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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		
	$\mathbb{E}[Y\|E=0]$ (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	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

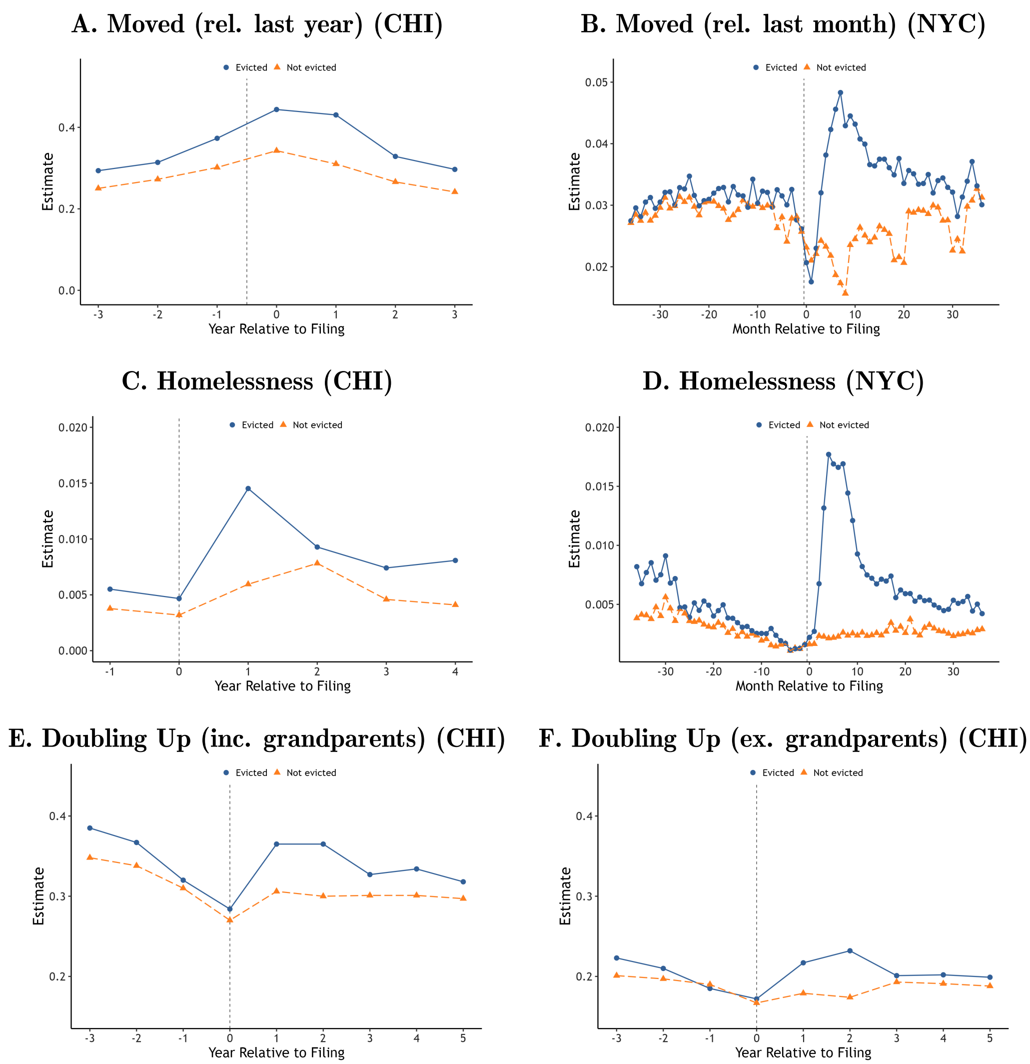
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Case school year" is the school year in which the case was filed and the upcoming year for cases filed in the summer. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete 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)									
$\delta^\star \in [-0.05, 0.05]$			$[-0.095, -0.086]$			$[-0.119, -0.112]^{*}$			$[-0.115, -0.108]^{**}$
