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. To do so, we assemble new datasets 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 moving in with grandparents or other adults. Eviction also disrupts school engagement, causing increased absences and school changes. Lastly, we find that eviction reduces high school course credits and high school graduation. We use a novel bounding method to show that the graduation finding is not driven by differential attrition. The disruptive effects of eviction are worse for boys. Our evidence suggests that the impact of eviction on children runs through the disruption to the home environment and school engagement rather than deterioration in school or neighborhood quality. These effects 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 particularly widespread: 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). 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 (NCHE, 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 potentially 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 exposed to eviction or to follow them over time with administrative data. Second, tenants facing eviction often experience multiple correlated sources of economic distress, such as unemployment or worsening health (Collinson et al., 2024). Comparisons between evicted and nonevicted children may therefore confound the effects of eviction with the effects of this distress.

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—which allow us to identify children in households facing eviction and to follow them over time. To separate the effect of eviction from the distress that accompanies it, we use a research design based on the random assignment of cases to judges who systematically vary 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. We find that eviction destabilizes children's housing situation, weakens their attachment to school, and reduces high school completion.

We construct linked data from the near-universe of eviction court records filed in Cook County, IL, between 2000 and 2019 and in New York, NY, between 2007 and 2017.[3] We link these records to public school records in Chicago and New York, the homeless services

[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 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.

system in each city, and restricted Census data (Chicago only). The K-12 education records cover absenteeism, grade retention, school changes, test scores, course credit completion, and high school graduation. We also use these 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 services 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 let us study outcomes at longer horizons, and allow us to follow children who move out of Cook County.

We first provide new descriptive evidence on children's exposure to eviction and the composition of their households. The linked Cook County Census records reveal that over half (53-63%) 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 household, and 20% live in a grandparent-headed household. Moreover, we find that approximately 1 in 3 children facing eviction live in a doubled-up household prior to the eviction (1 in 5 when excluding grandparents).

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 before the court case, children in households receiving an eviction order are more disadvantaged than children in households that avoid eviction, with higher move rates, lower test scores, and higher absences. Move rates and, in New York, absences are also already rising for this group in the years before filing. After filing, the evicted group experiences sharp additional increases in homelessness, doubling up, and moving relative to the nonevicted group, and in New York the gap in absences and school switching also widens. Because evicted and nonevicted children differ in both levels and trajectories before the case, these comparisons are descriptive. Our causal analysis instead relies on the random assignment of cases to judges.

Our instrumental variables (IV) analysis considers four sets of outcomes: children's home environment, school attachment and engagement, educational achievement, and high school completion. We begin with the home environment, which we study using the linked education records. We find that eviction increases moves in the year after filing by 13 percentage points (p-value <0.01), a 60 percent increase relative to the nonevicted mean. These effects persist through the following year, 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, we find no evidence that it moves children into higher-poverty neighborhoods.

Eviction also increases children's likelihood of experiencing homelessness. The IV estimates for homelessness, as measured in Homeless Management Information System (HMIS) data, imply that eviction increases homelessness in the year after filing by 4.7 percentage points in Cook County and 3.1 percentage points in New York (p-values 0.08 and 0.13, respectively). These effects grow in the following year, when evicted children are 6.6 percentage points more likely to be homeless than nonevicted children in Cook County and 5.1 percentage points more likely to be homeless in New York (significant at the 10 and 5 percent levels, respectively). Both estimates represent large increases relative to the nonevicted means of 1 percent in Chicago and 2.3 percent in New York. In Chicago, we also examine the impact of eviction on whether the school district flags the child as living in an unstable housing situation. The point estimates for this outcome also suggest that eviction increases housing instability, although they are imprecise.

In the Census sample, we explore impacts of eviction on children's living arrangements and family structure, measured an average of 5.6 years after 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 point 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, we find no evidence that eviction affects whether children live with their mother or father, although these estimates are somewhat imprecise.

Turning to measures of school attachment and engagement, we examine eviction's impact on absenteeism, school changes, and grade retention. Using our IV approach, we find that eviction increases absenteeism in the first school year following the case filing. 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 10 percent or more of school days—as a result of eviction. These effects persist in the second school year after 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 school year, eviction increases the likelihood of being retained at least once since filing by 5.2 percentage points (p-value 0.07).

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 allow us to rule out negative effects larger than 0.11 standard deviations with 95 percent confidence in both the first and second school years after filing, though we generally cannot rule out moderate 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 standard number of credits attempted) by 14.2 percentage points in the first 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 these decreases 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, because the shorter time horizon limits the potential extent of attrition, we estimate that eviction reduces high school graduation by 12.6 percentage points (p-value 0.013), 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 an additional consistency check, we use the estimated impacts on intermediate outcomes to predict changes in graduation rates. The predicted effects are similar in magnitude to our direct IV estimates.

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 evidence that eviction increases the likelihood of a missing graduation status. To account for this, we develop a procedure that bounds the effect of eviction on graduation for compliers who remain in the district regardless of eviction status.[4] The procedure then traces how this bound responds to the assumed gap in graduation rates, absent eviction, between these compliers who remain in the district and those who migrate out in response to eviction. Applying it, we find that eviction reduces graduation for compliers who always remain, even as we allow for large gaps in graduation rates between the two groups. In particular, their graduation rates would have to differ by more than 40 percentage points to overturn our finding of a statistically significant reduction.

The effects of eviction on schooling outcomes differ by gender, with more disruptive effects for boys. Compared to girls, boys experience larger increases in chronic absenteeism and school switching as a result of eviction. The negative effects of eviction on high school completion also appear to be driven by boys. These results are consistent with boys being more susceptible to family disadvantage and negative shocks (Heckman, 2008; Bertrand and Pan, 2013; Autor et al., 2019). In our linked Census samples, we find that girls are more likely to move into multigenerational households and into lower-poverty neighborhoods after eviction, with point estimates more than twice those for boys, though the difference between girls and boys is not statistically significant. Our interpretation is that girls have greater access to family support

[4] Because roughly 80 percent of compliers remain in the district regardless of eviction status, the bounds are informative for the large majority of compliers.

networks compared to boys, and this better stabilizes their school engagement after negative events.

Our paper contributes to a growing literature on eviction. We build upon prior work in Chicago and New York, which studied the effects of eviction on adult tenants and found increases in homelessness and reductions in earnings and credit scores (Collinson et al., 2024). We extend this work by examining the effects of eviction on children. 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).[5] Prior research, using longitudinal survey data, has shown that a parent's eviction is negatively associated with children's educational and health outcomes (Leifheit et al., 2020; Schwartz et al., 2022). A separate literature finds similar associations for residential moves (Pribesh and Downey, 1999; Ziol-Guest and McKenna, 2014). Cassidy et al. (2025) use linked Medicaid claims data and the expansion of subsidized legal aid in eviction court in New York to study the effects of court outcomes on shelter use and children's health. They find that children in families who receive a judgment for possession are more likely to appear at homeless shelter addresses and substantially more likely to have a mental health-related claim. 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 before 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 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 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 involuntary 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

[5] 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).

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 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 high school completion as a result of juvenile incarceration (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.

