
\end{aligned}
\tag{4}
$$

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 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[59]. 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 and monotonicity is uncontroversial. As usual, 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.

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 of families in my sample have multiple stays during my study period (see Table A.2). When these families' treatment statuses vary

[59] In related work, Dong (2015) offers corrections when the running variable is a dicretized 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.

across these stays, comparing own outcomes when placed locally and distantly is exacting way to estimate treatment effects. Implementing my family fixed effects estimator is a straightforward modification of Equation 1 to include individual student dummies, α_i. For family i in shelter spell p,

$$Y_{ip} = \alpha_i + \tau^{FE} N_{ip} + \mathbf{X}_{ip}\boldsymbol{\beta} + +\varepsilon_{ip} \tag{5}$$

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 administratively-derived covariates that capture broad classes of cross-spell variation, including placement criteria. In addition, prior research suggests homeless spells are largely based on luck (O'Flaherty, 2010); it follows that those with multiple bad hands be representative of homeless families in general.

6 Results

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 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 the indicator for in-borough placement. The difference between untreated (out-of-borough) and treated (in-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 terms of family composition, as well as education and 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: they average third grade; out-of-borough, the average is second. Commensurately, family heads are older, too.

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

6.2 OLS Results

Tables 3A and 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 3A analyzes stays and returns—the most salient outcomes in the homeless services domain. Table 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. As would be expected under random assignment, covariates beyond placement factors make little difference in the results. 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.

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 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. Part of the policy impetus is to keep families better connected to jobs and resources; these outcomes are evidence of policy effectiveness. 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.

These findings remain consistent in my shelter fixed effects 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.

Tables 4A and 4B, organized identically to the main results, present additional robustness checks, examining the same set of outcomes for all three samples for treatment measured as within-school-district placement and school-shelter distance, in miles. 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. But being very close to home may be more important for jobs than it is for other outcomes.

Distance treatment broadly confirms my main results, demonstrating that genuine proximity effects—rather than borough quirks—are at work. 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[60].

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 5A and 5B present my main policy IV results. Similar in organization to Tables 3A and 3B, with outcomes in rows and separated into three panels, the first three columns assess the ineligibility rate instrument while the latter three analyze 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

[60]Table A.6 repeats Tables 3A and 3B for several alternative outcome definitions.

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 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 impossible. Weak instrument bias is clearly not the culprit. The results instead point to vast heterogeneity. 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 A.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. Such challenges also 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.

The evidence for highly elastic policy responses among compliers suggest the potential for efficiency gains through improved targeting of local placements. Effective targeting requires understanding who these highly responsive families are. Table 6A, compares 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[61]. About 8 percent of my Full sample are ineligibility rate compliers and 10 percent comply with the aversion ratio (see Tables A.7 and A.8). The most notable contrast is borough of origin. 57 percent of compliers are from the Bronx, compared with 39 percent of non-compliers. Compliers also tend to be medium-large families: 39 percent have four or five members, compared with 28 percent of non-compliers. They are 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 (45 percent of Bronx entrants are Black vs. 55 percent overall; just 21 percent of Bronx placements are in commercial hotels.) Other comparisons are imprecisely estimated[62]. It should also be noted that these complier characteristics are indicative but not unqualified: majorities of large, young, and Bronx families are non-compliers, after all, so unobservables and characteristic interactions are clearly implicated.

The aversion ratio story is similar; this is not surprising, given ineligibility policy factors heavily. Aversion ratio compliers are more likely to originate from the Bronx (55 percent vs. 39 percent) and less likely to be in commercial hotels (14 percent vs. 29 percent). Family size and race contrasts lose statistical precision, though the point estimates are similar, with gaps of +6 pp for family size of 4–5 and −9 pp for Black. 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[63].

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 (29 percent) out-of-borough families are placed. 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-

[61] See Tables A.19 and A.20 for comparisons of additional characteristics.

[62] Differences between this depiction of ineligibility rate compliers and that discussed in Cassidy (2019) 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.

[63] 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 Cassidy (2019), where the unit of complier comparison is school-age children.

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.

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 variation over time and their trends may pick up the effects of complementary policies (e.g., improved shelter quality), which would overstate the impact of neighborhood placements per se. In my main results, I control for macro patterns with a year cubic. Table A.13 details a sensitivity analysis OLS and IV to alternative time trends: year dummies, linear year trends, and monthly splines of varying flexibility. The OLS results are little changed. However, IV is sensitive; the choice of time control is not innocuous. Sufficiently flexible trends can quickly generate multicollinearity with my instruments. My preferred specification—a cubic in years—strikes a balance between flexibility and overfitting; the three-knot monthly spline confirms the main results. Figures A.3 and A.4 visually assess IV and confirm this impression: first stages are strong regardless of the time trend, but flexible functions absorb most of the informative variation in reduced forms. The robust relationship between the instruments and treatment is evidence in favor of the exclusion restriction; the outcome-time association suggests the sensitivity of results is explained by overfitting. Additional robustness checks for the ineligibility and aversion instruments are detailed in Tables A.11 and A.12, respectively. My main results are confirmed.

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.

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 2–4 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 -5 to 0; the above-threshold regression is fit on the full [0,12] interval[64].

[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.

The top left panel of Figure 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, top right). Exits and returns do not display decisive breaks (bottom panels) .

