The Effects of Eviction on Children*

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Abstract

Eviction may be an important channel for the intergenerational transmission of poverty, and concerns about its effects on children are often raised as a rationale for tenant protection policies. We study how eviction impacts children's home environment, school engagement, educational achievement, and high school completion by assembling new data sets linking eviction court records in Chicago and New York to administrative public school records and restricted Census records. To disentangle the consequences of eviction from the effects of correlated sources of economic distress, we use a research design based on the random assignment of court cases to judges who vary in their leniency. We find that eviction increases children's residential mobility, homelessness, and likelihood of doubling up with grandparents or other adults. Eviction also disrupts school engagement, causing increased absences and school changes. While we find little impact on elementary and middle school test scores, eviction substantially reduces high school course credits. Lastly, we find that eviction reduces high school graduation and use a novel bounding method to show that this finding is not driven by differential attrition. The disruptive effects of eviction appear worse for older children and boys. Our evidence suggests that the impact of eviction on children runs through the disruption to the home environment or school engagement rather than deterioration in school or neighborhood quality, and may be moderated by access to family support networks.

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1 Introduction

Disruptions to the home environment—following eviction, foreclosure, divorce, or other changes in household composition—are common among low-income families in the United States. Eviction is a particularly widespread phenomenon: an estimated 2.7 million U.S. households (5-6% of renter households) have an eviction filing per year (Gromis et al., 2022), and these households include an estimated 2.9 million children (Graetz et al., 2023). Moreover, during the 2020-21 school year, public schools identified roughly one million students who experienced homelessness, representing 2.2% of all students enrolled in public schools (NCES, 2022).[1] Many U.S. states and cities have adopted policies aimed at stabilizing children's home environments, including eviction prevention and financial assistance programs, motivated in part by the potential damaging effects of eviction on children.[2]

Despite its importance for policy decisions, we know little about how eviction impacts children. Prior research has faced two main challenges in evaluating a causal link between eviction and children's outcomes. First, eviction records do not contain information about children present in the home, making it difficult to study children affected by eviction or to follow them over time with administrative data. Second, tenants facing eviction often face multiple, correlated sources of economic distress, such as unemployment or worsening health (Collinson et al., 2024). Therefore, comparisons between evicted and nonevicted children may be affected by omitted variables, complicating researchers' ability to study the causal impact of eviction.

In this paper, we provide the first comprehensive analysis of the causal link between eviction and children's home environment, school attachment and engagement, educational achievement, and high school completion. We use linked administrative data from two major U.S. cities—Chicago, IL, and New York, NY—and a research design that leverages the random assignment of cases to judges who vary systematically in their propensity to evict. For cases that are marginal to judge assignment, this design allows us to estimate the causal impact of an eviction order on the outcomes of children.

Our linked data is constructed using the near-universe of eviction court records filed in Cook County, IL, between 2000 and 2016 and in New York, NY, between 2007 and 2017.[3] We link these records to public school records in Chicago and New York, the homelessness services system in each city, and restricted Census data (Chicago only). The K-12 education records allow us to examine impacts on absenteeism, grade retention, school changes, test scores, credit

[1] This homelessness metric is defined in accordance with the McKinney-Vento Act as "lack[ing] a fixed, regular, and adequate nighttime residence" (42 U.S.C. Section 11434(a)(2)).

[2] For example, Seattle's 2021 School-Year Eviction Defense ordinance, which limits eviction of households with school-age children during the school year and states that "the Seattle City Council is committed to protecting children and students from the destructive impacts of eviction" (seattle.gov, 2021).

[3] For brevity, we refer to these locations as Chicago and New York.

completion, and high school completion. We also use these educational records to track changes in residential address, neighborhood quality, and, in Chicago, the school district's flag for the student living in an unstable housing situation. The linkages to administrative data on the homeless system allow us to measure impacts on child homelessness. Finally, our linkages to the 2000 and 2010 Decennial Censuses allow us to study the impact of eviction on the child's household size, their likelihood of doubling up (which we define as living with additional adults excluding cohabiting partners), living in a multigenerational household, living with their mother or father, and their neighborhood's poverty rate. Additionally, the linked Census data enables us to study outcomes at longer horizons and to follow children who move out of Cook County.

First, we provide new descriptive evidence on children's exposure to eviction and the composition of their households. The linked Cook County Census records reveal that just over half—53-56%—of eviction cases involve households with children. Approximately 45% of children facing eviction live in a single-mother headed household, 25% live in a two-parent, married household, and 20% live in a grandparent-headed household. Moreover, we find that roughly 1 in 6 children facing eviction live in a doubled-up household prior to the eviction.

Next, we use our linked records to compare evicted and nonevicted children and characterize trends in their housing situation, living arrangements, and schooling outcomes in the years leading up to and following an eviction filing. This within-court comparison shows that prior to the court case children in households receiving an eviction order are more disadvantaged and more likely to have recently moved than children in households that avoid eviction, with higher move rates, lower test scores, and higher absences. Their trends in schooling measures, living arrangements, and housing instability are broadly parallel prior to the eviction filing, but after filing we find striking increases in homelessness, doubling up, and rates of moving for the evicted group relative to the nonevicted group. In New York, we also find a widening gap between evicted and nonevicted children in absences and rates of switching schools after filing. The trends for most academic outcomes, including grade retention and test scores, however, do not substantially diverge after the eviction case relative to before.

Our instrumental variables (IV) analysis considers four sets of outcomes: children's home environment, school attachment and engagement, educational achievement, and high school completion. First, we study impacts on the home environment using the linked education records. We find that eviction increases moves in the case year by 13 percentage points (p–value <0.01), an approximate doubling relative to the nonevicted mean. These effects persist through the next two academic years, despite relatively high move-out rates among children in families who are not evicted. The point estimates for the effects of eviction on the likelihood of changing addresses are similar in magnitude to previous work on adults in eviction court (Collinson et al., 2024), which relied on alternative data sources for address histories. While eviction increases residential mobility, it appears to have little short- or medium-run effect on neighborhood poverty levels for families with children.

Eviction also increases children's likelihood of experiencing homelessness. The IV estimates for homelessness, as measured in HMIS data, imply that eviction increases homelessness in the year after filing by 7.0 percentage points in Cook County and 3.1 percentage points in New York (p–values 0.04 and 0.13, respectively). These effects grow in the following year, when evicted children are 7.7 percentage points more likely to be homeless than nonevicted children in Cook County and 5.1 percentage points more likely to experience homelessness than nonevicted children in New York. Both estimates are statistically significant at the 5 percent level and represent large increases in homelessness relative to the nonevicted mean of 0.9 percent in Chicago and 2.3 percent in New York. In Chicago, we also examine the impact of eviction on the child being flagged as living in an unstable housing situation. These point estimates also suggest that eviction increases housing instability, although they are imprecisely estimated.

In the Census sample, we explore impacts of eviction on children's living arrangements and family structure, which are not captured in school administrative data, and are measured in our Census sample at an average of 5 years after the filing. We find that eviction increases the likelihood that children live in a doubled-up household by 16.9 percentage points, relative to the nonevicted mean of 21.9 percent. The increase in doubling up is also reflected in a 13.2 percentage points increase in the child's likelihood of living in a multigenerational household, relative to the nonevicted mean of 9.7 percent. Both increases are significant at the 5 percent level. Despite these changes in living arrangements, eviction does not appear to disrupt family structure: we find no evidence that it affects whether children live with their mother or father.

Turning to measures of school attachment and engagement, we examine eviction's impact on absenteeism, grade retention, and school changes. Using our IV approach, we find that eviction increases absenteeism. These effects appear in the first full school year following the case filing and are significant at the 5 percent level. Eviction increases the percentage of absent days by 2.4 points (an 18 percent increase relative to the nonevicted mean, representing 4.3 school days). We also find an uptick in chronic absenteeism—students missing more than 10 percent of school days—as a result of eviction. These effects persist for two full school years after case filing. We also find evidence that eviction increases school changes, although these effects are primarily driven by Chicago. Finally, we explore impacts on grade retention. By the second full year, eviction increases the likelihood of being retained at least once since filing by 5.3 percentage points (p-value 0.06).

Next, we study educational achievement, focusing first on standardized test scores for elementary and middle school students. We find little evidence that eviction negatively impacts these students' test scores, as measured by grades 3-8 statewide math and reading exams. The IV point estimates generally rule out large negative effects, but we often cannot rule out moderate negative or positive effects. We also find evidence that eviction increases the likelihood of missing a standardized test, which is consistent with the effects on absences.

For high school students, we examine whether eviction impacts credit completion in both districts, and grade point average (GPA) in Chicago. Eviction reduces credits earned (as a share of the modal number attempted) by 14.4 percentage points in first full school year following the case, and this reduction persists in the second year (p–values 0.02 and 0.08). For Chicago students, the reductions in credits are accompanied by decreases in GPA, though the decreases in GPA are not statistically significant. The timing of these effects coincides with increases in chronic absenteeism that we find for these older children.

Lastly, we examine effects on high school completion. Focusing on older children as they are less likely to have a missing graduation status, we estimate that eviction reduces high school graduation by 12.5 percentage points (p–value 0.01), relative to the nonevicted mean of 67.6%. The point estimates are comparable across sites, though only the New York results are statistically significant on their own. As additional validation, we predict how our estimated impacts on intermediate measures—including absences, test scores, school changes, and credits earned—would translate into changes in graduation rates, using estimates from a regression of graduation on intermediate outcomes, and we find similar magnitudes to our direct IV estimates of the effects of eviction on graduation.

A challenge in analyzing longer-run outcomes is that migration out of the district may lead to differential attrition. While we do not find differential mobility out of the district in the first two years after the case, we find some evidence that eviction increases the likelihood of a missing graduation status in our data. To account for this, we develop a bounding procedure to characterize the sensitivity of our LATE estimates to differences in the graduation rates between students who migrate out of the school district in response to eviction and those who always remain in the district. Applying this procedure, we find that eviction causes a reduction in graduation rates even as we allow for large gaps in the graduation rates between these two groups of children. In particular, graduation rates must differ by more than 30 percentage points between these two groups to overturn our finding that eviction causes a statistically significant reduction in graduation rates.

The effects of eviction on schooling outcomes differ by gender, with more consistently disruptive effects for boys. Our estimates suggest that boys experience larger increases in absences, chronic absenteeism, and reductions in credits earned as a result of eviction than do girls. The negative effects of eviction on high school completion also appear to be driven primarily by boys. These results are consistent with boys being more susceptible to family disadvantage and negative shocks (Heckman, 2006; Bertrand and Pan, 2013; Autor et al., 2019). In our linked Census samples, we find that girls are more likely than boys to move into a multigenerational household and into lower-poverty neighborhoods after eviction. One interpretation is that girls have greater access to family support networks compared to boys, and this better stabilizes their school engagement after negative events.

Our paper contributes to a growing literature on eviction. Prior work in Chicago and New

York has shown that eviction negatively impacts adult tenants, increasing their likelihood of becoming homeless and negatively impacting earnings and credit scores (Collinson et al., 2024). Researchers have documented that children in the U.S. are commonly exposed to eviction (Lundberg and Donnelly, 2019; Graetz et al., 2023) and that neighborhood-level eviction rates are correlated with the proportion of rental households with children (Desmond et al., 2013).[4] Prior research, using longitudinal survey data, has also shown that a parent's eviction is negatively associated with children's educational and health outcomes (Pribesh and Downey, 1999; Ziol-Guest and McKenna, 2014; Leifheit et al., 2020; Schwartz et al., 2022). Our paper contributes by using court records linked at the individual level to administrative schooling records and Census records, which allow us to follow children beginning several years prior to the court case through several years after the case. We provide new descriptive evidence characterizing children's home and school environments in the years surrounding the eviction case, and we provide the first causal evidence on the effects of eviction on key home and educational outcomes for children in two major urban areas, advancing our understanding of the social costs of eviction.

Our work also contributes to the literature on housing and child outcomes more generally. Several studies have examined the effects of housing voucher receipt or public housing admission on child outcomes (Currie and Yelowitz, 2000; Jacob et al., 2015; Schwartz et al., 2020; Pollakowski et al., 2022). Relative to these interventions, which provide ongoing rent subsidies and may involve voluntary moves, we examine the impact of eviction, a court process that requires households to relocate immediately without assistance. Related research examines the consequences of voluntary moves out of public housing through the Moving to Opportunity Experiment (MTO) (Sanbonmatsu et al., 2011; Ludwig et al., 2013; Chetty et al., 2016) and involuntarily displacement through public housing demolitions (Jacob, 2004; Chyn, 2018). Moves through MTO produced large changes in the neighborhood environment, while eviction in our setting causes households to relocate to similarly poor neighborhoods. Still, the results of MTO are consistent with our findings: older children who moved out of public housing with a voucher were considerably less likely to report having a high school diploma (Sanbonmatsu et al., 2011) and follow-up work on MTO finds lower rates of college-going and lower earnings for older children (Chetty et al., 2016). Jacob (2004) finds that older children displaced by public housing demolition had higher dropout rates, and Chyn (2018) finds that children displaced at younger ages were less likely to drop out from high school. We contribute to this literature by studying the disruptive effects of eviction, which affects millions of households annually in the U.S.

Lastly, we contribute to a larger literature on the short- and long-run consequences of

[4] Other disruptions to the child's home environment, including exposure to foster care and the juvenile court system have been studied using linked administrative data and causal research designs (Doyle, 2007; Aizer and Doyle, 2015; Gross and Baron, 2022).

economic hardship and traumatic events for child outcomes. These include studies of the effects of placement in foster care (Doyle, 2007, 2008; Bald et al., 2022), juvenile incarceration (Aizer and Doyle, 2015), safety net bans (Mueller-Smith et al., 2023), and proximity to shootings (Ang, 2020; Cabral et al., 2020), as well as research examining the effects of spillovers from household-level shocks, such as parental job loss (Oreopoulos et al., 2008; Rege et al., 2011; Stevens and Schaller, 2011; Hilger, 2016); parental income volatility (Hardy, 2014; Hardy and Marcotte, 2018), and parental incarceration (Bhuller et al. 2018; Dobbie et al. 2018; Norris et al. 2021; Arteaga 2023). We contribute to this work by focusing on court-ordered eviction, a common but understudied disruption experienced by low-income households. Our estimated effects of eviction on absences and chronic absenteeism are comparable to the effects found in studies of exposure to school shootings (Cabral et al., 2020) or officer-involved killings (Ang, 2020), and the effects of home removal by child protective services (Bald et al., 2022). Our finding that eviction reduces children's likelihood of graduating high school echoes findings of lower higher school completion as a result of juvenile detention (Aizer and Doyle, 2015) or criminal history-based bans from the U.S. safety net (Mueller-Smith et al., 2023). To ensure that our findings on longer-term graduation are robust to differential missingness in the data, we develop and apply a novel bounding procedure and show that our qualitative conclusions are unaffected.

The remainder of the paper is organized as follows. Section 2 characterizes Cook County and New York City's eviction court process and the policy environment surrounding student homelessness. Section 3 describes our sample and linked administrative data. Section 4 provides new descriptive evidence on student outcomes in the years before and after eviction. Section 5 develops our instrumental variables research design, and Section 6 presents the results of this analysis. Section 7 concludes.

2 Background and Institutional Details

This section describes the legal process of eviction in Chicago and New York and provides information on laws and school programs aimed at supporting children experiencing housing instability. Additional information on the eviction court process is provided in Collinson et al. (2024).

The eviction process begins with the landlord serving the tenant a written notice, which indicates the reason for terminating the lease and the number of days before the landlord may proceed with filing a court case. Non-payment of rent is the most commonly stated reason for eviction. The court filing is a matter of public record and is the first entry we observe in our court data. When the landlord proceeds to file an eviction case, they must file the court case in the district determined by the location of the rental unit. Cases are randomly assigned to courtrooms within a district, and judge assignments to courtrooms are set in advance, hence random assignment to a courtroom is effectively random assignment to a judge.

The eviction case proceeds with one or more court hearings and concludes with the judge's decision either to issue an eviction order—which requires the tenant to vacate the property—or to dismiss the case. Our definition of eviction, used throughout the paper, is a case ending in an eviction order by a judge. Thus, we study the impact of an eviction order relative to the alternative of the case being dismissed. A case is dismissed if the tenant wins on the merits, the landlord and tenant reach an agreement, or the landlord decides not to pursue the case further. Once the landlord obtains the eviction order, they may file the judgment with the Sheriff or Marshal, who executes the lockout, returning possession of the property to the landlord. Collinson et al. (2024) show that while residential mobility among tenants in eviction court is high both before and after an eviction filing, an eviction increases the tenant's likelihood of moving residences and the tenant's likelihood of experiencing homelessness.

In response to growing concerns about families experiencing homelessness, the U.S. Congress created the McKinney-Vento Education for Homeless Children and Youth program in 1987 (McKinney-Vento, henceforth), which was reauthorized in 2002 by the No Child Left Behind Act. Under McKinney-Vento, the federal government funds local education agencies to *"identify homeless children, remove barriers to enrollment in school, and provide services to increase opportunities for academic success"* (Cunningham et al., 2010). These services include allowing the child to remain in the school they attended when they were last permanently housed and access to transportation to continue attending this school. McKinney-Vento also requires school districts to report data on homelessness and unstable housing situations among enrolled public school students.

Despite the resources for homeless students created by McKinney-Vento, eviction may impact a child's academic progress. The relocation itself may disrupt the child's home and neighborhood environment, since the eviction process is swift and families must move with limited time and resources, and with the penalty of having a public eviction record.[5] In Collinson et al. (2024), we find that eviction reduces a tenant's earnings and worsens their financial health, which may in turn impact children (Gennetian et al., 2018). Eviction may increase the likelihood of a child moving to a higher-poverty neighborhood, and the housing disruption may impact the child's ability to attend school and their ability to focus on their studies due to distraction or stress. Physical relocation may also increase the likelihood of switching to a new school or dropping out of school entirely.

[5] A 2017 survey conducted by TransUnion found that about 85% of landlords run eviction background checks on all applicants, and landlords who screen tenants say eviction history is the second-most important factor in their leasing decision, after income and employment history (TransUnion SmartMove, 2017).

3 Data Collection and Linkage

Our analysis uses eviction court records in Cook County, IL, and New York, NY, which we link to public school records to measure outcomes related to the home environment, school attachment and engagement, educational achievement, and high school graduation for public school students in the household. We additionally link the Cook County court data to Decennial Census records to study the impact of eviction on children's living arrangements, household structure, and neighborhood environment. This section describes our data sources, sample construction, data linkage, and main outcomes. We provide additional details in Appendix B.

3.1 Court records

Our court records include the near-universe of eviction court cases in Cook County from 2000-2019 and in New York from 2007-2017. We describe these data in detail in Collinson et al. (2024) and summarize them here. The case-level data include the names of tenants on the lease and the address of the rental unit, and we use these identifiers to link tenants to administrative records. We observe other elements of the court cases, including the case type, filing date, courtroom and date assignment, the name of the landlord, the amount of damages sought by the landlord (ad damnum amount), and whether an eviction order was issued. In Cook County, we observe the name of the judge assigned to the case, while in New York we observe the courtroom. We define an eviction as a case ending in an eviction order.

We impose similar restrictions on the court samples as in Collinson et al. (2024). We drop eviction cases associated with businesses, cases associated with co-ops or condominiums, cases with a missing defendant name, address, or district, and cases involving more than $100,000 in claimed damages. See Appendix B.1 for additional details.

An important challenge in studying the effects of eviction on children is that household members who are not named on the lease typically do not appear in the eviction court records. To construct our analysis samples of children in these households, we link the court records at the tenant level to other administrative records, including public school data and Decennial Census records, which we describe in the next subsections. Our unit of analysis is the child-case, so children with multiple cases enter the analysis sample once for each case.

3.2 Education records

We use administrative schooling records in Chicago Public Schools from years 2000-2019 and New York City Department of Education from 2005-2017, for grades K-12. The Chicago dataset provides annual observations for all variables, while the New York City data provides some variables at a monthly frequency. We study trends in outcomes around the filing using the monthly data (when available) in New York and the annual data in Chicago. Our analysis of the causal effects of eviction uses annual data from each site. Across both sites and in each school year, we observe the student's enrollment status and, conditional on enrollment, we observe student and school outcomes. We now briefly discuss our approach; see Appendix B.2 for additional details.

Chicago. The Chicago Public Schools data include annual information on attendance, school and grade of enrollment, grade progression and retention, and final enrollment status. The data additionally includes information on race, gender, age, whether the student has an individualized education plan (IEP), and whether the student qualifies for free or reduced price lunch. Starting in the fall of 2003, the data also includes information on residential addresses. For grades 3-8, the data include reading and math scores on statewide standardized tests, and, for grades 9-12, the data include GPA (starting in the fall of 2008) and credits earned (starting in the fall of 2014). We also observe a flag for the student living in an unstable housing situation, based on the McKinney-Vento data, which most commonly reflects doubling up.[6] Hereafter, we refer to this indicator as "the McKinney-Vento flag."

Court and Chicago Public Schools records were linked by staff at Chapin Hall, a research institute at the University of Chicago. The linkage was done using tenants' names and addresses from court records and the students' addresses and names of their legal guardians from Chicago Public Schools records. The linkage only used Chicago Public Schools records occurring prior to the filing date, and resulted in 77,256 unique student-case matches.[7] Because address data are available only starting in the fall of 2003, we restrict to eviction cases filed in 2003-2019.

New York City. The New York City Department of Education (NYCDOE) data include annual information on attendance, school and grade of enrollment, grade progression and retention, and final enrollment status. As in Chicago, the data additionally includes information on race, gender, age, whether the student has an individualized education plan (IEP), and whether the student qualifies for free or reduced price lunch. Starting in the fall of 2007, the data also includes information on residential addresses. For grades 3-8, the data include reading and math scores on statewide standardized tests (starting in the fall of 2006), and, for grades 9-12, the data include credits earned (starting in the fall of 2008). We also observe a flag for homelessness, based on HMIS data, which is an indicator for the student being listed on a family application for shelter bed as recorded by the Department of Homeless Services.

[6] Doubled-up students account for around 90% of students included in McKinney-Vento reporting (CCH, 2024). The definition of doubled-up that is used to determine a student's McKinney-Vento status is "children and youths who are sharing the housing of other persons due to loss of housing, economic hardship, or a similar reason" (42 U.S.C. Section 11434(b)(2)). This definition differs from the measure we construct based on Census data when we study children's living arrangements (see Section 3.3). The Census measure is based on household composition and does not use information about the reason for the living situation.

[7] Appendix Tables B.1 and B.2 show that the correlation between a case being linked and the stringency of the randomly assigned judge is statistically insignificant in both Chicago and New York.

Court and student records from NYCDOE were linked by staff at the Center for Innovation through Data Intelligence (CIDI) working with the research team. Parents or guardians were linked to court records using names and addresses in the student records. This linkage was restricted to students appearing in the school records at the eviction filing address before the filing date, and resulted in 278,879 unique student-case matches.

Time indexing. The academic year runs from early September to late June in both Chicago and New York, so we use September 1 to June 30 to define the academic term, and July 1 to August 31 as the summer. We index the school year based on the year of the spring term, with the previous summer also belonging to that school year. For example, the 2009 school year begins in the summer of 2008 and ends in the spring term of 2009.

Since most outcomes in the schooling data are defined annually over the entire academic year, while eviction filings occur throughout the calendar year, our analysis requires mapping school outcomes into years relative to eviction filing. We index results to the school year and take relative year 0 (RY0) as the school year in which the case is filed. For homelessness outcomes derived from HMIS records, we know the specific dates the outcome occurs, so we define RY0 as the first 12 months after the case filing. Lagged relative years (i.e., RY-1, RY-2, etc.) and lead relative years (i.e., RY1, RY2, etc.) are defined relative to RY0.

We use this time indexing to study trends around an eviction filing in Section 4. In the IV and OLS analyses of Section 6, we provide results for three outcome periods: the case school year, the first post-filing school year, and the second post-filing school year. These outcome periods correspond to RY0, RY1, and RY2 for cases occurring during the school year. For cases occurring during the summer, because we observe a complete school year of outcomes after filing, we define both the case school year and the first post-filing year as R0 and the second post-filing school year as R1. Because homelessness outcomes are defined in calendar time relative to the case filing date, the outcome periods always correspond to RY0, RY1, and RY2.

Construction of outcome variables. Using the schooling records, we study outcomes in four different domains: the home environment, school attachment and engagement, educational achievement, and high school completion.

To characterize the home environment, we study an indicator for the child not having the same residential address in the outcome period as their pre-case school year (RY-1), the number of times the child changed addresses since their pre-case school year (measured annually), and the Census tract-level poverty rate of the child's address in a school year, which we refer to as the neighborhood poverty rate. When plotting trends over time, we show annual (or monthly) move rates, and in the IV/OLS analysis, we study moves relative to the pre-case school year, and cumulative moves since the pre-case school year. In Chicago, we also study whether students are in an unstable housing situation using the McKinney-Vento flag. In New York, the education sample is linked to an indicator for the child being listed on a family application for a shelter bed as recorded in HMIS data. In Chicago, using the linked Census sample described below, the homelessness outcome is any child interaction with the HMIS system.

We study several key outcomes related to school attachment and engagement, including the percent of days that are absent in the school year, and an indicator for chronic absenteeism, which, following the U.S. Department of Education's practice, we define as the student missing more than 10 percent of days in which they are enrolled in school (Chicago Public Schools, 2022; New York State Education Department, 2025; U.S. Department of Education, 2025). To measure school switching, we construct an indicator for the child not being at the same school as they were in the pre-case school year. For this outcome, we drop student-case years where a school change would be mechanical; i.e., we drop all observations for which the pre-case school does not offer the student's grade in the outcome period. To measure grade retention, we define retention in a given school year as being in the same grade in the *following* year as the current year; hence, retention in RY1 means the student will repeat the same grade in RY2. In the analysis of trends, we present annual rates of grade retention, and in the IV/OLS analyses, we study an indicator for the student being retained in at least one year between the pre-case year (RY-1) and the outcome period.[8] We also construct an indicator for the student transferring out of the district to evaluate attrition from the sample.

The educational achievement outcomes include test scores from statewide reading and math tests, measured for grades 3-8 and standardized in the grade and school year. We also have academic outcomes for high school students, including credits earned as a share of the modal number of credits attempted ("credits," henceforth), and high school grade point average (GPA), which is available only for Chicago.

Finally, we use measures of high school diploma receipt to study impacts on high school completion. In particular, we use an indicator for whether a student has a final status of graduated (for those aged 18 and older in our panels) as our primary measure of high school completion. In appendix results, we also examine impacts on on-time graduation, which is an indicator for a student having graduated four years after entering 9th grade. All results using the education records, except for the transfer outcome, are conditional on being in the school system.

3.3 Census records

Tenants in our Cook County court records were linked by the Census Bureau to their unique Protected Identification Key (PIK), which allows us to link tenants at the individual-level to

[8] In Appendix A.3 and Appendix A.6, we also study impacts on school quality. For this, we use a measure of school average achievement across math and reading test scores in the student's school-year. These are based on math and reading scores standardized for each grade-year across the district to have mean 0 and standard deviation 1.

other restricted data sets held in the Census Bureau Research Data Centers (RDCs).[9] The PIK rate of tenants in the Cook County court records is 52 percent. We study which case characteristics are predictive of a match in Appendix D of Collinson et al. (2024). We construct Census-based analysis samples for several purposes: (1) to characterize the number of children facing eviction, (2) to present trends of key outcomes around the eviction filing, and (3) to study the impact of eviction on household and neighborhood outcomes (i.e., the IV and OLS analyses). This section describes the samples used for the IV and OLS analyses. We provide additional details on all Census samples in Appendix B.3.

To build our main causal analysis sample of children using the Census records, we first link tenants who are 19 and older in the case year to their 2000 Decennial Census records. We next collect the PIKs of all children in the household who are age 0-18 as of the case filing, and link these children forward to their 2010 Decennial Census records.[10] We restrict the sample to cases between July 2000 and December 2009, so that the 2000 Decennial precedes the case and the 2010 Decennial follows the case. We additionally restrict the sample to children who are 0-18 as of the 2010 Decennial, so that our analysis sample consists of children who have not aged out of the household. We also drop a small number of observations where children have age discrepancies of greater than 1 year between the age in the 2000 Decennial plus 10 and the 2010 Decennial, and the few cases where the child is named in the court case directly. For analyses of household outcomes (but not neighborhood outcomes), we drop children living in group quarters in the 2010 Decennial, because household relationships are not available for these individuals.[11] The final Census sample is a child-case level dataset with approximately 49,000 observations, where Census observation counts here and throughout the paper are rounded according to Census disclosure rules.[12]

We do not place restrictions on the relationship between the child and the tenant in constructing the child-case sample, because children may live in complex family arrangements. Table B.4 provides summary statistics on the relationship of the child to the tenant in the 2000 Decennial, based on the household interrelationships variable. The majority of children in our sample are the child of the linked tenant (81 percent), which includes biological children, adoptive children, and step children, 7 percent are the grandchild of the tenant, 2 percent are

[9]Due to restrictions in our data sharing agreement with the New York courts system, we are unable to bring the New York courts data into the Census RDC for analysis.

[10]In the baseline 2000 Decennial linkage, we drop tenants who live in group quarters, since household relationships are not available for these individuals. Children who have multiple household members named on a lease only enter the sample once.

[11]The proportion of children living in group quarters is 1.4 percent (shown in Table B.5), and includes those who are incarcerated, living in college dormitories, military barracks, nursing facilities, or emergency shelters.

[12]Table B.4 presents additional information on the construction of the linked Census sample. The table shows that judge stringency is not predictive of the tenant linking to the 2000 Decennial. We link approximately 68 percent of our Decennial 2000 child sample to their 2010 Decennial records. In the same table, we also show that conditional on being in the baseline sample, judge stringency is not a statistically significant predictor of a link to the 2010 Decennial.

the nephew or niece of the tenant, and 8 percent are a younger sibling of the tenant.

The child homelessness (HMIS) sample from Cook County differs from the Census sample previously described because we have the complete history of HMIS records and we can construct a child-case panel. We begin with the PIK'd tenants that are linked separately to the 2010 Decennial and to the 2000 Decennial. We collect the PIKs of all children in these households, avoiding double counting children that are present in both linkages. We restrict the sample of children to those who are the child of the household head, because when we link to the HMIS data we restrict the HMIS data to children of the household head. We restrict to case years between 2010 and 2016 to overlap with the HMIS sample years, and we restrict to children who are 18 or under as of the Decennial year, the HMIS year, and the case year. We use this sample for the trends in HMIS outcomes in Cook County and for the IV/OLS analysis of HMIS outcomes for Cook County. The main outcome using this data is any interaction with the HMIS system, which is a somewhat broader homelessness measure than the shelter application outcome used in New York.[13]

Construction of outcome variables. We use the Census sample to study the impact of eviction on household living arrangements, family structure, and the neighborhood environment. The key outcomes are measured in the 2010 Decennial and include: the total number of people in the household, an indicator for the household being multigenerational (i.e., having three generations in the household), an indicator for the grandparent being the household head, and an indicator for the household being doubled up.

We define doubled-up households in two ways: (1) doubling up (including grandparents) are households with an additional adult (19 and older) who is not the household head or their cohabiting partner; (2) doubling up (excluding grandparents) is defined identically but does not count adults who are the adult child of the household head, and does not count adults who are the adult parent of the household head. We construct these two measures because a child living with their grandparents is common in our data, and because it is unclear whether an increase in the likelihood of living in a multigenerational household is a negative outcome for children.[14]

We study additional family outcomes, including an indicator for the mother of the reference child living in the household, and an indicator for the father of the reference child living in the household. We also construct an indicator for single mother-headed household, in which the household head is the mother of the reference child and has no spouse in the household, and we construct an analogous indicator for single father-headed household. The key neighborhood outcomes are the Census-tract level poverty rate, an indicator for living outside of Cook County,

[13] While we observe shelter entry in the Cook County sample, we use the broader HMIS measure to stay above the Census disclosure requirements for sample sizes.

[14] These two measures are defined as in Pilkauskas et al. (2014), and they are also invariant to whether the parent or grandparent is labeled the household head in the Census.

IL, and an indicator for living outside Illinois.[15] We measure the neighborhood poverty rate using the 2009-2011 ACS.

4 Children in Eviction Court

This section uses our linked samples to provide new descriptive evidence on children in eviction court. We use the Census sample to provide estimates of the proportion of households in eviction court with children in the household, and to characterize these children's households, including their family living arrangements. We then leverage the panel dimension of our education samples to present trends in children's home environment, school attachment and engagement, and educational achievement outcomes over time relative to eviction filing.

4.1 How many children face eviction annually?

Using linked Census data, we first estimate the proportion of households in eviction court with children. For this exercise, we link cases filed in 2000-2004 to the 2000 Decennial and cases filed in 2008-2012 to the 2010 Decennial.[16] We focus on only five years of cases in each linkage so that these cases occur close to the Census observation date. If a tenant has multiple cases in a year, we select one case to not over-count tenants with multiple cases. In cases with multiple tenants listed in the case filing, we select one tenant per case since our goal is a household-level measure, and we do not want to overweight households with multiple tenants.[17]

Using the 2000 Decennial linkage, we find that 60-63 percent of households facing eviction have children age 0-18, and that households with children have on average 2.5 children (Table A.7). Using the 2010 Decennial linkage, we find that 53-56 percent of households facing eviction have children age 0-18, and that households with children have on average 2.3 children.

What do these estimates imply about the total number of children facing eviction per year? Using the estimate of 2.7 million eviction cases nationwide per year (Gromis et al., 2022), and assuming, based on our estimates, a national proportion of households of 50 percent and 2.3 children per household (conservative estimates based on our numbers), we estimate that approximately 3.1 million children face eviction each year. This estimate is within the

[15] These migration indicators are useful for validating the education analysis using Chicago Public Schools and New York public school records, since we do not measure students' educational records if they move out of the school district, but using the Census we can measure the child's location throughout the United States.