Eviction Process. The eviction process begins with the landlord serving the tenant a written notice stating 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 files the case, they must do so in the district determined by the location of the rental unit. Cases are randomly assigned to courtrooms within a court district, or "court" as we refer to it throughout, and judge assignments to courtrooms are set in advance. Random assignment to a courtroom is therefore effectively random assignment to a judge.[6]

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. Hearings are typically brief, lasting only a few minutes on average (Doran et al., 2003). However, obtaining an eviction order often takes a couple of months. The median time between filing and eviction order is about 2 months in Chicago and about 3.5 months in New York. 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.[7] 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.

There are several reasons an eviction may impact children's schooling experience. The relocation itself may disrupt the child's home and neighborhood environment, since families must move under duress with limited time and resources. The penalty of having a public eviction record may hamper subsequent housing search.[8] Additional housing instability—such as subsequent moves, doubling up, or homelessness—could impact the child's ability to attend school and their ability to focus on their studies due to distraction or stress. Collinson et al. (2024) find that eviction reduces adult earnings, limits access to credit, and worsens health, which in turn could impact children (Gennetian et al., 2018). Changing addresses may also increase the likelihood of switching to a new school or dropping out altogether.

Policies for Housing-Unstable Students. 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

[6] Some types of cases, such as those involving the public housing authority or condominiums, are not randomly assigned to courtrooms, and we exclude these from the analysis, as we detail in Appendix B.1.2.

[7] Tenants who fail to appear for a scheduled hearing will typically receive a "default judgment" against them, which would constitute an eviction order in our settings.

[8] 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).

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). Students identified as homeless by a McKinney-Vento coordinator are guaranteed the right to remain enrolled at their school of origin, though they are also free to transfer to a new zoned school. They are entitled to free transportation to their chosen school. McKinney-Vento also requires school districts to report data on homelessness and unstable housing situations among enrolled public school students.

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. Our data linkages are summarized in Appendix Figure B.1.

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 impose similar restrictions on the court samples as in Collinson et al. (2024), dropping eviction cases associated with businesses, co-ops or condominiums, and cases with a missing defendant name, address, or court. 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 as well as the amount of any monetary judgments issued, while in New York we observe the courtroom and do not observe money judgments. We define an eviction as a case ending in an eviction order.

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 (CPS) from years 2000-2019 and New York City Department of Education from 2005-2017, for grades K-12.[9] 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 include 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 include 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.[10] 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.[11] Because address data are available only starting in the fall of 2003, we restrict to eviction cases filed in 2003-2019.

[9] The availability of different outcomes varies across years, as we detail in Appendix Table B.3.

[10] Doubled-up students account for around 80% 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.

[11] 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.

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 include 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 include 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 a shelter bed as recorded by the Department of Homeless Services.

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 term runs from early September to late June in both Chicago and New York. We refer to each academic term as a school year and index it by the calendar year in which it ends. Most schooling outcomes are measured over an entire school year, while eviction cases are filed throughout the calendar year.

For cases filed during the academic term, outcomes measured throughout that school year mix pre- and post-filing months. Our IV and OLS analyses therefore measure outcomes in the first and second post-filing school years. The first post-filing school year is the first school year whose academic term takes place entirely after the filing date, and the second post-filing school year is the school year that follows.[12] We also report results for the school year of the filing (the "case year") in Appendix A.4.1.

To study trends around the filing in Section 4, we assign each school year a "relative year" (r) based on its distance from the school year of the filing: $r = 0$ is the school year that contains the filing date; $r = -1, -2$, etc. are the school years before it, and $r = 1, 2$, etc. are the school years after it. For cases filed during the summer months, $r = 0$ is the school year beginning immediately after that summer. Relative years $r = -1, -2$, etc. are the school years before $r = 0$, and $r = 1, 2$, etc. are the school years after it. This convention ensures that the academic term in $r = -1$ is entirely before the filing while the academic term in $r = 0$ always includes post-filing months. The trend analyses report outcomes for relative years $r = -3$ through $r = 3$. Appendix B.2.3 provides further detail on these conventions.

[12] Two outcomes include exact dates and, unlike other outcomes, are not recorded at an annual frequency: homelessness in the HMIS records and, in New York, transfers out of the school system. For these outcomes, year 1 is the first 365 days after the filing date and year 2 is the next 365 days.

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 ($r = -1$), the number of times the child changes 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 both districts, student addresses may not fully capture the extent of residential instability for several reasons: families may choose not to update the school district with a new address until they have reestablished permanent accommodations, some families may temporarily stay in motels, shelters, or doubled-up situations with family or friends, and others may forget to notify the school or incorrectly report move dates. If evicted families are (weakly) more likely to move and (weakly) less likely to update their address upon moving than nonevicted families, then our observed move rates and IV estimates will be biased downward (as we show in Appendix E). We also explore other measures of housing instability. In Chicago, we 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 they 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 10 percent or more 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 as the previous year; hence, retention in $r = 1$ means the student is in the same grade as they were in $r = 0$. 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 ($r = -1$) and the outcome period.[13] Additionally, the records from both districts provide

[13]In Appendix A.2.2 and Appendix A.3, 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

discharge information documenting when a student transferred out of the district, which we use 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, and averaged across the two subjects. We also have academic outcomes for high school students, including credits earned as a share of the standard 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 graduating on time, 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 other restricted datasets held in the Census Bureau Research Data Centers (RDCs).[14] The PIK rate of tenants in the Cook County court records is 52 percent.[15] 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.[16] We restrict the sample to cases between July 2000 and December 2009, so that the 2000 Decennial precedes the case

and reading scores standardized for each grade-year across the district to have mean 0 and standard deviation 1.

[14]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.

[15]The PIK rate is limited by the available identifiers in court records (names and addresses only), but is comparable to prior studies linking court records to Census data (Graetz et al., 2023). Appendix Table B.5 shows that males, Hispanics, and residents of high-poverty neighborhoods are less likely to receive PIKs.

[16]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.

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 between Decennials, and the few cases where the child is named in the court case directly. For the analysis of household outcomes, we drop children living in group quarters in the 2010 Decennial, because household relationships are not available for these individuals; for the analysis of neighborhood poverty and out-of-county moves, we measure the outcome regardless of whether the child is living in group quarters.[17] The final Census sample is a child-case level dataset with approximately 48,000 observations, where Census observation counts here and throughout the paper are rounded according to Census disclosure rules.[18]

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 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, de-duplicating children that are present in both linkages. 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.[19]

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

[17]The proportion of children living in group quarters is 1.4 percent (shown in Table B.6), and includes those who are incarcerated, living in college dormitories, military barracks, nursing facilities, or emergency shelters.

[18]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.

[19]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.

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) is defined as an indicator for if households include an additional adult (19 and older) who is not the household head or their spouse or 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.[20]

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, IL, and an indicator for living outside Illinois.[21] 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 use 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

[20]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.

[21]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.

cases filed in 2008-2012 to the 2010 Decennial.[22] We focus on only five years of cases in each linkage so that these cases occur close to the Census observation date. Because our goal is a household-level measure, we retain one tenant per case and one case per tenant-year, so that households with multiple listed tenants or multiple filings are not overweighted.[23]

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.9). 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. For comparison, among all Cook County households, the share with children age 0-18 is 35 percent in 2000, and households with children have an average of 2.1 children.