Figures 3 and 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 8A and 8B formalize the RD analysis. 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 Columns 2 excludes the threshold in assessing a symmetric two-year window ($A_{ip} = \{-2, -1, 1, 2\}$). Columns 3 and 4 provide "global" linear fits ([-3,12] bandwidth), allowing for different slopes on either side of the threshold, with the latter column 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 8A)[66]. Subsidized exits follow a similar pattern: school-age compliers are 35–66 pp more likely to leave shelter with a subsidy. There is little evidence of impact on shelter returns.

Similarly, as the graphical analysis presaged, there are few stable, precisely-measured entry-year impacts on benefits and employment (Table 8B, Panel B1). The exception, as by might by now 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 are suggestive of mild negative impacts.

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

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

Exit-year effects are generally sharper (Table 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 5 demonstrates this is not the case: the frequency of shelter entry is smooth around the treatment threshold. 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 6–8 repeat the RD plots for these characteristics, while Table A.18 provides 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 interestingly peak among families whose oldest children are five. On the other hand, year of shelter entry (families whose oldest children are older 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 unexplained discontinuities at the threshold. 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[69]. Overall, families around the school-starting threshold are largely comparable; most differences are expected and can be addressed by the inclusion of covariates. 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-

[67] Tables A.14 and A.15 provide additional Wald and Linear specification permutations, respectively. Table A.16 reproduces the RD analysis for my three alternative samples. Table A.17 replicates the RD analysis distance treatment across all four samples. The main conclusions remain unchanged.

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

[69] A formal complier characterization exercise, detailed in Table A.22, confirms these impressions. Families placed in-borough only when they have school age children disproportionately have Bronx and Brooklyn origin, Tier II placements, fewer members, and younger heads.

starting families are an important subpopulation in their own right.

6.5 Family FE Results

My final identification strategy relies upon aa different sort of natural experiment: multiple homeless spells. Prior research suggests families with multiple homeless spells within a short period are substantially similar to those with single spells. At the least, multiple spells are not uncommon: 10,390 families, about a fifth of my sample, have more than one shelter stay during the 2010–2016 period. This fact increases the likelihood they are representative of all homeless families, while also defining a policy-relevant subsample (multi-spell families) for which they definitely are. In addition, shelter scarcity quasi-randomness continues to apply to each spell as well.

Tables 9A and 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 for these outcomes are also in the expected directions.

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 Cassidy (2019), 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 constraints exhibit greater policy responsiveness.

This pattern of outcomes is consistent with a search effort model of shelter behavior: homeless families respond to program incentives by allocating effort to their highest-value priorities. Local shelter is more desirable, so families use more of it, diverting housing search hours to other activities, like work and school.

The challenge for policymakers is partly philosophical. The current policy objective is to place all families locally, to the extent capacity constraints (and other limitations) 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 objective is to maximize the well-being of participants while they are participating, then loosening resource constraints through benefit enhancements (local placement) 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 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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9 Tables

Table 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; rows with equal indentation are mutually exclusive for purposes of this table.

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

Table 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 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 3B: OLS Main Results

Outcome	Outcome Mean (1)	Full Sample: Base (2)	Placement (3)	Main (4)	Shelter (5)	Non-DV: Main (6)	Pre-2015: 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 4A: OLS Robustness

	School District			Distance		
	Full	Non-DV	Pre-2015	Full	Non-DV	Pre-2015
	(1)	(2)	(3)	(4)	(5)	(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 3A. Columns give samples; super-columns 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 4B: OLS Robustness

	School District			Distance		
	Full	Non-DV	Pre-2015	Full	Non-DV	Pre-2015
	(1)	(2)	(3)	(4)	(5)	(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 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 5A: IV Main Results

	Ineligibility Rate			Aversion Ratio		
	Placement	Main	Shelter	Placement	Main	Shelter
Outcome	(1)	(2)	(3)	(4)	(5)	(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 3A. Instruments are indicated by supercolumns. Standard errors clustered at family group level in parentheses. First-stage F-stats 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 3A and 3B. * $p < 0.10$, ** $p < 0.05$

Table 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 3A. Instruments are indicated by supercolumns. Standard errors clustered at family group level in parentheses. First-stage F-stats 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 3A and 3B. * $p < 0.10$, ** $p < 0.05$

Table 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 the Cassidy (2019). Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family.

Table 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 the Cassidy (2019). Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family.

Table 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 the Cassidy (2019). Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family.

Table 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 the Cassidy (2019). Standard errors (in parentheses) and differences in means (with t-stats in brackets) are calculated from 200 bootstrap replications, clustering by family.