[16] This exercise uses a different sample than the IV/OLS Census analysis sample (a child-case dataset), because this exercise is based on a sample of linked *tenants* to the Decennial Censuses, while all other Census analyses are based on the Census sample of children described in Section 3.3.

[17] In cases with multiple tenants, we present estimates using three alternative rules for selecting one tenant per case: (i) randomly choosing the tenant, (ii) choosing the Census household head, (iii) choosing the female first and, if there are multiple female adults, choosing one at random. The results are slightly sensitive to which of these three rules we adopt, because children are more likely to live with a female parent.

range of estimates reported in Graetz et al. (2023), which is based on a linkage of eviction cases nationwide to the American Community Survey.[18] Restricting to one case per household would reduce the estimated number of cases per year by approximately 5-10 percent, bringing down our estimated total number of children facing eviction annually to approximately 2.8 million. We emphasize that these estimates are based on data from Cook County only, and the proportion of households with children or the number of children per household may differ across geography. Nevertheless, these estimates represent a useful starting point given the paucity of linked administrative data in this setting.

4.2 Summary statistics: Census sample

We present summary statistics of our IV/OLS Census sample in Table 1. For this linked sample, the average age of the child is 8.6 years at the time of the case and 14.2 in 2010, and thus these outcomes are measured on average 5 years after the case.

Approximately 77 percent of the children in our Census sample are Black—a similar proportion to the CPS sample (shown in Table 2) —and the modal family living arrangement is a single-mother household. In 2000, 43.6 percent of children in the nonevicted group are living in a single-mother household, compared to 45.4 percent of the evicted group. In the evicted group, 4.9 percent of children are in single-father households, and 20.7 percent are the grandchild of the household head, while these numbers are 4.7 percent and 19.1 percent for the nonevicted group, respectively.[19]

Of children in the evicted group, 36.8 percent live in a doubled-up household in the baseline, using the measure that includes grandparents, compared to 33.7 percent for the nonevicted group. The doubling-up measure excluding grandparents is 19.1 percent for the evicted group, and 16.8 percent for the nonevicted group in the baseline. Children facing eviction, on average, live in high-poverty neighborhoods. The neighborhood poverty rate of children in our Census sample is 26.9 percent at the time of the case for the evicted group and 27.4 percent for the nonevicted group.

4.3 Summary statistics: education samples

Our linked education samples echo the finding that children in eviction court are economically disadvantaged. Table 2 presents summary statistics of our linked education samples in New York and Chicago. We first report average characteristics for children linked to cases that end in an eviction (columns 1 and 5) and for children linked to cases that do not end in an eviction

[18]Both this exercise and the exercise in Graetz et al. (2023) assume that tenants assigned PIKs are equally likely to have children as tenants without PIKs.

[19]Of children in the evicted group, 0.8 percent are foster children of the household head in 2000, compared to 0.7 percent for the nonevicted group. The 2010 Decennial does not record foster child as a separate response category, so we are unable to study foster care as an outcome.

(columns 2 and 6). We then report average characteristics of students enrolled in public school in Chicago and New York, weighted by grade-year-school (columns 3 and 7) and grade-year (columns 4 and 8) to match the eviction court sample.[20]

The baseline differences of children in court are not notably different by case outcome, although cases ending in eviction have slightly higher absenteeism, slightly lower test scores, and higher rates of address changes in the year prior to the case. In contrast, there are large differences between the students who are matched to eviction court cases and the broader student population. For example, children facing eviction filings are 15-20 percentage points more likely to be chronically absent (missing more than 10% of school days) than the grade-year average in the year prior to their cases. They also have reading and math test scores at baseline that are approximately 0.3 to 0.4 s.d. below the grade-year average. Children facing eviction also live in census tracts with higher poverty rates and attend schools with lower average test scores compared to students in the same grades and years.

Students linked to eviction court cases also differ from students in the same school-grade-year, with higher rates of chronic absenteeism and lower test scores in the year prior to the case. Students facing eviction court are 7 to 14 percentage points more likely to be chronically absent in the pre-case school year compared to peers from the same schools. They also have test scores that are around 0.08 to 0.16 s.d. lower than these peers.

The demographic profile of children in our Chicago and New York education samples are generally similar with a couple of notable differences. First, the proportion of children who are Black is much lower, and the proportion of children who are Hispanic is much higher, in New York compared to Chicago. Second, in New York children facing eviction have higher levels of retention compared to Chicago. Retention rates are 11.7-12.2 percent per year in New York compared to 5.5-5.9 percent per year in Chicago.

4.4 Trends around an eviction filing

We use the linked data to study trends in children's home environment and schooling outcomes relative to eviction filing, separately by whether the child's household is evicted or not. All subsequent analyses are restricted to children whose households are in eviction court.

For the panel data linked to schooling outcomes we estimate the regression:

$$Y_{i,r} = \alpha + \sum_{r=-3;r\neq-1}^{3} \beta_r + \sum_{r=-3}^{3} \delta_r \times E_i + \gamma_{i,t} + \psi_{i,r} + \text{age}_{i,r} + \varepsilon_{i,r}, \tag{4.1}$$

[20]To define pre-case year variables for students who are not in our court samples, we assign these students placebo filing dates that are randomly drawn from students in our court samples with the same year of birth. This ensures that the non-court sample of students have the same distribution of ages in their placebo court filing dates. We then report statistics for the full Chicago and New York samples of students (with placebo filing dates) weighted to match the court sample's distribution of grade-year-school (columns 3 and 7) and grade-year (columns 4 and 8) for each measure.

where i indexes the individual student, r indexes relative year to filing (as defined in Section 3), and t is the calendar in which the case was filed. E_i is an indicator for the case ending in an eviction order, β_r are coefficients on indicators for time relative to the case filing (we omit the time period prior to the eviction year), and δ_r are coefficients on indicators for relative time interacted with the eviction order. To control for time and case location, we include court district interacted with case calendar year fixed effects ($\gamma_{i,t}$) and school year at r fixed effects ($\psi_{i,r}$). To control for age trends, we include age at r fixed effects ($\text{age}_{i,r}$). For New York City outcomes that we observe with monthly frequency, we instead estimate a regression analogous to equation (4.1) where r indexes relative month to filing and t is the calendar month in which the case was filed.

To study household structure and living arrangements, we use the Chicago sample linked to Census records. Although we do not have a panel of outcomes, we can use variation in the staggered timing of the case filing date relative to the 2010 Decennial Census to estimate a regression like (4.1) that omits all controls to avoid multicollinearity.[21]

Figures 1-3 display regression estimates of β_r and $\beta_r + \delta_r$, with the nonevicted group mean in the omitted period added to both sets of coefficients. Adding the mean allows us to interpret the plotted values as relative time- and group-specific means that have been re-weighted to match the time and case location characteristics of the nonevicted group in the omitted period (see Appendix A.3 for the derivation); adding the mean also makes it easier to interpret the magnitudes of the trends and differences between the evicted and convicted groups.

Home environment. Figure 1 presents measures of housing instability, including moves, homelessness, and doubling up. In each case, we find a significant uptick in housing instability in the years immediately after filing for the evicted group with little change among the nonevicted. Panel A shows, for Chicago, the likelihood of having a residential address different from the prior year. While both evicted and nonevicted children have high annual move rates, there is a 7 percentage point gap between evicted and nonevicted households in the year prior to filing. This gap widens by a further 5 percentage points by the first year after filing before returning to pre-filing gaps after two years. Panel B shows that monthly move rates for New York increase sharply for the evicted group in the months after the case filing, and only decrease to pre-filing rates after two years. In contrast, the move rates for the nonevicted group decrease

[21] To construct this Census sample, we first link adult tenants to their 2010 Census responses and then create an analysis sample of all children in these households who are 18 and under at the time of the 2010 Census. This analysis sample excludes children in group quarters, because group quarters have one household identifier assigned to all individuals in residence, meaning we cannot identify children from the same household as the linked tenant. If eviction induces tenants to enter group quarters, there will be a compositional change following eviction in which more disadvantaged children exit the sample, likely attenuating the difference between evicted and nonevicted after filing. Panel F of Appendix Figure A.4 shows the trends for residing in group quarters for the children who are and are not evicted. We find that there is a small increase in both groups living in group quarters after the eviction case, and that the increase is approximately 1.5 percentage points larger for those whose cases end in eviction orders, consistent with our HMIS analysis of homelessness, described below.

after filing before increasing to pre-filing rates after two years.[22] These findings echo the findings in Collinson et al. (2024) of high move rates for both evicted and nonevicted households after the eviction filing.

Panels C and D of Figure 1 plot child interactions with the homelessness system. The Chicago analysis uses the Census sample and the outcome is any interaction with the HMIS system, while in New York the outcome is applications for homeless shelters, which we observe monthly rather than annually. In the pre-filing periods, homeless rates are low in both cities and similar for evicted and nonevicted (though slightly elevated for those who are evicted). In New York, the evicted group experiences a spike in homelessness from a baseline near zero to 1.77 percent per month 4 months after the case is filed, before declining to approximately 0.75 percent in month 12. In Chicago, interactions with the HMIS system similarly increase for the evicted group from approximately 0.55 percent annually in the year prior to the case to 1.45 percent in the year after the eviction case. In both cities, homelessness also increases for the nonevicted group, but the increases are much smaller.

Figure 1, Panels E and F, use the Chicago Census sample to show the share of children living in doubled-up households, with the first panel depicting the outcome including grandparents and the second panel showing the outcome excluding grandparents. The share of children living in doubled-up households declines in the years leading up to eviction filing, for both groups. In years 1-2 after filing, however, evicted children are more likely to move into doubled-up households. Overall, the gap between evicted and nonevicted in doubling up widens by 4-6 percentage points in years 1-2 relative to the year prior to filing.[23]

Overall, these results show that while evicted children have slightly higher pre-filing rates of moving, homelessness, and doubling-up relative to nonevicted children, after filing, evicted children experience a notable increase in all three of these measures of housing instability. Perhaps surprisingly, eviction does not lead to pronounced differences in neighborhood poverty rates, shown in Panel B of Appendix Figure A.4. In addition, eviction does not disrupt household structure. For both evicted and nonevicted children, the share living with their mother is high and stable over time at 85 percent and the share living with their father is low starting around 40 percent two to three years before the case and declines slightly over time (see Appendix Figure A.4, Panels C and D).[24]

[22]Appendix Figure A.5 presents annual trends for New York outcomes. These annual trends are broadly similar to the Chicago annual trends.

[23]Consistent with the above results, Appendix Figure A.2 shows that in the Chicago education sample, the McKinney-Vento flag as well as separate McKinney-Vento subcategories for living at a homeless shelter and doubling up all increase for the evicted group from the year before the case to the year after the case, while the nonevicted group experiences a smaller increase.

[24]Appendix Figure A.4 provides two additional robustness results. First, Panel A shows that children in evicted households experience an increase in the total number of people in the household relative to the nonevicted, which is consistent with the trends for doubling up. Second, Panel E shows no evidence of differential migration out of Cook County. About 15-20 percent of the evicted and nonevicted children live outside Cook County five years after filing.

School attachment and engagement. Figure 2 shows trends for absences, school-switching, and retention for the education sample. Panel A shows, for Chicago, that both evicted and nonevicted children have rising rates of absences: the evicted group misses just under 11 percent of school days 3 years prior to the case, which rises to about 12.5 percent per year in the case year; the nonevicted group misses about 9.7 percent of school days 3 years prior to the case, which rises to about 11 percent. Overall, the gap between evicted and nonevicted is just over one percentage point and widens only modestly. The trends in monthly absence rates in New York in Panel B show a more striking change after filing. While absenteeism rates for the nonevicted group remain stable over the entire horizon, the rates for the evicted rise in the 30 months preceding the case, increase by a little less than 1 percentage point in the 8 months after filing, and subsequently decrease.

Panel C shows the annual probability of switching schools in Chicago. The evicted group is more likely to switch schools even three years before the case, and this gap grows from a 3 percentage point difference in the year prior to eviction to a 4 percentage point difference in the year of the case and in the year after the case. The monthly probability of switching schools in New York shows a similar divergence after filing, with evicted children exhibiting similar rates of school-switching compared to nonevicted children in the months prior to filing; the difference grows to a peak of approximately 1 percentage point 8 months after filing, and remains elevated for another 10 months.[25] Lastly, Panels E and F show annual retention rates. The rates are slightly higher for the evicted group throughout the period, and we find a widening of the gap by 1 percentage point in the year of the case for Chicago.

These plots highlight that evicted children have higher rates of absences and school-switching than nonevicted children in the years preceding filing. At the same time, evicted children also experience greater increases in absences and school-switching in the immediate aftermath of the case compared to nonevicted children.

Educational achievement. Turning to achievement, Figure 3 depicts trends for evicted and nonevicted children. Panels A and B show results for mandatory reading tests administered yearly from 3rd to 8th grade. All test scores have been standardized to have a mean of 0 and a standard deviation (s.d.) of 1 for all students in the district in each grade year. While reading scores are approximately 0.05 s.d. lower for evicted children compared to nonevicted children in the years before filing, this difference remains relatively stable in the post-period.

Panels C and D show similar results for math scores. As with reading scores, evicted students score about 0.05 s.d. lower on math scores in both cities the years before the test. In New York, these gaps are relatively constant, while the gaps increase by about 0.04 s.d. in the year of the case in Chicago.

[25] Appendix Figure A.3 shows measures of school-level average test scores. The gap in school-level test scores between evicted and nonevicted children begins to widen one or two years before the case and increases by less than 0.01 s.d. in the first three years following the case.

Using data for high school students, we additionally show in Panels E and F trends in credits earned in both Chicago and New York. During the pre-period, the gap in credits between evicted and nonevicted children is negligible. In the period after filing, the gap in credits earned between the evicted and nonevicted groups widens to 0.02 to 0.03 by 3 years after filing. Finally, Panel G reports GPA for high schoolers using data from Chicago. Evicted students have lower GPAs than nonevicted students by about 0.03 in the year before filing. This gap widens in the year of case filing to approximately 0.05, and in the subsequent years to approximately 0.08 points.

Taken together, we find that academic achievement is relatively stable around the eviction filing in both locations. Students who will be evicted have somewhat lower performance in the years before filing and have similar gaps post-filing. For high school credits and GPA, we find a slight widening between evicted and nonevicted groups after filing.

5 Empirical Framework

This section describes our instrumental variables approach to estimating the causal effect of eviction on children. We discuss the assumptions underlying our research design and provide evidence supporting these assumptions.

5.1 Instrumental variables

The challenge for interpreting OLS in this setting is that eviction may be correlated with children's unobservables or the timing of unobserved shocks that affect children's outcomes. Our analysis in Section 4 shows that children who are evicted are more disadvantaged than those who are not evicted, and our aim here is to develop an instrument that is independent of these sources of disadvantage.

We follow a common approach used in court settings and leverage the random assignment of cases to judges for identification. Our instrument $Z_{j(i)}$ is the leave-one-out mean stringency of judge j assigned to individual i's case. We estimate the following two-stage least squares model:

$$E_i = \gamma Z_{j(i)} + X_i'\alpha + \epsilon_i \tag{5.1}$$

$$Y_i = \beta E_i + X_i'\delta + \nu_i \ , \tag{5.2}$$

where the regression is run separately for each outcome and time period.[26] In equation 5.1, E_i

[26]To leverage the largest possible samples in our analysis, the sample for each outcome and time period includes all children with an observed outcome in that time period. For example, when studying test scores for students in grades 3-8, the regression sample for the case school year includes children who are in grades 3-8 at the time of filing, the regression sample for school year 1 includes children who are in grades 2-7 at the time of filing, etc.

is an indicator for whether the child-case i ends in an eviction, Y_i is the observed outcome, and X_i is a set of controls for child and case characteristics. For this analysis, we impose the same restrictions as in Collinson et al. (2024) and remove cases that are not randomly assigned or that are assigned to judges/courtrooms that hear substantially fewer cases than is typical in the setting.[27] If the IV assumptions are satisfied and equations 5.1–5.2 are correctly specified (see Blandhol et al., 2022), the TSLS estimand for β captures a positively-weighted average effect of eviction among compliers, where compliers are defined as children whose case outcome would have changed had their case been assigned to another judge.

In the analysis of education records, the controls include court-year and child age-at-filing fixed effects, court variables, demographics, and outcome-specific lags.[28] The lags are constructed by averaging over relative years -3 to -1. We impute zeros for missing controls and we additionally control for indicators for each variable being missing. Standard errors in the education records are clustered at the judge-by-year level.

In the analysis of Census records, the controls include indicators for age-at-case, a female indicator, indicators for Black, white, or Hispanic, and family structure indicators in the baseline 2000 Decennial, including indicators for single-mother household, single-father household, two-parent household, grandparent-headed household, and the household being doubled-up. We also include Census-tract-level controls based on the address listed in the case filing, including share in poverty, share white, share Black, share Hispanic, and an indicator for missing Census covariates. Standard errors in the Census analysis are two-way clustered at the judge and household level.

The controls are not necessary for identification in our setting, but we include them to improve precision. We evaluate the robustness of our IV estimates to excluding lagged outcomes and to excluding all controls other than district-year fixed effects in Appendix C and find that our results are quite similar across all three specifications.

[27] Specifically, we remove cases filed during a week in which only a single judge (Chicago) or courtroom (New York) is hearing cases. We also drop the following cases in New York that are not randomly assigned to courtrooms: cases involving public housing units, cases assigned based on zip code through several policy initiatives, cases for family members of active military personnel, and cases involving the District Attorney's office or the New York City Police Department. We also restrict to cases in which the judge presides over 100 cases in the year (Chicago) or in which the courtroom has 500 cases in the year (New York).

[28] Both the Chicago and New York analyses include rent claim amount and indicators for legal representation. The demographics controlled for in Chicago are an indicator for Black, white (non-Hispanic), Hispanic, female, an indicator for free or reduced lunch prior to RY0, and indicators for speech and learning disabilities (IEPs). The demographics controlled for in the New York analysis include the same variables as in Chicago, plus indicators for being born in New York, speaking Spanish, and speaking another language. The New York analysis also uses court-year-quarter instead of court-year fixed effects.

5.2 The judge stringency instrument

We construct judge stringency using the yearly leave-one-out mean eviction rate for the initial judge assignment (Chicago) or courtroom assignment (New York). We use all court records, not just the linked sample, to construct the instrument.[29] Judge stringency is strongly predictive of whether a child's case ends in an eviction order. Figure A.1 shows the distribution of judge stringency (residualized by court-year-quarter) across cases in Chicago and New York. The variation in judge stringency is substantial and similar across settings: a 7 percentage point difference between the 10th percentile and 90th percentile of judge stringency in Chicago and a 6 percentage point difference in New York.

5.3 Validating the IV design

We next discuss tests of the assumptions for judge stringency to be a valid instrument and for the IV estimand to reflect a positive weighted average of local treatment effects on compliers.

Relevance. To assess the relevance of our instrument, columns 1 and 3 of Table A.1 report the first-stage estimates from equation 5.1 for each of our three samples, controlling for district-year fixed effects. Judge stringency has a large and statistically significant impact on the probability of eviction, with an F-statistic for the first stage of 129.2 in Chicago and 362.7 in New York, providing evidence against weak instruments in our setting. Columns 2 and 4 show that the first stage remains largely unchanged when adding additional controls, suggesting that judge stringency is uncorrelated with individual and case characteristics. Table A.1 shows the first stage for the Cook County Census sample.

Exogeneity. Table A.2 presents evidence that case and child characteristics are not predictive of judge stringency, which lends empirical support to the random assignment of judges in our setting. Columns 1 and 3 estimate a child-case regression of the eviction judgment on case and child characteristics, showing that these characteristics are predictive of the case ending in an eviction order in both Chicago and New York, while columns 2 and 4 show that these child and case characteristics are not predictive of the judge stringency instrument. We conduct an F-test that all coefficients are jointly equal to zero in both Chicago and New York and cannot reject the null hypothesis in either setting, consistent with random assignment. The balance table and F-test for the Census analysis sample are presented in Table A.3.

Exclusion. A key assumption in our setting is that judge stringency affects children's outcomes only through the eviction order. As noted in Collinson et al. (2024), judges may influence other

[29] There are 130 judges (321 judge-year pairs) in Cook County and 29 courtrooms (261 courtroom-year pairs) in New York City over our sample period.

aspects of the case, including the judgment amount, if the landlord is seeking payment for arrears, or granting tenants additional time before the bench trial. The multi-dimensionality of judge discretion can make it challenging to estimate the impact of court orders on outcomes (Mueller-Smith, 2015; Bhuller et al., 2020; Humphries et al., 2024). In Collinson et al. (2024), we provide evidence supporting exclusion holding in our setting (see Appendix G.3). In particular, we create measures of stringency in granting stays of the eviction order and stringency in the judgment amount, and we show that the correlation between different dimensions of judge stringency is low. We also show that our main stringency instrument is not predictive of the money judgment in cases where the tenant is evicted, and that controlling for the additional stringency measures has little impact on the first stage.

Monotonicity. The monotonicity assumption requires that evicted tenants would also have been evicted by a more stringent judge, and that nonevicted tenants would not have been evicted by a less stringent judge. One potential threat to this assumption is the possibility that some judges are harsh for some types of cases, or for some groups of individuals, while being more lenient toward others. We test the monotonicity assumption in two ways. First, we perform the standard test that the first-stage estimates should be non-negative for subsamples of cases. The second test we conduct is to estimate the judge stringency measure using one subpopulation and using that as our instrument for the complementing sub-population (Bhuller et al., 2020; Norris et al., 2021). We find that the first-stage estimates are all positive and largely unchanged. We additionally construct a judge stringency measure using cases that do not have a match to our educational records and re-estimate the first stage using this alternative instrument and education samples; we again find the first-stage estimates are largely unchanged. These exercises lend support for the monotonicity assumption in our setting. See Appendix Tables A.4 and A.5 for results using the education samples, and Appendix Table A.6 for results using the Census sample.

5.4 Combining estimates across cities

Our data use agreements do not allow us to pool observations from Cook County and New York City. We therefore estimate each regression separately by location and report the location-specific estimates and also the average point estimates across the two locations. The results are observation-weighted, to reflect the average effect across children in our two cities. Given that the New York sample is larger, the New York weight is approximately 0.8 to 0.85. We calculate the standard errors for the combined estimates as

$$\widehat{SE}_{\text{combined}} = \sqrt{\omega^2 \times \widehat{SE}^2_{NYC} + (1-\omega)^2 \times \widehat{SE}^2_{CC}},$$

where ω reflects the observation weight.

Under the assumptions outlined in Section 5.1, the combined estimates can be interpreted as the average of the effect of eviction for children in complier cases in Cook County and New York City.

6 Estimates of Causal Effects

In this section, we present our main estimates of the effects of eviction on children's outcomes. We study four outcome domains: the home environment, school attachment and engagement, educational achievement, and high school completion. To study the home environment using CPS and NYCDOE data, our main outcomes are residential moves, homelessness, an unstable living situation, and neighborhood poverty rates. Additionally, we use linked Census data to study impacts on children's living arrangements, household structure, and neighborhood poverty rates. To study school attachment and engagement, our main outcomes are absenteeism, school switching, and grade retention. We then examine effects on academic achievement, including test scores in grades 3-8 and credits earned and GPA in high school. Finally, we investigate the effects on high school graduation.

6.1 Home environment

Table 3 shows our estimates of the effects of eviction on the home environment using the education data. The nonevicted mean, OLS estimates, and IV estimates are shown in columns 1-3 for Chicago and in columns 4-6 for New York City. Columns 6-8 present the combined estimates. The top panel of Table 3 reports estimates for the case school year, and the middle and bottom panels report estimates for the first and second post-filing school years, respectively.

We first examine impacts on the child's likelihood of moving out of their pre-filing address. Our IV estimates show that, for complier children, eviction increases the likelihood of moving by 12.5 percentage points in the case year. The point estimate for Chicago is larger than it is for New York in absolute terms, but both estimates imply an approximately 80 percent increase in the likelihood of moving, relative to the nonevicted mean. This effect persists through the next two years: the combined estimate is 13.6 percentage points in the first post-filing school year and 17.4 percentage points in the second year, and both estimates are significant at the one percent level. Because eviction may cause residential mobility beyond the initial move, we also examine effects on children's cumulative number of moves. We find that eviction increases the number of moves by 0.2 in the first full year after the case and 0.4 two years later, suggesting that eviction causes residential churn beyond the initial move.[30]

[30] Appendix Table A.14 reports effects separately for children in grades 1-5 and 6-12 in the school year of the case. For both groups, eviction causes similar increases in the likelihood of moving out by the second full school year, though the results suggest that children with filings in grades 1-5 experience earlier moves and a greater number of moves. We do not find evidence that eviction causes either group to move to neighborhoods with higher poverty rates.

While we find that eviction increases the child's likelihood of moving, we do not find evidence that eviction leads children to move to higher-poverty neighborhoods. We estimate small and fairly precise null effects on neighborhood poverty, measured by census tract poverty rates. In the case year, we can rule out increases in the neighborhood poverty rate larger than 3.4 percentage points with 95 percent confidence. Overall, these estimates are similar to those in Collinson et al. (2024) for the population of adult tenants in Chicago and New York facing eviction.

Homelessness. Table 3 also reports impacts on homelessness and the McKinney-Vento flag for being housing unstable. We find consistent evidence that eviction increases child homelessness. The IV estimates imply that eviction increases homelessness by 3.3 percentage points in the first year after filing (a 100 percent increase relative to the non-evicted mean, and statistically significant at the 10 percent level) and by 5.3 percentage points in the following year (a 150 percent increase, and significant at the 5 percent level). In addition, in Chicago, we find that eviction increases the likelihood that a child is flagged as housing unstable by 7.9 percentage points in year 0, by 10.3 percentage points in year 1, and by 7.8 percentage points in year 2, although these estimates are imprecise and are not statistically significant. These effects on homelessness and the McKinney Vento flag are similar in magnitude to the increases at filing found in Section 4.4, in our analysis of trends. Together, these results indicate that while homelessness is rare among children, eviction causes a large relative increase in child homelessness.

Household structure and living arrangements. We next use the linked Census sample to study the impact of eviction on children's living arrangements, family structure, and neighborhood environment. This sample includes children aged 0-18 years in the case year, and is therefore younger on average than the education records sample.[31] We emphasize that because the Census sample is based on a 2000 Decennial linkage and the outcomes are recorded in the 2010 Decennial, with the cases occurring in between, the outcomes are measured an average of 5.5 years after the case year, a longer time horizon than the education records analysis.

The results are presented in Table 4. We find a positive, though insignificant, effect of eviction on household size of 0.7 relative to the nonevicted mean of 4.8. We find that eviction increases the likelihood that children live in a doubled-up household—in the measure that includes grandparents—by 16.9 percentage points, relative to the nonevicted mean of 21.9 percent.[32] The increase in household size and doubling up is also reflected in a large increase

[31] We present the Census results restricting to the school-aged sample of 6-18 year-olds in Table A.12 and find similar results to those presented here.

[32] The effect on the doubling up measure that excludes grandparents is 10.2 percentage points, relative to the nonevicted mean of 13.7 percent, an effect size that is similar in magnitude, although not statistically significant.

in the child's likelihood of living in a multigenerational household—a 13.2 percentage points increase relative to the nonevicted mean of 9.7 percent. Taken together, these results show that eviction increases the likelihood that children move into doubled-up households, often with grandparents or other extended family.

Turning to household structure, we do not find evidence that eviction increases the likelihood that children live in a single mom-headed household. We also find no evidence that eviction impacts the likelihood that children live with their mother or father, and no statistically significant effect on the likelihood of children living with a non-relative household head. These estimates are somewhat imprecise but are consistent across outcomes, and suggest that eviction does not disrupt the child's family structure.

Looking at the neighborhood environment, we find that eviction has a negative impact on the child's neighborhood poverty rate, by 5.1 percentage points relative to a nonevicted mean of 23.5 percent. This result is consistent with the neighborhood poverty result in the second year after filing using the Chicago education sample presented in Table 3. We find no statistically significant impact on the probability that the child is living out of the county, and no statistically significant impact on the probability that the child is living out of state. Although these out-of-county estimates are imprecise, they lend supportive evidence against selection bias in the education records analysis.

As a whole, we find evidence that eviction causes children to move in with their grandparents and to somewhat lower-poverty neighborhoods. To investigate whether the same children who move in with their grandparents also move to lower-poverty neighborhoods, we first construct an indicator for whether the child moves to a lower-poverty neighborhood relative to their case address. We use this outcome to construct indicators for moving to a multigenerational household in a lower-poverty neighborhood and for moving to a non-multigenerational household in a lower-poverty neighborhood. Table A.13 reports IV estimates for these three outcomes. We find that eviction increases the probability of moving to a multigenerational household in a lower-poverty neighborhood by 6.2 percentage points, which is 63 percent of the 9.9 percentage point total increase in the probability of moving to a lower-poverty neighborhood.

6.2 School attachment and engagement

We now examine impacts on children's school attachment and engagement, focusing on absenteeism, switching schools, grade retention, and transferring out of the district. Previous research postulates that increased absenteeism and school switching are important channels through which housing instability could impact schooling (Pribesh and Downey, 1999; Hanushek et al., 2004; Fantuzzo et al., 2012; Welsh, 2018; Todres and Meeler, 2021). Moreover, research suggests that absenteeism, grade retention, and school-switching causally impact longer-run outcomes, such as high school graduation (Jacob and Lefgren, 2009; Schwartz et al., 2017; Liu et al., 2021; Goldman and Gracie, 2024). Little quasi-experimental evidence exists, however, on the link between children's housing situation and their school engagement.

Our OLS and IV estimates are shown in Table 5. The first row reports the impact of eviction on switching schools within the district. The combined IV estimate implies that eviction increases the likelihood of switching schools by 7.6 percentage points in the year of the case (significant at 5 percent), a 46.6 percent increase relative to the nonevicted mean of 16.3 percent. This effect is driven almost entirely by impacts in Chicago. The effects on school switching persist in the following school year and are broadly consistent with elevated rates of school changes among evicted children that we show in Section 4.4. While we find that eviction increases school switching, we find no evidence that it impacts school quality (see Appendix Table A.10).

Next, we consider impacts on absenteeism, including the fraction of days the child is absent and the likelihood the child is chronically absent (i.e., absent for more than 10 percent of days). Our combined IV estimate in the case year suggests a 0.9 percentage point increase in the fraction of days absent, though the estimate is not statistically significant. In the first post-filing school year, we find larger effects, with eviction increasing the fraction of days absent by 2.4 percentage points, or 18 percent of the nonevicted mean (significant at the 5 percent level). Similarly, we find that eviction increases the likelihood of a child being chronically absent by 9 percentage points (21 percent of the nonevicted mean), with particularly large increases in Chicago. The impact on days absent persists into the second post-filing school year. These estimates are similar in magnitude to those found in studies of officer-involved killings (Ang, 2020), school shootings (Cabral et al., 2020), and protective services removals (Bald et al., 2022), and are larger than effects from parental incarceration (Norris et al., 2021).

Turning to grade retention, we find some evidence that eviction increases the likelihood that a child is held back by a grade. The combined IV estimate implies that eviction increases the child's likelihood of being retained in their grade by 2.4 percentage points one year after eviction, an effect that is not statistically significant. By the second post-filing year, eviction increases the likelihood of being retained at least once by 5.3 percentage points, a 34 percent increase, which is significant at the 10 percent level.

Finally, we explore whether eviction causes children to transfer out of the district. We find that eviction has little effect on transferring in the case year, or in the first post-filing school year: the combined IV estimates are 0.002 and 0.001, respectively. We find evidence that eviction leads to higher rates of transferring in the second year, however, by 2.9 percentage points, which is statistically significant at the 10 percent level.

Together, these results provide evidence that eviction increases school switching, causes an uptick in absenteeism that results in a substantial increase in chronic absenteeism, and also appears to increase grade retention. In Appendix Table A.15, we report effects separately for children in grades 1-5 and 6-12 at the time of filing. The effects on school-switching and absenteeism are larger for children in middle or high school at the time of filing. The estimates for retention are similar for both groups by the second full year, though children with filings in grades 1-5 experience increases in retention earlier.

6.3 Educational achievement

Test performance. We now examine whether eviction affects standardized test scores. For both sites, we observe test scores on statewide math and reading exams during grades 3-8, which have been shown to predict long-run outcomes such as earnings (Chetty et al., 2011). Given the effect on absences that we document above, we also explore impacts on whether a student misses a scheduled test.

We find no statistically significant effect of eviction on math or reading test scores, reported in Table 6. In the case school year, the IV estimate is positive for reading (0.08 s.d.) and virtually zero for math scores. In the first post-filing year, the estimates for reading and math are both slightly positive (0.05 s.d.). In the second post-filing year, both estimates are moderately positive (about 0.15 s.d. each) and again not statistically different than zero. Although somewhat imprecise, our estimates allow us to reject moderate to large negative and positive effects. In particular, in the case school year we can reject reductions in reading and math scores larger than -0.06 s.d. and -0.15 s.d., respectively, with 95 percent confidence. In the first post-filing year, we can reject negative effects larger than one-fifth of a standard deviation in reading and math scores, with 95 percent confidence.

These estimates, while imprecise, provide some evidence that eviction does not have large negative effects on conventional cognitive skills measures for elementary and middle school students. One factor that could limit strong takeaways is if eviction causes students to miss standardized test dates. Although the IV estimate on the likelihood of missing a test is an insignificant 2.4 percent points in the case school year, it increases to 6.5 percentage points ($p < .01$) in the first post-filing year. If eviction causes lower-performing students to miss the test, our test score estimates will be biased upwards. Alternatively, eviction may not have a large impact on cognitive skills, but instead may primarily impact non-cognitive skills. This latter interpretation is consistent with Jacob (2004), who finds that public housing demolitions do not affect test scores, but reduce graduation.