What do these estimates imply about the total number of children facing eviction per year? Using the estimate of 2.7 million unique households facing eviction filing nationwide per year (Gromis et al., 2022), and assuming, based on our estimates, a national proportion of households with children 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 range of estimates reported in Graetz et al. (2023), which is based on a linkage of eviction cases nationwide to the American Community Survey.[24] 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 5.6 years after the case. We also construct a comparison sample of children from renter households in Cook County in 2000 where the youngest child would be 18 or younger in 2010, which we present in column 3.

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

[22] 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.

[23] 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.

[24] 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.

a single-mother household. In 2000, 43.6 percent of children in the evicted group are living in a single-mother household, compared to 45.4 percent of the nonevicted 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.[25] These shares are far higher than the shares for all children in Cook County, among whom 47.1 percent are Black, 33.1 percent are in a single-mother household, and 11.3 percent are the grandchild of the household head.

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. As mentioned in Section 3.3, certain groups (men, Hispanic tenants, residents of lower-income neighborhoods) are underrepresented in our linked sample relative to the full court population. If these groups have different living arrangements, it could affect the representativeness of our estimates. 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.

What proportion of children in our Census sample have multiple eviction cases? Approximately 68 percent have one case, 20 percent have two cases, and the remainder have three or more cases (see Appendix Table A.10). We provide and discuss subgroup analyses below for cases that are the child's first.

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 (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.[26]

[25] 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.

[26] 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.

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 10% or more 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 profiles of children in our Chicago and New York education samples are generally similar with a couple of notable differences. First, New York has a lower proportion of Black children and a higher proportion of Hispanic children 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 descriptive 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:

$$Y_{i,r} = \alpha + \beta_r \mathbb{1}[r \neq -1] + \delta_r E_i + X'_{i,r}\gamma + \varepsilon_{i,r}, \tag{4.1}$$

where i indexes the individual student, $-3 \leq r \leq 3$ indexes relative year to filing (as defined in Section 3), 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, $X_{i,r}$ includes court district interacted with case calendar year fixed effects and school year at r fixed effects. To control for age trends, $X_{i,r}$ additionally includes age at r fixed effects. 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.

To study household structure and living arrangements, we use the Chicago sample linked to Census records. Although we do not have a panel for Census outcomes, we present pseudo-event studies, using 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.[27]

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.2.2 for the derivation); adding the mean also makes it easier to interpret the magnitudes of the trends and differences between the evicted and nonevicted groups.

Home environment. Figure 1 presents measures of housing instability, including moves, homelessness, and doubling up. In each case, we find an increase 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 after filing before increasing to pre-filing rates after two years.[28] 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

[27]To construct this pseudo-event study 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. Panel F of Appendix Figure A.5 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.

[28]Appendix Figure A.6 presents annual trends for New York outcomes. These annual trends are broadly similar to the Chicago annual trends.

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 0.8 percent annually in the year prior to the case to 1.54 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.[29]

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.5. In addition, eviction does not change the probability of living with either parent. 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.5, Panels C and D).[30]

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 larger change after filing. While absenteeism rates for the nonevicted

[29]Consistent with the above results, Appendix Figure A.3 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.

[30]Appendix Figure A.5 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.

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.[31] 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 trends in average test scores from 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. Test scores are approximately 0.05 s.d. lower for evicted children compared to nonevicted children in the years before filing, and this difference remains relatively stable in the post-period.

Using data for high school students, we additionally show in Panels C and D 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 E 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.

Overall, 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.

[31] Appendix Figure A.4 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.

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 often on different trajectories in the lead-up to filing. Therefore, our aim here is to develop an instrument that is independent of these differences.

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 eviction rate of judge j assigned to individual i's case. We estimate the following population regression model using two-stage least squares:

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

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

where the regression is run separately for each outcome and time period.[32] In equation 5.1, E_i 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.[33] If the IV assumptions are satisfied and equations 5.1–5.2 are correctly specified (see Blandhol et al., 2026), 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.

[32] 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.

[33] 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 or condos, 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 at least 100 cases in the year (Chicago). In New York, all regular courtrooms hear more than 500 cases per year after removing courtrooms with non-randomly-assigned cases.

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.[34] 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 are clustered at the judge-by-year level.[35]

In the analysis of Census records, the controls include court-year fixed effects, 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 are clustered at the judge-by-year level.

Other than the court-by-time fixed effects, 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 court-year fixed effects in Appendix C and find that our results are similar across all three specifications.

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.[36] 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.

[34]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 $r = 0$, 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.

[35]In Appendix Tables C.24 and C.25, we show that the standard errors are similar under judge-level clustering.

[36]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.

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, 3, and 5 of Table A.1 report the first-stage estimates from equation 5.1 for each of our three samples, controlling for court-year fixed effects alone. Judge stringency has a large and statistically significant impact on the probability of eviction, with an F-statistic for the first stage of 149.1 in the Chicago education sample, 361.8 in the New York education sample, and 94.9 for the Cook County Census sample, providing evidence against weak instruments in our setting. Columns 2, 4, and 6 show that the first stage remains largely unchanged when adding additional controls, suggesting that judge stringency is uncorrelated with individual and case characteristics.

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.

Monotonicity. The monotonicity assumption of Imbens and Angrist (1994) 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. More recently, Frandsen et al. (2023) show that a weaker, average monotonicity condition is sufficient for IV to have a causal interpretation in randomized judge designs.[37] It would violate average monotonicity, for example, if judges who do not evict are stricter on average than those who do. We test the average monotonicity assumption in two ways. First, we perform the standard test that the first-stage estimates should be non-negative for subsamples of cases. Second, we 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 similar to our main estimates. As part of this exercise, we include a split that constructs judge stringency measures using cases that do not match to our educational records and re-estimate the first stage using this alternative instrument and

[37] Where average monotonicity only requires that potential treatment status as a function of judge assignment is positively correlated with judge propensity.

education samples; we again find the first-stage estimates are positive and similar to our main estimates. The results of these exercises are consistent with average monotonicity holding in our setting (Frandsen et al., 2023). 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.

Exclusion. A key assumption in our setting is that judge stringency affects children's outcomes only through the eviction order. However, judges may influence other aspects of the case, including the judgment amount, if the landlord is seeking payment for arrears, or providing tenants additional time by granting stays. 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., 2025). In Appendix A.1.4, we provide evidence supporting the exclusion restriction. In particular, we create measures of stringency in granting stays of the eviction order (in NYC and Cook County) and stringency in the judgment amount (in Cook County), and we show in Appendix Tables A.7 and A.8 that the correlation between different dimensions of judge stringency is low. Consistent with this finding, in Section 6.7 ("Robustness") we show that controlling for the additional stringency measures has little impact on our IV estimates.