Table 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 grade year (end-of-school-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 grade year 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 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 grade year (end-of-school-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 grade year 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 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 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. * $p < 0.10$, ** $p < 0.05$

10 Figures

Figure 1

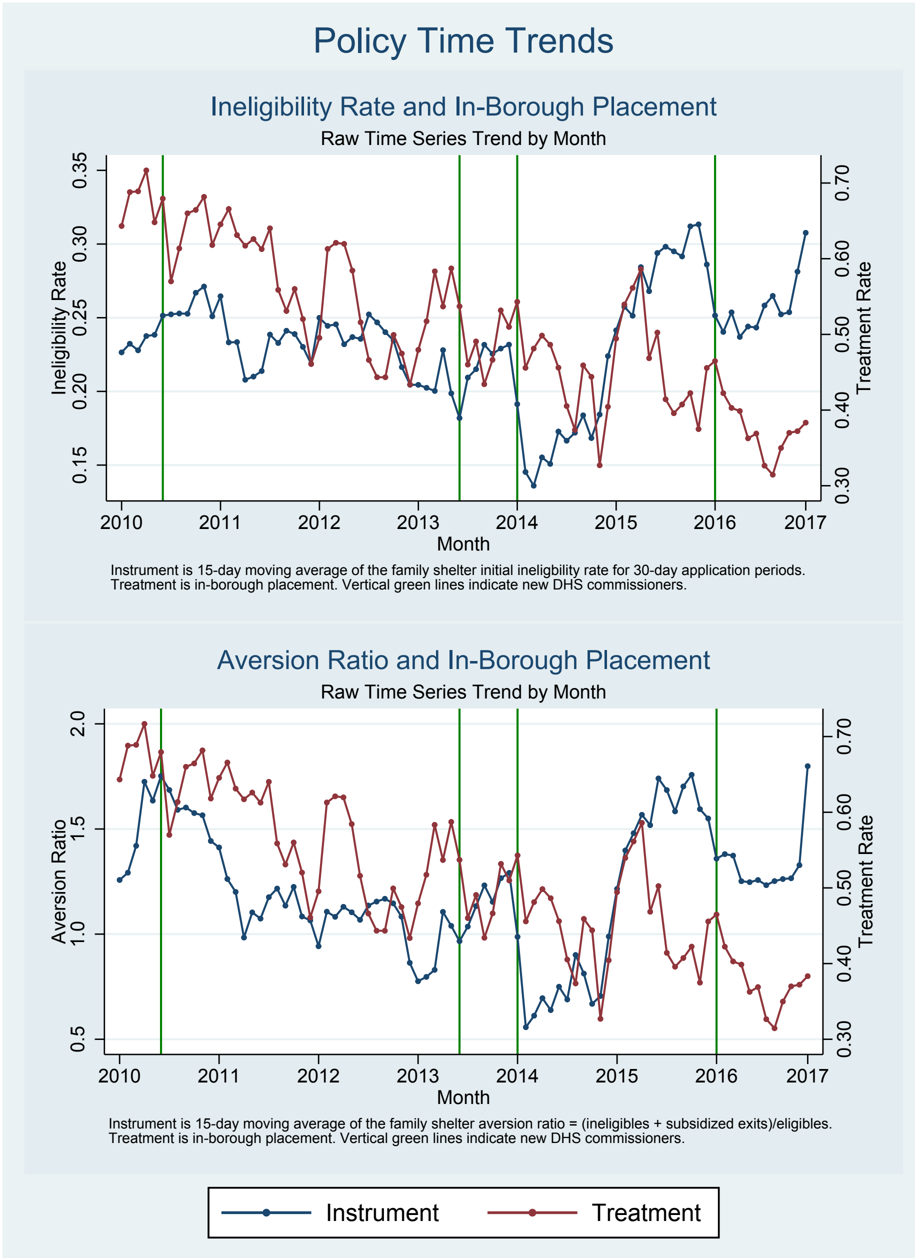
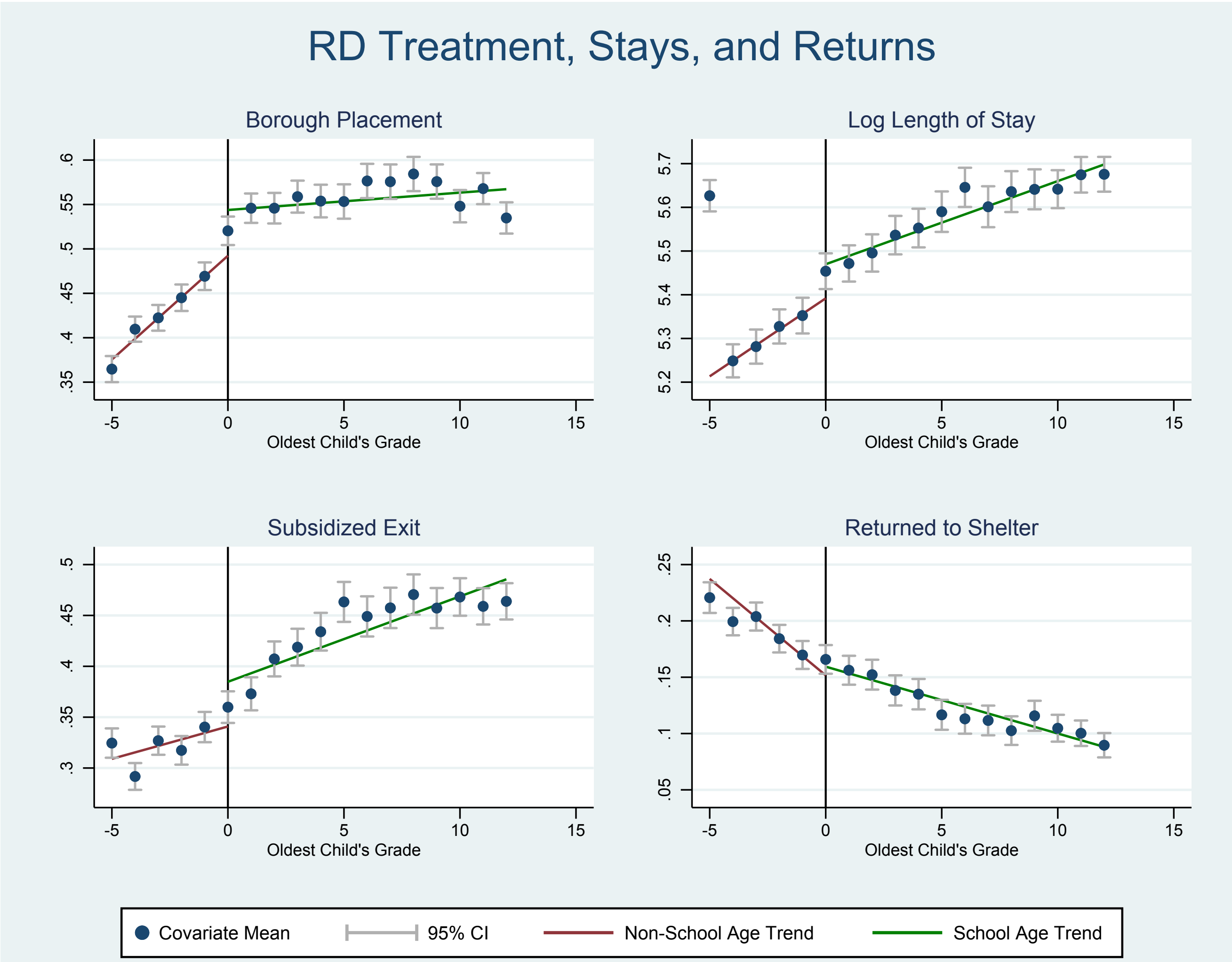