High school credits and GPA. Finally, for older students in our sample, we can study whether eviction impacts high-school course completion, and course grades (in Chicago only). Credits and GPA in high school are likely to capture a mix of both cognitive and non-cognitive skills (Jackson, 2018; Mulhern, 2023). Furthermore, in both districts, high school GPA and completed course credits directly determine whether a student can earn a high school diploma.

In Table 7, we report effects on course credits earned (as a share of the credits typically needed to progress) in high school for Chicago, New York, and combined estimates, and impacts on high school GPA in Chicago. The combined estimates in the case year imply that eviction reduces credits earned, as a percentage of credits needed, by 8.8 percentage points, though the estimate is not statistically significant. In the first year after the case, the IV point estimate grows to a 14.4 percentage point reduction in credits earned (a 17 percent reduction relative to the nonevicted mean) and is statistically significant at the 5 percent level. The IV estimates for Chicago and New York are both statistically significant, implying a 27.6 percentage point reduction in credits and a 13.8 reduction in credits, respectively, and these effects persist into the second year. The IV estimates for the effects on GPA generally point to reductions of approximately one-quarter of a letter grade, but these effects are imprecisely estimated, and are not statistically significant.

6.4 High school graduation

In this subsection, we examine whether the disruptive effects of eviction extend to high school completion. A challenge in studying longer-run outcomes using school records is missing outcomes for students who move out of the district. We study a subsample of children who are more likely to have a non-missing graduation status: those aged 18 years or older by the end of our sample period and enrolled in at least middle school (grade 6 or higher) at the time of the court case. We provide evidence on the extent of attrition among this sample below, and we develop a method in Section 6.4.1 to provide bounds on graduation effects under varying assumptions about the severity of differential attrition.

Table 8, Panel A, reports estimated effects of eviction on graduation. The combined IV estimate indicates that eviction reduces the likelihood of graduating by 12.5 percentage points, relative to the nonevicted mean of 67.6 percent, and with a 95% confidence interval of -2.5 to -22.5 percentage points. The IV point estimates for Chicago and New York are similar at -10.3 percentage points and -12.8 percentage points, respectively, though only the New York estimate is statistically significant. The OLS estimates also imply that eviction reduces the likelihood of graduating, but the combined estimate of -3.9 percentage points is smaller than the IV estimate. We examine impacts on *on-time* graduation—i.e., graduating within 4 years of starting 9th grade—in Appendix Table A.11, and we obtain a smaller estimate of -0.049 that is not statistically significant. These results suggest that eviction does not cause students to shift from on-time to delayed graduation, but instead causes students to shift from delayed graduation to dropping out.

These graduation effects are consistent with our finding that eviction reduces high school course credits while increasing—especially among older students—absenteeism and school-switching. To examine whether our estimated impacts of graduation align with the effects we observed on intermediate outcomes, we perform the following back-of-the-envelope calculation. We first regress graduation on middle-school and 9th-grade intermediate outcomes—absenteeism, residential mobility, test scores, high school credits, and GPA—using the sample of all public school students in Chicago and New York. These intermediate outcomes are highly predictive of graduation, jointly obtaining R-squared values of 0.36 and 0.53 for Chicago and New York. Second, we use the coefficients from these regressions, along with our IV estimates of the impact of eviction on intermediate outcomes, to predict the impact of eviction on the likelihood of graduation (see Appendix A.5 for details). This back-of-the-envelope calculation yields a predicted impact of -12.0 percentage points. This estimate is similar to the -12.5 percentage point effect we estimate above and suggests that the impacts on intermediate outcomes can broadly rationalize the effects on graduation.

Overall, our estimates of the impact of eviction on graduation are similar in magnitude to the estimated effects of juvenile incarceration on high school completion (Aizer and Doyle, 2015) and to the disruptive effects of moving found among older children in the MTO program (Sanbonmatsu et al., 2011), and slightly larger than the effects of involuntary displacement from public housing (Jacob, 2004).[33]

We present estimates of the effect of eviction on attrition—i.e., having a missing graduation status—in Table 8, Panel A. While neither site-specific IV estimate is statistically significant, the combined estimate for the effects of eviction on having a missing graduation status is a 6.6 percentage point increase, which is significant at the 10 percent level, and is 50.4 percent of the non-evicted mean of 13.1 percent. Using these estimates, we develop bounds on the graduation effects in the next subsection.

6.4.1 Bounding approach to account for attrition

We now investigate the potential bias from differential attrition. Lee (2009) develops a method for bounding treatment effects when sample selection depends on treatment. Although Lee's method has been extended to the instrumental variable setting (Chen and Flores, 2015; Bartalotti et al., 2023), these approaches do not develop estimation and inference procedures for a non-binary instrument, and the resulting bounds allow for unlikely scenarios such as one in which none of the students who exit the district due to eviction graduate.[34] In this subsection, we develop an alternative bounding approach that only requires estimating three LATE-like parameters (using TSLS), involves straightforward inference, and allows us to consider a range of scenarios, including highly conservative ones, by varying a single parameter: the difference in graduation rates in the nonevicted state between students who exit due to eviction and those who do not.

[33] Aizer and Doyle (2015) estimate that juvenile incarceration causes a 13 percentage point reduction in high school completion. Sanbonmatsu et al. (2011) find that older children who moved to low-poverty with a voucher in MTO were 14 percentage points less likely to report having a high school diploma. Jacob (2004) estimates that public housing demolitions increased dropout rates by 3.6-8.5 percentage points depending on the year of measurement.

[34] Chen and Flores (2015) consider a binary instrument. Bartalotti et al. (2023) discuss identification and estimation with a non-binary instrument, but do not consider inference. Estimation and inference are challenging with a non-binary instrument because the estimators are extremum functions of the conditional distribution functions $Y|E = e, S = 1, Z = z$ and $S = 1, E = e|Z = z$.

Graduation is only observed for students who do not transfer out of the school district. Let $S \equiv S(E)$ be an indicator for a student staying in the district, where E denotes whether the student is evicted. If eviction decreases the likelihood of staying in the sample, as suggested by the estimates in Panel A of Table 8, then, on average, $S(0) > S(1)$, and the observed samples for the evicted and nonevicted group may be differentially selected, in a way that may correlate with graduation.

Our bounding approach requires a monotonicity assumption that eviction weakly increases the likelihood of leaving the school district for all students.[35] To explain the intuition for our approach, we implicitly condition on covariates and suppose that individuals are randomly assigned to either a stricter judge ($Z = z_1$) or a more lenient one ($Z = z_0$). We discuss below how to implement the approach using covariates and the full range of judge stringency values in Appendix D.

We first define:

$$\mu \equiv \mathbb{E}[Y(1)|T = c, S(1) = 1] - \mathbb{E}[Y(0)|T = c, S(0) = 1], \tag{6.1}$$

where Y denotes graduation, $T = c$ denotes Z-compliers (i.e., those evicted by the stricter judge but not the more lenient one). In words, μ is the difference between the average evicted potential outcome for compliers who stay when evicted and the average nonevicted potential outcome for compliers who stay when nonevicted. In Appendix D, we show that μ equals the difference between two straightforward TSLS estimands.

Because the latter moment contains both compliers who stay when evicted and compliers who leave only when evicted, we can rewrite (6.1) as:

$$\mu = \underbrace{\mathbb{E}[Y(1) - Y(0)|T = c, S(0) = 1, S(1) = 1]}_{\text{LATE-AO}} \tag{6.2}$$

$$- \underbrace{\left(\mathbb{E}[Y(0)|\underbrace{T = c, S(0) = 1, S(1) = 0}_{\text{OOU compliers}}] - \mathbb{E}[Y(0)|\underbrace{T = c, S(0) = 1, S(1) = 1}_{\text{AO compliers}}]\right)}_{\equiv\, \delta^\star} \times \pi, \tag{6.3}$$

where $\pi \equiv \mathbb{P}[S(1) = 0|T = c, S(0) = 1]$ is also identified by a TSLS estimand. This is the share of "observed-only-when-untreated" (OOU)—students who are *only* observed in our data when not evicted—among all compliers who are observed when untreated.

The term LATE-AO is the parameter of interest: the local average treatment effect of eviction on graduation for the always-observed (AO) compliers, i.e., students who are instrument compliers and who are in the observed sample regardless of their eviction status. While not identical to the estimands obtained if outcomes were observed for all students, the LATE-AO

[35]Formally, we assume $S(0) \geq S(1)$ for all students, though we could alternatively assume $S(1) \geq S(0)$, as described in Appendix D. Chen and Flores (2015) and Bartalotti et al. (2023) also maintain this monotonicity assumption.

informs the average causal effect of eviction for a well-defined population.

The only unknown quantity in equation 6.2 is $\delta^\star$: the difference in graduation rates in the nonevicted state between the OOU and AO compliers Although we cannot identify $\delta^\star$, we can bound it by assuming it lies inside a reasonable interval. For example, if we assume graduation rates, when not evicted, do not differ by more than 10 percentage points between the two types of students, then $\delta^\star \in [-0.1, 0.1]$. More generally, suppose $\delta^\star \in [\delta_L, \delta_U]$, then:

$$\underbrace{\mathbb{E}[Y(1) - Y(0)|T = c, S(0) = 1, S(1) = 1]}_{\text{LATE-AO}} \in [\mu + \pi\delta_L, \mu + \pi\delta_U].$$

We can thus use the TSLS-based estimates for μ and π along with the chosen values for δ_L and δ_U to bound the LATE-AO. Because the TSLS estimators—and thus the bound endpoints—are asymptotically normally distributed, we use results from Imbens and Manski (2004) to construct confidence intervals for the LATE-AO bounds (see Appendix D for details).

6.4.2 Estimated bounds

Panel B of Table 8 presents estimates based on our bounding approach for graduation.[36] Across rows, we vary the assumption on the interval that encompasses δ^*, i.e. on the largest possible gap in the nonevicted graduation rates between complier students who remain in the school districts irrespective of eviction status and students who only migrate out of the school districts when evicted. For example, the first row contains estimated bounds for the LATE-AO under the assumption that $|\delta^*| \leq 0.05$, which maintains that graduation rates in the nonevicted state for the two groups of students differ by at most five percentage points. Under this assumption, the estimated interval for the combined IV estimate is narrow and similar to the IV estimate $[-0.115, -0.108]$.

We continue to find that eviction causes a reduction in graduation rates even as we allow for large gaps in the graduation rates between these two groups of students. In particular, even when we allow for $|\delta^*| \leq 0.25$, the bounds continue to contain only negative values, and we reject (at the 10% level) that the LATE-AO is zero. We continue to reject (at the 10% level) that the LATE-AO is zero up until $[\delta_L, \delta_U] = [-.37, .37]$. Hence, our finding that eviction causes lower graduation rates changes only if students who exit the district in response to eviction have a graduation rate that is more than 37 percentage points higher than students who always stay in the district.

[36] We implement the bounding procedure by first estimating the city-specific bounds, and then estimating combined bounds as described in Section 5.4.

6.5 Interpreting the IV estimates

Under the assumptions described in Section 5, our IV approach recovers a weighted average of treatment effects for compliers, i.e., children whose case outcome would have changed had their case been assigned to a different judge. In this subsection we characterize the complier population. In particular, we use our data to describe the demographic characteristics and average pre-case outcomes of these compliers, as in Bhuller et al. (2020), and compare these characteristics to both evicted and nonevicted children. Then we follow Imbens and Rubin (1997) in estimating compliers' mean potential outcomes when they are not evicted to explore why their treatment effects may differ from those for the broader population of tenants in court.

Panel A of Table A.8 reports estimates of complier characteristics alongside characteristics of evicted and nonevicted students. Across a broad set of attributes, children involved in complier cases closely resemble those in cases that do not end in eviction, rather than those that do. For example, relative to evicted children, nonevicted children and compliers have lower ad damnum amounts and are substantially less likely to have recently moved. Nonevicted children and compliers also have greater academic attachment and higher achievement prior to the case, as reflected in lower retention and absenteeism rates, more credits, and higher test scores and GPAs. Lastly, Panel C shows that compliers, had they not been evicted, would have continued to experience housing stability and academic attachment comparable to nonevicted students in the year the case was filed. Overall, compared to all students with an eviction filed against them, compliers appear to be more stably housed and have greater academic attachment and achievement prior to the case.

Since compliers are less disadvantaged than the evicted group, and in some cases, even the nonevicted group, the effects of eviction on compliers may differ from the average effect of eviction across all tenants in court. Differences between populations may explain why some OLS estimates are smaller than the IV estimates. For example, the combined IV estimate for "Not at pre-case address" in the case school year is 0.125, compared to an OLS estimate of 0.095. Some differences are larger, such as "Doubled up (incl. grandparents)" for the Census sample with an IV estimate of 0.169 and an OLS estimate of 0.023. There are several reasons why compliers may experience larger effects of eviction compared to the average child in eviction court.

Compliers tend to owe significantly less in rental arrears at the time of the case compared to both evicted and nonevicted tenants, and the complier means in the case school year show somewhat lower rates of not being at the pre-case address (Panel C of Table A.8). Therefore, compliers may face fewer challenges in staying housed when their case does not end in eviction, which would imply that being evicted is a larger disruption to their family's housing environment. Additionally, the fact that complier children are less disadvantaged and more stably housed than the average evicted child could explain why they experience larger treatment effects. For example, compliers may have less experience dealing with disruption, eviction may be less anticipated, or their higher baseline outcomes may simply have more scope for deterioration.

6.6 Heterogeneity in effects by gender

A consistent theme in research on the role of family background and childhood environment in children's outcomes is that there are gender differences in the impact of family disadvantage or income shocks (Dahl and Lochner, 2012; Bertrand and Pan, 2013; Chetty et al., 2016; Autor et al., 2019; Barr et al., 2022). Motivated by these prior findings, we investigate whether the effects of eviction differ by gender.

Appendix Tables A.16–A.21 show the combined schooling results separately by child gender. Eviction appears to be more disruptive for boys. We find larger effects for boys on absenteeism, chronic absenteeism, and school-switching, although for many outcomes we cannot reject group equality. The effects of eviction on high school credits and graduation are also more pronounced for boys.

In Table A.17, we report the effects on household structure and living arrangements separately by child gender. We find stark differences in the subsequent living situations of girls and boys who experience eviction. Girls are much more likely to move into a multigenerational household or live in a grandparent-headed household as a result of eviction. These effects are also reflected in a larger effect on household size and doubling up. Moreover, eviction has a larger effect on moving to a lower-poverty neighborhood for girls relative to boys.

These results may reflect differences in the difficulty in securing or maintaining housing depending on the gender of the child. Prior work discusses how families with boys encounter more resistance from landlords in leasing to them, and elevated rates of police contact after lease-up, which may limit housing options for families with boys (Desmond et al., 2013; Desmond, 2016). Bertrand and Pan (2013) discuss the difficulty of rearing boys compared to girls. For related reasons, grandparents may be more willing to extend housing support when grandchildren are female. Our results indicate that girls may have access to additional support from extended family members, and are consistent with stronger family insurance helping to moderate the adverse effects of eviction through increased adult supervision and stability.

7 Conclusion

In this paper, we provide the first evidence of a causal link between eviction and children's outcomes. We find that eviction destabilizes children's housing situation—increasing residential mobility, doubling up with grandparents or other adults, and homelessness—and disrupts their schooling. The effects on schooling appear most clearly for measures related to school attachment and engagement, where we find increases in absences and school switching, and reductions in credits earned, outcomes that are frequently interpreted as being influenced by non-cognitive traits (Heckman et al., 2018; Jackson, 2018; Petek and Pope, 2023). In contrast, we find little evidence of direct effects on cognitive measures such as math or reading scores. As in previous work (Jackson, 2018), we find that the non-test-score measures are, in fact, better predictors of high school completion than conventional cognitive measures such as standardized test scores. In line with those findings, when we turn to impacts on high school graduation, we find that eviction leads to meaningful reductions in the likelihood of graduating high school, with much of the effect on graduation explained by its attendant effects on absences, school switching, and course credits.

Our results highlight how adverse shocks may have lasting effects on the educational attainment of low-income children. These findings relate to recent research exploring how adversity among disadvantaged youth can impact longer-term educational attainment (DeLuca et al., 2021). We shed new light on how low-income families weather negative shocks, finding evidence suggesting that these spillover effects of eviction are moderated by households moving in with extended family. This reinforces the need to better understand the role of family support networks and adult supervision in the lives and outcomes of low-income children.

In addition to contributing to knowledge on the role of an important dimension of poverty—housing insecurity—in the economic mobility of children, our results also inform debates around eviction and low-income housing policies. In particular, they suggest that the social cost of eviction may be amplified for families with children through reduced educational attainment. Whether eviction prevention policies or school policies to aid housing-insecure students could mitigate these effects remains an open question for future research.

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Tables and Figures

Table 1: Summary Statistics (Census Sample)

	Evicted (1)	Not evicted (2)
Demographics:		
Age at case	8.594	8.606
Age in 2010	14.150	14.170
Female	0.499	0.503
Black	0.768	0.769
Child relationship to household head (2000):		
Foster child	0.008	0.007
Grandchild	0.207	0.191
Household structure (2000):		
Single mom	0.436	0.454
Single mom (without cohabiting partner)	0.350	0.372
Single dad	0.049	0.047
Single dad (without cohabiting partner)	0.015	0.013
Two parent	0.252	0.258
Mom present	0.898	0.901
Dad present	0.412	0.403
Doubling up (including grandparents)	0.368	0.337
Doubling up (excluding grandparents)	0.191	0.168
Case characteristics:		
No attorney	0.976	0.933
Ad damnum	1.615	1.319
Neighborhood fraction Black	0.652	0.632
Neighborhood poverty rate	0.269	0.274
Observations	35,000	18,000

Notes: The table above presents sample averages for children in the linked Census sample used in the OLS/IV analysis. This sample consists of children who are in the same household in the 2000 Decennial Census as a tenant with a Cook County eviction filing in 2000-2009. We link these children to their their 2010 Decennial Census records. See Section 3.3 for details. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965.

Table 2: Summary Statistics (Education Sample)

	Chicago				New York			
	Evicted (1)	Not evicted (2)	Grade-Yr-Schl (3)	Grade-Yr (4)	Evicted (5)	Not evicted (6)	Grade-Yr-Schl (7)	Grade-Yr (8)
Student demographics								
Female	0.490	0.490	0.489	0.492	0.490	0.492	0.486	0.484
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Black	0.764	0.770	0.677	0.449	0.396	0.429	0.360	0.294
	(0.424)	(0.421)	(0.468)	(0.497)	(0.489)	(0.495)	(0.480)	(0.456)
Hispanic	0.190	0.182	0.252	0.414	0.531	0.510	0.511	0.399
	(0.393)	(0.386)	(0.434)	(0.492)	(0.499)	(0.500)	(0.500)	(0.490)
Age (in case school year)	10.603	10.785	10.923	11.035	11.040	11.332	10.866	10.839
	(4.122)	(4.228)	(4.255)	(4.235)	(3.415)	(3.415)	(4.008)	(4.038)
Student variables (at pre-case year)								
Changed address	0.377	0.300	0.210	0.143	0.212	0.110	0.123	0.115
	(0.485)	(0.458)	(0.407)	(0.350)	(0.409)	(0.312)	(0.328)	(0.319)
McKinney-Vento Flag	0.104	0.073	0.065	0.034				
	(0.305)	(0.261)	(0.246)	(0.182)				
Retained	0.059	0.055	0.044	0.031	0.117	0.122	0.076	0.065
	(0.236)	(0.228)	(0.204)	(0.174)	(0.296)	(0.304)	(0.265)	(0.246)
Percent absent	0.117	0.105	0.087	0.071	0.113	0.105	0.106	0.093
	(0.122)	(0.118)	(0.108)	(0.093)	(0.095)	(0.090)	(0.142)	(0.136)
Chronic Absent	0.412	0.358	0.280	0.210	0.445	0.406	0.302	0.248
	(0.492)	(0.479)	(0.449)	(0.407)	(0.497)	(0.491)	(0.459)	(0.432)
Math score	-0.410	-0.356	-0.250	-0.011	-0.394	-0.362	-0.240	0.000
	(0.885)	(0.899)	(0.935)	(1.001)	(0.905)	(0.887)	(0.958)	(1.000)
Reading score	-0.371	-0.307	-0.224	-0.009	-0.340	-0.307	-0.225	0.000
	(0.927)	(0.940)	(0.955)	(1.000)	(0.915)	(0.884)	(0.951)	(1.000)
Credits earned	0.888	0.894	0.922	0.934	0.884	0.903	0.890	0.920
	(0.223)	(0.232)	(0.204)	(0.182)	(0.364)	(0.361)	(0.433)	(0.410)
GPA	2.052	2.108	2.304	2.496				
	(1.008)	(1.024)	(1.071)	(1.131)				
School and neighborhood characteristics (at pre-case year)								
School's Avg. Test Scores	-0.203	-0.162	-0.188	0.022	-0.229	-0.231	-0.222	-0.003
	(0.393)	(0.432)	(0.407)	(0.482)	(0.347)	(0.351)	(0.371)	(0.447)
Tract poverty	0.321	0.321	0.307	0.251	0.299	0.303	0.289	0.224
	(0.138)	(0.148)	(0.143)	(0.137)	(0.121)	(0.118)	(0.129)	(0.134)
Observations	48,926	26,165	874,436	874,436	81,580	172,639	9,752,322	13,383,620

Notes: Columns (1)-(2) and (5)-(6) show summary statistics for students whose household had eviction cases filed against them who were evicted and not evicted in Chicago and New York. Columns (3)-(4) and (7)-(8) show statistics for the full Chicago and New York education samples of students (with placebo filing dates) weighted by grade-year-school and grade-year to match the court samples. For comparison, we define pre-case year variables for students who are not in our court samples by assigning them placebo filing dates that are randomly drawn from students in our court samples with the same year of birth. Student race and ethnicity variables are mutually exclusive. Age is the age at the time the case was filed. "Pre-case year" is defined as the school year prior to the case being filed. "Changed address" is an indicator for being at a different address than the prior shool year (i.e., two years before the case was filed). "McKinney-Vento Flag" is a district flag for the student being in an unstable living situation. "Retained" is an indicator for being enrolled in the same grade as the prior year. "Percent absent" is the percent of enrolled school days the student was absent, and "chronic absent" is an indicator for missing more than 10% of days. "Math score" and "reading score" are test scores from grades 3-8, standardized by grade-year to have a standard deviation of 1 and a mean of 0. The Chicago grade-year weighted test score means in (4) are not exactly zero because of noise introduced when assigning placebo filing dates. Credits earned is the number of credits earned divided by the standard number of credits needed and GPA is the grade point average, both of which are only observed in high school. "School's Avg. Test Scores" is the average of the standardized math and reading test scores in the student's school. Tract poverty is the Census tract poverty rate of the child's address in the given year based on estimates from the 5-year ACS. Because these 5-year estimates span 5 years, we match each school year with the 5-year estimate for which it is the midpoint (or closest ACS when this is not possible). Pre-case year student variables and school and neighborhood characteristics are defined among actively enrolled students. The sample is restricted to the education sample described in Section 3.

Table 3: Home Environment (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Case school year:									
Not at pre-case address	0.344	0.095***	0.299***	0.110	0.095***	0.087**	0.134	0.095***	0.125***
	(0.475)	(0.007)	(0.099)	(0.313)	(0.003)	(0.037)	(0.285)	(0.003)	(0.035)
Neighborhood poverty	0.322	0.000	0.006	0.301	-0.004***	0.009	0.303	-0.003***	0.009
	(0.148)	(0.002)	(0.032)	(0.119)	(0.001)	(0.014)	(0.106)	(0.001)	(0.013)
Homelessness†	0.007	0.009***	0.070**	0.017	0.062***	0.031	0.016	0.058***	0.033*
		(0.002)	(0.033)	(0.129)	(0.002)	(0.020)		(0.002)	(0.019)
McKinney Vento	0.082	0.039***	0.079						
	(0.275)	(0.004)	(0.070)						
Observations	18,147	42,776	41,276	160,613	238,610	238,610	179,552	281,075	279,075
Post-filing school year 1:									
Not at pre-case address	0.486	0.113***	0.141	0.191	0.148***	0.135**	0.223	0.142***	0.136***
	(0.500)	(0.007)	(0.095)	(0.393)	(0.004)	(0.053)	(0.356)	(0.003)	(0.047)
Number of moves	0.580	0.174***	0.097	0.304	0.247***	0.233**	0.332	0.234***	0.210**
	(0.649)	(0.009)	(0.172)	(0.654)	(0.007)	(0.092)	(0.592)	(0.006)	(0.082)
Neighborhood poverty	0.318	0.002	-0.007	0.301	-0.005***	0.024	0.303	-0.003***	0.018
	(0.147)	(0.002)	(0.024)	(0.120)	(0.001)	(0.017)	(0.106)	(0.001)	(0.014)
Homelessness†	0.009	0.001	0.077**	0.023	0.021***	0.051**	0.021	0.020***	0.053**
		(0.002)	(0.033)	(0.148)	(0.001)	(0.026)		(0.001)	(0.024)
McKinney Vento	0.118	0.059***	0.103						
	(0.323)	(0.004)	(0.075)						
Observations	16,006	39,125	37,825	130,583	193,622	193,622	146,382	230,811	229,186
Post-filing school year 2:									
Not at pre-case address	0.602	0.113***	0.126	0.264	0.168***	0.186**	0.302	0.157***	0.174***
	(0.490)	(0.008)	(0.090)	(0.441)	(0.004)	(0.078)	(0.395)	(0.004)	(0.065)
Number of moves	0.814	0.241***	0.179	0.573	0.424***	0.448**	0.595	0.395***	0.405**
	(0.789)	(0.015)	(0.199)	(1.026)	(0.010)	(0.190)	(0.934)	(0.009)	(0.163)
Neighborhood poverty	0.315	0.002	-0.011	0.300	-0.005***	0.028	0.302	-0.003***	0.019
	(0.145)	(0.002)	(0.025)	(0.121)	(0.001)	(0.021)	(0.106)	(0.001)	(0.017)
Homelessness†				0.024	0.013***	0.012			
				(0.153)	(0.001)	(0.029)			
McKinney Vento	0.150	0.064***	0.078						
	(0.357)	(0.005)	(0.080)						
Observations	13,127	36,709	36,709	105,760	156,814	156,814	101,487	165,265	165,265

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "Case school year" is the school year in which the case was filed and the upcoming year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "Not at pre-case address" is an indicator for not being at the same address as the pre-case school year. "Number of moves" is the total number of residential address changes recorded by the district since the pre-case school year. "Neighborhood poverty" is the poverty rate of the census tract of residence based on 5-year ACS data. "Homelessness" is an indicator for any HMIS contact. The † indicates that homelessness results for Chicago are from the Census sample as HMIS records are not linked to the CPS education sample. Observation counts for HMIS records are rounded in accordance with U.S. Census Bureau disclosure requirements and were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R11514. "McKinney Vento" is an indicator of the student being in an unstable living situation. All outcomes are defined among actively enrolled students, with the exception of homelessness for Chicago since it is from the Census sample. Columns (1)-(3) report results for Chicago, (4)-(6) report results for New York City, and (7)-(9) report combined results as described in Section 5.4. The first column reports the non-evicted mean, the second reports the coefficient on an eviction indicator from an OLS regression, and the third reports the TSLS estimate for eviction. Means are accompanied by standard deviations in parentheses, while OLS and TSLS estimates are accompanied by standard errors in parentheses. Standard errors are clustered at the judge×case-year level. Apart from the homelessness outcome for Chicago, regressions control for court-year and child age-at-filing fixed effects, court variables, demographics, and outcome-specific lags. Both the Chicago and New York analyses control for rent claim amount and indicators for legal representation. The demographics controlled for in Chicago are an indicator for Black, white (non-Hispanic), Hispanic, female, an indicator for free or reduced lunch prior to case year (RY0), and indicators for speech and learning disabilities (IEPs). The demographics controlled for in the New York analysis include the same variables as in Chicago, plus indicators for being born in NYC, speaking Spanish, and speaking another language. The outcome-specific lags are constructed by averaging over relative years -3 to -1. We impute zeros for missing controls and we additionally control for indicators for each variable being missing. The samples are restricted to the education analysis samples described in Section 5.1. The regression and sample specifications for the homelessness outcome for Chicago are as described in the notes of Table 4. For each column and time period, the final row reports the average sample size across outcomes. Table E.1 provides cell-specific observation counts, and Appendix C checks for robustness to excluding the lagged outcomes and to excluding all controls other than the fixed effects.

Table 4: Living Arrangements, Household Structure, and Geography (Census Sample)

	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	IV (3)
Living Arrangements			
Total household size	4.841	0.133***	0.686
		(0.023)	(0.480)
Doubling up (incl. grandparents)	0.219	0.023***	0.169**
		(0.006)	(0.077)
Doubling up (excl. grandparents)	0.137	0.007	0.102
		(0.005)	(0.072)
Multigenerational household	0.097	0.015***	0.132***
		(0.003)	(0.054)
Grandparent household head	0.086	0.017***	0.097**
		(0.003)	(0.048)
Household Structure			
Mother present	0.859	-0.024***	0.018
		(0.003)	(0.058)
Father present	0.308	0.006	0.003
		(0.004)	(0.101)
Single mother	0.572	-0.026***	-0.008
		(0.005)	(0.099)
Non-relative household head	0.015	0.002	-0.000
		(0.002)	(0.027)
Geography			
Neighborhood poverty rate	0.235	-0.003**	-0.051**
		(0.001)	(0.025)
Out of county	0.234	0.021***	-0.028
		(0.005)	(0.070)
Out of state	0.139	0.021***	-0.015
		(0.004)	(0.055)
Observations		52,500	48,000

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports results for the Census sample (Cook County) of OLS and two-stage least squares (IV) regressions to estimate the impact of eviction on living arrangements, family structure, and neighborhood. The first column reports the non-evicted mean, the second reports the coefficient on an eviction indicator from an OLS regression, and the third reports the TSLS estimate for eviction. Outcomes are listed on the left of each row and are measured as per the 2010 Decennial. The analysis sample consists of linked cases filed between July 2000 and December 2009 with children who are 0-18 as of the 2010 Decennial (see Section 3.3 and Section 5.1 for more details). Controls for all model specifications include indicators for age-at-case, a female indicator, indicators for Black, white, or Hispanic, and family structure indicators in the 2000 Decennial, including indicators for single-mother household, single-father household, two-parent household, grandparent-headed household, and the household being doubled-up. We also include Census-tract-level controls based on the address listed in the case filing, including share in poverty, share white, share Black, share Hispanic, and an indicator for missing Census covariates. Standard errors for regression model coefficients are included in parentheses and are two-way clustered on the judge and household. The final row reports the modal sample size, which equals the sample size for all outcomes except for neighborhood poverty rate, which has a slightly larger sample. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965. Results rounded following Census Bureau disclosure guidelines.

Table 5: School Attachment and Engagement (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Case school year:									
Not at pre-case school	0.262	0.049***	0.371***	0.153	0.014***	0.018	0.163	0.020***	0.076**
	(0.440)	(0.005)	(0.081)	(0.360)	(0.002)	(0.032)	(0.329)	(0.002)	(0.030)
Percent absent	0.113	0.008***	-0.010	0.126	0.004***	0.012	0.125	0.005***	0.009
	(0.127)	(0.001)	(0.022)	(0.149)	(0.000)	(0.009)	(0.139)	(0.000)	(0.008)
Chronic absent	0.382	0.043***	0.121	0.414	0.019***	0.052	0.412	0.022***	0.061
	(0.486)	(0.005)	(0.093)	(0.493)	(0.002)	(0.043)	(0.458)	(0.002)	(0.039)
Transferred out of school system	0.074	0.008***	-0.042	0.032	0.027***	0.013	0.037	0.023***	0.002
	(0.262)	(0.003)	(0.048)	(0.176)	(0.001)	(0.018)	(0.159)	(0.001)	(0.017)
Observations	18,227	51,522	51,522	188,313	278,879	278,879	206,540	330,401	330,401
Post-filing school year 1:									
Not at pre-case school	0.390	0.060***	0.280***	0.266	0.041***	0.038	0.278	0.044***	0.079**
	(0.488)	(0.007)	(0.088)	(0.442)	(0.003)	(0.044)	(0.403)	(0.002)	(0.040)
Percent absent	0.109	0.009***	0.028	0.135	0.007***	0.023**	0.133	0.007***	0.024**
	(0.124)	(0.001)	(0.020)	(0.166)	(0.001)	(0.011)	(0.154)	(0.001)	(0.010)
Chronic absent	0.361	0.046***	0.228***	0.424	0.027***	0.069	0.419	0.030***	0.090**
	(0.480)	(0.005)	(0.085)	(0.494)	(0.002)	(0.047)	(0.460)	(0.002)	(0.042)
Retained	0.105	0.012***	0.009	0.129	0.004**	0.027	0.127	0.005***	0.024
	(0.307)	(0.003)	(0.045)	(0.335)	(0.001)	(0.027)	(0.307)	(0.001)	(0.024)
Transferred out of school system	0.126	0.019***	-0.031	0.031	0.008***	0.008	0.042	0.010***	0.001
	(0.332)	(0.003)	(0.050)	(0.174)	(0.001)	(0.015)	(0.159)	(0.001)	(0.016)
Observations	16,469	46,519	46,519	170,736	251,730	251,730	187,205	298,249	298,249
Post-filing school year 2:									
Not at pre-case school	0.499	0.065***	0.258***	0.382	0.057***	0.037	0.394	0.058***	0.077
	(0.500)	(0.008)	(0.095)	(0.486)	(0.003)	(0.058)	(0.440)	(0.003)	(0.051)
Percent absent	0.105	0.009***	-0.006	0.142	0.005***	0.028*	0.139	0.005***	0.024*
	(0.123)	(0.001)	(0.022)	(0.175)	(0.001)	(0.015)	(0.163)	(0.001)	(0.014)
Chronic absent	0.343	0.038***	0.024	0.432	0.018***	0.062	0.426	0.021***	0.057
	(0.475)	(0.005)	(0.084)	(0.495)	(0.002)	(0.047)	(0.460)	(0.002)	(0.043)
Retained	0.144	0.015***	0.058	0.157	0.006***	0.052	0.156	0.007***	0.053*
	(0.351)	(0.004)	(0.053)	(0.364)	(0.002)	(0.032)	(0.335)	(0.002)	(0.028)
Transferred out of school system	0.163	0.020***	0.041	0.029	0.005***	0.026	0.043	0.008***	0.029*
	(0.369)	(0.004)	(0.056)	(0.168)	(0.001)	(0.016)	(0.155)	(0.001)	(0.017)
Observations	14,379	41,084	41,084	149,338	220,181	220,181	163,718	261,264	261,264

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Case school year" is the school year in which the case was filed and the upcoming year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "Not at pre-case school is an indicator for being enrolled at a different school relative to the school in the pre-case school year, not counting mechanical school changes due to progressing to a grade that is not available at the prior school. "Percent absent" is the proportion of days absent. "Chronic absent" is an indicator for missing more than 10% of school days. "Retained" is an indicator for the grade in the given year being less than what would be implied by a normal progression since the year before the case (RY-1). "Transferred out of school system" is an indicator for if the student exited the school district and transferred to another school. All outcomes are defined among actively enrolled students, with the exception of "transferred out of school system." Columns (1)-(3) report results for Chicago, (4)-(6) report results for New York City, and (7)-(9) report combined results as described in Section 5.4. The first column reports the non-evicted mean (with standard deviations in parentheses), the second reports the coefficient on an eviction indicator from an OLS regression, and the third reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while OLS and TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes. Table E.2 provides cell-specific observation counts, and Appendix C checks for robustness to excluding the lagged outcomes and to excluding all controls other than the fixed effects.