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 5.3, 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. Evictions could impact children's outcomes through a variety of channels: the timing, exigency, or frequency of moves; the indirect effects of more acute housing instability, such as doubling up or homelessness; trauma from forced removal (Desmond et al., 2013); the impacts of monetary judgments or the public record of eviction on subsequent housing or financial circumstances; or through the transmission of any effects of eviction on parents or caregivers, such as reduced earnings, worsened health, or weakened credit access (Collinson et al., 2024).

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. For these outcomes, we report effects for the first two post-filing school years (i.e., the first two full academic terms after filing).[38] 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 and IV estimates are shown in columns 1-2 for Chicago and in columns 3-4 for New York City. Columns 5-7 present the combined nonevicted mean, OLS estimates, and IV estimates across sites. The top panel of Table 3 reports estimates for the first post-filing school year, and the bottom panel reports estimates for the second post-filing school year.

We first examine impacts on the child's likelihood of moving out of their pre-filing address. The combined IV estimate shows that, for complier children, eviction increases the likelihood of a recorded move in the school data by 13.4 percentage points in the first school year following the case, which is statistically significant at the one percent level. The IV point estimates for Chicago and New York are nearly identical at 13.4 and 13.5, respectively. These effects persist: the combined estimate is 17.6 percentage points in the second post-filing school year. 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 0.21 ($p < 0.01$) in the first school year after the case and 0.41 ($p < 0.05$) two years after filing, suggesting that for some children eviction causes residential churn beyond the initial

[38] Appendix A.4.1 also reports estimates for the school year containing the filing; these are often similar in sign and magnitude but sometimes smaller, in part because they average over months prior to filing.

move.[39] The OLS estimates are generally similar for impacts on residential moves.[40]

This analysis relies on school district records, which have two limitations. As noted in Section 3, the records are based on family-reported updates and may miss some residential mobility. Appendix E shows that, as long as evicted families are weakly more likely to move and weakly less likely to update their address upon moving than nonevicted families, our estimates represent a lower bound on the effect of eviction on whether a child moves. The records are also coarse. They identify if someone moved but not the exact date, and they capture whether a move occurred but not its circumstances, such as whether it was made under duress, how much advance warning the family received, or whether they had time to remove their possessions. We therefore interpret these estimates as evidence that eviction orders trigger residential moves, rather than as precise estimates of the effect's magnitude.[41]

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 first school year following the case, we can rule out increases in the neighborhood poverty rate larger than 4.4 percentage points with 95 percent confidence.

Homelessness. Table 3 also reports impacts on homelessness and the McKinney-Vento flag for being housing unstable. Recall that the homelessness outcome in the HMIS data is constructed in calendar time relative to the filing date, rather than at an annual school-year frequency. We find consistent evidence that eviction increases child homelessness. The IV estimates imply that eviction increases homelessness by 3.2 percentage points in the first year after filing (a 200 percent increase relative to the nonevicted mean, and statistically significant at the 10 percent level) and by 5.2 percentage points in the following year (a 250 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 9.0 percentage points in year 1, and by 7.7 percentage points in year 2, although these estimates are imprecise and not statistically significant. These effects on homelessness and the McKinney-Vento flag are qualitatively similar to the patterns 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.

[39]Appendix Table A.23 reports effects separately for children in grades 1–5 and 6–12 in the school year of the case. The results suggest that children in grades 1-5 move earlier and more frequently due to eviction, and, by year 2, move to somewhat higher-poverty neighborhoods, compared to older children.

[40]Throughout this section we report both IV and OLS estimates; we discuss the relative sizes of the IV and OLS estimates in more detail in Section 6.4, where we characterize the complier population.

[41]This data limitation only applies to the address data, and not to the data used for the other main outcomes, which we believe do not suffer from underreporting.

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.[42] As noted in section 4.2, 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 therefore measured an average of 5.6 years after the case year, a longer time horizon than the education records analysis.

The results are presented in Table 4. We find that eviction leads to an uptick in household size of 0.7 members relative to the nonevicted mean of 4.8, which is marginally significant. 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 ($p < 0.05$).[43] The increase in household size and doubling up is also reflected in a large increase in the child's likelihood of living in a multigenerational household, a 13.2 percentage point increase relative to the nonevicted mean of 9.7 percent ($p < 0.05$). 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 mother-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 (shown in Appendix Table A.15). 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 reduces children's neighborhood poverty rate exposure by 5.1 percentage points, with a p-value of 0.074, relative to a nonevicted mean of 23.5 percent. This estimate is larger than the insignificant estimates from the Chicago education sample, possibly because this estimate is measured later and for a population that includes younger children. Both sets of estimates show no evidence that children end up in worse neighborhoods on average.

In Appendix Table A.15, we examine whether eviction causes children to move outside of Cook County or outside of Illinois. 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 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

[42] We present the Census results restricting to the school-aged sample of 6-18 year-olds in Table C.26 and find similar results to those presented here.

[43] 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.

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. Appendix 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, and grade retention. Previous research suggests 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 estimates of the effects of eviction on school attachment and engagement are shown in Table 5. The first row reports the impact of eviction on switching schools within the district, where we exclude mechanical moves that occur when schools do not offer higher grades. The combined IV estimate implies that eviction increases the likelihood of switching schools by 7.7 percentage points in the first school year (with a 95 percent confidence interval from 0.06 percentage points to 15 percentage points). This effect is driven largely by impacts in Chicago. The effects on school switching in the following school year are similar, but noisier and not statistically significant. This pattern is 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 reduces school quality (see Appendix Table A.11).

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 10 percent or more of school days). Our combined IV estimate implies that eviction increases the fraction of days absent by 2.4 percentage points, or 18 percent of the nonevicted mean (significant at the 5 percent level) in the first school year following a case. The IV point estimates for Chicago and New York are similar with a 3 and 2.3 percentage point increase in absenteeism, respectively. Similarly, we find that eviction increases the likelihood of a child being chronically absent by 8.8 percentage points (21 percent of the nonevicted mean; significant at the 5 percent level), with particularly large increases in Chicago. The impact on days absent persists at 2.4 percentage points into the second post-filing school year (significant at the 10 percent level). 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 suggestive 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 2.2 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 since filing by 5.2 percentage points, which is significant at the 10 percent level.

Finally, in Appendix Table A.14 we explore whether eviction causes students to transfer out of the district. We find no evidence that eviction causes students to transfer in the two years after the case. The combined IV estimate is -0.007 in the first year after filing, and 0.021 in the second, neither of which is statistically significant.

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.22, 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. We cannot reject equality for any individual outcome, but a joint test across outcomes rejects equality across the two grade groups ($p < 0.05$).

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 (e.g., Chetty et al., 2011). We average standardized math and reading scores to improve the precision of our resulting estimates. 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 standardized test scores, reported in Table 6. In the first school year after the case, the IV estimate is slightly positive at 0.06 s.d., but is not statistically significant. In the second year after filing, the estimates for the effects of eviction on test scores are similar at 0.07 s.d., but again not statistically different from zero. Although somewhat imprecise, our estimates allow us to rule out large negative effects, but we often cannot rule out moderate negative or positive effects. In particular, we can reject reductions in test scores larger than 0.11 s.d., with 95 percent confidence in both the first and second year after filing.