Figure 2



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

Figure 3

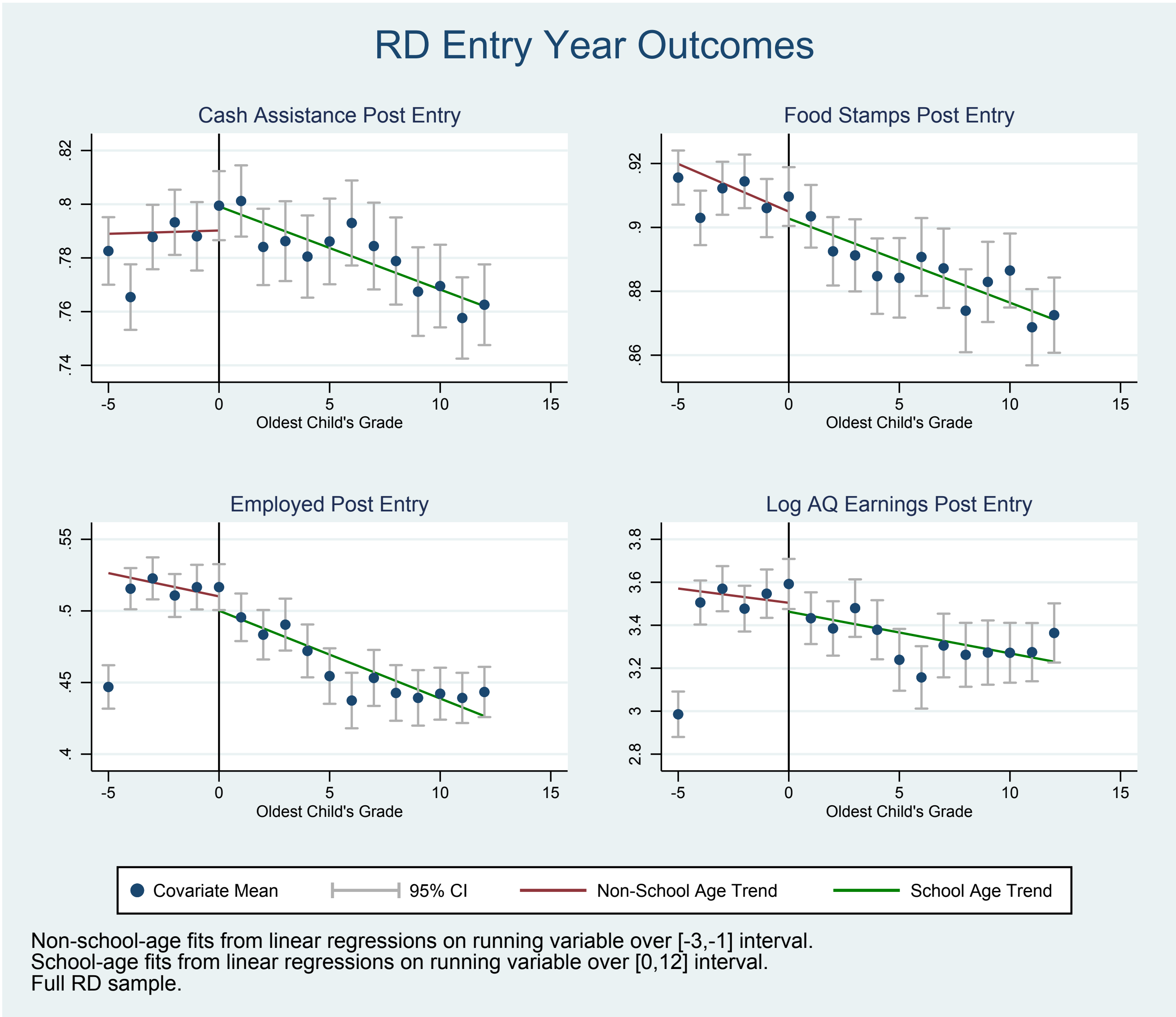

RD Entry Year Outcomes
Cash Assistance Post Entry
Food Stamps Post Entry
Employed Post Entry
Log AQ Earnings Post Entry
Oldest Child's Grade
Covariate Mean
95% CI
Non-School Age Trend
School Age Trend
Non-school-age fits from linear regressions on running variable over [-3,-1] interval.
School-age fits from linear regressions on running variable over [0,12] interval.
Full RD sample.