			$(-0.251, 0.07)$			$(-0.207, -0.024)$			$(-0.193, -0.029)$
$\delta^\star \in [-0.1, 0.1]$			$[-0.1, -0.081]$			$[-0.123, -0.109]^{*}$			$[-0.119, -0.104]^{**}$
			$(-0.256, 0.075)$			$(-0.21, -0.02)$			$(-0.196, -0.025)$
$\delta^\star \in [-0.25, 0.25]$			$[-0.114, -0.066]$			$[-0.133, -0.098]^{*}$			$[-0.13, -0.093]^{*}$
			$(-0.275, 0.093)$			$(-0.221, -0.007)$			$(-0.208, -0.012)$

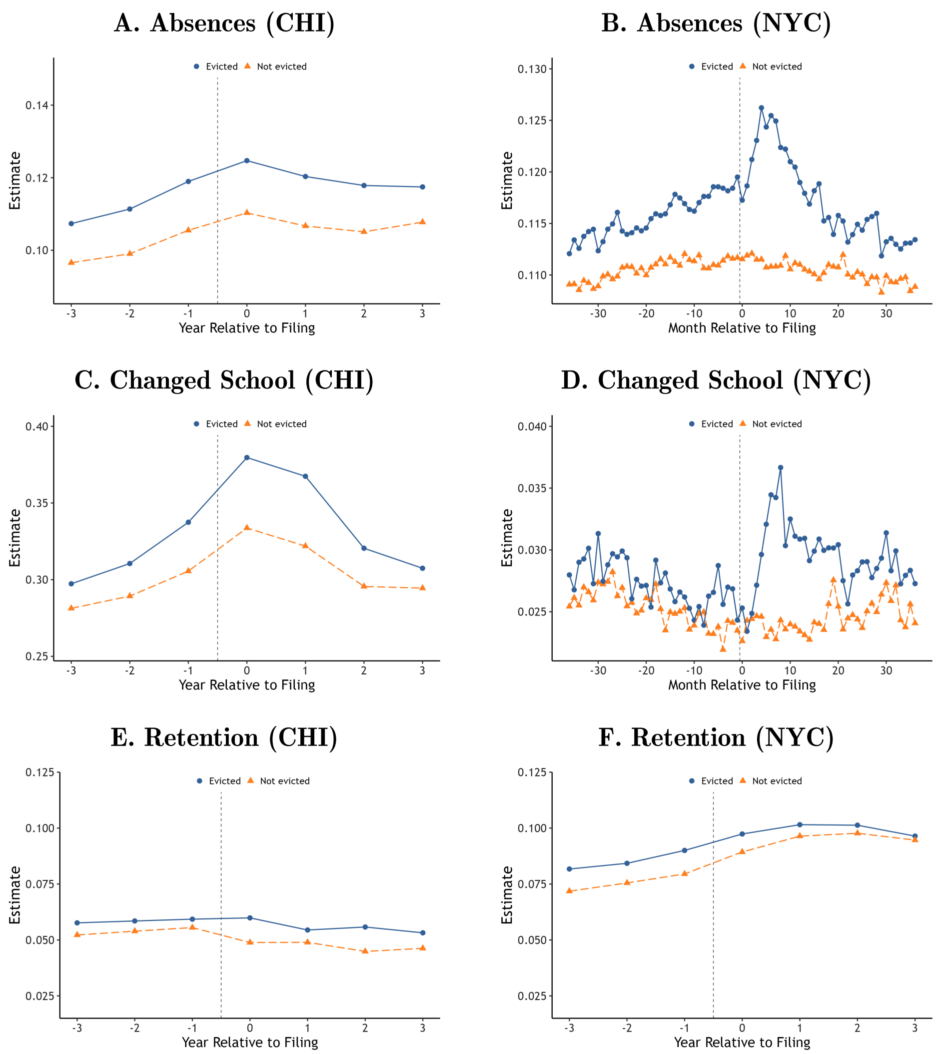
Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Columns (1)-(3) report results for Chicago, (4)-(6) report results for New York City, and (7)-(9) report combined results as described in Section 5.4. In Panel A, "Graduation" is an indicator for if the student graduated conditional on seeing the student through 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



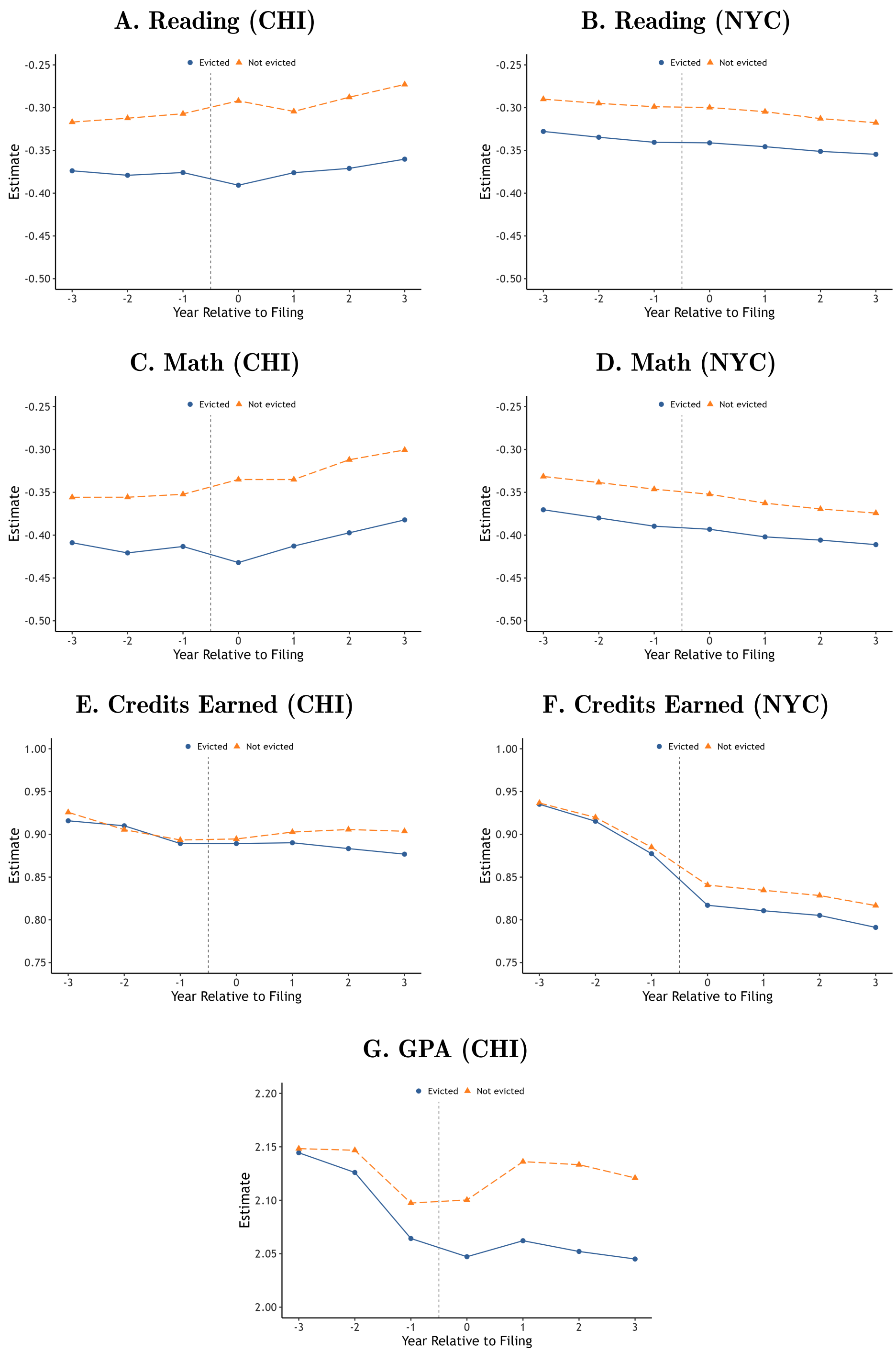
Notes: Each panel displays trends in outcomes relative to eviction filing. Panel A (B) plots trends for the Chicago education (New York education) sample for an indicator for being observed at an address other than the address of residence in the prior year (month). Panel C (D) plots trends for the Census (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.