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 limits strong takeaways is that eviction causes students to miss test dates. In the year after filing, the IV estimate on the likelihood of missing a test is a 6.3 percentage point increase ($p < 0.01$). In the second school year after filing, the IV estimate implies that evicted children are 7.3 percentage points more likely to miss a scheduled test ($p < 0.05$).

If eviction causes lower-performing students to miss the test, our test score estimates will be biased upward. To explore this, we split children by whether their pre-filing scores are above or below the sample median and re-estimate our main specification for each subgroup (Appendix Table A.35). Below-median performers are about 11 percentage points more likely to miss a test due to eviction in year 1, while above-median performers have small and insignificant effects.[44] This compositional shift may bias our overall test score estimates upward. Indeed, the point estimates for impacts on test scores are typically positive when estimated among the groups with the largest effects on missing tests. At the same time, we do not find any statistically significant reductions in test scores from eviction, even among students who are no more likely to miss tests in response to eviction, suggesting eviction could operate primarily through non-cognitive channels. This is consistent with Jacob (2004), who finds that public housing demolitions do not affect test scores but increase dropping out among older students.

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 in high school for Chicago, New York, and combined estimates, and impacts on high school GPA in Chicago.[45] The combined IV estimate implies that eviction reduces credits earned, as a share of the standard number of credits attempted, by 14.2 percentage points with a 95 percent confidence interval of -26 to -2 percentage points. This is 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 23.6 and 13.8 percentage point reduction in credits, respectively, and these effects persist into the second year. The IV estimates for the effects on

[44]Similarly, in Appendix Table A.36, we find that below-median performers are also more likely to be absent as a result of eviction.

[45]As we note in Section 3, GPA is only available in Chicago starting in 2008, and credits beginning in 2014.

GPA generally point to reductions of approximately one-quarter of a letter grade, but these effects are imprecisely estimated, and are not statistically significant.

Summarizing Schooling Impacts. To summarize effects on schooling across outcome domains, we construct indices for cognitive and non-cognitive outcomes. The cognitive index is the average of standardized math and reading scores, normalized to have a mean of zero and a standard deviation of one. Our non-cognitive measure, which we label the attachment and engagement index, is the normalized sum of standardized measures of absences, school changes, credits (high school), missing tests (elementary/middle school), and (for Chicago) GPA. We flip the sign of measures such as absences, school changes, and missing tests, so that higher values mean more attachment and engagement. For both indices, we averaged the outcomes over years 1 and 2 after filing. By pooling across years, we reduce the likelihood of having entirely missing values for measures such as test scores. Because the cognitive index is only available for grades 3–8, we report the results separately for elementary/middle school and high school. The results appear in Figure 4. The combined estimate of the effects of eviction on the cognitive index is close to zero at -0.02 standard deviations, which is not statistically significant.[46] In contrast, the effect on the attachment and engagement index for the same grades is large at -0.31 standard deviations and statistically significant, with similar reductions in attachment and engagement during high school years. This analysis reaffirms the pattern of effects we document above, where eviction reduces children's school engagement, but does not appear to impact cognitive skills directly.

6.4 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 outcomes depend on judge assignment. To better understand these estimates, we characterize the complier population and explore why our IV estimates are sometimes larger than the OLS estimates. We begin by describing the demographic characteristics and average pre-case outcomes of compliers, following Bhuller et al. (2020), and then use the framework of Imbens and Rubin (1997) to estimate compliers' mean potential outcomes when not evicted.

Who are the compliers? Panels A and B of Table A.21 show that complier families differ from both typical evicted and nonevicted cases across a range of observable characteristics. Compliers owe substantially less in rental arrears, with a mean of \$2,300, compared to \$4,100 for evicted families and \$3,500 for nonevicted families. They also tend to have longer tenures at their pre-filing address: just 12.8 percent were at a different address two years before

[46]Note, our cognitive index results are different from the test score results in Table 6 because we pool across years 1 and 2 after filing.

filing, compared to an average of 23.7 percent among evicted families and 13.8 percent among nonevicted families. Similar to nonevicted children, compliers also have greater academic attachment prior to the case, as reflected in lower retention and absenteeism rates, more credits, and higher GPAs. Panel C further shows that compliers, had they not been evicted, would have continued to experience housing stability comparable to nonevicted students in the year the case was filed.

Compliers may also differ along unobserved dimensions. By definition, compliers are families whose case outcome hinges on judge assignment, a fact that likely reflects their active involvement in contesting or settling their cases. This engagement itself may signal that compliers have more limited outside options if evicted. Together, compliers are more stably situated in the lead-up to eviction, yet may have comparatively fewer alternatives if evicted, leaving them with potentially more to lose from eviction. This possibility of compliers being somewhat better off at baseline, but suffering larger effects from negative shocks, has been found in other settings, such as the impact of pre-trial detention on labor market attachment (Dobbie et al., 2018).

Why do IV estimates exceed OLS? For several outcomes, our IV estimates are larger in magnitude than the corresponding OLS estimates. For example, the combined IV estimate for moving out of the pre-case address after two years is 0.176, compared to an OLS estimate of 0.157. This difference is larger for other measures, such as chronic absenteeism, where the IV estimate implies an 8.8 percentage point increase, compared to a 3 percentage point increase with OLS.

Differences between populations may explain why some OLS estimates are smaller than the IV estimates, as could the nature of selection bias in this context. In general, the LATE will exceed the OLS estimate when compliers fare better than never-takers in the absence of eviction, or if compliers fare worse than always-takers when evicted.[47] Compliers owe less in rental arrears than never-takers, suggesting smaller unobserved shocks prior to filing. If never-takers owe more because they experience more severe shocks prior to filing, they may have worse subsequent outcomes even in the absence of eviction, making compliers' counterfactual outcomes relatively favorable. Relative to never-takers, compliers' lower arrears and longer tenures could suggest that they have better landlord relationships and face fewer housing

[47] With a binary instrument and no covariates, OLS recovers $\mathbb{E}[Y|E=1]-\mathbb{E}[Y|E=0]$, where E is eviction. Expressing $\mathbb{E}[Y|E=e]=\mathbb{E}[Y(e)|E=e]$ for $e=0,1$ as weighted sums of never-takers (NT), compliers (C), and always-takers (AT) and subtracting, one obtains

$$\underbrace{\mathbb{E}[Y|E=1]-\mathbb{E}[Y|E=0]}_{\text{OLS}}=\underbrace{\mathbb{E}[Y(1)-Y(0)|C]}_{\text{LATE}}+\mathbb{P}[NT|E=0]\times\underbrace{\big(\mathbb{E}[Y(0)|C]-\mathbb{E}[Y(0)|NT]\big)}_{\text{difference in } Y(0) \text{ between never-takers and compliers}}$$
$$+\,\mathbb{P}[AT|E=1]\times\underbrace{\big(\mathbb{E}[Y(1)|AT]-\mathbb{E}[Y(1)|C]\big)}_{\text{difference in } Y(1) \text{ between always-takers and compliers}}.$$

challenges when not evicted. Relative to always-takers, compliers are more likely to be tenants who actively contested or sought to settle their cases.[48] To the extent that tenants contest because eviction would be especially harmful to them, compliers may be selected on the basis of having the most to lose.