Figure 4

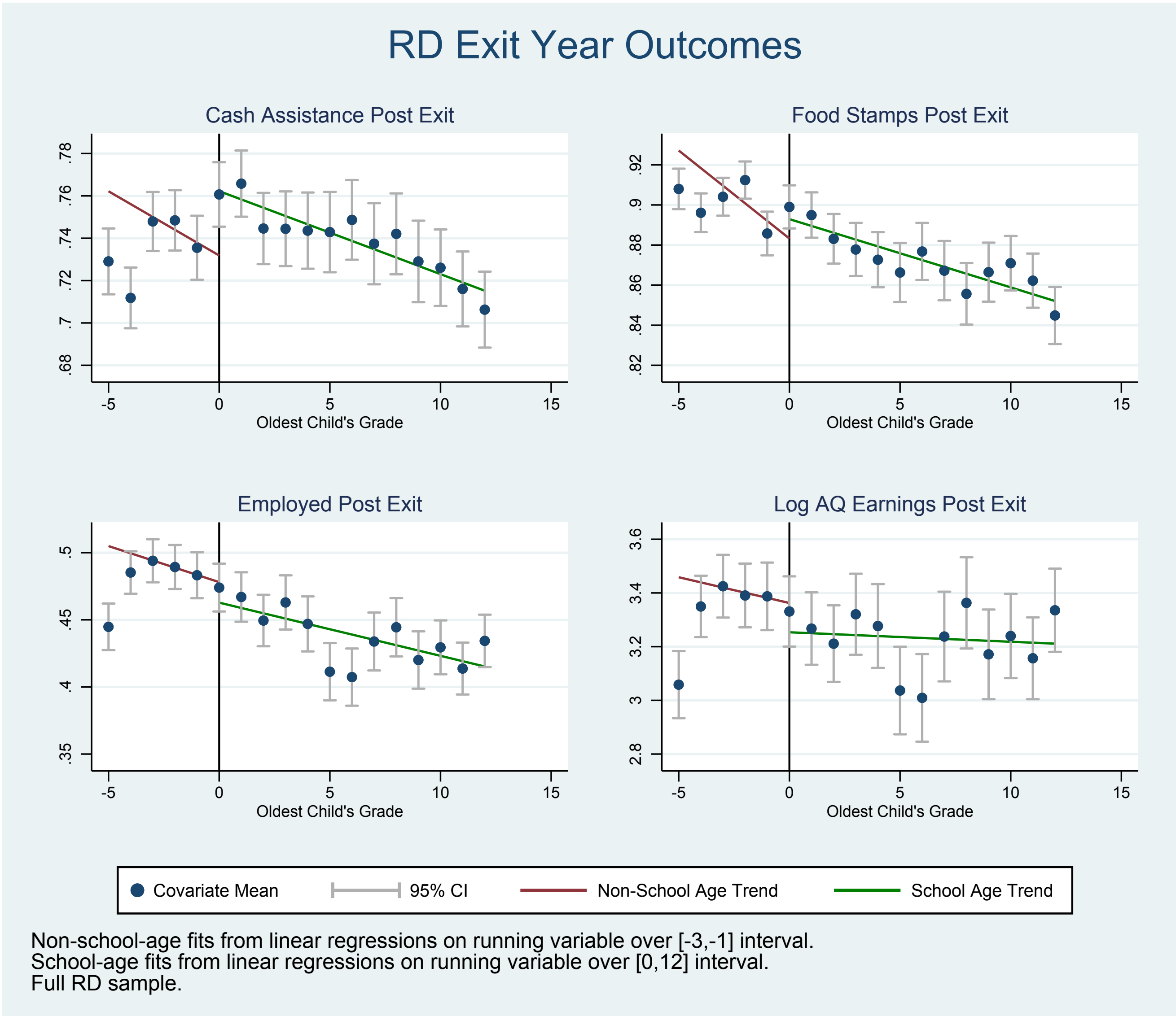

RD Exit Year Outcomes
Cash Assistance Post Exit
.68 .7 .72 .74 .76 .78
-5 0 5 10 15
Oldest Child's Grade
Food Stamps Post Exit
.82 .84 .86 .88 .9 .92
-5 0 5 10 15
Oldest Child's Grade
Employed Post Exit
.35 .4 .45 .5
-5 0 5 10 15
Oldest Child's Grade
Log AQ Earnings Post Exit
2.8 3 3.2 3.4 3.6
-5 0 5 10 15
Oldest Child's Grade
Covariate Mean
95% CI
Non-School Age Trend
School Age Trend
Non-school-age fits from linear regressions on running variable over [-3,-1] interval.
School-age fits from linear regressions on running variable over [0,12] interval.
Full RD sample.