Table 6: Elementary and Middle School Test Scores (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Case school year:									
Reading test score	-0.305	-0.039***	0.248*	-0.313	-0.017***	0.046	-0.312	-0.020***	0.079
	(0.938)	(0.009)	(0.129)	(0.889)	(0.004)	(0.082)	(0.812)	(0.004)	(0.071)
Math test score	-0.351	-0.045***	-0.141	-0.368	-0.015***	0.026	-0.367	-0.020***	-0.001
	(0.891)	(0.008)	(0.138)	(0.885)	(0.004)	(0.087)	(0.808)	(0.004)	(0.076)
Missed test	0.058	0.004	-0.002	0.052	0.009***	0.030	0.053	0.009***	0.024
	(0.234)	(0.003)	(0.052)	(0.222)	(0.001)	(0.021)	(0.203)	(0.001)	(0.020)
Observations	9,571	27,796	27,796	94,910	141,356	141,356	104,482	169,151	169,151
Post-filing school year 1:									
Reading test score	-0.321	-0.039***	-0.164	-0.309	-0.014***	0.080	-0.310	-0.018***	0.040
	(0.941)	(0.010)	(0.156)	(0.883)	(0.005)	(0.125)	(0.806)	(0.005)	(0.108)
Math test score	-0.358	-0.047***	-0.171	-0.367	-0.023***	0.082	-0.366	-0.027***	0.040
	(0.896)	(0.010)	(0.151)	(0.888)	(0.005)	(0.114)	(0.809)	(0.004)	(0.098)
Missed test	0.057	0.006*	-0.004	0.114	0.016***	0.078***	0.109	0.014***	0.065***
	(0.232)	(0.003)	(0.051)	(0.318)	(0.001)	(0.025)	(0.291)	(0.001)	(0.023)
Observations	8,622	24,933	24,933	86,379	128,636	128,636	95,001	153,569	153,569
Post-filing school year 2:									
Reading test score	-0.319	-0.046***	0.106	-0.303	-0.021***	0.140	-0.305	-0.025***	0.134
	(0.935)	(0.012)	(0.155)	(0.881)	(0.005)	(0.132)	(0.797)	(0.005)	(0.111)
Math test score	-0.344	-0.061***	0.148	-0.368	-0.025***	0.158	-0.365	-0.031***	0.156
	(0.891)	(0.012)	(0.184)	(0.887)	(0.005)	(0.131)	(0.802)	(0.005)	(0.112)
Missed test	0.056	0.001	0.040	0.183	0.008***	0.083**	0.172	0.007***	0.076**
	(0.231)	(0.003)	(0.050)	(0.387)	(0.001)	(0.041)	(0.353)	(0.001)	(0.036)
Observations	8,177	24,120	24,120	76,737	114,926	114,926	84,914	139,046	139,046

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Case school year" is the school year in which the case was filed and the upcoming year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "Reading test score" is the standardized test score on reading tests administered between 3rd and 8th grade (the grades with consistent mandatory testing in our sample), where scores have been standardized to have a mean of zero and standard deviation within each grade-school year for all students enrolled in that grade and school year in the district who took the test. "Math test score" is constructed similarly to the reading test score. "Missed test" is defined as an indicator that is equal to one if a student was actively enrolled in grades 3-8 but does not have one or both test scores. All outcomes are defined among actively enrolled students. Columns (1)-(3) report results for Chicago, (4)-(6) report results for New York City, and (7)-(9) report combined results as described in Section 5.4. The first column reports the non-evicted mean (with standard deviations in parentheses), the second reports the coefficient on an eviction indicator from an OLS regression, and the third reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while OLS and TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes. Table E.2 provides cell-specific observation counts, and Appendix C checks for robustness to excluding the lagged outcomes and to excluding all controls other than the fixed effects.

Table 7: High School Credit Accumulation and GPA (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Case school year:									
Credits	0.898	-0.003	-0.227	0.850	-0.018***	-0.081	0.851	-0.017***	-0.088
	(0.226)	(0.007)	(0.177)	(0.417)	(0.003)	(0.056)	(0.406)	(0.003)	(0.054)
GPA	2.120	-0.051***	-0.428						
	(1.019)	(0.013)	(0.279)						
Observations	2,324	6,137	6,137	47,481	68,604	68,604	48,701	71,768	71,768
Post-filing school year 1:									
Credits	0.900	-0.001	-0.276**	0.830	-0.015***	-0.138**	0.832	-0.015***	-0.144**
	(0.225)	(0.008)	(0.126)	(0.426)	(0.003)	(0.064)	(0.417)	(0.003)	(0.062)
GPA	2.140	-0.058***	-0.291						
	(1.037)	(0.020)	(0.329)						
Observations	2,282	6,054	6,054	51,807	74,312	74,312	53,002	77,454	77,454
Post-filing school year 2:									
Credits	0.910	-0.025***	-0.134	0.824	-0.020***	-0.143*	0.826	-0.020***	-0.143*
	(0.221)	(0.009)	(0.091)	(0.429)	(0.003)	(0.084)	(0.418)	(0.003)	(0.080)
GPA	2.145	-0.080***	-0.243						
	(1.034)	(0.024)	(0.309)						
Observations	2,287	6,243	6,243	50,141	71,766	71,766	51,399	75,165	75,165

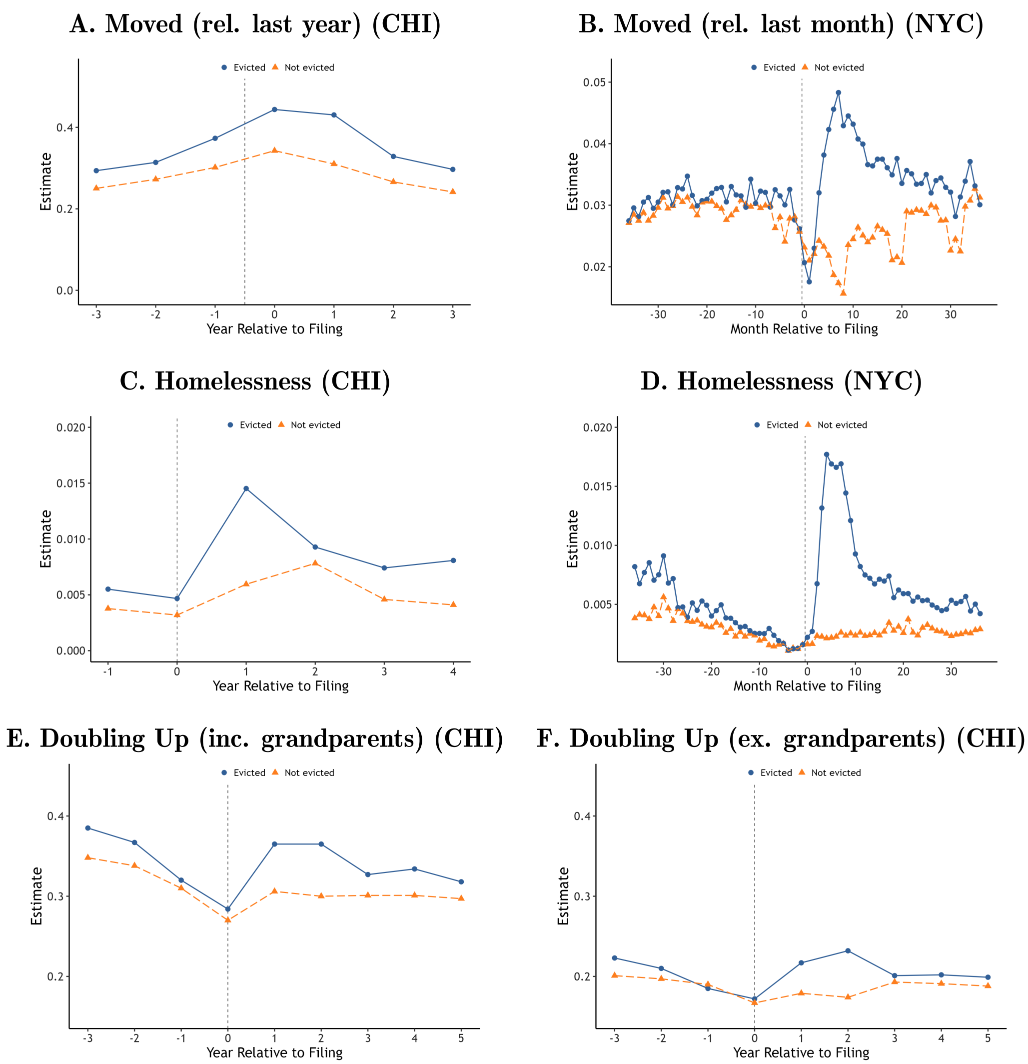
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Case school year" is the school year in which the case was filed and the upcoming year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "Credits" is the number of credits completed divided by the standard number of credits required to progress to the next grade in each district, which is 7 in Chicago and typically 14 in New York. "GPA" is the grade point average of the student, which is only available in Chicago. Both variables are only defined in high school, i.e., grades 9-12, and outcomes are defined among actively enrolled students. Columns (1)-(3) report results for Chicago, (4)-(6) report results for New York City, and (7)-(9) report combined results as described in Section 5.4. The first column reports the non-evicted mean (with standard deviations in parentheses), the second reports the coefficient on an eviction indicator from an OLS regression, and the third reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while OLS and TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes. Table E.2 provides cell-specific observation counts, and Appendix C checks for robustness to excluding the lagged outcomes and to excluding all controls other than the fixed effects.

Table 8: High School Graduation (Education Sample; Filing in Grades 6 to 12)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. OLS and IV estimates									
Graduated	0.753	-0.041***	-0.103	0.670	-0.039***	-0.128**	0.676	-0.039***	-0.125**
	(0.431)	(0.007)	(0.095)	(0.470)	(0.003)	(0.056)	(0.439)	(0.003)	(0.051)
Graduation status not observed	0.207	0.020***	0.085	0.123	0.029***	0.063	0.131	0.027***	0.066*
	(0.405)	(0.006)	(0.092)	(0.329)	(0.002)	(0.039)	(0.302)	(0.002)	(0.036)
Observations	7,628	20,960	20,960	89,700	129,452	129,452	97,328	150,413	150,413
Panel B. Bounds on LATE-AO (90% CIs in parentheses)									
$\delta^\star \in [-0.05, 0.05]$			$[-0.095, -0.086]$			$[-0.119, -0.112]^{*}$			$[-0.115, -0.108]^{**}$
			$(-0.251, 0.07)$			$(-0.207, -0.024)$			$(-0.193, -0.029)$
$\delta^\star \in [-0.1, 0.1]$			$[-0.1, -0.081]$			$[-0.123, -0.109]^{*}$			$[-0.119, -0.104]^{**}$
			$(-0.256, 0.075)$			$(-0.21, -0.02)$			$(-0.196, -0.025)$
$\delta^\star \in [-0.25, 0.25]$			$[-0.114, -0.066]$			$[-0.133, -0.098]^{*}$			$[-0.13, -0.093]^{*}$
			$(-0.275, 0.093)$			$(-0.221, -0.007)$			$(-0.208, -0.012)$

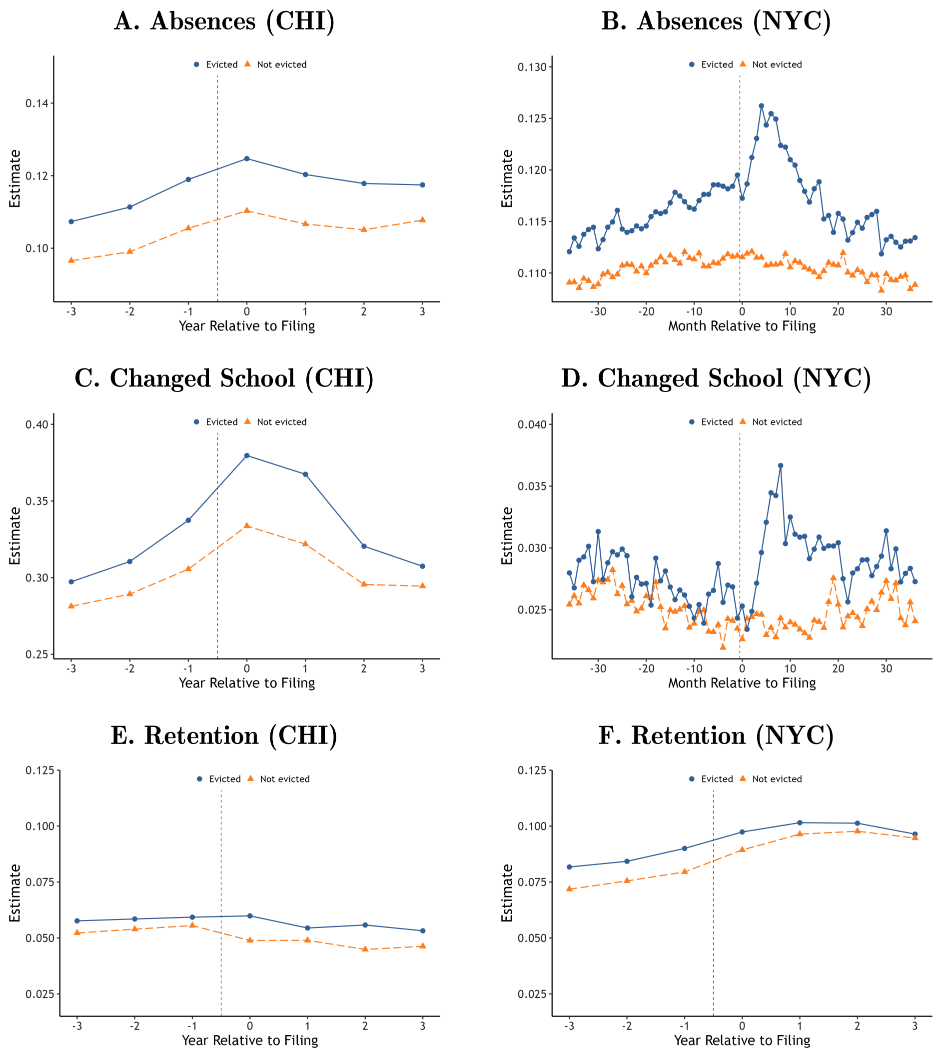
Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Columns (1)-(3) report results for Chicago, (4)-(6) report results for New York City, and (7)-(9) report combined results as described in Section 5.4. In Panel A, "Graduation" is an indicator for if the student graduated conditional on seeing the student through at least age 18. "Graduation status not observed" is an indicator for if graduation status for a student who we could have seen through at least age 18 (i.e. is at least age 18 by the end of our panel) is unknown, predominantly due to moving out of the district. The first column reports the non-evicted mean (with standard deviations in parentheses), the second reports the coefficient on an eviction indicator from an OLS regression, and the third reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while OLS and TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3, except that we do not include outcome-specific lagged outcomes as controls because there is no such measure, and we additionally restrict to students in grades 6-12 during the case school year and who are at least age 18 by the end of our panel. For each column in Panel A, the final row reports the average sample size across outcomes. Table E.5 provides cell-specific observation counts, and Appendix C checks for robustness to excluding all controls other than the fixed effects. In Panel B, we present estimated bounds and 90% confidence intervals for the LATE-AO of graduation as developed in Section 6.4.1 for difference choices of $[\delta_L, \delta_U]$ intervals. The first row takes $[\delta_L, \delta_U] = [-0.05, 0.05]$, the second row takes $[\delta_L, \delta_U] = [-0.1, 0.1]$, and the final row takes $[\delta_L, \delta_U] = [-0.25, 0.25]$. The bound endpoints are estimated using TSLS specifications similar to those in Panel A of this table. Details on estimation and inference can be found in Appendix D.

Figure 1: Housing Environment Relative to Time of Eviction Filing



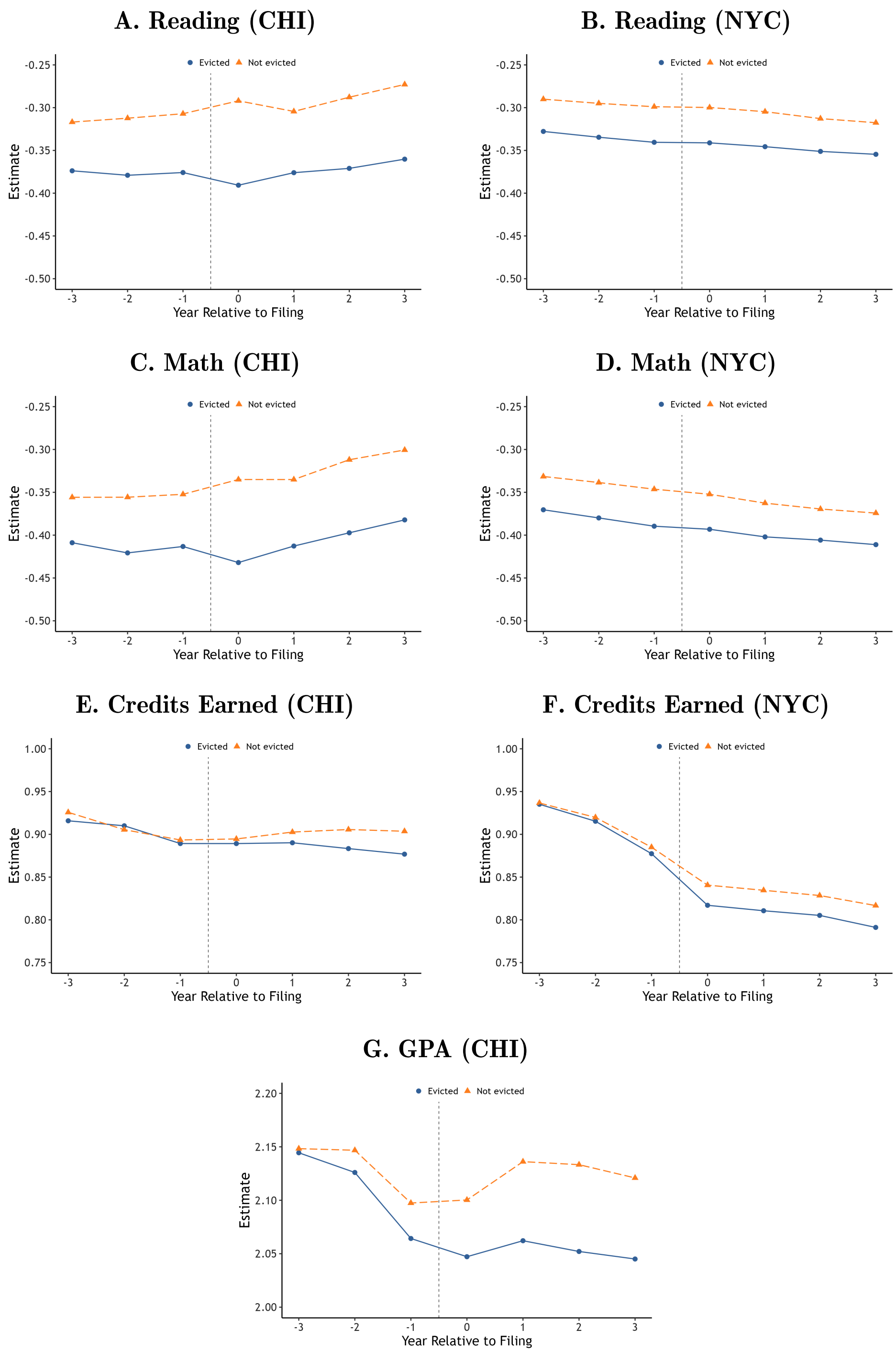
Notes: Each panel displays trends in outcomes relative to eviction filing. Panel A (B) plots trends for the Chicago education (New York education) sample for an indicator for being observed at an address other than the address of residence in the prior year (month). Panel C (D) plots trends for the Census (New York education) sample for being in a homeless shelter in the year (month). Panel E plots trends for the Census sample for doubling up, i.e. living in a household with a household head and an additional adult who is not their cohabiting partner. Panel F plots trends for the Census sample for a measure of doubling up that does not count adults who are the adult parents of the household head. Panels A, B, and D display annual or monthly trends from -3 to 3 years relative to filing for children in the education samples using the panel structure of the data. Panel C displays annual trends from -1 to 4 years relative to filing for children in the Census sample using the panel structure of the homelessness Census data. Panels E and F display annual trends from -3 to 5 years relative to filing for children in the Census sample using variation in the staggered timing of the eviction filing relative to the 2010 Census. See equation 4.1 and related discussions in Section 4.4 for additional details about sample and specification details. Results in Panels C, E, and F are approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R11514.

Figure 2: Schooling Environment Relative to Time of Eviction Filing



Notes: Each panel displays trends in outcomes relative to eviction filing. Panel A (B) plots trends for the Chicago (New York) education sample for the proportion of days absent in the year (month). Panel C (D) plots trends for the Chicago (New York) education sample for an indicator for being observed at a school other than the school in the prior year (month), not counting mechanical school changes due to progressing to a grade that is not available at the prior school. Panel E (F) plots trends for the Chicago (New York) education sample for an indicator for being retained relative to the previous year. All Panels display annual or monthly trends from -3 to 3 years relative to filing for children in the education samples using the panel structure of the data. See equation 4.1 and related discussions in Section 4.4 for additional details about sample and specification details.

Figure 3: Academic Achievement Relative to Time of Eviction Filing



Notes: Each panel displays trends in outcomes relative to eviction filing. Panel A (B) plots trends for the Chicago (New York) education sample for the standardized test score on reading tests administered between 3rd and 8th grade, where scores have been standardized to have a mean of zero and standard deviation within each grade-school year for all students enrolled in that grade and school year in the district who took the test. Panel C (D) plots trends for the Chicago (New York) education sample for the standardized test score on math tests administered between 3rd and 8th grade, where scores have been standardized similarly to the reading test scores. Panel E (F) plots trends for the Chicago (New York) education sample for the number of credits completed divided by the standard number of credits required to progress to the next grade in each city, which is 7 in Chicago and typically 14 in New York. Panel G plots trends for the Chicago education sample for the grade point average, which is only available in Chicago. All Panels display annual trends from -3 to 3 years relative to filing for children in the education samples using the panel structure of the data. See equation 4.1 and related discussions in Section 4.4 for additional details about sample and specification details.

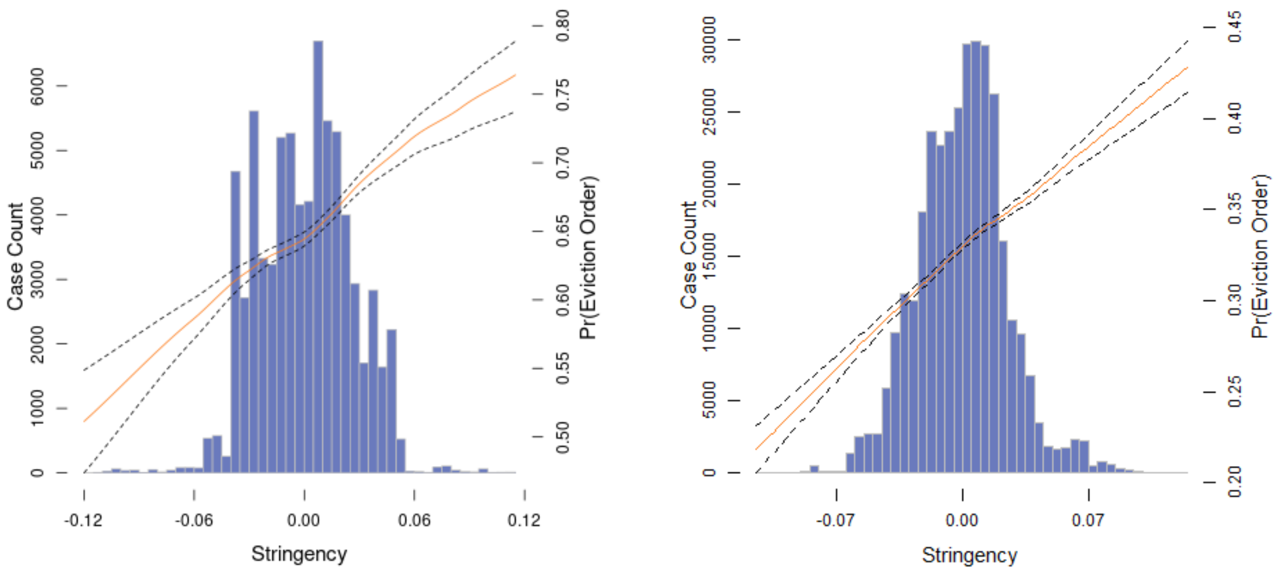
A Additional Tables and Figures

A.1 Validity of the empirical design

First stage. Figure A.1 shows the distribution of judge stringency (residualized by court-year-quarter). Table A.1 presents first stage estimates for our three samples.

Figure A.1: Judge Stringency in the Linked Education Sample

(a) Chicago **(b) New York**



Notes: For each location, this figure shows a histogram of the mean-standardized distribution of judge stringency, $Z_{j(i)}$, with the number of cases indicated along the left vertical axis. Each panel also depicts fitted values of the first-stage regression of eviction on judge stringency and district-year fixed effects (solid line, plotted along the right vertical axis). Dotted lines indicate 95% confidence intervals. The probability of eviction is plotted on the right y-axis against the mean-standardized year-specific judge stringency for judges who see at least 100 cases per year, shown on the x-axis.

Table A.1: First Stage (Education and Census Samples)

	Chicago Public Schools		New York		Census	
	(1)	(2)	(3)	(4)	(5)	(6)
Judge Stringency	1.058***	1.048***	0.842***	0.836***	0.939***	0.931***
	(0.090)	(0.088)	(0.044)	(0.044)	(0.100)	(0.097)
Controls	No	Yes	No	Yes	No	Yes
Observations	74,296	74,296	254,218	254,218	49,000	49,000
F-statistic	137.159	140.646	361.833	362.726	87.510	92.310

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports results from the first stage regression of eviction on judge stringency for Chicago and New York using the linked student sample and the Census analysis sample. Columns (1) and(3) include our judge stringency measure with district-year and age at filing date fixed effects. Columns (2) and (4) add controls (age race, gender age, free/reduced price lunch, and IEP status), three-year average lagged outcomes (percent absent, math and reading scores, indicator for retained), and dummies for missing controls. Column (5) represents a regression at the child-case level of an eviction indicator on judge stringency, and controls only for district-year fixed effects. Column (6) adds the main IV controls. Standard errors are shown in parentheses and are clustered at the judge-year level. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965.

Balance. Table A.2 presents balance tests for the education samples. Table A.3 presents balance tests for the Census sample.

Table A.2: Balance Test (Education Sample)

	Chicago		New York	
	Evicted (1)	Stringency (2)	Evicted (3)	Stringency (4)
Amount Owed	0.013***	0.000	0.061***	0.000
	(0.001)	(0.000)	(0.002)	(0.000)
Tenant has Lawyer	-0.233***	0.000	-0.156***	0.000
	(0.014)	(0.001)	(0.014)	(0.001)
Female	0.003	0.000	0.000	-0.000
	(0.003)	(0.000)	(0.002)	(0.000)
Black	0.022	0.001	-0.025***	-0.000
	(0.016)	(0.001)	(0.006)	(0.000)
Hispanic	0.041**	0.002**	-0.004	-0.000
	(0.017)	(0.001)	(0.007)	(0.000)
Age	-0.003***	0.000	-0.005***	-0.000
	(0.001)	(0.000)	(0.000)	(0.000)
FRP Lunch	0.011	-0.001	0.007**	0.000
	(0.007)	(0.001)	(0.003)	(0.000)
Learning IEP/Disabled	-0.007	-0.000	0.001	-0.000
	(0.009)	(0.000)	(0.003)	(0.000)
Speech IEP/Impairment	-0.004	-0.001	-0.006	-0.000
	(0.013)	(0.001)	(0.004)	(0.000)
IEP			-0.012***	0.000
			(0.004)	(0.000)
Born in NYC			-0.041***	-0.000
			(0.003)	(0.000)
Pct Absent	0.232***	0.001	0.236***	0.000
	(0.028)	(0.001)	(0.012)	(0.001)
Reading Score	-0.013***	0.000	-0.006***	0.000
	(0.005)	(0.000)	(0.002)	(0.000)
Math Score	-0.008*	-0.000	-0.001	-0.000
	(0.005)	(0.000)	(0.002)	(0.000)
Retained	0.015	0.001	-0.007	-0.000
	(0.012)	(0.001)	(0.005)	(0.000)
Observations	74,296	74,296	254,220	254,220
Joint F-statistic	40.544	0.996	64.847	0.831
P-Value	0.000	0.462	0.000	0.691

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Columns (1) and (3) present results from a regression of eviction on student characteristics and lagged outcomes from the school year before the case. Columns (2) and (4) show the same exercise with judge stringency as the dependent variable. All regressions include indicators for each right-hand side variable having a missing value, which are not reported in the table, and fixed effects for district-year of the case. We perform an F-test on the null hypothesis that all coefficients are equal to zero and report the p-value. Standard errors are shown in parentheses and are clustered at the judge-year level.

Table A.3: Balance Test (Census Sample)

	Evicted (1)	Judge stringency (2)
Age at case	-0.000	0.000
	(0.001)	(0.000)
Female	-0.003	-0.000
	(0.004)	(-0.000)
Black	-0.003	-0.001
	(0.014)	(-0.001)
White	0.020	0.000
	(0.016)	(0.000)
Hispanic	0.009	-0.001
	(0.010)	(-0.001)
Single mom (2000)	-0.016	-0.000
	(0.010)	(-0.000)
Single dad (2000)	-0.004	-0.004
	(0.018)	(0.000)
Two parent (2000)	-0.010	-0.010
	(0.014)	(-0.001)
Grandchild of h.h. head (2000)	-0.010	-0.000
	(0.010)	(-0.000)
Doubling up (2000)	0.025***	-0.000
	(0.010)	(-0.000)
Neighborhood poverty rate	-0.152***	-0.002
	(0.022)	(-0.002)
Neighborhood fraction Hispanic	0.229***	0.002
	(0.024)	(0.002)
Neighborhood fraction White	0.111**	0.001
	(0.051)	(0.001)
Neighborhood fraction Black	0.249***	0.002
	(0.047)	(0.002)
Missing neighborhood char.	0.582***	0.004
	(0.166)	(0.004)
Observations	53,000	49,000
F-statistic	19.060	0.760
P-value	0.000	0.717

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. The table above reports tests of balance for our Census analysis sample. The left column presents a regression where the dependent variable is the eviction indicator, which is regressed on our main controls and district-year of the case fixed effects. The right column presents the same regression except the dependent variable is judge stringency. We perform an F-test on the null hypothesis that all coefficients are equal to zero and report the p-value. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965.

Monotonicity checks. We now provide the results of tests of the monotonicity assumption. We first consider the education samples. Table A.4 reports the coefficient on judge stringency from running the first stage on the sample listed in the first column. In both cities, the first stage coefficient is positive for all subsamples and is relatively stable across subsamples. Following Bhuller et al. (2020) and Norris et al. (2021), Table A.5 runs the first stage regression where judge stringency is calculated on one sub-population of kids (e.g., female) and then estimates the first-stage on the complementary sub-population (e.g., not female). The coefficients are all positive. Lastly, Table A.6 replicates these analyses for the Census sample, obtaining the same conclusions.

Table A.4: Single-Sample Monotonicity Checks (Education Sample)

Sample	Chicago (1)	New York (2)
Main	1.066***	0.848***
	(0.095)	(0.04)
Kid is female	1.092***	0.827***
	(0.107)	(0.056)
Kid is Black	1.005***	0.768***
	(0.118)	(0.069)
Kid is Hispanic	1.182***	0.92***
	(0.201)	(0.057)
Defendant is female	1.102***	0.874***
	(0.12)	(0.049)
Defendant is Black	1.028***	0.834***
	(0.134)	(0.071)
Defendant is Hispanic	0.97***	0.876***
	(0.232)	(0.055)
Attorney	0.769**	0.306
	(0.388)	(0.902)
No attorney	1.084***	0.847***
	(0.098)	(0.04)

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports results from Chicago and New York for the first-stage regressions of eviction on judge stringency. We use the IV sample. For each row, we calculate judge stringency using the analysis sample and run the first stage on the subsample listed. Standard errors are shown in the second column and are clustered at the judge(courtroom)-year level.