6.5 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 that we do not observe outcomes for students who move out of the district, and this missingness is more common at longer horizons. 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 6.5.1 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 12.6 percentage points, relative to the nonevicted mean of 67.6 percent, and with a 95% confidence interval of -22.6 to -2.6 percentage points. The IV point estimates for Chicago and New York are similar at -11.3 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.12, and we obtain a smaller estimate of -4.7 percentage points 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 residential mobility. To examine whether our estimated impacts on graduation align with the effects we observed on intermediate outcomes, we perform the following back-of-the-envelope calculation. First, we 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.

[48] In contrast, always-takers are likely to be cases where judges have little scope for disagreement, such as instances where the tenant never shows up in court.

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 exercise is equivalent to a linear case of the surrogate index of Athey et al. (2025). This calculation yields a predicted impact of -12.0 percentage points. This estimate is similar to the -12.6 percentage point effect we estimate above. Interpreting the estimate from the back-of-the-envelope calculation as a valid treatment effect of eviction on graduation would require additional assumptions.[49] We interpret these results more narrowly as suggesting that the impacts on intermediate outcomes from the previous sections can broadly rationalize the effects on graduation we find in this section.

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 moderately larger than the effects of involuntary displacement from public housing (Jacob, 2004).[50]

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 nonevicted mean of 13.1 percent. Using these estimates, we develop bounds on the graduation effects in the next subsection.

6.5.1 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, 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.[51] In this subsection, we develop an alternative bounding approach that

[49] In particular, Athey et al. (2025) show that full surrogate index interpretation would require that there are no unobserved factors that confound the relationship between the intermediate outcomes and graduation, and that eviction has no direct effect on graduation (i.e., the effect of eviction on graduation operates only through the intermediate outcomes).

[50] 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 neighborhoods 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.

[51] Engberg et al. (2014); Chen and Flores (2015); Abdulkadiroğlu et al. (2018) consider a binary instrument and maintain a monotonicity assumption like the one we impose. 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 conditional distribution functions

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.

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.[52] 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 $Y = Y(E)$ 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

at each value of the instrument support. These challenges motivate our approach, which does not require recovering these conditional distribution functions.

[52]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.

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 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).

6.5.2 Estimated bounds

Panel B of Table 8 presents estimates based on our bounding approach for graduation.[53] Across rows, we vary the assumption on the interval that encompasses $\delta^\star$, 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 $|\delta^\star| \leq 0.05$, 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 $[-0.117, -0.110]$.

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 $|\delta^\star| \leq 0.25$, 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 $[\delta_L, \delta_U] = [-.41, .41]$. Hence, our finding that eviction causes lower graduation rates changes only if students who exit the district in response to

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

eviction have a graduation rate that is more than 40 percentage points higher than students who always stay in the district.

The LATE-AO is only informative about compliers who stay in their school district regardless of eviction status. That said, this represents over 80 percent of compliers. The overall LATE could therefore differ from the LATE-AO depending on both the effect of eviction among students who would leave the district regardless of eviction status (13 percent of compliers) and the additional disruption faced by students who leave the district because of eviction (7 percent). For example, moves out of the district could attenuate the overall effect if they reflect movement to a better school environment, but eviction-induced district changes could make the effect more negative if they compound the disruption caused by eviction.

6.6 Heterogeneity

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.

Table 9 summarizes the combined schooling results by child gender. Eviction appears to be more disruptive for boys. We find larger effects for boys on absenteeism, chronic absenteeism, school-switching, credits earned, and high school graduation, although for many individual outcomes we cannot reject group equality. A joint test rejects the null that the differences across all schooling outcomes are zero (p = 0.017), indicating that the schooling effects of eviction differ by gender when the outcomes are considered together.[54]

In Table A.26, we report the effects on household structure and living arrangements separately by child gender. The point estimates differ by gender, though we cannot reject equality of the effects for boys and girls. The estimated effects for girls on moving into a multigenerational household and living in a grandparent-headed household are larger, consistent with larger estimated effects on household size and doubling up. The estimated effect on moving to a lower-poverty neighborhood is also larger for girls than for boys.

These results may reflect differences in the difficulty of securing or maintaining housing by the child's gender. 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. The point estimates suggest that girls may have access to additional support from extended

[54]Appendix Tables A.25–A.30 show the full set of schooling results separately by child gender.

family members, and are consistent with stronger family support networks moderating the adverse effects of eviction.

In Appendix Sections A.6.4 and A.6.5, we explore heterogeneity by other dimensions: student race, and whether the filing is their first observed eviction case, respectively. We do not find consistent evidence that the effects of eviction differ by student race. When restricting the sample to the first observed case, the estimates are similar or modestly larger for the education outcomes, and notably larger for the Census outcomes, though estimated with larger standard errors. The first-case estimates provide suggestive evidence that impacts may be larger for children facing eviction for the first time.

6.7 Robustness

In this subsection, we briefly summarize a range of exercises evaluating the robustness of our results including: controlling for other dimensions of judge behavior, alternative constructions of the instrument, sensitivity to the choice of controls and sample, and multiple hypothesis testing.

As we discuss in section 5.3, a concern in randomized judge designs is that judges might influence outcomes through channels other than binary case outcomes. In our setting, this could be through setting different judgment amounts or granting stays for the tenant. To evaluate whether these other margins of judicial discretion are affecting our results, we construct other stringency measures as the leave-one-out mean of: judgment amounts (in Cook County) and rate of granting stays (in Cook County and NYC).[55] We then re-estimate our main results including controls for these alternative stringencies. The results appear in Appendix Tables C.1-C.6, and show that our results are not sensitive to the inclusion of controls for other stringencies. This suggests that the exclusion restriction is unlikely to be violated by judges' decision-making on other case margins. Additionally, Appendix Tables C.13-C.17 contain our reduced-form results, which are similar to our IV estimates, but do not rely on the exclusion or monotonicity assumptions needed for interpreting the IV estimate as a weighted average of local average treatment effects.[56] The reduced-form estimates provide transparent evidence that the assignment of a stricter judge has meaningful consequences for children's housing stability and schooling.

We also assess robustness to the choice of controls and the sample. Specifically, we examine sensitivity to (i) the choice of controls, (ii) the minimum case threshold used to construct the instrument in Cook County, where some judges hear relatively few cases, and (iii) restricting to a balanced panel of students observed in all post-filing years.[57] Appendix Tables C.1–C.6

[55]We do not observe judgment amounts in NYC.

[56]In Appendix Figure A.7, we also plot the reduced-form effects over time for education measures that we can measure in each year, which further illustrates that the instrument is uncorrelated with outcomes in pre-period.

[57]Our main specification restricts the Cook County sample to judges who hear at least 100 cases in the calendar

report estimates without lagged outcomes and without any controls; Appendix Tables C.7–C.12 report Cook County estimates that vary the minimum case threshold; and Appendix Table C.23 reports estimates for the balanced panel. The estimates are all similar to our main estimates, which indicates that our findings are not sensitive to specification or sample restrictions.