Figure 5

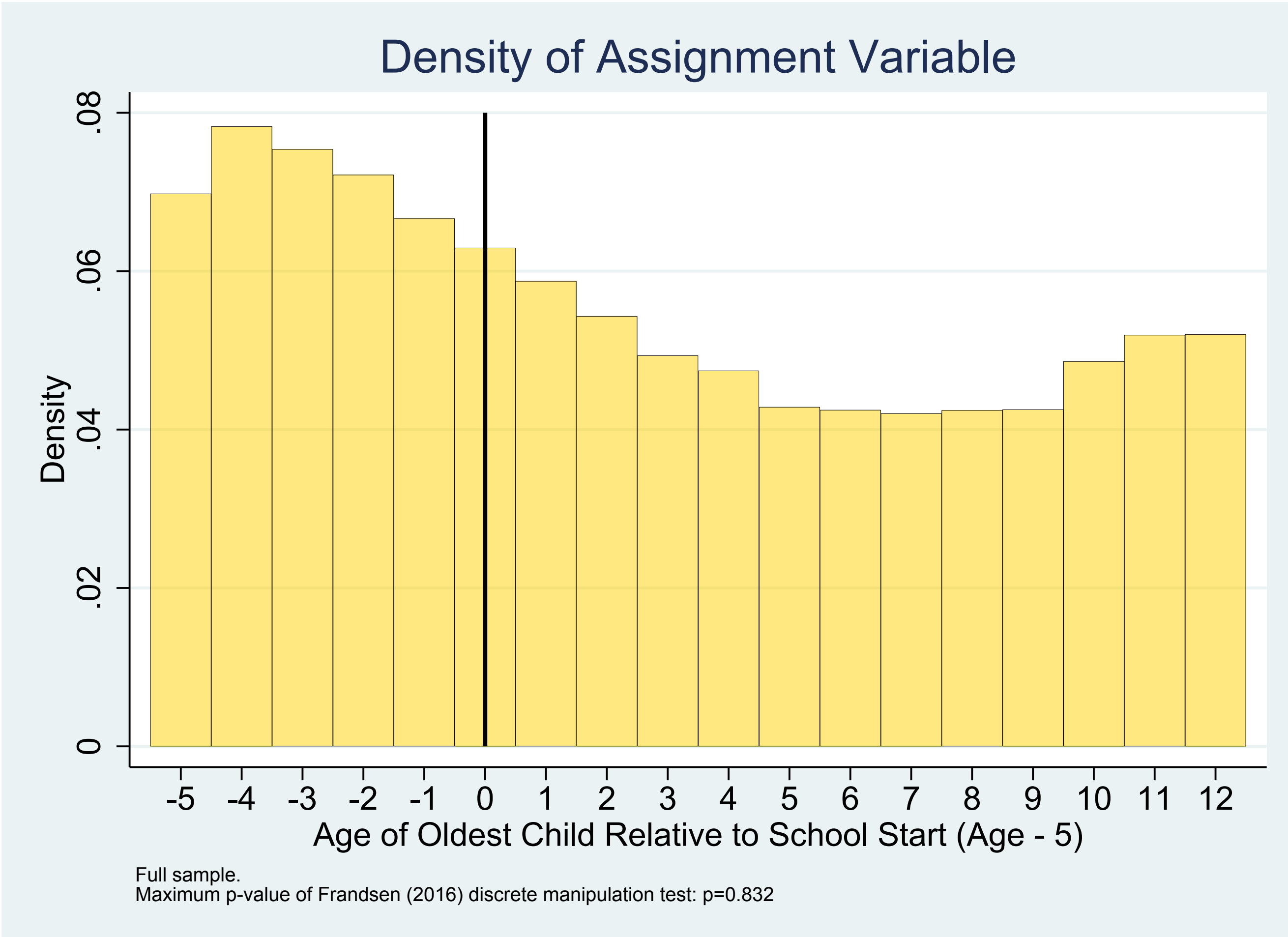