Table A.5: Split-Sample Monotonicity Checks (Education Sample)

Stringency Sample	First Stage Sample	Chicago (1)	New York (2)
DOE/CPS link	Not DOE/CPS link	0.426***	0.624***
		(0.04)	(0.035)
Not DOE/CPS link	DOE/CPS link	1.188***	0.857***
		(0.104)	(0.047)
Defendant is female	Defendant is not female	0.542***	0.924***
		(0.165)	(0.049)
Defendant is not female	Defendant is female	0.35***	0.676***
		(0.105)	(0.05)
Defendant is black	Defendant is not black	0.341***	0.813***
		(0.117)	(0.055)
Defendant is not black	Defendant is black	0.257***	0.63***
		(0.089)	(0.067)
Defendant is Hispanic	Defendant is not Hispanic	0.326***	0.465***
		(0.108)	(0.071)
Defendant is not Hispanic	Defendant is Hispanic	0.188	0.96***
		(0.152)	(0.059)

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. For each city, this table reports results for first stage regressions of eviction on judge stringency. For each row, we calculate judge stringency using the subsample listed under "Stringency Sample", and we run the first stage on the subsample listed under "First Stage Sample". For example, the first row depicts a first stage on the sample of cases linked to our education records, with our measure of judge stringency calculated based on the sample of cases not linked to education records. Sample restrictions include judges who see 50 or more cases in addition to IVOLS restrictions. "Joint Action Case" is an indicator for if the case was a joint action case and is specific to Chicago. Standard errors are depicted in parentheses and are clustered at the judge(courtroom)-year level.

Table A.6: Single- and Split- Sample Monotonicity Checks (Census Sample)

Sample	Coefficient	Standard Errors	Observations
Male	1.037***	0.117	24,500
Male (out-of-sample instrument)	0.996***	0.152	26,500
Female	0.830***	0.106	24,500
Female (out-of-sample instrument)	0.793***	0.113	26,500
Age K-12	0.915***	0.101	37,500
Age K-12 (out-of-sample instrument)	0.875***	0.126	41,000

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. The table above reports monotonicity tests of the Census analysis sample. The first row reports the first-stage regression for the subgroup of male children. The second row reports the first-stage regression for the male subgroup, using all other cases to construct the judge stringency instrument. The next rows perform the same exercise for the subgroup of female children, and for the subgroup of children aged 6-18 (the K-12 subgroup). Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965.

A.2 Estimating proportion of households facing eviction with children

Table A.7: Census Sample: Estimating the Proportion of Households Facing Eviction With Children

	2010 Decennial	2000 Decennial
	(1)	(2)
Share of households with children		
Option 1: Randomized	0.558	0.630
Option 2: Household head	0.534	0.597
Option 3: Female first	0.560	0.631
Number of children per household (among those with children)		
Option 1 (random)	2.342	2.472

Notes: The table above merges tenants in case years indicated in the column heading to the Decennial Census year indicated in the column heading. In cases with more than one tenant listed, we select one tenant according to three different rules (indicated in the row): (i) randomly choosing the tenant, (ii) choosing the Census household head, (iii) choosing the female first and if there are multiple female adults we choose one at random. We then report the proportion of these tenants with children aged 0-18 in the household, and, in the last row, the number of children per household (for those with children). Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965.

A.3 Additional details and results on trends around eviction filing

Figures 1-3 and the figures in this Appendix display regression estimates of β_r and $\beta_r + \delta_r$ from the regression in (4.1), with the non-evicted group mean in the omitted period added to both sets of coefficients. As discussed in Section 4.4, the exact specification depends on whether we consider annual data from the education sample, monthly data from the education sample, or annual data from the Chicago Census sample. However, in all cases, we can interpret the plotted estimates as means that have been re-weighted to match the time and case location characteristics of the non-evicted group in the omitted period. To see why, let $X_{i,-1}$ collect the additional fixed effects in (4.1) for a student i in the omitted period and assume (4.1) is correctly specified so that

$$\mathbb{E}[Y_{i,r}|E_i = e, X_{i,-1}] = \alpha + \sum_{r=-3;r\neq-1}^{3} \beta_r + \sum_{r=-3}^{3} \delta_r \times \mathbb{1}[E_i = e] + \gamma X_{i,-1}.$$

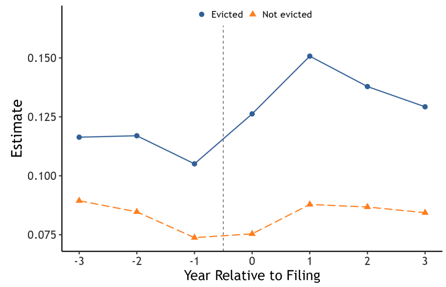
In this notation, $\mathbb{E}\left[\mathbb{E}[Y_{i,r}|E_i = e, X_{i,-1}]\middle|E_i = 0\right]$ is the mean of $Y_{i,r}$ for the non-evicted group in the baseline period. Using the above equation, this value equals

$$\begin{aligned}\mathbb{E}\left[\mathbb{E}[Y_{i,r}|E_i = e, X_{i,-1}]\middle|E_i = 0\right] =& \beta_r \mathbb{1}[r \neq -1] + \delta_r \mathbb{1}[e = 1] + \mathbb{E}[\alpha + X_{i,-1}|E_i = 0] \\ =& \beta_r \mathbb{1}[r \neq -1] + \delta_r \mathbb{1}[e = 1] + \mathbb{E}[Y_{i,-1}|E_i = 0],\end{aligned}$$

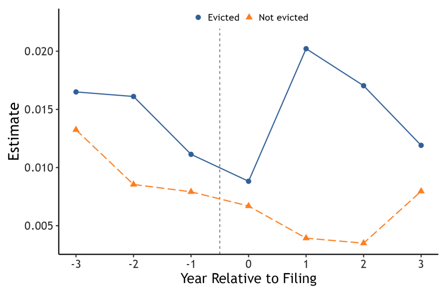
which is what we are plotting (noting that we set $\beta_{-1} = 0$ when plotting). Similar arguments hold for the monthly education sample and annual Census sample specifications.

Figure A.2: Chicago Education Sample: McKinney-Vento

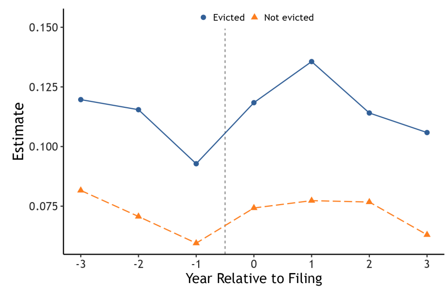
A. McKinney-Vento Flag (CHI)



B. McKinney-Vento Shelter (CHI)



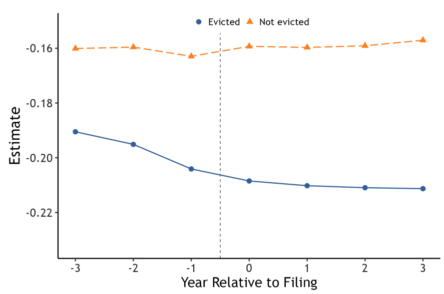
C. McKinney-Vento Doubling Up (CHI)



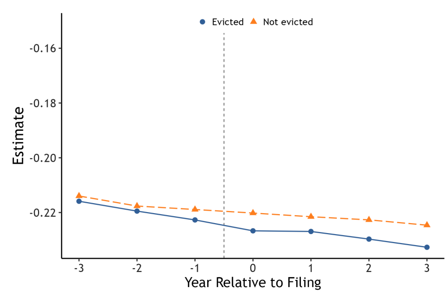
Notes: Each panel displays annual trends from -3 to 3 years relative to eviction filing for children in the Chicago education sample using the panel structure of the data. Panels A plots trends for a McKinney-Vento flag for student homelessness, which primarily indicates living doubled-up or at a homeless shelter. The definition of "doubled-up" that is used to determine a student's McKinney-Vento status is "children and youths who are sharing the housing of other persons due to loss of housing, economic hardship, or a similar reason" (42 U.S.C. Section 11434(b)(2)). Panels B and C plot trends separately for each of these two living situations. See equation 4.1 and related discussions in Section 4.4 for additional details about the sample and specification.

Figure A.3: Education Sample: School Average Test Scores

A. School's Avg. Test Scores (CHI)

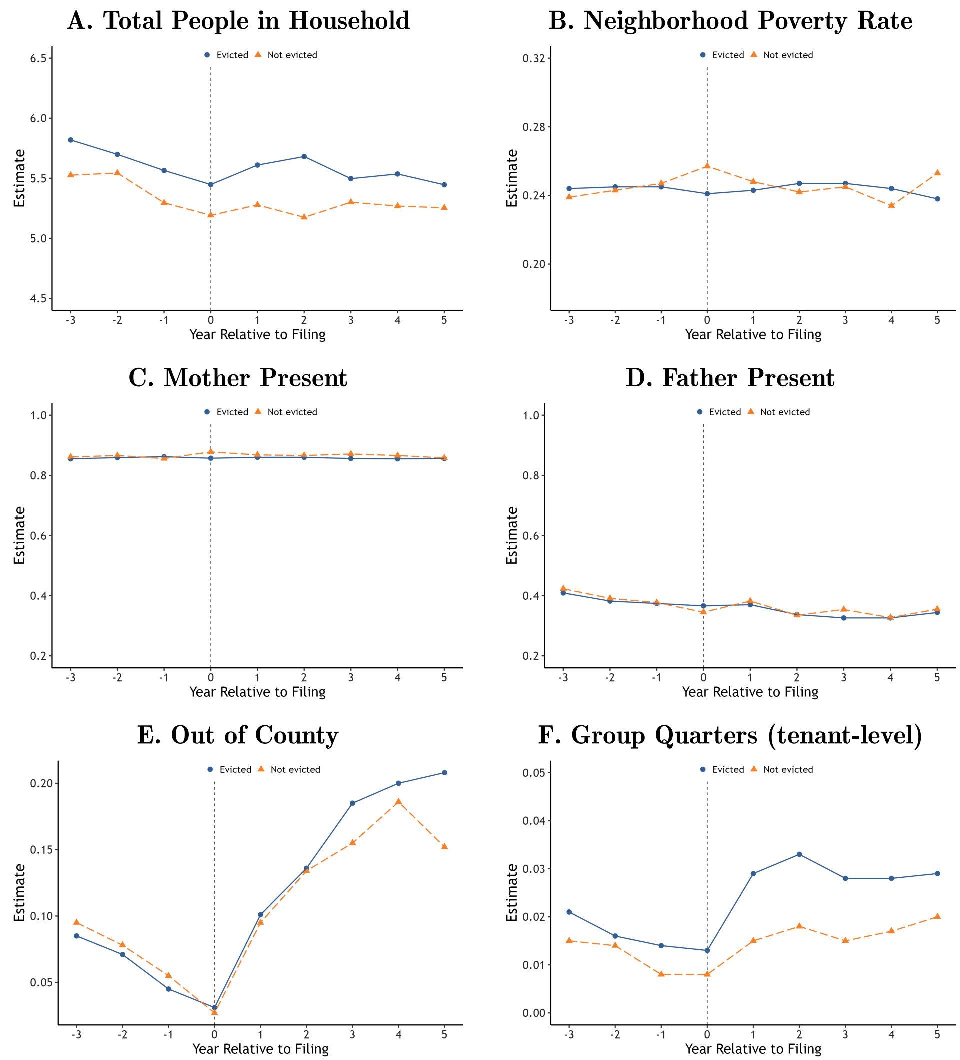


B. School's Avg. Test Scores (NYC)



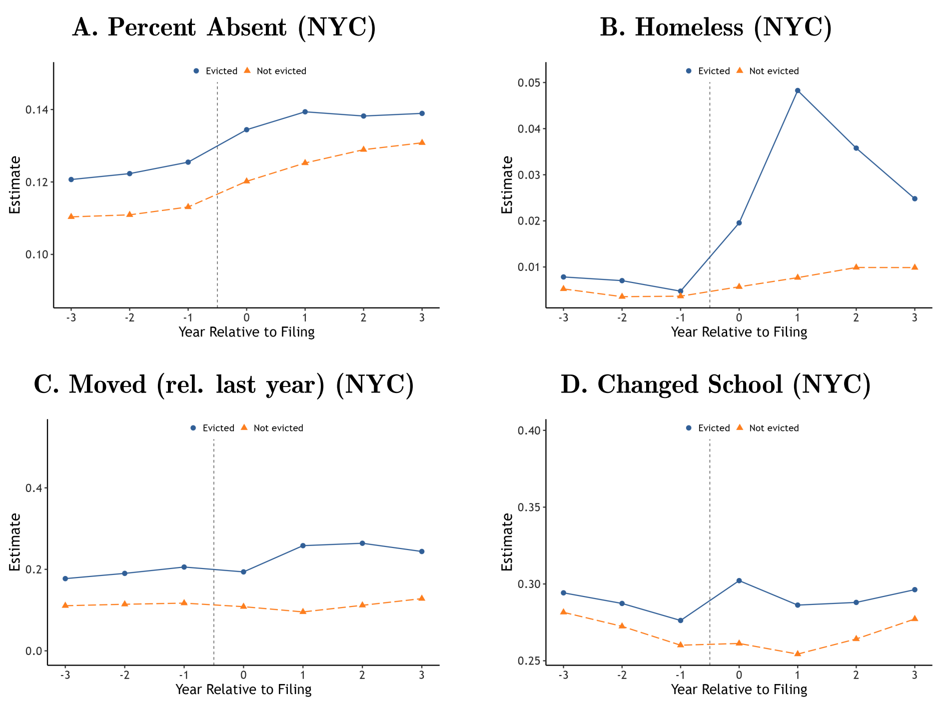
Notes: Each panel displays trends in outcomes relative to eviction filing. Each panel displays trends in outcomes relative to eviction filing. Panel A (B) plots trends for the Chicago (New York) education sample for the average achievement of the school attended as measured by average test scores and is defined among actively enrolled students. See equation 4.1 and related discussions in Section 4.4 for additional details about the sample and specification.

Figure A.4: Census Sample: Additional Trends



Notes: The panels above use variation in the year of filing relative to the 2010 Census to present trends for children around the time of filing. The sample includes all children age 18 and under (at the time of filing and at the time of the 2010 Census) in households with a tenant linked to the 2010 Census. The panels plot child-case averages in relative time for evicted and nonevicted groups. No controls are included in this regression. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R11514.

Figure A.5: New York Education Sample: Yearly Trends



Notes: Each panel displays annual trends from -3 to 3 years relative to eviction filing for children in the New York education sample using the panel structure of the data. Panel A presents the results for the percentage of days the student was absent during the year. Panel B displays the results for an indicator reflecting applications to homeless shelters within the year. Panel C shows the results for an indicator for being observed at an address other than the address of residence in the prior year. Panel D reports the results for an indicator of a change in the school attended compared to the previous year. See equation 4.1 and related discussions in Section 4.4 for additional details about the sample and specification.

A.4 Characterizing compliers

Table A.8: Characterizing Compliers

	Chicago			New York		
	Evicted	Non-evicted	Complier	Evicted	Non-evicted	Complier
Panel A. Characteristics						
Female	0.491	0.491	0.501	0.49	0.492	0.479
			(0.031)			(0.039)
Black	0.764	0.769	0.764	0.396	0.429	0.432
			(0.05)			(0.05)
Ad damnum	2109	1739	1675	4407	3764	2415
			(280)			(1223)
Ad damnum below median	0.466	0.563	0.621	0.421	0.542	0.525
			(0.06)			(0.049)
Panel B. Pre-case outcomes						
Not at pre-case address (in -2)	0.379	0.302	0.317	0.212	0.11	0.096
			(0.05)			(0.025)
Retained (in -1)	0.058	0.052	0.032	0.117	0.122	0.088
			(0.019)			(0.03)
Chronic absent (in -1)	0.414	0.359	0.3	0.445	0.406	0.41
			(0.068)			(0.043)
Percent absent (in -1)	0.117	0.105	0.098	0.113	0.105	0.108
			(0.016)			(0.008)
Reading test score (in -1)	-0.372	-0.307	-0.256	-0.34	-0.307	-0.385
			(0.101)			(0.101)
Math test score (in -1)	-0.411	-0.352	-0.226	-0.394	-0.362	-0.362
			(0.091)			(0.105)
Credits earned (in -1)	0.889	0.893	0.981	0.887	0.907	0.92
			(0.287)			(0.055)
GPA (in -1)	2.046	2.097	2.12			
			(0.336)			
Panel C. Case school year outcomes if not evicted						
Not at pre-case address		0.344	0.342		0.11	0.079
			(0.061)			(0.026)
Chronic absent		0.382	0.342		0.414	0.444
			(0.062)			(0.039)
Percent absent		0.113	0.122		0.126	0.128
			(0.015)			(0.011)
Reading test score		-0.305	-0.376		-0.313	-0.393
			(0.112)			(0.083)
Math test score		-0.351	-0.284		-0.368	-0.479
			(0.106)			(0.083)
Credits earned		0.898	1.083		0.856	0.902
			(0.104)			(0.061)
GPA		2.12	2.385			
			(0.282)			

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. This table reports evicted, non-evicted, and complier means in the Chicago and New York education samples. Evicted and non-evicted means for Y_i (denoting either a characteristic or outcome) are estimated from sample averages. The complier mean for Y_i is estimated from a TSLS regression with $Y_i(1-E_i)$, treatment $(1-E_i)$, instrument $Z_{j(i)}$, and the same fixed effects as our main IV specification (we omit controls because Y_i is often one of these controls). With a binary instrument (and no controls), this TSLS estimand identifies $\mathbb{E}[Y_i(0)|\text{complier}]$, irrespective of whether Y_i is binary, discrete, or continuous (Abadie, 2003). When Y_i is a treatment-invariant characteristic, this is the mean of that characteristic for compliers, and when Y_i is an outcome, this is the non-evicted mean potential outcome for compliers. Panel A variables are defined as in Table 2, with the additions of "Ad damnum" denoting the amount the landlord is claiming the tenant owes in the case and "Ad damnum below median" denoting an indicator for the ad damnum being less than the median amount. Panel B variables are defined as in Table 2, and are evaluated either in the year prior to the case year (RY-1) or the year before (RY-2). Panel C variables are defined as in Tables 3-7, and are evaluated in the "case school year" (i.e., the school year in which the case was filed and the upcoming year for cases filed in the summer). The samples are restricted to the education analysis samples described in Section 5.1, and, in particular, remove cases that are not randomly assigned or assigned to judges/courtrooms that hear substantially fewer cases than is typical in the setting, which is why some of the evicted and non-evicted moments do not exactly match those in Table 2.

A.5 What predicts graduation?

Table A.9 studies what intermediate outcomes predict high school graduation, focusing on outcomes measured in grades 6-8 (middle school), as well as 9th grade, where we observe additional outcomes, such as credit scores and GPA. The regression is not restricted to children facing eviction. The regression is restricted to individuals for whom graduation and the intermediate outcomes are observed, as described in the table notes. The first column considers only math and reading test scores (averaged across grades 6-8). When considering only test scores, we find that they explain 6 percent of the variation in the graduation outcome in Chicago and 13 percent in New York and have sizable coefficients (with a one standard deviation increase in test scores predicting a 4 to 13 percentage point increase in the probability of graduating). When only considering the proportion of days absent, a chronic absenteeism indicator, and changing residential address in middle school, they explain 11 percent of the variation in graduation rates in Chicago and 23 percent in New York. When including all these measures in a single regression, they explain 14 and 27 percent of the variation in the outcome, respectively. These three regressions imply that absenteeism and residential mobility better predict graduation than test scores, and adding middle school absenteeism and mobility to the regression with test scores notably increases the R-squared.

The fourth and fifth columns of Table A.9 additionally add 9th-grade credits earned, GPA, proportion of days absent, and changing residential address. Column 4 omits test scores yet has large R-squared values of 0.36 for Chicago and 0.53 for New York. Column 5 adds middle-school test scores to the prior regression and only marginally increases its predictive power, with the R-squared rising by 0.0005 in Chicago and 0.005 in New York. Similarly, the coefficient on test scores are substantially smaller than those discussed in the prior paragraph, ranging from -0.0024 (and statistically insignificant) for math scores in Chicago to 0.026 for reading scores in New York. Overall, these results suggest that the outcomes we measure in middle school and 9th grade are quite predictive of graduation.

One question is how our IV estimates on intermediate outcomes compare to our estimates on high school graduation. Using Table A.9 and our IV estimates for the intermediate schooling and housing outcomes, we can construct a back-of-the-envelope estimate of how impacts on intermediate outcomes may affect graduation. Consider a student who is evicted before the start of 8th grade. First, we will consider the regression in column (3) of Table A.9 that includes middle school math and reading scores, percent of days absent, a chronic absenteeism indicator, and an indicator for changing residential addresses. We can multiply the combined IV estimates for each of these outcomes in post-filing school year 1 by the weighted average of the regression coefficients in column (3) for Chicago and New York.[37] Doing so yields a predicted change in graduation of -5.0 percentage points, though this does not include intermediate outcomes such

[37] We use the weights used to combine our IV estimates for high school graduation. Results are similar when using equal weights.

as credits or GPA, which are only observed in high school.

Second, we calculate the predicted change in graduation using the more complete set of predictors in column (5) of Table A.9. As this column uses predictors from middle school and high school, we will need to use IV estimates from both post-filing year 1 and post-filing year 2. Specifically, as we are considering a student evicted before the start of 8th grade, we use the IV estimates for post-filing year 1 for the middle school outcomes (i.e., those in Panel A), as this is when the student would be in 8th grade, and post-filing year 2 for the 9th-grade outcomes (i.e., those in Panel B) as this is when the student would be in 9th grade. We then multiply these combined IV estimates by the coefficients in column 5 of Table A.9, where doing so yields a predicted change in graduation of -12.0 percentage points, which is similar to the -12.5 percentage point reduction in high school graduation we estimate. Overall, this exercise suggests that the impact on intermediate outcomes we estimate can broadly rationalize our estimated change in graduation rates.

Table A.9: Middle and High School Predictors of Graduation

	Chicago					New York				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Intercept	0.8637	0.9774	0.9531	0.31	0.3125	0.7881	0.9732	0.9422	0.2431	0.269
	(0.002)	(0.0033)	(0.0034)	(0.0127)	(0.0127)	(6e-04)	(8e-04)	(8e-04)	(0.0035)	(0.0035)
Panel A. Grades 6-8 variables										
Math test score	0.0566***		0.0285***		-0.0024	0.1324***		0.0564***		0.0085***
	(0.0036)		(0.0035)		(0.0032)	(9e-04)		(9e-04)		(7e-04)
Reading test score	0.031***		0.0303***		0.0139***	0.0374***		0.0498***		0.0259***
	(0.0037)		(0.0035)		(0.0031)	(9e-04)		(8e-04)		(7e-04)
Percent absent		-1.9082***	-1.5759***	-0.176**	-0.1833**		-2.1211***	-1.764***	-0.2858***	-0.2227***
		(0.0828)	(0.0828)	(0.0752)	(0.0754)		(0.0184)	(0.0173)	(0.0137)	(0.0137)
Chonic absent		-0.1191***	-0.1174***	-0.063***	-0.0599***		-0.1515***	-0.1362***	-0.0346***	-0.0354***
		(0.0146)	(0.0144)	(0.0125)	(0.0125)		(0.0038)	(0.0036)	(0.0028)	(0.0028)
Changed address		-0.0518***	-0.0276***	-0.0109	-0.0082		-0.0269***	-0.0085***	-0.0106***	-0.0048***
		(0.0092)	(0.0091)	(0.0079)	(0.0079)		(0.0022)	(0.0022)	(0.0018)	(0.0017)
Panel B. Grade 9 variables										
Credits				0.5627***	0.5785***				0.761***	0.7247***
				(0.0136)	(0.014)				(0.0034)	(0.0034)
GPA				0.0374***	0.0307***					
				(0.002)	(0.0025)					
Percent absent				-0.4981***	-0.4951***				-0.1762***	-0.204***
				(0.0289)	(0.0289)				(0.0053)	(0.0053)
Chonic absent				-0.0388***	-0.0391***				-0.0849***	-0.0791***
				(0.0059)	(0.0059)				(0.0019)	(0.0019)
Changed address				0.003	0.0033				-0.009***	-0.0066***
				(0.0043)	(0.0043)				(0.0014)	(0.0014)
R squared	0.0621	0.1104	0.1356	0.3552	0.356	0.1317	0.2286	0.2721	0.5273	0.5319
Observations	25546	25558	25546	25558	25546	462787	462787	462787	462787	462787

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The columns of this table report coefficients from regressions of various variables on an indicator for graduating. In all cases, the sample is all students – irrespective of interacting with the court system – in CPS (for Chicago) and NYDOE (for New York) who: (i) are observed in grades 6-9 and have non-missing test score, absenteeism, mobility, and credit variables, (ii) do not transfer out of the district, and (iii) are age 18+ by the end of our panels. For this sample, we observe average test scores, absenteeism, and mobility in grades 6-8, we observe credits, GPA (for Chicago), absenteeism, and mobility in grade 9, and we observe final graduation status (graduated, dropped out, or still enrolled). For each city, the five columns report regressions of different subsets of the considered variables on graduation: column (1) reports coefficients from a regression that only includes average math and reading test scores in grades 6-8, column (2) reports coefficients from a regression that only includes average absenteeism and mobility in grades 6-8, column (3) reports coefficients from a regression that includes all considered variables in grades 6-8, column (4) reports coefficients form a regression that only includes average absenteeism and mobility in grades 6-8 and all considered variables in grade 9, and column (5) reports coefficients from a regression that includes all considered variables in grades 6-8 and grade 9. All variables enter the regressions linearly, and no fixed effects are included. Note that the observation count for Chicago are small (relative to the number of students in CPS) because credits are only observed in 2014- while the panel ends in 2019, so that, for this exercise, we effectively consider students who are in 9th grade in 2014-2015 and able to graduate in 2019.

A.6 Additional IV/OLS results

Table A.10: School Average Test Scores (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	IV (3)	$\mathbb{E}[Y\|E=0]$ (4)	OLS (5)	IV (6)	$\mathbb{E}[Y\|E=0]$ (7)	OLS (8)	IV (9)
Case school year:									
School's Avg. Test Scores	-0.160	-0.022***	-0.055	-0.226	-0.002**	0.014	-0.219	-0.007***	-0.001
	(0.446)	(0.003)	(0.047)	(0.369)	(0.001)	(0.022)	(0.332)	(0.001)	(0.020)
Observations	17,403	50,573	50,573	140,058	188,074	188,074	157,461	238,647	238,647
Post-filing school year 1:									
School's Avg. Test Scores	-0.158	-0.026***	-0.015	-0.230	-0.001	0.063*	-0.221	-0.007***	0.046
	(0.454)	(0.004)	(0.053)	(0.378)	(0.001)	(0.036)	(0.338)	(0.001)	(0.030)
Observations	14,709	42,480	42,480	114,214	152,106	152,106	128,923	194,586	194,586
Post-filing school year 2:									
School's Avg. Test Scores	-0.152	-0.034***	0.032	-0.230	-0.004*	0.056	-0.220	-0.011***	0.051
	(0.459)	(0.005)	(0.059)	(0.388)	(0.002)	(0.048)	(0.346)	(0.002)	(0.040)
Observations	12,094	35,271	35,271	89,188	118,029	118,029	101,282	153,300	153,300

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "Case school year" is the school year in which the case was filed and the following year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "School average test score" is the average achievement of school attended as measured by average test scores and is defined among actively enrolled students. Columns (1)-(3) report results for Chicago, (4)-(6) report results for New York City, and (7)-(9) report combined results as described in Section 5.4. The first column reports the non-evicted mean (with standard deviations in parentheses), the second reports the coefficient on an eviction indicator from an OLS regression, and the third reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while OLS and TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3.

Table A.11: More on High School Graduation (Education Sample; Filing in Grades 6 to 12)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	IV (3)	$\mathbb{E}[Y\|E=0]$ (4)	OLS (5)	IV (6)	$\mathbb{E}[Y\|E=0]$ (7)	OLS (8)	IV (9)
Dropped out	0.233	0.038***	0.080	0.212	0.028***	0.134**	0.213	0.029***	0.128**
	(0.423)	(0.007)	(0.089)	(0.409)	(0.003)	(0.056)	(0.382)	(0.003)	(0.051)
Graduated on time	0.711	-0.040***	-0.083	0.568	-0.037***	-0.044	0.578	-0.037***	-0.049
	(0.453)	(0.007)	(0.091)	(0.495)	(0.003)	(0.067)	(0.463)	(0.003)	(0.060)
Observations	6,021	16,202	16,202	83,384	119,604	119,604	89,405	135,807	135,807

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "Case school year" is the school year in which the case was filed and the following year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed."Dropped out" is an indicator for if the student dropped out of high school, conditional on seeing the student through at least age 18. Dropping out is not the exact reverse of graduating because some students may appear as "still enrolled" in the education records. "Graduated on time" is an indicator for students who graduate within four years of first entering 9th grade and is defined for students who are actively enrolled in 9th grade at some point in our panels. Columns (1)-(3) report results for Chicago, (4)-(6) report results for New York City, and (7)-(9) report combined results as described in Section 5.4. The first column reports the non-evicted mean (with standard deviations in parentheses), the second reports the coefficient on an eviction indicator from an OLS regression, and the third reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while OLS and TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3. For each column, the final row reports the average sample size across outcomes.

Table A.12: Living Arrangements, Household Structure, and Geography (Census Sample; ages 6-18)

	$\mathbb{E}[Y \mid E=0]$ (1)	OLS (2)	IV (3)
Living Arrangements			
Total household size	4.809	0.139***	0.808
		(0.025)	(0.543)
Doubling up (incl. grandparents)	0.212	0.029***	0.254***
		(0.006)	(0.081)
Doubling up (excl. grandparents)	0.134	0.010**	0.192***
		(0.005)	(0.072)
Household Structure			
Mother present	0.855	-0.025***	0.006
		(0.004)	(0.065)
Father present	0.302	0.004	-0.004
		(0.005)	(0.093)
Single mother	0.577	-0.029***	0.006
		(0.006)	(0.094)
Non-relative household head	0.015	0.003**	-0.004
		(0.001)	(0.026)
Multigenerational household	0.090	0.017***	0.146***
		(0.003)	(0.058)
Grandparent household head	0.082	0.017***	0.074
		(0.003)	(0.060)
Geography			
Neighborhood poverty rate	0.234	-0.002	-0.054**
		(0.002)	(0.027)
Out of county	0.224	0.016***	-0.078
		(0.005)	(0.078)
Out of state	0.133	0.016***	0.146
		(0.003)	(0.059)
Observations		40,000	37,000

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. This table reproduces Table 4 except restricts the sample to age 6-18, to allow for comparison with the education K-12 analysis. Observation counts reflect the modal observation counts. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965.

Table A.13: Neighborhood and Multigenerational Households (Census Sample)

	Pr[Lower poverty neighborhood] (1)	Pr[Lower poverty neighborhood $\cap$ multigen] (2)	Pr[Lower poverty neighborhood $\cap$ non-multigen] (3)
IV: Evicted	0.099	0.062*	0.037
	(0.066)	(0.036)	(0.075)
Dependent variable mean	0.371	0.037	0.333

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. This table estimates the impact of eviction on living in a lower-poverty Census tract in 2010 relative to the case address (column 1), using our IV specification of Table 4. We interact this outcome with an indicator for the 2010 household being multigenerational and estimate the impact on this interacted outcome in column 2; we also interact this outcome with an indicator for the 2010 household not being multigenerational and estimate the impact on this interacted outcome in column 3. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965.

A.7 Grade at filing IV splits

Appendix Tables A.14–A.15 show pooled (across Chicago and New York) education results separately for younger students with case filings during grades 1-5 (capturing students with filings during elementary school) and for older students with case filings during grades 6-12 (capturing students with filings during middle and high school). We only present home environment and schooling attachment and engagement results because the other main tables present outcomes are primarily observed for only one of these two groups (e.g., credits are only observed for students in grades 9-12). We focus on the combined estimate due to limited sample sizes in the site-by-grade results.

Table A.14: Home Environment by Grade at Filing (Education Sample)

	Grades 1-5 at Filing		Grades 6-12 at Filing		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	P-value of
	(1)	(2)	(3)	(4)	(2) = (4)
Case school year:					
Not at pre-case address	0.147	0.191***	0.118	0.053	0.031
	(0.303)	(0.048)	(0.269)	(0.043)	
Neighborhood poverty	0.306	0.012	0.300	0.001	0.610
	(0.109)	(0.015)	(0.106)	(0.016)	
Observations	78,494	130,087	82,713	130,620	
Post-filing school year 1:					
Not at pre-case address	0.238	0.209***	0.200	0.043	0.074
	(0.373)	(0.061)	(0.337)	(0.071)	
Number of moves	0.360	0.378***	0.295	0.011	0.020
	(0.623)	(0.107)	(0.558)	(0.115)	
Neighborhood poverty	0.305	0.022	0.300	0.016	0.827
	(0.110)	(0.018)	(0.106)	(0.019)	
Observations	67,959	111,674	58,685	92,820	
Post-filing school year 2:					
Not at pre-case address	0.315	0.176**	0.276	0.195**	0.880
	(0.412)	(0.084)	(0.373)	(0.098)	
Number of moves	0.636	0.611***	0.528	0.201	0.214
	(0.975)	(0.207)	(0.876)	(0.257)	
Neighborhood poverty	0.302	0.039*	0.300	-0.017	0.071
	(0.110)	(0.022)	(0.105)	(0.022)	
Observations	58,660	95,568	38,525	61,421	

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "Case school year" is the school year in which the case was filed and the following year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "Not at pre-case address" is an indicator for not being at the same address as the pre-case school year. "Number of moves" is the total number of residential address changes recorded by the district since the pre-case school year. "Neighborhood poverty" is the poverty rate of the census tract of residence based on 5-year ACS data. All outcomes are defined among actively enrolled students. Columns (1)-(2) report Chicago and New York combined results for students with case filings in grades 1-5, and (3)-(4) report combined results for students with case filings in grades 6-12. Combined results are constructed as described in Section 5.4. The first column reports the non-evicted mean, and the second reports the TSLS estimate for eviction. Means are accompanied by standard deviations in parentheses, while TSLS estimates are accompanied by standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes.