Because we study the effects of eviction across many outcomes, some individually significant estimates may reflect chance. We address this concern in two ways. First, we report, in each main table that features multiple primary outcomes, the p-value from a joint test of the null that the combined IV effects on those outcomes are all zero within a post-filing year. These tests reject the null for most outcome domains and years. For example, the home-environment outcomes (Table 3) yield p-values of 0.023 in year 1 and below 0.001 in year 2, and the Census living-arrangement outcomes (Table 4) yield a p-value of 0.013. The school-engagement outcomes have a p-value of 0.067 in year 1, but 0.404 in year 2. Second, we compute Benjamini–Hochberg (BH) p-values, which control the false discovery rate within outcome domain and year, and report them alongside our unadjusted p-values in Appendix Tables C.18–C.22. Most of our statistically significant estimates remain significant after this adjustment, while a few marginal estimates lose significance.

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 non-cognitive measures are better predictors of high school completion than conventional cognitive measures such as standardized test scores. When we turn to impacts on high school graduation, we find that eviction reduces the likelihood of graduating high school. We develop bounds on the effect for the subgroup of compliers who always remain in the district to show that this parameter estimate is robust to potential non-random attrition. Much of the effect on graduation can be accounted for by eviction's effects on intermediate outcomes such as 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

year. In New York City, all courtrooms hear more than 500 cases annually after the initial exclusion of courtrooms that involve non-random case assignment.

adversity among disadvantaged youth can impact longer-term educational attainment. 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. Our findings reinforce the need to better understand the role of family support networks and adult supervision in the lives and outcomes of low-income children, and contribute new evidence to a literature on gender differences in susceptibility to disadvantage.

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 specific eviction prevention policies could mitigate these effects, given the potential equilibrium effects of such policies, 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)	Cook County (3)
Demographics			
Age at case	8.594	8.606	
Age in 2010	14.150	14.170	13.984
Female	0.499	0.503	0.491
Black	0.768	0.769	0.471
Child relationship to household head (2000)			
Foster child	0.008	0.007	0.006
Grandchild	0.207	0.191	0.113
Household structure (2000)			
Single mom	0.436	0.454	0.331
Single mom (excl. cohabiting)	0.350	0.372	0.283
Single dad	0.049	0.047	0.066
Single dad (excl. cohabiting)	0.015	0.013	0.021
Two parent	0.252	0.258	0.485
Mom present	0.898	0.901	0.860
Dad present	0.412	0.403	0.562
Doubling up (incl. grandparents)	0.368	0.337	0.202
Doubling up (excl. grandparents)	0.191	0.168	0.174
Case characteristics			
No attorney	0.976	0.933	
Ad damnum	1.615	1.319	
Neighborhood characteristics (2000)			
Fraction Black	0.652	0.632	0.422
Poverty rate	0.269	0.274	0.277
Observations	35,000	18,000	302,539

Notes: Columns (1) and (2) present 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 2010 Decennial Census records. See Section 3.3 for details. Column (3) presents averages sourced from IPUMS for all children in Cook County renter households in the 2000 Decennial Census where the youngest child would be 18 or younger in 2010, weighted by person weights. Ad damnum is the amount the landlord is claiming the tenant owes in the case, divided by 1,000 to have a scale similar to the rest of the variables. Results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965. Results rounded following Census Bureau disclosure guidelines.

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	0.119	0.125	0.076	0.065
	(0.236)	(0.228)	(0.204)	(0.174)	(0.324)	(0.331)	(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 school 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 10% or more of school 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 attempted, 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		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Not at pre-case address	0.486	0.134	0.191	0.135**	0.223	0.141***	0.134***
	(0.500)	(0.087)	(0.393)	(0.053)	(0.416)	(0.003)	(0.047)
Number of moves	0.580	0.100	0.304	0.233**	0.332	0.234***	0.210***
	(0.649)	(0.157)	(0.654)	(0.092)	(0.659)	(0.006)	(0.081)
Neighborhood poverty	0.318	-0.011	0.301	0.024	0.303	-0.003***	0.017
	(0.147)	(0.022)	(0.120)	(0.017)	(0.124)	(0.001)	(0.014)
Homelessness†	0.008	0.047*	0.017	0.031	0.016	0.056***	0.032*
		(0.027)	(0.129)	(0.020)		(0.002)	(0.019)
McKinney-Vento	0.118	0.090					
	(0.323)	(0.065)					
Joint test††:							0.023
Observations	17,640	38,503	133,959	198,957	151,786	237,334	235,334
Post-filing school year 2:							
Not at pre-case address	0.602	0.136	0.264	0.186**	0.302	0.157***	0.176***
	(0.490)	(0.085)	(0.441)	(0.078)	(0.459)	(0.004)	(0.065)
Number of moves	0.815	0.193	0.573	0.448**	0.596	0.394***	0.407**
	(0.789)	(0.188)	(1.026)	(0.190)	(1.009)	(0.009)	(0.163)
Neighborhood poverty	0.315	-0.014	0.300	0.028	0.302	-0.003***	0.018
	(0.145)	(0.024)	(0.121)	(0.021)	(0.124)	(0.001)	(0.017)
Homelessness†	0.010	0.066*	0.023	0.051**	0.021	0.019***	0.052**
		(0.035)	(0.148)	(0.026)		(0.001)	(0.023)
McKinney-Vento	0.150	0.077					
	(0.357)	(0.069)					
Joint test††:							<0.001
Observations	16,038	34,950	110,233	163,387	125,813	196,985	194,610

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "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 in Chicago and an indicator for being listed on a family application for a shelter bed in New York. 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-FY26-P2476-R12909. Results rounded following Census Bureau disclosure guidelines. "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)-(2) report results for Chicago, (3)-(4) report results for New York City, and (5)-(7) report combined results as described in Section 5.4. Columns (1), (3), and (5) report the nonevicted mean, column (6) reports the coefficient on an eviction indicator from an OLS regression, and columns (2), (4), and (7) report 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 ($r = 0$), 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 F.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. Appendix A.4.1 reports estimates for the school year containing the filing. Appendix Table C.18 reports these estimates with Benjamini–Hochberg-adjusted p-values. ††The joint test reports the p-value from a test of the null hypothesis that the IV coefficients for all outcomes in a given year are jointly equal to zero.

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

	$\mathbb{E}[Y \mid E=0]$ (1)	OLS (2)	IV (3)
Living Arrangements			
Total household size	4.841	0.133***	0.686*
		(0.024)	(0.400)
Doubling up (incl. grandparents)	0.219	0.023***	0.169**
		(0.005)	(0.077)
Doubling up (excl. grandparents)	0.137	0.007	0.102
		(0.004)	(0.066)
Multigenerational household	0.097	0.015***	0.132**
		(0.003)	(0.054)
Household Structure			
Mother present	0.859	-0.024***	0.018
		(0.004)	(0.052)
Father present	0.308	0.006	0.003
		(0.006)	(0.082)
Single mother	0.572	-0.026***	-0.008
		(0.006)	(0.085)
Geography			
Neighborhood poverty rate	0.235	-0.003	-0.051*
		(0.002)	(0.029)
Joint test†:			0.013
Observations		48,000	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 nonevicted 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 clustered at the judge×case-year level. 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. Results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909. Results rounded following Census Bureau disclosure guidelines. †The joint test reports the p-value from a test of the null hypothesis that the IV coefficients for all outcomes under "Living Arrangements" are jointly equal to zero. Appendix Table C.19 reports these estimates with Benjamini–Hochberg-adjusted p-values. For an extended version of this table with additional results, see Appendix Table A.15.