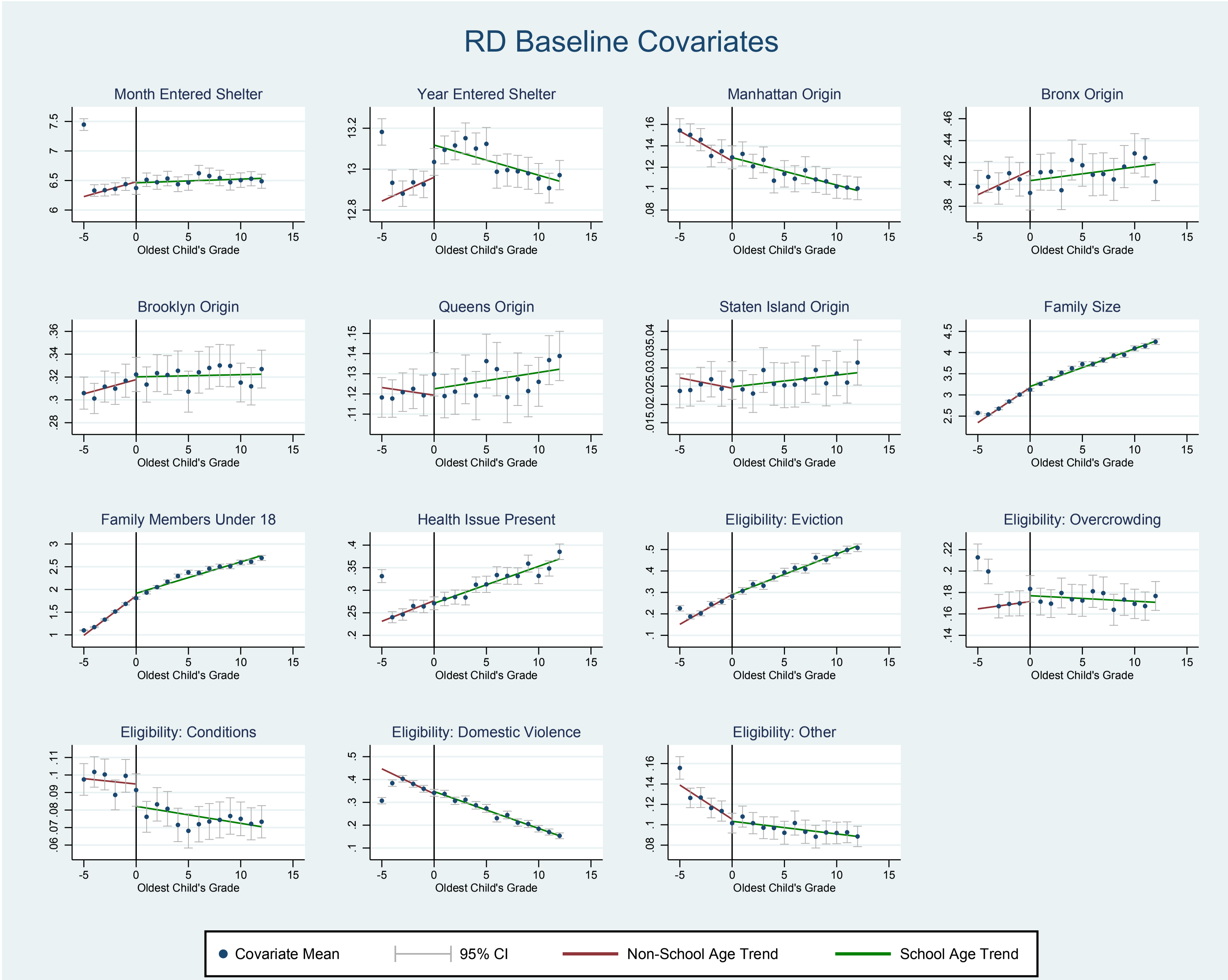
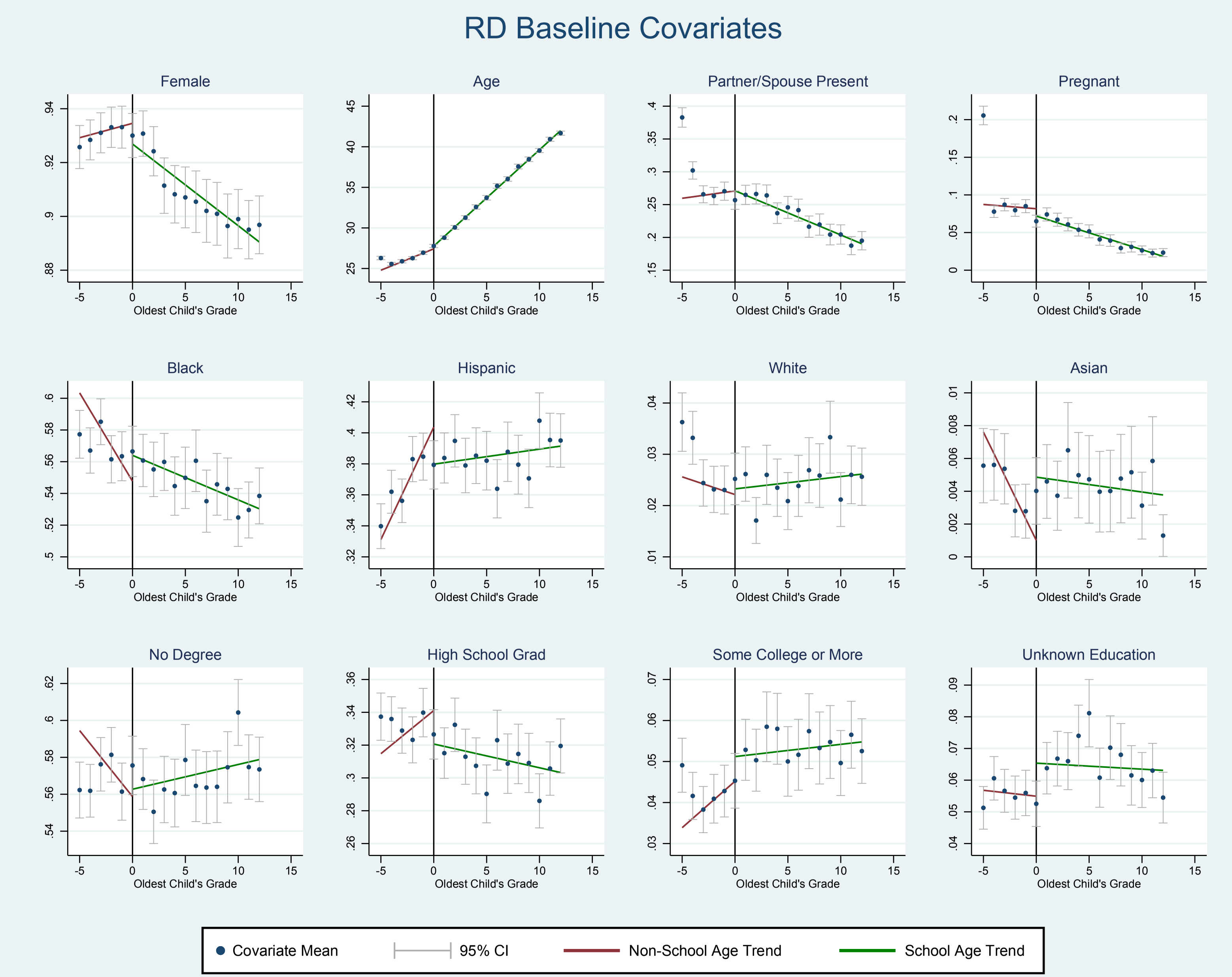