Table A.15: Schooling Attachment and Engagement by Grade at Filing (Education Sample)

	Grades 1-5 at Filing		Grades 6-12 at Filing		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	P-value of (2) = (4)
Case school year:					
Not at pre-case school	0.147	0.061	0.169	0.058	0.966
	(0.309)	(0.045)	(0.342)	(0.039)	
Percent absent	0.088	0.009	0.152	0.009	0.960
	(0.070)	(0.007)	(0.171)	(0.013)	
Chronic absent	0.338	0.078	0.461	0.060	0.798
	(0.443)	(0.053)	(0.470)	(0.045)	
Transferred out of school system	0.039	-0.006	0.035	0.020	0.440
	(0.166)	(0.027)	(0.157)	(0.019)	
Observations	86,177	142,178	113,996	175,384	
Post-filing school year 1:					
Not at pre-case school	0.261	0.014	0.286	0.135***	0.126
	(0.389)	(0.059)	(0.417)	(0.052)	
Percent absent	0.087	0.014*	0.172	0.034**	0.300
	(0.072)	(0.008)	(0.193)	(0.017)	
Chronic absent	0.327	0.044	0.493	0.136***	0.222
	(0.440)	(0.056)	(0.472)	(0.050)	
Retained	0.089	0.082***	0.153	0.000	0.086
	(0.261)	(0.031)	(0.338)	(0.036)	
Transferred out of school system	0.045	-0.022	0.038	0.027	0.106
	(0.167)	(0.025)	(0.156)	(0.017)	
Observations	81,411	133,609	99,894	153,026	
Post-filing school year 2:					
Not at pre-case school	0.348	0.026	0.456	0.158**	0.200
	(0.421)	(0.073)	(0.464)	(0.073)	
Percent absent	0.088	0.010	0.190	0.036	0.325
	(0.077)	(0.009)	(0.208)	(0.024)	
Chronic absent	0.328	0.000	0.521	0.119**	0.142
	(0.441)	(0.062)	(0.472)	(0.052)	
Retained	0.117	0.071*	0.187	0.060	0.855
	(0.295)	(0.038)	(0.366)	(0.047)	
Transferred out of school system	0.047	0.028	0.038	0.031	0.927
	(0.166)	(0.025)	(0.148)	(0.019)	
Observations	75,979	124,404	82,011	125,571	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Case school year" is the school year in which the case was filed and the following year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "Not at pre-case school is an indicator for being enrolled at a different school relative to the school in the pre-case school year, not counting mechanical school changes due to progressing to a grade that is not available at the prior school. "Percent absent" is the proportion of days absent."Chronic absent" is an indicator for missing more than 10% of school days. "Retained" is an indicator for the grade in the given year being less than what would be implied by a normal progression since the year before the case (RY-1). "Transferred out of school system" is an indicator for if the student exited the school district and transferred to another school. All outcomes are defined among actively enrolled students, with the exception of "transferred out of school system." Columns (1)-(2) report Chicago and New York combined results for students with case filings in grades 1-5, and (3)-(4) report combined results for students with case filings in grades 6-12. Combined results are constructed as described in Section 5.4. The first column reports the non-evicted mean (with standard deviations in parentheses), and the second reports the TSLS estimate for eviction. Means include standard deviations in parentheses, TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes.

A.8 Gender IV splits

Appendix Tables A.16–A.21 show pooled (across Chicago and New York) education results separately by child gender. We focus on the combined estimate due to limited sample sizes in the site-by-gender results.

Table A.16: Home Environment by Gender (Education Sample)

	Girls		Boys		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	P-value of (2) = (4)
Case school year:					
Not at pre-case address	0.134	0.134***	0.134	0.128***	0.921
	(0.286)	(0.047)	(0.285)	(0.042)	
Neighborhood poverty	0.304	0.012	0.303	0.006	0.774
	(0.106)	(0.015)	(0.106)	(0.016)	
Observations	82,230	134,134	84,967	138,566	
Post-filing school year 1:					
Not at pre-case address	0.222	0.150**	0.223	0.124**	0.757
	(0.355)	(0.061)	(0.356)	(0.059)	
Number of moves	0.331	0.279***	0.333	0.151	0.374
	(0.592)	(0.107)	(0.593)	(0.097)	
Neighborhood poverty	0.304	0.026	0.302	0.012	0.567
	(0.107)	(0.019)	(0.106)	(0.017)	
Observations	64,908	105,458	66,676	108,616	
Post-filing school year 2:					
Not at pre-case address	0.303	0.252***	0.300	0.105	0.219
	(0.395)	(0.085)	(0.395)	(0.085)	
Number of moves	0.597	0.770***	0.594	0.075	0.018
	(0.932)	(0.213)	(0.936)	(0.202)	
Neighborhood poverty	0.303	0.023	0.301	0.016	0.803
	(0.107)	(0.025)	(0.106)	(0.020)	
Observations	50,006	81,334	51,472	83,911	

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "Case school year" is the school year in which the case was filed and the following year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "Not at pre-case address" is an indicator for not being at the same address as the pre-case school year. "Number of moves" is the total number of residential address changes recorded by the district since the pre-case school year. "Neighborhood poverty" is the poverty rate of the census tract of residence based on 5-year ACS data. All outcomes are defined among actively enrolled students. Columns (1)-(2) report Chicago and New York combined results for girls, (3)-(4) report results for boys. Combined results are constructed as described in Section 5.4. The first column reports the non-evicted mean, and the second reports the TSLS estimate for eviction. Means are accompanied by standard deviations in parentheses, while TSLS estimates are accompanied by standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes.

Table A.17: Living Arrangements, Household Structure, and Geography by Gender (Census Sample)

	Girls		Boys		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	P-value of (2) = (4)
Living Arrangements:					
Total household size	4.820	1.255*	4.860	0.233	0.250
		(0.709)		(0.535)	
Doubling up (incl. grandparents)	0.214	0.283**	0.225	0.084	0.207
		(0.134)		(0.083)	
Doubling up (excl. grandparents)	0.131	0.204	0.143	0.026	0.279
		(0.148)		(0.072)	
Household Structure:					
Mother present	0.870	0.101	0.847	-0.049	0.185
		(0.077)		(0.083)	
Father present	0.298	-0.102	0.318	0.084	0.268
		(0.143)		(0.088)	
Single Mother	0.583	0.040	0.560	-0.052	0.591
		(0.135)		(0.105)	
Non-relative household head	0.016	-0.011	0.015	0.007	0.708
		(0.041)		(0.025)	
Multigenerational household	0.096	0.183***	0.098	0.091	0.309
		(0.065)		(0.063)	
Grandparent household head	0.085	0.159***	0.086	0.048	0.188
		(0.063)		(0.056)	
Geography:					
Neighborhood poverty rate	0.237	-0.089**	0.232	-0.021	0.213
		(0.045)		(0.031)	
Out of county	0.234	-0.087	0.234	0.007	0.499
		(0.111)		(0.084)	
Out of state	0.138	-0.037	0.140	-0.008	0.795
		(0.081)		(0.077)	
Observations		24,000		24,000	

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. This table reports results for the Census sample (Cook County) of two-stage least squares (IV) regressions as in Table 4 except it presents estimates separately by the child's gender. Observation counts reflect the modal observation counts. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965.

Table A.18: Schooling Attachment and Engagement by Gender (Education Sample)

	Girls		Boys		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	P-value of (2) = (4)
Case school year:					
Not at pre-case school	0.158	0.030	0.167	0.118***	0.123
	(0.325)	(0.040)	(0.332)	(0.041)	
Percent absent	0.122	0.007	0.128	0.012	0.742
	(0.136)	(0.010)	(0.142)	(0.012)	
Chronic absent	0.401	0.013	0.423	0.116**	0.145
	(0.456)	(0.045)	(0.460)	(0.054)	
Transferred out of school system	0.034	-0.015	0.039	0.019	0.289
	(0.152)	(0.023)	(0.164)	(0.023)	
Observations	101,974	162,966	104,544	167,401	
Post-filing school year 1:					
Not at pre-case school	0.272	-0.013	0.283	0.160***	0.031
	(0.400)	(0.056)	(0.405)	(0.057)	
Percent absent	0.130	0.014	0.137	0.033**	0.363
	(0.151)	(0.014)	(0.157)	(0.014)	
Chronic absent	0.408	0.018	0.430	0.163***	0.053
	(0.457)	(0.049)	(0.462)	(0.056)	
Retained	0.112	0.040	0.142	0.012	0.562
	(0.291)	(0.035)	(0.322)	(0.033)	
Transferred out of school system	0.038	-0.031	0.046	0.035*	0.043
	(0.150)	(0.026)	(0.168)	(0.020)	
Observations	92,233	146,822	94,948	151,389	
Post-filing school year 2:					
Not at pre-case school	0.383	-0.030	0.404	0.170**	0.042
	(0.437)	(0.067)	(0.443)	(0.072)	
Percent absent	0.136	0.008	0.143	0.040**	0.205
	(0.160)	(0.017)	(0.165)	(0.019)	
Chronic absent	0.413	0.028	0.438	0.075	0.566
	(0.458)	(0.057)	(0.463)	(0.060)	
Retained	0.138	0.051	0.174	0.061	0.861
	(0.317)	(0.039)	(0.350)	(0.040)	
Transferred out of school system	0.040	0.032	0.047	0.026	0.840
	(0.147)	(0.023)	(0.162)	(0.021)	
Observations	80,497	128,360	83,192	132,863	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Case school year" is the school year in which the case was filed and the following year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "Not at pre-case school is an indicator for being enrolled at a different school relative to the school in the pre-case school year, not counting mechanical school changes due to progressing to a grade that is not available at the prior school. "Percent absent" is the proportion of days absent."Chronic absent" is an indicator for missing more than 10% of school days. "Retained" is an indicator for the grade in the given year being less than what would be implied by a normal progression since the year before the case (RY-1). "Transferred out of school system" is an indicator for if the student exited the school district and transferred to another school. All outcomes are defined among actively enrolled students, with the exception of "transferred out of school system." Columns (1)-(2) report Chicago and New York combined results for girls, (3)-(4) report combined results for boys. Combined results are constructed as described in Section 5.4. The first column reports the non-evicted mean (with standard deviations in parentheses), and the second reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes.

Table A.19: Elementary and Middle School Test Scores Test Scores by Gender (Education Sample)

	Girls		Boys		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	P-value of
	(1)	(2)	(3)	(4)	(2) = (4)
Case school year:					
Reading test score	-0.179	-0.004	-0.443	0.140	0.311
	(0.768)	(0.101)	(0.836)	(0.101)	
Math test score	-0.315	-0.105	-0.418	0.072	0.254
	(0.781)	(0.108)	(0.831)	(0.112)	
Missed test	0.044	0.002	0.061	0.046*	0.232
	(0.186)	(0.027)	(0.217)	(0.025)	
Observations	51,501	83,266	52,946	85,832	
Post-filing school year 1:					
Reading test score	-0.173	-0.148	-0.444	0.123	0.170
	(0.769)	(0.148)	(0.823)	(0.131)	
Math test score	-0.312	-0.001	-0.419	0.041	0.824
	(0.786)	(0.127)	(0.828)	(0.140)	
Missed test	0.098	0.052*	0.119	0.085**	0.442
	(0.279)	(0.028)	(0.302)	(0.033)	
Observations	46,761	75,467	48,201	78,047	
Post-filing school year 2:					
Reading test score	-0.167	0.185	-0.442	0.044	0.514
	(0.754)	(0.153)	(0.820)	(0.154)	
Math test score	-0.312	0.278**	-0.418	0.038	0.264
	(0.782)	(0.141)	(0.819)	(0.162)	
Missed test	0.158	0.073*	0.185	0.077**	0.952
	(0.342)	(0.044)	(0.363)	(0.039)	
Observations	42,072	68,641	42,820	70,368	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Case school year" is the school year in which the case was filed and the following year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "Reading test score" is the standardized test score on reading tests administered between 3rd and 8th grade (the grades with consistent mandatory testing in our sample), where scores have been standardized to have a mean of zero and standard deviation within each grade-school year for all students enrolled in that grade and school year in the district who took the test. "Math test score" is constructed similarly to the reading test score. "Missed test" is defined as an indicator that is equal to one if a student was actively enrolled in grades 3-8 but does not have one or both test scores. All outcomes are defined among actively enrolled students. Columns (1)-(2) report Chicago and New York combined results for girls, (3)-(4) report combined results for boys. Combined estimates are constructed as described in Section 5.4. The first column reports the non-evicted mean (with standard deviations in parentheses), and the second reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes.

Table A.20: High School Credit Accumulation by Gender (Education Sample)

	Girls		Boys		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	P-value of (2) = (4)
Case school year:					
Credits	0.895	-0.080	0.805	-0.151	0.750
	(0.391)	(0.071)	(0.418)	(0.210)	
Observations	24,845	36,704	23,815	35,005	
Post-filing school year 1:					
Credits	0.874	-0.099	0.788	-0.243**	0.290
	(0.402)	(0.082)	(0.427)	(0.107)	
Observations	26,804	39,226	26,176	38,191	
Post-filing school year 2:					
Credits	0.870	-0.067	0.782	-0.277**	0.205
	(0.405)	(0.104)	(0.427)	(0.130)	
Observations	25,801	37,750	25,577	37,386	

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "Case school year" is the school year in which the case was filed and the following year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "Credits" is the number of credits completed divided by the standard number of credits required to progress to the next grade in each district, which is 7 in Chicago and typically 14 in New York. "Credits" is only defined in high school, i.e. grades 9-12, and outcomes are defined among actively enrolled students. Columns (1)-(2) report Chicago and New York combined results for girls, (3)-(4) report combined results for boys. Combined estimates are constructed as described in Section 5.4. The first column reports the non-evicted mean (with standard deviations in parentheses) and the second reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3.

Table A.21: High School Graduation by Gender (Education Sample; Filing in Grades 6 to 12)

	Girls		Boys		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	P-value of (2) = (4)
Graduation	0.729	-0.051	0.621	-0.211***	0.112
	(0.417)	(0.064)	(0.456)	(0.078)	
Graduation status not observed	0.122	0.073	0.140	0.057	0.819
	(0.294)	(0.047)	(0.310)	(0.051)	
Observations	48,846	75,638	48,429	74,706	

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "Case school year" is the school year in which the case was filed and the following year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "Graduation" is an indicator for if the student graduated conditional on seeing the student through at least age 18. "Graduation status not observed" is an indicator for if graduation status for a student who we could have seen through at least age 18 (i.e., at least age 18 by the end of our panel) is unknown, predominantly due to moving out of the district. Columns (1)-(2) report Chicago and New York combined results for girls, (3)-(4) report combined results for boys. Combined estimates are constructed as described in Section 5.4. The first column reports the non-evicted mean (with standard deviations in parentheses) and the second reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3, except that we do not include outcome-specific lagged outcomes as controls because there is no such measure, and we additionally restrict to students in grades 6-12 during the case school year and who are at least age 18 by the end of our panel. For each column and time period, the final row reports the average sample size across outcomes.

B Data and Linkage

B.1 Court data

This subsection provides details about the Chicago and New York court record data sets. These data sets are largely the same as those used by Collinson et al. (2024), except that we observe additional years of data. In what follows, we describe the data and sample restrictions we impose.

B.1.1 Overview of the data

We use the near-universes of court records for Chicago (for the period 2000-2023) and for New York (for the period 2007-2017). For both cities, court records include the residential address of the disputed housing unit and the names of tenant(s). Other key elements we observe in the court records include case type, filing date, courtroom and date assignment, name of the landlord, attorneys' names, the amount claimed by the landlord (ad damnum amount), and whether an eviction order was granted. Below, we describe the court variables that are most relevant for our analyses.

- *Filing date* is the date the landlord filed the eviction case.
- *Tenant name(s)* are the names of the tenants listed by the landlord when filing the case. Names are cleaned to standardize case and punctuation.
- *Case address* is the property address provided by the landlord when filing the case. In Chicago, addresses are standardized using the SmartyStreet address standardization API. In New York, addresses are standardized using HUD's Geocoding Service Center, which uses Pitney and Bowes' Core-1 Plus address-standardizing software.
- *District* is the district in which the case is filed. Both Cook County and New York divide their regions into multiple court districts, each with their own court house and eviction courtrooms. Landlords cannot choose the district in which they file their case and are required to file eviction cases in the district in which the property is located.
- *Judge* is the judge that is assigned to the case. In both Chicago and New York, cases are randomly assigned to a courtroom and time slot. Since judges are designated to specific courtrooms, the randomization to courtroom and time effectively randomizes judge assignment. In Chicago, our data includes the identity of the judge overseeing that courtroom at the date and time of the case. In New York, we do not observe the identity of the judge assigned to the case and instead use the term 'judge' to refer to the

courtroom assigned to the case. We use the judge or courtroom assigned at the time of filing, regardless of any subsequent changes in judge assignment.

- *Eviction* is an indicator for the case ending in eviction. We construct this using docket entries indicating either an order for possession or a ruling in favor of the plaintiff (landlord). See Collinson et al. (2024) for more details.
- *Has attorney* is an indicator for the tenant being represented by an attorney.
- *Case type* describes whether the landlord is only seeking possession of the property (single action in Chicago, holdover in New York) or whether the landlord is seeking payment of rental arrears in addition to possession of the property (joint action in Chicago, nonpayment in New York).
- *Ad damnum* is the amount of money the plaintiff (landlord) is seeking in payment of rental arrears upon filing joint action in Chicago or nonpayment in New York. We windsorize any amounts greater than \$100,000, as these may partially reflect entry errors and extreme outliers.

B.1.2 Baseline sample restrictions

The following restrictions mirror the restrictions imposed by Collinson et al. (2024).

Chicago. The full sample consists of 675,747 cases. We impose a few baseline restrictions. First, we impose that the defendant is not a business and the property is not a condo. We drop businesses because we are interested in the impacts of eviction on residential tenants. We drop condos as they typically represent the eviction of condo owners, rather than renters. Second, we drop cases with missing names or only "unknown occupants," as we are not able to link these cases to our other samples. Third, we drop the rare cases with more than \$100,000 in damages, as there is a small right tail involving very large damages, which we believe are a combination of entry errors and outlier cases. Lastly, we drop cases for which we cannot determine whether the case ends with eviction or not (see Collinson et al. (2024) for details). These restrictions result in a sample of 600,914 cases, which corresponds to our baseline sample prior to linking.

New York. The full sample consists of 1,192,988 calendared cases. We impose a few baseline restrictions, outlined in (Collinson et al., 2024) and described below. First, we restrict the sample to cases involving residential property, excluding businesses because we are interested in the impacts of eviction on residential tenants. We also drop cases involving condos or co-ops, as these are not randomly assigned to courtrooms. We use annual administrative data from the New York City Department of Finance to identify buildings with condos or co-ops. Second, we drop cases with missing names, as we are not able to link these cases to our other samples. Finally, we drop cases involving the public housing authority (NYCHA) because these are not randomly-assigned to courtrooms These restrictions result in a sample of 774,464 cases, which corresponds to our baseline sample prior to linking.

B.2 Education data

This subsection provides details about Chicago and New York education data. We describe the data, their linkage to court records, and the sample restrictions we impose. We then discuss variable construction and the time indexing of the data.

B.2.1 Overview of the data

The calendar year can be divided into the academic term, which includes the fall and spring seasons, and the summer. In both Chicago and New York, the academic term has typically run from early September to late June, so we use September 1 to June 30 to define the academic term and July 1 to August 31 to define summer. We index the school year based on the year of the spring term, with the previous summer also belonging to that school year. For example, the 2009 school year includes the summer of 2008 and concludes with the spring term of 2009.

Chicago. We use administrative schooling records from Chicago Public Schools (CPS). The data contains student-year level records from 2000-2019, for grades K-12. As we discuss below, we only observe address data for student-year records in the 2003-2019 calendar years, which corresponds to records for 1,246,650 unique students. We can only link these students to eviction records.

New York. We use administrative schooling records from New York City's Department of Education (NYCDOE). The NYCDOE contains student-year level records from 2005-2018, for grades K-12. We observe panel data for 2,859,983 unique students.

B.2.2 Linkage and sample restrictions

Chicago. The link between the baseline sample of court records and student records was conducted by Chapin Hall based on names and addresses from both data sets. The baseline sample of court records contains the names of the tenants on the case, the case address, and the case filing date. CPS records contain, for each school year and season in the 2003-2019 calendar years, the names of enrolled students, their residential address, and the names of any parents or guardians. Prior to linkage, the following steps were implemented:

- Names and addresses from CPS were cleaned and standardized following similar procedures to those implemented for court records. When a record had multiple first or last names, multiple aliased records were created with each name.
- All string fields were converted to uppercase, and soundex codes were calculated for fields representing student last name, guardian first name, guardian last name, street name, and city name.

BigMatch software and human supervision were jointly used to define a match as any permutation of CPS and Court records where all the following were satisfied:

- The date of the CPS address record was between 120 and 0 days prior to the court filing data;
- House number, street number, direction, address, suffix, and city showed acceptable agreement based on fuzzy matching and human supervision for setting weights and cutoffs on linkage metrics; and
- Either (i) guardian name matched Court defendant name in soundex or (ii) for the subset of years when unit numbers were captured in CPS data, student last name matched the defendant's and the address either included no unit number or, if it did, it matched the unit from the Court record.

Given these matches, we de-duplicate student-case pairs that arise when a student is linked to more than one defendant in the same case (but we keep observations of children linked to more than one case).

This linkage results in 77,256 unique student-case matches, comprised of 58,650 unique students and 46,530 unique cases. We use this linked sample for our analysis of trends around an eviction case filing. The 46,530 unique linked cases are part of the 488,761 unique cases from our baseline court data that were filed between the 2003-2019 calendar years (and thus are eligible to be potentially linked to CPS data). The implied linkage rate of 9.5% is likely substantially below the share of families who face an eviction filing, in part because we require that the children attend CPS schools in the months prior to the eviction filing. This rate is also likely an under-estimate of the share of families with children in CPS who face eviction filing due to our strict matching criteria that were designed to increase the chances of avoiding measurement error.

For our IV/OLS analyses, we impose two additional restrictions similar to those imposed by Collinson et al. (2024). First, we impose that a single judge can be clearly identified from the randomly assigned room and time, and that the district had at least two active judges seeing cases during the week of the initial hearing. These restrictions help guarantee that we can identify the judge and that there was more than one judge to whom the case could have been assigned. Second, we impose that the assigned judge saw at least 100 cases that year, which ensures that the judge sees a sufficient number of cases to accurately estimate stringency. These reductions results in 74,733 unique student-case matches, comprised of 56,999 unique students and 44,998 unique cases. We use this sample in our IV/OLS analyses.

Because the linkage is based on address information prior to the case filing date, it should not be correlated with the randomly assigned judge. To examine this possibility, Table B.1 reports the estimates of regressions of the indicator for the case being linked to a student separately on eviction status and on the instrument. All regressions include judge×year fixed effects, and we consider specifications both with and without the court controls included in our IV/OLS results (indicator for the tenant being without an attorney and the ad damnum amount). In both specifications, the coefficient on the stringency instrument is statistically insignificant.

Table B.1: Linked to CPS records

	(1)	(2)	(3)	(4)
Evicted	0.0010	0.0018		
	(0.0013)	(0.0013)		
Stringency			0.0242	0.0242
			(0.0223)	(0.0222)
No attorney		-0.0263^{***}		-0.0261^{***}
		(0.0035)		(0.0035)
Ad Damnum		-0.0020^{***}		-0.0020^{***}
		(0.0001)		(0.0001)
Court controls	No	Yes	No	Yes
Observations	431,044	431,044	431,044	431,044

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Court records are restricted to the analysis samples used in our IV/OLS regressions that were eligible to be linked to CPS records (i.e., filed in the 2003-2019 calendar years). All regressions include district-year fixed effects. Standard errors are clustered at the judge-year level.

New York. The link between the baseline sample of court records and student records was conducted with the help of the Center for Innovation through Data Intelligence (CIDI) working on premise at the DOE using names and addresses from both data sets. The baseline sample of court records contains the names of the tenants on the case, the case address, and the case filing date. The K-12 records contain, for each school year from 2005-2017, the names of enrolled students, their residential address, and the names of any parents or guardians. Prior to linkage, the following steps were implemented:

- Names from DOE records were cleaned and standardized to remove non-numeric characters

- Addresses were geocoded to the building level and assigned a building identification number using the NYC Geobat geocoding tool
- A soundex transformation of last names was created for blocking in the record linking procedure

The record linkage then proceeded in the following manner:

- In the first step, tenants from the court records were exact matched on building identification number and soundex of the last name to the school records
- In the second step, all matches with move-in dates *after* the eviction filing were discarded
- In the third step, Jaro-Winkler string distances were calculated between names in the court and school records; and only records with first name and last name scores exceeding 0.87 were retained as a tenant matches
- In the final step, any student who was discharged from the district *before* the filing date was discarded

As in Collinson et al. (2024), we apply three sample restrictions for our IV/OLS analysis. First, we drop cases arising in courts with only one judge (Staten Island) or in one of two specialized courts in Red Hook and Harlem, since we cannot construct the instrument for these cases. Second, we drop cases where the courtroom (and hence the judge) are not randomly assigned. These include cases involving the public housing authority, cases assigned based on zip code through several policy initiatives, and cases involving drugs or members/family members of the active military. Third, we require that the courtroom hears at least 500 total cases in a year, which results in a very small number of cases being dropped. Functionally, this restriction drops courtrooms that are used only sporadically, which may feature non-random set of cases.

After applying sample restrictions, the resulting linked sample consists of 278,879 unique student-case matches, comprised of 206,789 unique students and 185,509 unique cases. We use this linked sample for all of our analysis.

To examine whether linkage is correlated with the instrument, Table B.1 reports the estimates of regressions of the indicator for the case being linked to a student separately on eviction status and on the instrument. All regressions include district-year fixed effects, and we consider specifications with and without the court controls included in our IV/OLS results (indicator for the tenant being without an attorney and the ad damnum amount). In both specifications, the coefficient on the instrument is statistically insignificant.

Table B.2: Linked to DOE records

	(1)	(2)	(3)	(4)
Evicted	-0.017	-0.018		
	(0.0015)	(0.0015)		
Stringency			-0.025	-0.024
			(0.018)	(0.018)
No Attorney		0.090		0.088
		(0.0066)		(0.0066)
Ad Damnum		-1.5e-06		-2.5e-06
		(6.8e-06)		(6.3e-06)
Observations	774,464	774,451	774,464	774,451

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Court records are restricted to the analysis samples used in our IV/OLS regressions. All regressions include district-year fixed effects. Standard errors are clustered at the judge-year level.

B.2.3 Variable construction and time indexing

Since most outcomes in the schooling data are defined annually over the entire academic year, while evictions filings occur throughout the calendar year, we need to map school outcomes into years relative to eviction filing. We index results to the school year and take relative year 0 (RY0) as the school year in which the case is filed. If the case is filed in the summer, RY0 is the academic year that starts that fall. For homelessness outcomes derived from HMIS records, we know the specific dates the outcome occurs, so we define RY0 as the first 365 days after the case filing. Lagged relative years (i.e., RY-1, RY-2, etc.) and lead relative years (i.e., RY1, RY2, etc.) are defined relative to RY0.

We use the panel data to construct variables in different ways based on the nature of the data. First, we define time-invariant characteristics, such as gender and race. We then construct time-variant variables. Lastly, to define high school completion variables, we consider the last observed status given our panel data. We now discuss the construction of variables in more detail.

Time-invariant characteristics. These variables are observed for all students in our linked sample (see Appendix Table B.3 for years of availability of this data by city). We consider the following variables:

- *Gender* is an indicator for the student's reported gender being female.
- *Race and ethnicity* are the reported race and ethnicity of the student. For Chicago it takes one of the following values: Black, Hispanic, White, or other[38]. Similarly, for New

[38]This is constructed from a single field collected by CPS that captures the following mutually exclusive race/ethnic categories: Black/African American, Hispanic, White, Asian, Native American/Alaskan, Hawaiian/Pacific Islander, and Multi-Racial.

York, race and ethnicity are consolidated, but may also take the value “Asian” or “Native American.”

- *Free or reduced lunch* for Chicago and New York is an indicator for the student qualifying for free or reduced lunch in a school year prior to the one that contains the case filing date.[39]
- *Individualized Education Program (IEP) and disability status* takes values for overall IEP status and for corresponding learning accommodations. In Chicago, IEP codes are used to distinguish students with speech-related accommodations, emotional or behavioral disorders, or other learning considerations such as dyslexia or autism. For New York, we consider an indicator for IEP, and indicators for the following disability statuses: learning disability, emotional disability, and speech impediment.
- *Language status* is observed for New York and is used to construct indicators for Spanish-speaking and for other-language-speaking.
- *Born in New York City* is observed for New York and is an indicator for being born in New York City.

Time-varying variables. All time-varying variables—except for homelessness outcomes derived from HMIS records (and transferring out for New York)—are defined over the academic term. Because the indexing depends on whether a case occurs during the academic term (the fall and spring seasons) or during the summer, we discuss each separately. To capture the potential for eviction to have an immediate impact on students, we focus on cases filed during the academic term and define relative (school) year 0 (RY0) as the school year during which the case is filed. Lagged relative years (i.e., RY-1, RY-2, etc.) and lead relative years (i.e., RY1, RY2, etc.) are defined relative to RY0. In this way, RY1 is the first full academic term after an eviction filing. For instance, a case filed in the fall of 2010 or the spring of 2011 occurs during the 2011 school year, and thus RY-1 is 2010, RY0 is 2011, and RY1 is 2012.

For cases filed during the summer, we define RY0 as the school year with the academic term that begins right after the summer. Lagged relative years (i.e., RY-1,RY-2,etc.) and lead relative years (i.e., RY1, RY2, etc.) are defined relative to RY0. For instance, consider a case filed in the summer of 2011. Such a case occurs during the 2012 school year, and thus RY-1 is 2011 (the school year that ended before the case filing), RY0 is 2012 (the school year that begins after the case filing), and RY1 is 2013.

For homelessness outcomes derived from HMIS records (and transferring out for New York), we know the specific dates the outcome occurs, so we define RY0 as the first 365 days after

[39] For precise definitions of school years relative to the case filing date, see the next paragraph on school year indexing.

the case filing. Lagged relative years (i.e., RY-1, RY-2, etc.) and lead relative years (i.e., RY1, RY2, etc.) are defined relative to RY0.

We use this indexing to define the following variables for relative year t (RYt) where, unless specified otherwise, we implicitly condition on the student being enrolled in RYt (see Appendix Table B.3 for years of availability of the below variables):

Based on characteristics data:

- *Age (in RYt)* is the age in RYt, and is known irrespective of enrollment at t.

Based on enrollment data:

- *Grade (in RYt)* is the grade (K-12) in RYt.
- *Predicted grade in RYt* is the grade in RYt implied by a normal progression from their grade in RY-1. This is equal to the grade in RY-1 plus (t+1).
- *Not at pre-case school (in RYt)* is an indicator that is one if the school identifier in RY-1 is different from the school identifier in RYt, not counting mechanical school changes due to progressing to a grade that is not available at the prior school.
- *Number of school changes (in RYt)* is the sum of year-on-year school changes starting from RY-1 and ending with RYt, where a school change is defined as in the previous bullet. Thus, not at pre-case school in RY0 and number of school changes in RY0 are equal.
- *Transferred out of school system (in RYt)* is an indicator that is one if the student is marked as having transferred out of the school system in RYt. This variable does not condition on being enrolled in RYt.

Based on address data:

- *Not at pre-case address (in RYt)* for Chicago is an indicator that is one if the distance between the latitude and longitude coordinates in RY-1 and RYt exceeds a minimum threshold, which we set to 100m and which is not sensitive to the choice of threshold.[40] We consider the address measured in the spring season of the school year. For New York, it is an indicator that is one if the census block in RY-1 is different from the census block in RYt. The addresses are recorded in June of he respective school year.
- *Number of moves (in RYt)* is the sum of year-on-year address changes starting from RY-1 and ending with RYt, where an address change is defined as in the previous bullet. Thus, not at pre-case address in RY0 and number of moves in RY0 are equal.

[40]Chapin Hall used actual address data when conducting the link between court records and CPS student records, but this data is not available for subsequent use.

- *Neighborhood poverty (in RYt)* for Chicago is the tract-level percent of households below the federal poverty line obtained from American Community Survey (ACS) 5-year aggregates. Because each estimate spans 5 years, we match each school year with the 5-year estimate for which it is the midpoint. For instance, if RYt has school year 2014, we pull the poverty rate from the 2012-2016 5-year ACS.[41] For New York, we rely on ACS 5-year estimates for the period 2006-2010.

Based on McKinney-Vento data:

- *McKinney Vento (in RYt)* for Chicago is a flag that comes from two sources. First, a binary indicator of homelessness is available in the student "master file" records. Second, starting in school year 2016, data is available from the Students in Temporary Living Situations (STLS) program, which includes both a binary indicator of student status as well as additional information on temporary living arrangements (such as homeless shelter or being doubled-up) and whether the student is accompanied by an adult. McKinney-Vento data is not observed for New York.