Density of Assignment Variable
Density
0
.02
.04
.06
.08
-5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8 9 10 11 12
Age of Oldest Child Relative to School Start (Age - 5)
Full sample.
Maximum p-value of Frandsen (2016) discrete manipulation test: p=0.832

Figure 6



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.

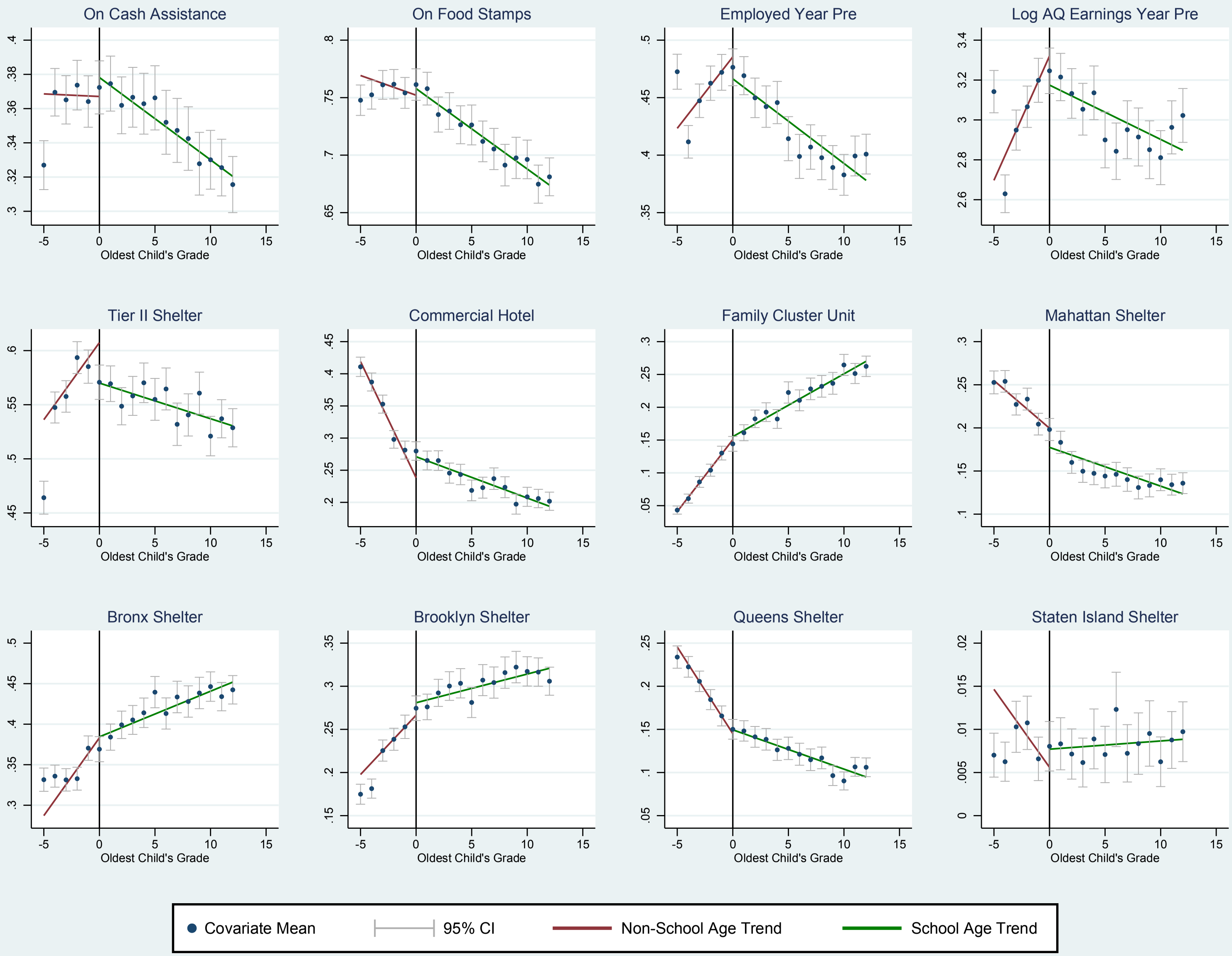
Figure 7



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 8

RD 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.