Based on HMIS data:

- *Homelessness (in RYt)* for New York is an indicator for the student child being listed on a family application for shelter bed as recorded by the Department of Homeless Services. HMIS data is not linked to CPS data, and is thus not directly observed for Chicago. However, as described in Appendix B.3, we do measure it for a different sample of children.

Based on school attendance data:

- *Percent absent (in RYt)* is the number of days absent in RYt divided by the number of school days in RYt in which the student was enrolled in the district.
- *Chronically absent (in RYt)* is an indicator that is one if percent absent in RYt is greater than or equal to 10%, which is the threshold used by the Department of Education to measure chronic absenteeism (see, for example, Chicago Public Schools 2022, New York State Education Department 2025, or U.S. Department of Education 2025).
- *Retained (in RYt)* is an indicator of whether the grade in RYt is less than the predicted grade based on RY-1, i.e. less than what would be implied by a normal progression given the grade in RY-1. In addition to requiring that a student is enrolled in RYt, we also require that the student's grade implied by a normal progression from their grade in RY-1 lies within grades 1-12. This requirement helps avoid issues arising from students who

[41] For school years that do not have corresponding ACS data from this strategy, we use the most recently-available ACS data at the time of writing. For instance, for school year 2021, we used ACS 5-year data from 2017-2021.

graduate leaving the sample while those who are retained remaining, creating a sample selection issue that obfuscates the interpretation of this variable.

Based on test scores data: Standardized state reading/English and Language Arts (ELA) and math tests are administered in grades 3-8 for both Chicago and New York.[42] In both cities, tests are required to be completed over certain time windows of the year.

For Chicago, the different standardized test regimes that span the panel are the Illinois Standards Achievement Test (ISAT), Partnership for Assessment of Readiness for College and Careers (PARCC), and the Illinois Assessment of Readiness (IAR), and their testing windows are determined by CPS administration of Illinois State Board of Education guidance.[43] For New York, all standardized test scores come from the New York State Assessment. Since 2005-06, the New York State Education Department applies the ELA and mathematics testing programs to Grades 3-8. Previously, state tests were administered in Grades 4 and 8 and citywide tests were administered in Grades 3, 5, 6, and 7.

In both cities, students are assessed with both math and reading test, and the "scale score" values are used in our analysis. We define math and reading test scores in RYt to be the tests that occur in the academic term corresponding to RYt. Given these test scores, we define the following variables:

- *Reading (math) test score (in RYt)* is the reading (math) test score in RYt as described above. To aid with comparability across cities and with interpretability, we separately Z-score the reading and math scores over their test score distributions for all students in the school system in the considered grade×school-year.
- *Missed test (in RYt)* is an indicator for whether a student is missing a math test score or a reading test score. This sample conditions on students who are enrolled in grades 3-8 in RYt and thus is defined as missing a test score when the student was expected to take the test.
- *Average achievement of school attended (in RYt)* for Chicago is the average of math and reading test scores taken over all students – irrespective of being linked to eviction records – in the considered student's school in the school year given by RYt. For instance, for a student with RYt corresponding to the school year 2011, it is the school-specific average of all test scores from school year 2011. As test scores are for grades 3-8, we do not have measures for schools that do not include these grades.

[42] While other tests–including those for college admissions such as the SAT, or other formative assessments–are tested in these and other grades, we do not include them in the analysis because they are less consistently assessed, and do not have the same higher stakes as these Federally-required standardized tests in grades 3-8.

[43] ISBE, 2023. *Assessment Communications.* https://www.isbe.net/Pages/Assessment-Communications.aspx.

Based on credits data:

- *Credits (in RYt)* is the number of completed credits in RYt divided by the standard number of credits required to progress to the next grade. This normalization is done to make the Chicago and New York results comparable. We implement this by dividing the number of completed credits by the modal number of completed credits in the student's grade (computed using all students in the school system in the considered grade). In Chicago, this standard number of credits required to progress to the next grade is always 7. In New York, this number is typically 14. Credits are observed for grades 9-12 in Chicago and for grades 7-12 in New York. To ensure comparability across cities, we restrict New York credits data to grades 9-12.

Based on GPA data:

- *GPA (in RYt)* for Chicago is the average (unweighted) GPA over the academic year of RYt. GPA is measured on a four-point scale and for grades 9-12. It is not observed for New York.

High school completion measures We measure high school completion by using the last observed enrollment status of high school students as recorded in our panels. This final observed enrollment status for a high school student can take one of the following values: still enrolled, dropped out, graduated, or transferred out of the school system. Because we do not observe whether a student who transfers out is still enrolled in another school, dropped out, or graduated from a school outside the district, we restrict attention to the sample of students who are enrolled in high school at some point in the panel and do not transfer out of the system (see Appendix D for details on the bounding approach we use to address transfers and attrition). Lastly, to focus on students who are expected to graduate by the end of our panels, we additionally condition on students who are at least 18 years old by the end of our panels. We define the following outcomes:

- *Graduation* is an indicator that is one for students whose final status is recorded as graduated. The sample is as described above.
- *Graduation status not observed* is an indicator for being in the sample described above among those who are age 18 or older by the end of our panels.
- *Dropped out* is an indicator that is one for students whose final status is recorded as dropped out. The sample is as described above.
- *Graduated on time* is an indicator that is one for students who graduate within four years of first entering 9th grade, and zero otherwise. The sample is as described above and

additionally restricts to students who are actively enrolled in 9th grade at some point in our panels.

Table B.3: Data sources and availability

	Chicago			New York		
	Has?	Grades	School years	Has?	Grades	School years
Characteristics data	Yes	K-12	2000-2019	Yes	K-12	2005-2018
Enrollment data	Yes	K-12	2000-2019	Yes	K-12	2005-2018
Address data	Yes	K-12	2004-2019	Yes	K-12	2007-2017
McKinney-Vento data	Yes	K-12	2009-2019	No	-	-
HMIS data	No†	-	-	Yes	-	2003-2019
Attendance data	Yes	K-12	2009-2019	Yes	K-12	2005-2018
Credits data	Yes	9-12	2015-2019	Yes	6-12	2008-2017
GPA data	Yes	9-12	2009-2019	No	-	-
Test scores data	Yes	3-8	2000-2019	Yes	3-8	2006-2017

Notes: School years are indexed by the year of the spring season of the academic term. The † indicates that although HMIS results are not available for the Chicago education sample (since HMIS records are not linked to this sample), these results are available for the Chicago Census sample.

B.3 Census data

This subsection provides details about how the Census samples are constructed and how the key variables are defined.

B.3.1 Overview of the data

We use linked Census data for three main purposes: (1) to characterize what proportion of households facing eviction have children (Section 4.1); (2) to estimate staggered event studies and present trends of household and neighborhood outcomes (Section 4.4); (3) to study the impact of eviction on Census household and neighborhood outcomes (Section 6.1); and (4) to estimate event studies and study the impact of eviction on child homelessness, using linked HMIS data (Sections 4.4 and 6.1).

B.3.2 Linkage and sample restrictions

All Census samples begin with the processing of Cook County eviction court records. The Census Bureau has applied its Person Identification Validation System (PVS) to the names and addresses of tenants in eviction court records. This procedure uses probabilistic matching to assign a unique anonymized identifier to individuals called a protected identification key (PIK), which we use to link to other Census datasets.

To construct the samples used to characterize the proportion of households with children (used in Section 4.1), we take the PIK'd court records and link them to the 2010 Decennial, and, separately, to the 2000 Decennial. Because the goal is to consider children in the household at the time of filing, we restrict the sample to cases occurring between 2000-2004 for the 2000 Decennial linkage, and to cases occurring between 2008-2012 for the 2010 Decennial linkage. We restrict the linkage to tenants who are not in group quarters, so that children in the household can be observed, and also to one case per tenant-year to not overweight tenants with multiple cases per year. And we also restrict to one tenant per case in cases with multiple tenants, using different rules for selecting the tenant: (i) randomly selecting one tenant, (ii) selecting the Census household head, (iii) choosing the female tenant first, and if there are more than one female tenant choosing the tenant at random. We then present the proportion of households with children aged 0-18, and the number of children per household among those with at least 1 child. Table B.4 presents additional information on the construction of the linked Census sample. We link approximately 68 percent of our Decennial 2000 child sample to their 2010 Decennial records. In the same table, we also show that judge stringency is not predictive of the tenant linking to the 2000 Decennial and that for the sample of children in tenants' households in the 2000 Decennial, judge stringency is not predictive of linking to a 2010 Decennial record.

To construct the Census event study sample (used in Section 4.4), excluding the HMIS analysis, we link the PIK'd tenant-case records at the individual level to tenants' 2010 Decennial records. We use case years between 2007 and 2015 to study children beginning 3 years prior to and continuing until 5 years after the 2010 Decennial. We restrict to tenants who are not in group quarters in the 2010 Decennial, because the household relationship variable is missing for those in group quarters.[44] Next, we construct a child-case level dataset using any children in the tenant's household aged 0-18 regardless of their relationship to the tenant.[45]

To construct the IV/OLS Census sample (used in Section 6.1) for all household and neighborhood outcomes excluding the HMIS analysis, we link tenants in PIK'd court records at the individual level to their 2000 Decennial records. We restrict the sample to tenants with case filings between 2000 and 2009 so that the 2000 Decennial precedes the court case and the 2010 Decennial follows the court case. We restrict tenants to those not in group quarters. We then collect all the PIKs of children in these households. We restrict children to those who are 18 or under as of the case year, and 18 or under as of the Decennial 2010. The purpose of this restriction is to

[44]Panel B of Appendix Figure A.4 uses the tenant-level linkage and shows an event study depicting the proportion of tenants in group quarters by year relative to filing. This figure shows that evicted tenants are about 1 percentage point more likely to be in group quarters in the 1-5 years after eviction filing.

[45]Note that if there are multiple tenants on the court case and they are not living together as of the 2010 Decennial, we will include any child living in either household as of the 2010 Decennial in our analysis sample. Note that if there are multiple tenants on the court case and they *are* living together as of the 2010 Decennial, we include each child only once in the analysis dataset.

study the impact of eviction on children who are still children as of the outcome period. We make a few other minor sample restrictions: (1) requiring an age discrepancy of no more than 1 year between the child's age in the 2000 Decennial plus 10 years, and the child's age in the 2010 Decennial; (2) requiring at least one of the linked tenants in the household to be over 18 as of the case year; and (3) the court case restrictions in described in Section 3 such as dropping bulk filings, cases in which landlords are claiming over $100,000, etc. The analysis sample is a child-case dataset. The household relationship outcomes (i.e., doubling up, multigenerational household, mother/father present, etc.) are missing for children in group quarters as of the 2010 Decennial, while the neighborhood outcomes (neighborhood poverty rate, out-of-county indicator, out-of-state indicator) are measured regardless of whether the child is living in group quarters.[46]

To construct the Cook County child-HMIS sample, we begin with the PIK'd tenants, linked to the 2010 Decennial, and to the 2000 Decennial. We collect the PIKs of all children in the household and avoid double counting children in both linkages. We restrict the sample of children to those who are the child of the household head, because when we link to the HMIS data we will restrict the HMIS data to children who are the child of the household head. We restrict to case years between 2010 and 2016 (inclusive), to overlap with the HMIS sample years, and we restrict to children who are 18 or under as of the Decennial year, the HMIS year, and the case year.

B.3.3 Variable construction and time indexing

We now provide detailed definitions for the household and neighborhood outcomes in the 2010 Decennial:

- *Single mother.* Takes a value of one if the reference child is the child of a female household head and there is no spouse present.
- *Single mother (without cohabiting partner).* Takes a value of one if the reference child is the child of a female household head and there is no spouse present and there is no cohabiting partner present.
- *Single father.* Takes a value of one if the reference child is the child of a male household head and there is no spouse present.
- *Single father (without cohabiting partner).* Takes a value of one if the reference child is the

[46] Only 1.4 percent of the non-evicted group are in group quarters in 2010, and there is no statistically significant impact of being evicted on the likelihood of being in group quarters: our main IV specification produces an insignificant point estimate of -1.2 percentage points, and our main OLS specification produces an insignificant point estimate of 0.1 percentage point, as shown in Table B.5.

Table B.4: Decennial Census Linkage Details

A. Match rates and baseline relationships	Mean
Match rate (tenant): Court records to Decennial 2000	0.67
Match rate (child): Decennial 2000 to Decennial 2010	0.68
Child linked through:	
Mother	0.62
Father	0.19
Grandparent	0.07
Aunt or Uncle	0.02
Sibling	0.08

B. Eviction and match rates	Matched to 2000		Matched to 2010	
	(1)	(2)	(3)	(4)
Eviction	-0.033***		-0.006	
	(0.003)		(0.004)	
Stringency		-0.039		-0.122
		(0.046)		(0.106)
Observations	203,000	187,000	78,000	72,000

Notes: Panel A. provides summary statistics of the data linkage procedure that creates the Census sample. We present the overall match rate of the tenant sample to the Decennial 2000 court records, and the match rate of the children in the Decennial 2000 to the Decennial 2010. We also provide the proportion of the child sample linked through each relative. Panel B. estimates a regression of an indicator of the tenant matching to the Decennial 2000 records on the eviction indicator (column 1) and on the stringency measure (column 2). We then use the sample of children in the Decennial 2000 and regress an indicator for those children linking to the Decennial 2010, on eviction (column 3) and on stringency (column 4). Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965.

Table B.5: Group Quarters (Census Sample)

	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)
Group Quarters	0.014	0.001	-0.012
		(0.001)	(0.019)
Observations	18,000	53,000	49,000

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports results for the Census sample (Cook County) of OLS and two-stage least squares (IV) regressions to estimate the impact of being evicted on the likelihood of being in group quarters. The first column reports the non-evicted mean, the second reports the coefficient on an eviction indicator from an OLS regression, and the third reports the TSLS estimate for eviction. The regression and sample specifications are as described in the notes of Table 4. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R11514.

child of a male household head and there is no spouse present and there is no cohabiting partner present.

- *Multigenerational household.* Takes a value of one if any of the conditions are met: (i) the reference child is the child of the household head and the household head has a parent, parent-in-law, or grandparent present, (ii) the reference child is the grandchild of the household head and a child (biological, adopted, or step), parent, or parent-in-law of the household head is present.
- *Doubling-up (including grandparents).* Takes a value of one if there is any adult (19 and older) in the household that is not the child, spouse, or cohabiting partner of the household head.

- *Doubling-up (excluding grandparents).* This measure excludes parents or parents-in-law of the household head.
- *Mother present.* This measure takes a value of one if (i) the reference child is the child of a female household head, (ii) the reference child is the child of a male household head and the female spouse is present (note this would include step-mothers), (iii) the reference child is the grandchild of the household head and the female child of the household head is present (note this condition may erroneously include aunts), (iv) the reference child is the brother or sister of the household head and the female parent of the household head is present (note this may include step-mothers).
- *Father present.* This measure is analogous to *mother present.*
- *Out of county.* An indicator equal to 1 if the child is living outside of Cook County, IL.
- *Out of state.* An indicator equal to 1 if the child is living outside of Illinois.

In the linked Census-HMIS data, we define event time in months relative to the case month, so year 0 refers to an HMIS outcome between 12 months prior and 1 month prior to the case month, year 1 refers to an HMIS outcome between the case month and 11 months after the case, year 2 refers to an HMIS outcome between 12 months after the case and 23 months after the case, etc. The homelessness outcome in the HMIS sample refers to any interaction with the homelessness system.

C Specification Details and Robustness

This appendix presents robustness results to our main specification. Tables C.1-C.4 present IV results for our education samples under two different specifications in addition to our main specification. In the first specification ('No X'), we include no controls (but keep the fixed effects). In the second specification ('Base X'), we include court and education covariates, but no lagged controls. Apart from these differences in how we incorporate covariates, these specifications are like our main specification described in Section 5. The third specification ('Main IV') is our main specification that we include for ease of comparison.[47] Overall, we find that both the estimates and standard errors across all three specifications are quite similar.

Table C.5 presents IV results for our Chicago Census sample under a 'No X' specification that includes no controls (but keeps the fixed effects). Apart from this difference, this specification is like our main specification described in Section 5. The second specification ('Main IV') is our main specification that we include for ease of comparison. We again find that the estimates and standard errors across both specifications are quite similar.

Lastly, Tables C.1-C.5 also present reduced form estimates under our main specification ('Main RF'). These estimates are similar to our IV estimates, consistent with our first stage being close to unity.

[47]Because high school completion variables have no notion of lagged outcomes, the main and 'Base X' specifications are equal, and we thus omit presenting both.

Table C.1: Robustness to Controls and RF: Home environment (Education Sample)

	Chicago				New York				Combined			
	No X IV (1)	Base X IV (2)	Main IV (3)	Main RF (4)	No X IV (5)	Base X IV (6)	Main IV (7)	Main RF (8)	No X IV (9)	Base X IV (10)	Main IV (11)	Main RF (12)
Case school year:												
Not at pre-case address	0.303***	0.298***	0.299***	0.346***	0.081**	0.077**	0.087**	0.073**	0.121***	0.117***	0.125***	0.122***
	(0.102)	(0.100)	(0.099)	(0.112)	(0.036)	(0.037)	(0.037)	(0.032)	(0.034)	(0.035)	(0.035)	(0.033)
Neighborhood poverty	-0.017	0.006	0.006	0.006	0.008	0.009	0.009	0.008	0.002	0.008	0.009	0.007
	(0.034)	(0.032)	(0.032)	(0.035)	(0.014)	(0.014)	(0.014)	(0.011)	(0.013)	(0.013)	(0.013)	(0.011)
Homelessness†	0.067**		0.070**	0.060**	0.023	0.024	0.031	0.026	0.026		0.033*	0.028*
	(0.032)		(0.033)	(0.027)	(0.021)	(0.021)	(0.020)	(0.017)	(0.019)		(0.019)	(0.016)
McKinney Vento	0.108	0.115	0.079	0.081								
	(0.077)	(0.074)	(0.070)	(0.073)								
Observations	42,276	49,868	41,276	42,276	238,610	238,610	238,610	238,610	280,408	272,758	279,075	280,408
Post-filing school year 1:												
Not at pre-case address	0.148	0.142	0.141	0.175	0.126**	0.121**	0.135**	0.111**	0.130***	0.125***	0.136***	0.123***
	(0.095)	(0.095)	(0.095)	(0.119)	(0.052)	(0.053)	(0.053)	(0.046)	(0.046)	(0.047)	(0.047)	(0.044)
Number of moves	0.109	0.097	0.097	0.118	0.218**	0.208**	0.233**	0.192**	0.199**	0.189**	0.210**	0.179**
	(0.174)	(0.172)	(0.172)	(0.210)	(0.089)	(0.091)	(0.092)	(0.078)	(0.079)	(0.081)	(0.082)	(0.074)
Neighborhood poverty	-0.023	-0.007	-0.007	-0.008	0.024	0.024	0.024	0.019	0.014	0.017	0.018	0.013
	(0.026)	(0.024)	(0.024)	(0.027)	(0.017)	(0.017)	(0.017)	(0.013)	(0.015)	(0.014)	(0.014)	(0.012)
Homelessness†	0.077**		0.077**	0.060***	0.043	0.045*	0.051**	0.041**	0.046*		0.053**	0.042**
	(0.031)		(0.033)	(0.022)	(0.027)	(0.027)	(0.026)	(0.021)	(0.025)		(0.024)	(0.019)
McKinney Vento	0.131	0.144*	0.103	0.107								
	(0.081)	(0.075)	(0.075)	(0.079)								
Observations	37,825	42,407	37,825	37,825	193,622	193,622	193,622	193,622	229,186	214,124	229,186	229,186
Post-filing school year 2:												
Not at pre-case address	0.145	0.138	0.126	0.163	0.171**	0.170**	0.186**	0.140**	0.166***	0.164***	0.174***	0.144***
	(0.094)	(0.091)	(0.090)	(0.117)	(0.074)	(0.076)	(0.078)	(0.059)	(0.063)	(0.063)	(0.065)	(0.053)
Number of moves	0.197	0.179	0.179	0.231	0.410**	0.404**	0.448**	0.337**	0.376**	0.368**	0.405**	0.320**
	(0.210)	(0.199)	(0.199)	(0.258)	(0.181)	(0.183)	(0.190)	(0.147)	(0.156)	(0.157)	(0.163)	(0.130)
Neighborhood poverty	-0.014	-0.011	-0.011	-0.014	0.031	0.027	0.028	0.020	0.020	0.018	0.019	0.013
	(0.029)	(0.025)	(0.025)	(0.031)	(0.021)	(0.021)	(0.021)	(0.015)	(0.018)	(0.017)	(0.017)	(0.014)
Homelessness†					0.003	0.006	0.012	0.009				
					(0.029)	(0.029)	(0.029)	(0.022)				
McKinney Vento	0.101	0.115	0.078	0.082								
	(0.088)	(0.083)	(0.080)	(0.084)								
Observations	36,709	36,709	36,709	36,709	156,814	156,814	156,814	156,814	165,265	165,265	165,265	165,265

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Table shows robustness of IV results to specification choice. "No X" includes only court-by-year fixed effects, "Base X IV" includes a limited set of demographic controls, and "Main IV" replicates our main specification. "Main RF" presents reduced form estimates under our main specification. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R11514.

Table C.2: Robustness to Controls and RF: Schooling Disruption and Engagement (Education Sample)

	Chicago				New York				Combined			
	No X IV (1)	Base X IV (2)	Main IV (3)	Main RF (4)	No X IV (5)	Base X IV (6)	Main IV (7)	Main RF (8)	No X IV (9)	Base X IV (10)	Main IV (11)	Main RF (12)
Case school year:												
Not at pre-case school	0.369***	0.378***	0.371***	0.429***	0.021	0.018	0.018	0.015	0.078***	0.077***	0.076**	0.083***
	(0.080)	(0.081)	(0.081)	(0.088)	(0.032)	(0.032)	(0.032)	(0.027)	(0.030)	(0.030)	(0.030)	(0.027)
Percent absent	0.007	0.006	-0.010	-0.010	0.006	0.004	0.012	0.010	0.006	0.004	0.009	0.007
	(0.027)	(0.026)	(0.022)	(0.022)	(0.014)	(0.013)	(0.009)	(0.008)	(0.012)	(0.012)	(0.008)	(0.007)
Chronic absent	0.231**	0.224**	0.121	0.123	0.036	0.029	0.052	0.044	0.061	0.054	0.061	0.054
	(0.116)	(0.114)	(0.093)	(0.095)	(0.049)	(0.049)	(0.043)	(0.037)	(0.045)	(0.045)	(0.039)	(0.034)
Transferred out of school system	-0.036	-0.037	-0.042	-0.045	0.014	0.013	0.013	0.011	0.005	0.003	0.002	-0.000
	(0.048)	(0.048)	(0.048)	(0.051)	(0.018)	(0.018)	(0.018)	(0.016)	(0.018)	(0.017)	(0.017)	(0.016)
Observations	51,522	51,522	51,522	51,522	278,879	278,879	278,879	278,879	330,401	330,401	330,401	330,401
Post-filing school year 1:												
Not at pre-case school	0.283***	0.285***	0.280***	0.349***	0.044	0.042	0.038	0.033	0.084**	0.083**	0.079**	0.086**
	(0.087)	(0.090)	(0.088)	(0.107)	(0.044)	(0.044)	(0.044)	(0.038)	(0.039)	(0.040)	(0.040)	(0.036)
Percent absent	0.047**	0.046**	0.028	0.032	0.025*	0.022	0.023**	0.019**	0.028**	0.025**	0.024**	0.021**
	(0.023)	(0.023)	(0.020)	(0.022)	(0.014)	(0.014)	(0.011)	(0.010)	(0.013)	(0.013)	(0.010)	(0.009)
Chronic absent	0.342***	0.337***	0.228***	0.257***	0.071	0.062	0.069	0.058	0.105**	0.097**	0.090**	0.083**
	(0.095)	(0.095)	(0.085)	(0.094)	(0.050)	(0.050)	(0.047)	(0.040)	(0.045)	(0.045)	(0.042)	(0.037)
Retained	0.015	0.012	0.009	0.011	0.028	0.027	0.027	-0.002	0.026	0.025	0.024	0.000
	(0.044)	(0.044)	(0.045)	(0.055)	(0.027)	(0.027)	(0.027)	(0.026)	(0.024)	(0.024)	(0.024)	(0.024)
Transferred out of school system	-0.024	-0.030	-0.031	-0.034	0.009	0.009	0.008	0.007	0.003	0.001	0.001	-0.001
	(0.050)	(0.050)	(0.050)	(0.054)	(0.015)	(0.015)	(0.015)	(0.013)	(0.016)	(0.016)	(0.016)	(0.015)
Observations	46,519	46,519	46,519	46,519	251,730	251,730	251,730	244,350	298,249	298,249	298,249	290,869
Post-filing school year 2:												
Not at pre-case school	0.260***	0.263***	0.258***	0.315***	0.047	0.038	0.037	0.031	0.085*	0.078	0.077	0.082*
	(0.096)	(0.099)	(0.095)	(0.113)	(0.058)	(0.058)	(0.058)	(0.049)	(0.050)	(0.051)	(0.051)	(0.045)
Percent absent	0.007	0.008	-0.006	-0.008	0.035**	0.032*	0.028*	0.023*	0.032**	0.029*	0.024*	0.019*
	(0.023)	(0.023)	(0.022)	(0.026)	(0.017)	(0.017)	(0.015)	(0.013)	(0.015)	(0.015)	(0.014)	(0.012)
Chronic absent	0.091	0.092	0.024	0.028	0.090*	0.077	0.062	0.051	0.090**	0.079*	0.057	0.048
	(0.088)	(0.086)	(0.084)	(0.100)	(0.047)	(0.049)	(0.047)	(0.039)	(0.043)	(0.044)	(0.043)	(0.037)
Retained	0.066	0.065	0.058	0.075	0.055*	0.052	0.052	0.018	0.056**	0.054*	0.053*	0.028
	(0.054)	(0.053)	(0.053)	(0.069)	(0.032)	(0.032)	(0.032)	(0.028)	(0.028)	(0.028)	(0.028)	(0.026)
Transferred out of school system	0.051	0.042	0.041	0.046	0.026	0.026	0.026	0.022	0.031*	0.029*	0.029*	0.026
	(0.057)	(0.056)	(0.056)	(0.062)	(0.016)	(0.016)	(0.016)	(0.014)	(0.017)	(0.017)	(0.017)	(0.016)
Observations	41,084	41,084	41,084	41,084	220,181	220,181	220,181	213,532	261,264	261,264	261,264	254,616

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Table shows robustness of IV results to specification choice. "No X" includes only court-by-year fixed effects, "Base X IV" includes a limited set of demographic controls, and "Main IV" replicates our main specification. "Main RF" presents reduced form estimates under our main specification.

Table C.3: Robustness to Controls and RF: High school Credit Accumulation and GPA (Education Sample)

	Chicago				New York				Combined			
	No X IV (1)	Base X IV (2)	Main IV (3)	Main RF (4)	No X IV (5)	Base X IV (6)	Main IV (7)	Main RF (8)	No X IV (9)	Base X IV (10)	Main IV (11)	Main RF (12)
Case school year:												
Credits	-0.207	-0.274	-0.227	-0.196	-0.071	-0.065	-0.081	-0.071	-0.077	-0.074	-0.088	-0.077
	(0.184)	(0.187)	(0.177)	(0.148)	(0.063)	(0.060)	(0.056)	(0.051)	(0.060)	(0.058)	(0.054)	(0.049)
GPA	-0.502	-0.404	-0.428	-0.437								
	(0.435)	(0.405)	(0.279)	(0.284)								
Observations	6,137	6,137	6,137	6,137	68,604	68,604	68,604	68,604	71,768	71,768	71,768	71,768
Post-filing school year 1:												
Credits	-0.261*	-0.306**	-0.276**	-0.316***	-0.121*	-0.120*	-0.138**	-0.124**	-0.127*	-0.128*	-0.144**	-0.131**
	(0.135)	(0.134)	(0.126)	(0.121)	(0.068)	(0.068)	(0.064)	(0.058)	(0.066)	(0.065)	(0.062)	(0.056)
GPA	-0.367	-0.402	-0.291	-0.329								
	(0.422)	(0.386)	(0.329)	(0.365)								
Observations	6,054	6,054	6,054	6,054	74,312	74,312	74,312	74,312	77,454	77,454	77,454	77,454
Post-filing school year 2:												
Credits	-0.119	-0.141	-0.134	-0.178	-0.140*	-0.142*	-0.143*	-0.112*	-0.139*	-0.142*	-0.143*	-0.115*
	(0.092)	(0.092)	(0.091)	(0.117)	(0.085)	(0.084)	(0.084)	(0.066)	(0.081)	(0.081)	(0.080)	(0.063)
GPA	-0.375	-0.377	-0.243	-0.322								
	(0.344)	(0.327)	(0.309)	(0.406)								
Observations	6,243	6,243	6,243	6,243	71,766	71,766	71,766	71,766	75,165	75,165	75,165	75,165

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Table shows robustness of IV results to specification choice. "No X" includes only court-by-year fixed effects, "Base X IV" includes a limited set of demographic controls, and "Main IV" replicates our main specification. "Main RF" presents reduced form estimates under our main specification.

Table C.4: Robustness to Controls and RF: Graduation (Education Sample; Grade 6 to 12)

	Chicago			New York			Combined		
	No X IV (1)	Main IV (2)	Main RF (3)	No X IV (4)	Main IV (5)	Main RF (6)	No X IV (7)	Main IV (8)	Main RF (9)
Graduation	-0.108	-0.103	-0.147	-0.137**	-0.128**	-0.107**	-0.133**	-0.125**	-0.112**
	(0.094)	(0.095)	(0.136)	(0.062)	(0.056)	(0.049)	(0.056)	(0.051)	(0.046)
Graduation status not observed	0.090	0.085	0.105	0.071*	0.063	0.054	0.074**	0.066*	0.062*
	(0.092)	(0.092)	(0.111)	(0.041)	(0.039)	(0.034)	(0.037)	(0.036)	(0.033)
Observations	20,960	20,960	20,960	129,452	129,452	129,452	150,413	150,413	150,413

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Table shows robustness of IV results to specification choice. "No X" includes only court-by-year fixed effects and "Main IV" replicates our main specification. "Main RF" presents reduced form estimates under our main specification.

Table C.5: Robustness to Controls and RF: Living Arrangements, Household Structure, and Geography (Census Sample)

	No X IV (1)	Main IV (2)	Main RF (3)
Living Arrangements			
Total household size	0.601	0.686	0.640
	(0.476)	(0.480)	(0.461)
Doubling up (incl. grandparents)	0.166**	0.169**	0.158***
	(0.079)	(0.077)	(0.068)
Doubling up (excl. grandparents)	0.102	0.102	0.095
	(0.072)	(0.072)	(0.064)
Multigenerational household	0.127**	0.132***	0.123***
	(0.058)	(0.054)	(0.053)
Grandparent household head	0.098*	0.097**	0.091**
	(0.052)	(0.048)	(0.045)
Household Structure			
Mother present	0.005	0.018	0.017
	(0.060)	(0.058)	(0.054)
Father present	-0.009	0.003	0.003
	(0.097)	(0.101)	(0.095)
Single mother	-0.005	-0.008	-0.008
	(0.108)	(0.099)	(0.094)
Non-relative household head	0.004	-0.000	-0.000
	(0.027)	(0.027)	(0.025)
Geography			
Neighborhood poverty rate	-0.061**	-0.051**	-0.048*
	(0.024)	(0.025)	(0.025)
Out of county	-0.014	-0.028	-0.026
	(0.064)	(0.070)	(0.065)
Out of state	-0.011	-0.015	-0.014
	(0.052)	(0.055)	(0.050)
Observations	48,000	48,000	48,000

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Table shows robustness of IV results to specification choice. The "No X" column specification includes only district-year fixed effects as controls, and the "Main IV" reproduces our main specification. The "Main RF" column shows the estimate of a regression of the outcome on the instrument with the main set of controls. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965. Results rounded following Census Bureau disclosure guidelines.

D Details on the Bounding Approach

D.1 Partial identification approach

We follow the notation and approach developed in Section 6.4.1. Let Y denote an outcome (e.g., graduation), S denote being observed in the sample, E denote eviction status (treatment), and Z denote the instrument. To develop our approach, we implicitly condition on covariates and suppose that individuals are assigned to instrument $Z = z_1$ or $Z = z_0$. We discuss implementation using covariates and the full range of instrument values Z in Appendix D.2.

The researcher observes the distribution (YS, S, D, Z), where $YS = Y$ when $S = 1$ and $YS = 0$ when $S = 0$ captures that the outcome is only observed for those in the sample. The observed data relates to potential variables via $Y = Y(E)$, $S = S(E)$, and $E = E(Z)$. Throughout, we will maintain the IV assumptions that Z is independent of potential variables $(Y(0), Y(1), S(0), S(1), \{E(z)\}_z)$ and that, for any pair of instrument (z, z'), $E(z) \geq E(z')$ for everyone or that $E(z) \leq E(z')$ for everyone. Additionally, we will impose the sample selection monotonicity assumption of Lee (2009) that $S(1) \leq S(0)$ for everyone. In Appendix D.4, we show how to adapt our results to the case where $S(1) \geq S(0)$ for everyone.

Intuitively, the reason we cannot identify a causal effect in this setting is that we have a sample selection problem arising from individuals who have $S(0) = 1$ and $S(1) = 0$. Although these individuals are part of the sample when not evicted (because $S(0) = 1$), they are not part of the sample when evicted (because $S(1) = 0$).

Letting $T = c$ denote the instrument compliers (i.e. those with $E(z_0) = 0, E(z_1) = 1$), our bounding approach starts by noting that

$$\frac{\mathbb{E}[YSE|Z = z_1] - \mathbb{E}[YSE|Z = z_0]}{\mathbb{E}[SE|Z = z_1] - \mathbb{E}[SE|Z = z_0]} = \mathbb{E}[Y(1)|T = c, S(1) = 1], \tag{D.1}$$

and

$$\frac{\mathbb{E}[YS(1-E)|Z = z_1] - \mathbb{E}[YS(1-E)|Z = z_0]}{\mathbb{E}[S(1-E)|Z = z_1] - \mathbb{E}[S(1-E)|Z = z_0]} = \mathbb{E}[Y(0)|T = c, S(0) = 1]. \tag{D.2}$$

See Appendix D.5 for the derivations. Define μ as the (point identified) difference, i.e.

$$\mu \equiv \frac{\mathbb{E}[YSE|Z = z_1] - \mathbb{E}[YSE|Z = z_0]}{\mathbb{E}[SE|Z = z_1] - \mathbb{E}[SE|Z = z_0]} - \frac{\mathbb{E}[YS(1-E)|Z = z_1] - \mathbb{E}[YS(1-E)|Z = z_0]}{\mathbb{E}[S(1-E)|Z = z_1] - \mathbb{E}[S(1-E)|Z = z_0]}. \tag{D.3}$$

Combining the above, we then have that

$$\mu = \mathbb{E}[Y(1)|T = c, S(1) = 1] - \mathbb{E}[Y(0)|T = c, S(0) = 1]. \tag{D.4}$$

This is not a treatment effect because compliers with $S(1) = 1$ may be different from those with $S(0) = 1$. However, using the assumption that $S(1) \leq S(0)$, one can show that

$$\mu = \underbrace{\mathbb{E}[Y(1) - Y(0)|T = c, S(0) = 1, S(1) = 1]}_{\text{LATE-AO}} \tag{D.5}$$

$$- \underbrace{\left(\mathbb{E}[Y(0)| \underbrace{T = c, S(0) = 1, S(1) = 0}_{\text{observed-only-when-untreated}}] - \mathbb{E}[Y(0)| \underbrace{T = c, S(0) = 1, S(1) = 1}_{\text{always-observed}}] \right)}_{\equiv\ \delta^\star} \tag{D.6}$$

$$\times \underbrace{\mathbb{P}[S(1) = 0|T = c, S(0) = 1]}_{\equiv \pi}. \tag{D.7}$$

Again, see Appendix D.5 for the derivation.

Thus, equation (D.4) equals the LATE-AO (the treatment effect for the compliers who would stay in the sample irrespective of treatment), plus a bias term that is the down-weighted difference ($\delta^\star$) between the means of $Y(0)$ for two groups. The first group is the is the observed-only-when-untreated compliers who would stay in the sample when not evicted, but would leave when evicted. The second group is the always-observed compliers who would stay in the sample irrespective of eviction status. Importantly, because both groups stay in the sample when not evicted, the $Y(0)$ is the potential outcome when not evicted and in the sample for both groups. This difference is down-weighted by the share (π) in (D.7), which is the share who would leave the sample when evicted among compliers who would stay in the sample when not evicted.

We showed above that we point identify μ. In Appendix D.5, we also show that π is point identified via

$$\pi = \frac{\mathbb{E}[S|Z = z_1] - \mathbb{E}[S|Z = z_0]}{\mathbb{E}[S(1 - E)|Z = z_1] - \mathbb{E}[S(1 - E)|Z = z_0]}. \tag{D.8}$$

Re-arranging (D.5)-(D.7), we have:

$$\mathbb{E}[Y(1) - Y(0)|T = c, S(0) = 1, S(1) = 1] = \overbrace{\mu}^{\text{known}} + \underbrace{\delta^\star}_{\text{unknown}} \overbrace{\pi}^{\text{known}}$$

Although we cannot generally identify $\delta^\star$ from the data, we can impose restrictions on the magnitude. Suppose that (D.6) is assumed to lie in a chosen interval $[\delta_L, \delta_U]$. Then it must be that

$$\mathbb{E}[Y(1) - Y(0)|T = c, S(0) = 1, S(1) = 1] \in [\mu + \delta_L\pi\ ,\ \mu + \delta_U\pi], \tag{D.9}$$

where all the components of the bounds are known. The bounding exercise we propose is to construct bounds for the LATE-AO in (D.9) for different choices of intervals $[\delta_L, \delta_U]$. In particular, a natural choice is to consider a $\delta > 0$ and take $\delta_L = -\delta$ and $\delta_U = \delta$. By varying this value of δ, we can assess the sensitivity of our conclusions to the choice of δ, where δ is the largest (absolute) difference that the expression in (D.6) (i.e. $\delta^\star$) can take. Additionally, δ has a simple interpretation, so that the choice of value can be guided by context-specific considerations.

D.2 Estimation and inference

D.2.1 Estimation

Given a choice of δ_L and δ_U, the bounds can be estimated by first estimating μ and π as in equations (D.3) and (D.8). These quantities can be obtained via TSLS regressions with judicious choices of "outcome" and "treatment." In particular, let $\hat{\mu}_1$ be the TSLS estimator with outcome YSE, treatment SE, and instrument Z. Similarly, define $\hat{\mu}_0$ as the TSLS estimator with outcome $YS(1-E)$, treatment $S(1-E)$, and instrument Z, and define $\hat{\pi}$ as the TSLS estimator with outcome S, treatment $S(1-E)$, and instrument Z.

With a binary instrument, the TSLS estimators $(\hat{\mu}_1, \hat{\mu}_0, \hat{\pi})$ equal the WALD estimators and are thus consistent estimators of the parameters in equations (D.1), (D.2), and (D.8). By the continuous mapping theorem, $\hat{\theta}_L \equiv (\hat{\mu}_1 - \hat{\mu}_0) + \hat{\pi}\delta_L$ and $\hat{\theta}_U \equiv (\hat{\mu}_1 - \hat{\mu}_0) + \hat{\pi}\delta_U$ are consistent estimators of the bounds in (D.9), which we denote as $\theta_L \equiv (\mu_1 - \mu_0) + \pi\delta_L$ and $\theta_U \equiv (\mu_1 - \mu_0) + \pi\delta_U$. Under the maintained assumptions, the LATE-AO $\theta^\star \equiv (m_1 - m_0) + \pi\delta^\star$ is in $[\theta_L, \theta_U]$.

With a non-binary instrument and with covariates X, we replace the above TSLS estimators with TSLS estimators that includes Z linearly and includes X linearly in the first and second stages. With some abuse of notation, we denote these TSLS estimators as $(\hat{\mu}_1, \hat{\mu}_0, \hat{\pi})$, which converge to the estimands (μ_1, μ_0, π). We can use them to construct the estimators $\hat{\theta}_L \equiv (\hat{\mu}_1 - \hat{\mu}_0) + \hat{\pi}\delta_L$ and $\hat{\theta}_U \equiv (\hat{\mu}_1 - \hat{\mu}_0) + \hat{\pi}\delta_U$, which are consistent estimators of $\theta_L \equiv (\mu_1 - \mu_0) + \pi\delta_L$ and $\theta_U \equiv (\mu_1 - \mu_0) + \pi\delta_U$. Letting $\theta^\star \equiv (m_1 - m_0) + \pi\delta^\star$, we again have that $\theta^\star \in [\theta_L, \theta_U]$. Assuming correctly-specified TSLS specifications, this approach can be interpreted as recovering a positively-weighted average of LATE-AO among compliers.

D.2.2 Inference

The vector of TSLS estimators $(\hat{\mu}_1, \hat{\mu}_0, \hat{\pi})$ are asymptotically jointly normal. By the delta method, $\hat{\theta}_L$ and $\hat{\theta}_U$ are jointly asymptotically normal, so that

$$\sqrt{n}\begin{bmatrix}(\hat{\theta}_L - \theta_L)\\(\hat{\theta}_U - \theta_U)\end{bmatrix} \xrightarrow{d} N\left(\begin{bmatrix}0\\0\end{bmatrix}, \begin{bmatrix}\sigma_L^2 & \rho\sigma_L\sigma_U\\ \rho\sigma_L\sigma_U & \sigma_U^2\end{bmatrix}\right). \tag{D.10}$$

To construct a $1-\alpha$ confidence interval of $\theta^\star$, we use the ideas of Imbens and Manski (2004); Stoye (2009). Let $z_{1-\alpha}$ be the $1-\alpha$ quantile of the standard normal distribution. Define $CI_{n,1-\alpha}$ as

$$CI_{n,1-\alpha} \equiv \left[\hat{\theta}_L - z_{1-\alpha}\frac{\hat{\sigma}_L}{\sqrt{n}}\ ,\ \hat{\theta}_U + z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}}\right]. \tag{D.11}$$

To show that $CI_{n,1-\alpha}$ is a valid (asymptotic) confidence interval, consider any distribution F of (YS, S, D, Z) such that $\pi > 0$. Also consider any $\delta_L < \delta_U$. In Appendix D.5, we derive that:

$$\lim_{n\to\infty} \mathbb{P}[\theta^\star \in CI_{n,1-\alpha}] \geq 1-\alpha. \tag{D.12}$$

The result follows the arguments in Imbens and Manski (2004), though we do not impose that the estimators for the bound endpoints need not be well-ordered with probability one.

To construct these confidence intervals, we only need to additionally obtain $\hat{\sigma}_L$ and $\hat{\sigma}_U$, which we do via bootstrap.

Uniform validity of $CI_{n,1-\alpha}$. Above, we showed that $CI_{n,1-\alpha}$ is a point-wise (asymptotically) valid confidence interval, which means that it is valid given an F with $\pi > 0$. In problems similar to ours, there has been interest in whether the confidence intervals are valid uniformly over some specified (large) set of distributions. In particular, Imbens and Manski (2004) show that $CI_{n,1-\alpha}$ is not uniformly valid. The key issue arises from considering sequences of distributions where $\theta_U - \theta_L \to 0$. They and Stoye (2009) accordingly consider additional assumptions on the set of considered distributions and modifications to $CI_{n,1-\alpha}$, which together ensure uniform validity.

Uniformity is necessarily obtained only under restrictions on the set of considered distributions F. The derivation of (D.12) suggests one possible uniform validity statement if we are willing to assume that $\pi \geq \underline{\pi} > 0$ for some pre-specified $\underline{\pi}$. In words, this assumes that the share of "observed-only-when-untreated" among the always-observed must be bounded above 0 by a known value. In Appendix D.5, we show that $CI_{n,1-\alpha}$ is uniformly valid over sets of distributions $\mathcal{F}$ such that this assumption holds and such that the estimators $\hat{\theta}_L$ and $\hat{\theta}_U$ satisfy certain uniformity conditions (see Appendix D.5 for a precise statement). In particular, for any such

$\mathcal{F}$, we show that:

$$\lim_{n\to\infty} \inf_{F\in\mathcal{F}} \mathbb{P}[\theta^\star \in CI_{n,1-\alpha}] \geq 1-\alpha. \tag{D.13}$$

We largely avoid further considerations of uniform validity for both theoretical and practical reasons. In our setting, $\theta_U - \theta_L \to 0$ occurs over any sequence of distributions where $\pi \to 0$. However, recalling that π is a TSLS/WALD estimand, studying this problem would additionally have to consider the problem of weak instruments when considering sequences of distributions for which $\pi \to 0$. This consideration would need to be accounted for in addition to the other considerations discussed by Imbens and Manski (2004); Stoye (2009).

From a practical perspective, we view our point-wise and uniform asymptotic validity results as sufficient. In particular, the problem of differential sample selection is often a concern for settings where π is a priori known to be non-trivial, which is what we maintain when imposing that $\pi \geq \underline{\pi}$. However, as a last robustness check to our inference results, we implement the confidence interval procedure proposed in Lemma 4 of Imbens and Manski (2004), noting that the required super-efficiency condition likely fails (since the bound endpoints need not be well-ordered with probability one as $\hat{\pi}$ is need not be positive with probability one).. Using this approach, we continue to reject that the LATE-AO for graduation contains zero at the 0.1 level even as we take $\delta_L = -0.10$ and $\delta_U = 0.10$.

D.3 Combining bounds across cities

To estimate bounds that combine across cities, we proceed as in Section 5.4. Let ω be the observation weight for New York City. Letting $\theta^\star_j$ denote the parameter of interest in city j and letting $\theta_{L,j}$ and $\theta_{U,j}$ denote the bounds for city j, it follows that

$$\theta^\star_{\text{combined}} \equiv \omega\theta^\star_{NYC} + (1-\omega)\theta^\star_{CC} \in [\omega\theta_{L,NYC} + (1-\omega)\theta_{L,CC}\ ,\ \omega\theta_{U,NYC} + (1-\omega)\theta_{U,CC}]\,. \tag{D.14}$$

We can estimate these bounds using the city-specific estimates. To do inference, we calculate the standard deviations of the endpoints of the estimated bounds as

$$\hat{\sigma}_{L,\text{combined}} = \sqrt{\omega^2\hat{\sigma}^2_{L,NYC} + (1-\omega)^2\hat{\sigma}^2_{L,CC}} \tag{D.15}$$

with an analogous expression for $\hat{\sigma}_{U,\text{combined}}$. The rest proceeds as above.

D.4 The case when $S(1) \geq S(0)$

We can easily adapt the above arguments to the case where the natural assumption is instead that $S(1) \geq S(0)$ always. Again, it is the case that (D.1) and (D.2) hold. Recycling the same notation and defining μ as before (see equation D.4), we now have that

$$\mu = \underbrace{\mathbb{E}[Y(1) - Y(0)|T = c, S(0) = 1, S(1) = 1]}_{\text{LATE-AO}} \tag{D.16}$$

$$- \underbrace{\left(\mathbb{E}[Y(1)|\underbrace{T = c, S(0) = 1, S(1) = 1}_{\text{always-observed}}] - \mathbb{E}[Y(1)|\underbrace{T = c, S(0) = 1, S(1) = 0}_{\text{observed-only-when-untreated}}]\right)}_{\equiv\ \delta^\star} \tag{D.17}$$

$$\times \underbrace{\mathbb{P}[S(0) = 0|T = c, S(1) = 1]}_{\equiv \pi}, \tag{D.18}$$

with

$$\pi = \frac{\mathbb{E}[S|Z = z_1] - \mathbb{E}[S|Z = z_0]}{\mathbb{E}[SE|Z = z_1] - \mathbb{E}[SE|Z = z_0]}. \tag{D.19}$$

These derivations follow the same steps as the derivations for (D.5)-(D.7) and for (D.8), but now instead using that $S(1) \geq S(0)$ always implies

$$\mathbb{E}[Y(1)|T = c, S(1) = 1] = \mathbb{E}[Y(1)|T = c, S(0) = 0, S(1) = 1](1 - \pi) \tag{D.20}$$

$$+ \mathbb{E}[Y(1)|T = c, S(0) = 1, S(1) = 1]\pi \tag{D.21}$$

and that

$$\mathbb{E}[Y(0)|T = c, S(0) = 1] = \mathbb{E}[Y(1)|T = c, S(0) = 1, S(1) = 1]. \tag{D.22}$$

Note that $\delta^\star$ is now the difference in average potential outcomes when treated between the always-observed and the observed-only-when-untreated. In particular, supposing that $\delta^\star \in [\delta_L, \delta_U]$, we again have that

$$\mathbb{E}[Y(1) - Y(0)|T = c, S(0) = 1, S(1) = 1] \in [\mu + \delta_L \pi\ ,\ \mu + \delta_U \pi]. \tag{D.23}$$

As above, we can estimate these bounds by letting $\hat{\mu}_1$ be the TSLS estimator with outcome YSE, treatment SE, and instrument Z, and $\hat{\mu}_0$ as the TSLS estimator with outcome $YS(1-E)$, treatment $S(1-E)$, and instrument Z. The only difference is that $\hat{\pi}$ is now the TSLS estimator with outcome S, treatment SE, and instrument Z. The estimated bounds are again $\hat{\theta}_L \equiv (\hat{\mu}_1 - \hat{\mu}_0) - \hat{\pi}\delta_L$ and $\hat{\theta}_U \equiv (\hat{\mu}_1 - \hat{\mu}_0) + \hat{\pi}\delta_U$, and the rest (including inference) proceeds as above.

D.5 Derivations

D.5.1 Identification

Deriving (D.1) and (D.2). To derive (D.1), note that $YSE = Y(1)S(1)E(Z)$, so that

$$\mathbb{E}[YSE|Z=z_1] - \mathbb{E}[YSE|Z=z_0] = \mathbb{E}[Y(1)S(1)(E(1)-E(0))] \tag{D.24}$$
$$= \mathbb{E}[Y(1)|T=c, S(1)=1]\,\mathbb{P}[T=c, S(1)=1]. \tag{D.25}$$

Also, $SE = S(1)E(Z)$ so that

$$\mathbb{E}[SE|Z=z_1] - \mathbb{E}[SE|Z=z_0] = \mathbb{P}[T=c, S(1)=1]. \tag{D.26}$$

Dividing the two, we have

$$\frac{\mathbb{E}[YSE|Z=z_1] - \mathbb{E}[YSE|Z=z_0]}{\mathbb{E}[SE|Z=z_1] - \mathbb{E}[SE|Z=z_0]} = \frac{\mathbb{E}[Y(1)|T=c, S(1)=1]\,\mathbb{P}[T=c, S(1)=1]}{\mathbb{P}[T=c, S(1)=1]} \tag{D.27}$$
$$= \mathbb{E}[Y(1)|T=c, S(1)=1], \tag{D.28}$$

as required. Analogous arguments hold for (D.2).

Deriving the decomposition in (D.5)-(D.7). Since $S(1) \leq S(0)$ wp1, we have that those with $S(1)=1$ are equivalently those with $S(0)=1, S(1)=1$. Thus,

$$\mu = \mathbb{E}[Y(1)|T=c, S(1)=1] - \mathbb{E}[Y(0)|T=c, S(0)=1] \tag{D.29}$$
$$= \mathbb{E}[Y(1)|T=c, S(0)=1, S(1)=1] - \mathbb{E}[Y(0)|T=c, S(0)=1], \tag{D.30}$$

Letting $\pi \equiv \mathbb{P}[S(1)=0|T=c, S(0)=1]$ and applying the law of iterated expectations,

$$\mathbb{E}[Y(0)|T=c, S(0)=1] \tag{D.31}$$
$$= \mathbb{E}[Y(0)|T=c, S(0)=1, S(1)=0]\pi \tag{D.32}$$
$$+ \mathbb{E}[Y(0)|T=c, S(0)=1, S(1)=1](1-\pi) \tag{D.33}$$
$$= \mathbb{E}[Y(0)|T=c, S(0)=1, S(1)=1] \tag{D.34}$$
$$+ \Big(\mathbb{E}[Y(0)|T=c, S(0)=1, S(1)=0] - \mathbb{E}[Y(0)|T=c, S(0)=1, S(1)=1] \Big)\pi. \tag{D.35}$$

Plugging this into (D.30) and re-arranging, we have

$$\begin{aligned}\mu =& \mathbb{E}[Y(1) - Y(0)|T = c, S(0) = 1, S(1) = 1]] && \text{(D.36)}\\ &- \Big(\mathbb{E}[Y(0)|T = c, S(0) = 1, S(1) = 0] - \mathbb{E}[Y(0)|T = c, S(0) = 1, S(1) = 1]\Big)\pi, && \text{(D.37)}\end{aligned}$$

as required.

Deriving (D.8). Observe that $S = S(0) + (S(1) - S(0))E(Z)$ so that

$$\mathbb{E}[S|Z = z_1] - \mathbb{E}[S|Z = z_0] = \mathbb{E}[(S(1) - S(0))(E(1) - E(0))] = -\mathbb{P}[T = c, S(0) = 1, S(1) = 0]. \tag{D.38}$$

Also since $S(1 - E) = S(0)(1 - E(Z))$, then

$$\mathbb{E}[S(1 - E)|Z = z_1] - \mathbb{E}[S(1 - E)|Z = z_0] = -\mathbb{E}[S(0)(E(1) - E(0))] = -\mathbb{P}[T = c, S(0) = 1]. \tag{D.39}$$

Dividing the two yields

$$\begin{aligned}\frac{\mathbb{E}[S|Z = z_1] - \mathbb{E}[S|Z = z_0]}{\mathbb{E}[S(1 - E)|Z = z_1] - \mathbb{E}[S(1 - E)|Z = z_0]} =& \frac{\mathbb{P}[T = c, S(0) = 1, S(1) = 0]}{\mathbb{P}[T = c, S(0) = 1]} && \text{(D.40)}\\ =& \mathbb{P}[S(1) = 0|T = c, S(0) = 1], && \text{(D.41)}\end{aligned}$$

as required.

D.5.2 Inference

Deriving point-wise validity of $CI_{n,1-\alpha}$. To show (D.12), note that

$$\begin{aligned}& \lim_{n\to\infty} \mathbb{P}[\theta^\star \in CI_{n,1-\alpha}] && \text{(D.42)}\\ =& \lim_{n\to\infty} \mathbb{P}[\hat{\theta}_L - z_{1-\alpha}\frac{\hat{\sigma}_L}{\sqrt{n}} \leq \theta^\star \leq \hat{\theta}_U + z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}}] && \text{(D.43)}\\ =& \lim_{n\to\infty} \mathbb{P}[\hat{\theta}_L - z_{1-\alpha}\frac{\hat{\sigma}_L}{\sqrt{n}} \leq \theta^\star] && \text{(D.44)}\\ &+ \mathbb{P}[\theta^\star \leq \hat{\theta}_U + z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}}] && \text{(D.45)}\\ &+ \mathbb{P}\left[\hat{\theta}_L - z_{1-\alpha}\frac{\hat{\sigma}_L}{\sqrt{n}} \leq \theta^\star \text{ OR } \theta^\star \leq \hat{\theta}_U - z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}}\right]. && \text{(D.46)}\end{aligned}$$

We know $\theta^\star \in [\theta_L, \theta_U]$. We have three cases: $\theta^\star = \theta_L$, $\theta^\star = \theta_U$, or $\theta^\star \in (\theta_L, \theta_U)$. If $\theta^\star = \theta_L$, then (D.44) becomes

$$\mathbb{P}\left[-z_{1-\alpha} \leq \frac{\sqrt{n}(\theta_L - \hat{\theta}_L)}{\hat{\sigma}_L}\right] \to 1-\alpha, \tag{D.47}$$

while (D.45)becomes

$$\mathbb{P}\left[\theta_L \leq \hat{\theta}_U + z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}}\right] = \mathbb{P}\left[\theta_U - \hat{\theta}_U \leq \theta_U - \theta_L + z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}}\right] \tag{D.48}$$

$$= \mathbb{P}\left[(\theta_U - \hat{\theta}_U) - z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}} \leq (\delta_U - \delta_L)\pi\right] \to 0, \tag{D.49}$$

where we use that $\theta_U - \theta_L = (\mu_1 - \mu_0) + \delta_U\pi - (\mu - \mu_0) - \delta_L\pi = (\delta_U - \delta_L)\pi$ and appealed to convergence in probability of the LHS term in the last line. This same argument holds for (D.46), so that $\lim_{n\to\infty}\mathbb{P}[\theta^\star \in CI_{n,1-\alpha}] = 1-\alpha$. The case of $\theta^\star = \theta_U$ proceeds analogously, and the above arguments also immediately imply that when $\theta^\star \in (\theta_L, \theta_U)$, then $\lim_{n\to\infty}\mathbb{P}[\theta^\star \in CI_{n,1-\alpha}] = 1$, as required.

Deriving uniformly validity of $CI_{n,1-\alpha}$. Fix some $\underline{\pi} > 0$. Let $\mathcal{F}$ be any set of distributions F such that:

$$\pi \geq \underline{\pi} \text{ for all } F \tag{D.50}$$

$$\hat{\theta}_L \xrightarrow{p} \theta_L \text{ and } \hat{\theta}_U \xrightarrow{p} \theta_U \text{ uniformly over } \mathcal{F} \tag{D.51}$$

$$\lim_{n\to\infty}\inf_{F\in\mathcal{F}}\mathbb{P}\left[\frac{\sqrt{n}(\theta_U - \hat{\theta}_U)}{\hat{\sigma}_U} \leq z_{1-\alpha}\right] \to 1-\alpha. \tag{D.52}$$

$$\lim_{n\to\infty}\inf_{F\in\mathcal{F}}\mathbb{P}\left[-z_{1-\alpha} \leq \frac{\sqrt{n}(\theta_L - \hat{\theta}_L)}{\hat{\sigma}_L}\right] \to 1-\alpha. \tag{D.53}$$

The first condition about π was discussed previously. The latter three assumptions are uniformity conditions about the estimators of the endpoints of the bounds. If these endpoints were sample means, then these kinds of assumptions would follow from Berry-Essen type results (see, e.g., Lemma 6 of Imbens and Manski (2004)). For any such $\mathcal{F}$, we claim that (D.13) holds. This follows from the above derivations of the point-wise validity of $CI_{n,1-\alpha}$. In particular, following those steps, we have that (D.13) is weakly greater than

$$\lim_{n\to\infty}\inf_{F\in\mathcal{F}}\mathbb{P}[\hat{\theta}_L - z_{1-\alpha}\frac{\hat{\sigma}_L}{\sqrt{n}} \leq \theta^\star] \tag{D.54}$$

$$+ \lim_{n\to\infty}\inf_{F\in\mathcal{F}}\mathbb{P}[\theta^\star \leq \hat{\theta}_U + z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}}] \tag{D.55}$$

$$+ \lim_{n\to\infty}\inf_{F\in\mathcal{F}}\mathbb{P}\left[\hat{\theta}_L - z_{1-\alpha}\frac{\hat{\sigma}_L}{\sqrt{n}} \leq \theta^\star \text{ OR } \theta^\star \leq \hat{\theta}_U - z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}}\right]. \tag{D.56}$$

We again have three cases for $\theta^\star$. Considering the case where $\theta^\star = \theta_L$, we showed in the point-wise derivations that (D.54) equals

$$\lim_{n\to\infty} \inf_{F\in\mathcal{F}} \mathbb{P}\left[-z_{1-\alpha} \le \frac{\sqrt{n}(\theta_L - \hat{\theta}_L)}{\hat{\sigma}_L}\right] \to 1-\alpha, \tag{D.57}$$

where the convergence follows from (D.52). As in the point-wise derivations, we can also show that (D.55) equals

$$\lim_{n\to\infty} \inf_{F\in\mathcal{F}} \mathbb{P}\left[(\theta_U - \hat{\theta}_U) - z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}} \le (\delta_U - \delta_L)\pi\right] \tag{D.58}$$

$$\ge \lim_{n\to\infty} \inf_{F\in\mathcal{F}} \mathbb{P}\left[(\theta_U - \hat{\theta}_U) - z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}} \le (\delta_U - \delta_L)\underline{\pi}\right] \to 1, \tag{D.59}$$

where the inequality used (D.50) and the convergence used (D.51). The case of $\theta^\star = \theta_U$ proceeds analogously, and these arguments also immediately imply that when $\theta^\star \in (\theta_L, \theta_U)$, then we have uniform convergence to unity. Thus, (D.13) converges at least to $1-\alpha$, as required.

E Observation Counts for Education IV/OLS Results

Table E.1: Observation Counts for Home Environment (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	IV (3)	$\mathbb{E}[Y\|E=0]$ (4)	OLS (5)	IV (6)	$\mathbb{E}[Y\|E=0]$ (7)	OLS (8)	IV (9)
Case school year:									
Not at pre-case address	16,150	45,435	45,435	141,882	210,186	210,186	158,032	255,621	255,621
Neighborhood poverty	21,166	60,459	60,459	155,281	229,437	229,437	176,447	289,896	289,896
Homelessness†	19,500	21,500	15,500	184,677	276,208	276,208	204,177	297,708	291,708
McKinney Vento	15,772	43,711	43,711						
Post-filing school year 1:									
Not at pre-case address	13,498	37,686	37,686	113,571	168,258	168,258	127,069	205,944	205,944
Number of moves	12,578	35,110	35,110	113,571	168,258	168,258	126,149	203,368	203,368
Neighborhood poverty	17,620	49,959	49,959	124,015	183,102	183,102	141,635	233,061	233,061
Homelessness†	19,500	26,000	19,500	171,174	254,870	254,870	190,674	280,870	274,370
McKinney Vento	16,834	46,872	46,872						
Post-filing school year 2:									
Not at pre-case address	11,073	30,949	30,949	87,349	129,205	129,205	98,422	160,154	160,154
Number of moves	8,963	24,599	24,599	87,349	129,205	129,205	96,312	153,804	153,804
Neighborhood poverty	14,665	41,567	41,567	95,061	140,269	140,269	109,726	181,836	181,836
Homelessness†				153,280	228,575	228,575			
McKinney Vento	17,808	49,720	49,720						

Notes: This table provides the observation counts for the "Home Environment" main IV/OLS results, as presented in Table 3. For each column and time period, the main table observation count reports the average of the counts reported in this table. †Observation counts for HMIS records are rounded in accordance with U.S. Census Bureau disclosure requirements and were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R11514.

Table E.2: Observation Counts for School Attachment and Engagement (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	IV (3)	$\mathbb{E}[Y\|E=0]$ (4)	OLS (5)	IV (6)	$\mathbb{E}[Y\|E=0]$ (7)	OLS (8)	IV (9)
Case school year:									
Not at pre-case school	17,882	50,847	50,847	174,710	258,723	258,723	192,592	309,570	309,570
Percent absent	14,818	41,175	41,175	190,680	282,529	282,529	205,498	323,704	323,704
Chronic absent	14,818	41,175	41,175	190,680	282,529	282,529	205,498	323,704	323,704
Transferred out of school system	25,390	72,890	72,890	197,182	291,736	291,736	222,572	364,626	364,626
Post-filing school year 1:									
Not at pre-case school	14,054	39,932	39,932	134,775	198,810	198,810	148,829	238,742	238,742
Percent absent	13,435	37,193	37,193	172,639	254,219	254,219	186,074	291,412	291,412
Chronic absent	13,435	37,193	37,193	172,639	254,219	254,219	186,074	291,412	291,412
Retained	16,746	47,134	47,134	176,446	259,665	259,665	193,192	306,799	306,799
Transferred out of school system	24,676	71,143	71,143	197,182	291,736	291,736	221,858	362,879	362,879
Post-filing school year 2:									
Not at pre-case school	10,303	29,852	29,852	92,695	136,893	136,893	102,998	166,745	166,745
Percent absent	11,978	33,535	33,535	151,735	223,371	223,371	163,713	256,906	256,906
Chronic absent	11,978	33,535	33,535	151,735	223,371	223,371	163,713	256,906	256,906
Retained	14,132	40,199	40,199	153,345	225,533	225,533	167,477	265,732	265,732
Transferred out of school system	23,505	68,297	68,297	197,182	291,736	291,736	220,687	360,033	360,033

Notes: This table provides the observation counts for the "School Attachment and Engagement" main IV/OLS results, as presented in Table 5. For each column and time period, the main table observation count reports the average of the counts reported in this table.

Table E.3: Observation Counts for Elementary and Middle School Test Scores (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	IV (3)	$\mathbb{E}[Y\|E=0]$ (4)	OLS (5)	IV (6)	$\mathbb{E}[Y\|E=0]$ (7)	OLS (8)	IV (9)
Case school year:									
Reading test score	9,408	27,302	27,302	93,371	139,092	139,092	102,779	166,394	166,394
Math test score	9,389	27,219	27,219	92,594	137,874	137,874	101,983	165,093	165,093
Missed test	9,917	28,866	28,866	98,766	147,101	147,101	108,683	175,967	175,967
Post-filing school year 1:									
Reading test score	8,477	24,493	24,493	83,045	123,687	123,687	91,522	148,180	148,180
Math test score	8,461	24,419	24,419	82,152	122,314	122,314	90,613	146,733	146,733
Missed test	8,928	25,888	25,888	93,941	139,907	139,907	102,869	165,795	165,795
Post-filing school year 2:									
Reading test score	8,055	23,738	23,738	71,626	107,328	107,328	79,681	131,066	131,066
Math test score	8,009	23,640	23,640	70,754	106,046	106,046	78,763	129,686	129,686
Missed test	8,466	24,982	24,982	87,831	131,403	131,403	96,297	156,385	156,385

Notes: This table provides the observation counts for the "Elementary School Test Scores" main IV/OLS results, as presented in Table 6. For each column and time period, the main table observation count reports the average of the counts reported in this table.

Table E.4: Observation Counts for High School Credit Accumulation and GPA (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	IV (3)	$\mathbb{E}[Y\|E=0]$ (4)	OLS (5)	IV (6)	$\mathbb{E}[Y\|E=0]$ (7)	OLS (8)	IV (9)
Case school year:									
Credits	1,220	3,164	3,164	47,481	68,604	68,604	48,701	71,768	71,768
GPA	3,427	9,110	9,110						
Post-filing school year 1:									
Credits	1,195	3,142	3,142	51,807	74,312	74,312	53,002	77,454	77,454
GPA	3,368	8,966	8,966						
Post-filing school year 2:									
Credits	1,258	3,399	3,399	50,141	71,766	71,766	51,399	75,165	75,165
GPA	3,316	9,087	9,087						

Notes: This table provides the observation counts for the "High School Credits and GPA" main IV/OLS results, as presented in Table 7. For each column and time period, the main table observation count reports the average of the counts reported in this table.

Table E.5: Observation Counts for High School Graduation (Education Sample; Filing in Grades 6 to 12)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Graduation	6,073	16,352	16,352	83,803	120,196	120,196	89,876	136,548	136,548
Graduation status not observed	9,183	25,569	25,569	95,597	138,709	138,709	104,780	164,278	164,278

Notes: This table provides the observation counts for the "High School Completion" main IV/OLS results, as presented in Table 8. For each column and time period, the main table observation count reports the average of the counts reported in this table.