Table 5: School Attachment and Engagement (Education Sample)

	Chicago		New York		Combined		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Not at pre-case school	0.391	0.271***	0.266	0.038	0.278	0.044***	0.077**
	(0.488)	(0.083)	(0.442)	(0.044)	(0.448)	(0.002)	(0.039)
Percent absent	0.109	0.030*	0.135	0.023**	0.133	0.007***	0.024**
	(0.124)	(0.018)	(0.166)	(0.011)	(0.163)	(0.001)	(0.010)
Chronic absent	0.361	0.213***	0.424	0.069	0.419	0.030***	0.088**
	(0.480)	(0.076)	(0.494)	(0.047)	(0.493)	(0.002)	(0.042)
Retained	0.105	-0.005	0.129	0.027	0.127	0.005***	0.022
	(0.307)	(0.045)	(0.335)	(0.027)	(0.333)	(0.001)	(0.024)
Joint test††:							0.067
Observations	14,459	40,469	164,125	241,728	178,584	282,198	282,198
Post-filing school year 2:							
Not at pre-case school	0.499	0.249***	0.382	0.037	0.394	0.058***	0.075
	(0.500)	(0.093)	(0.486)	(0.058)	(0.489)	(0.003)	(0.051)
Percent absent	0.105	-0.008	0.142	0.028*	0.139	0.005***	0.024*
	(0.123)	(0.021)	(0.175)	(0.015)	(0.172)	(0.001)	(0.014)
Chronic absent	0.343	0.011	0.432	0.062	0.426	0.021***	0.055
	(0.475)	(0.080)	(0.495)	(0.047)	(0.494)	(0.002)	(0.042)
Retained	0.144	0.051	0.157	0.052	0.156	0.007***	0.052*
	(0.351)	(0.053)	(0.364)	(0.032)	(0.363)	(0.002)	(0.028)
Joint test††:							0.404
Observations	12,132	34,363	137,378	202,292	149,510	236,655	236,655

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "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 10% or more 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 ($r = -1$). All outcomes are defined among actively enrolled students. Columns (1)-(2) report results for Chicago, (3)-(4) report results for New York City, and (5)-(7) report combined results as described in Section 5.4. Columns (1), (3), and (5) report the nonevicted mean, column (6) reports the coefficient on an eviction indicator from an OLS regression, and columns (2), (4), and (7) report 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 F.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. Appendix Table C.20 reports these estimates with Benjamini–Hochberg-adjusted p-values. ††The joint test reports the p-value from a test of the null hypothesis that the IV coefficients for all outcomes in a given year are jointly equal to zero.

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

	Chicago		New York		Combined		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Test score	-0.337	-0.133	-0.339	0.104	-0.338	-0.022***	0.059
	(0.853)	(0.127)	(0.805)	(0.095)	(0.810)	(0.004)	(0.081)
Missed test	0.057	-0.015	0.114	0.077***	0.109	0.014***	0.063***
	(0.232)	(0.047)	(0.318)	(0.025)	(0.312)	(0.001)	(0.022)
Observations	8,704	25,185	81,974	121,876	90,678	147,060	147,060
Post-filing school year 2:							
Test score	-0.330	0.114	-0.331	0.053	-0.331	-0.027***	0.068
	(0.846)	(0.144)	(0.796)	(0.109)	(0.803)	(0.005)	(0.089)
Missed test	0.057	0.036	0.183	0.080*	0.172	0.006***	0.073**
	(0.231)	(0.047)	(0.387)	(0.041)	(0.377)	(0.001)	(0.036)
Observations	8,252	24,341	68,729	102,560	76,981	126,901	126,901

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "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. "Test score" is defined as mean of reading and math scores 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 of one within each grade-school year for all students enrolled in that grade and school year in the district who took the test. "Missed test" is defined as an indicator that is equal to one if a student was actively enrolled in grades 3-8 but does not have one or both test scores. All outcomes are defined among actively enrolled students. Columns (1)-(2) report results for Chicago, (3)-(4) report results for New York City, and (5)-(7) report combined results as described in Section 5.4. Columns (1), (3), and (5) report the nonevicted mean, column (6) reports the coefficient on an eviction indicator from an OLS regression, and columns (2), (4), and (7) report 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 F.3 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		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Credits earned	0.900	-0.236**	0.830	-0.138**	0.832	-0.015***	-0.142**
	(0.225)	(0.098)	(0.426)	(0.064)	(0.423)	(0.003)	(0.062)
GPA	2.139	-0.276					
	(1.037)	(0.316)					
Observations	2,286	6,068	51,807	74,312	53,003	77,459	77,459
Post-filing school year 2:							
Credits earned	0.910	-0.165*	0.824	-0.143*	0.826	-0.020***	-0.144*
	(0.221)	(0.084)	(0.429)	(0.084)	(0.425)	(0.003)	(0.080)
GPA	2.143	-0.281					
	(1.033)	(0.299)					
Observations	2,294	6,263	50,141	71,766	51,401	75,173	75,173

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "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 earned" is the number of credits completed divided by the standard number of credits attempted 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)-(2) report results for Chicago, (3)-(4) report results for New York City, and (5)-(7) report combined results as described in Section 5.4. Columns (1), (3), and (5) report the nonevicted mean, column (6) reports the coefficient on an eviction indicator from an OLS regression, and columns (2), (4), and (7) report 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 F.4 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		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. OLS and IV estimates							
Graduation	0.753	-0.113	0.670	-0.128**	0.676	-0.039***	-0.126**
	(0.431)	(0.092)	(0.470)	(0.056)	(0.468)	(0.003)	(0.051)
Graduation status not observed	0.207	0.080	0.123	0.063	0.131	0.027***	0.066*
	(0.405)	(0.085)	(0.329)	(0.039)	(0.337)	(0.002)	(0.036)
Observations	7,646	21,014	89,700	129,452	97,346	150,467	150,467
Panel B. Bounds on LATE-AO (90% CIs in parentheses)							
$\delta^\star \in [-0.05, 0.05]$		$[-0.109, -0.1]$		$[-0.119, -0.112]^{*}$			$[-0.117, -0.11]^{**}$
		$(-0.258, 0.046)$		$(-0.207, -0.024)$			$(-0.195, -0.032)$
$\delta^\star \in [-0.1, 0.1]$		$[-0.114, -0.096]$		$[-0.123, -0.109]^{*}$			$[-0.121, -0.106]^{**}$
		$(-0.265, 0.05)$		$(-0.21, -0.02)$			$(-0.198, -0.028)$
$\delta^\star \in [-0.25, 0.25]$		$[-0.127, -0.082]$		$[-0.133, -0.098]^{*}$			$[-0.132, -0.095]^{*}$
		$(-0.286, 0.063)$		$(-0.221, -0.007)$			$(-0.21, -0.015)$

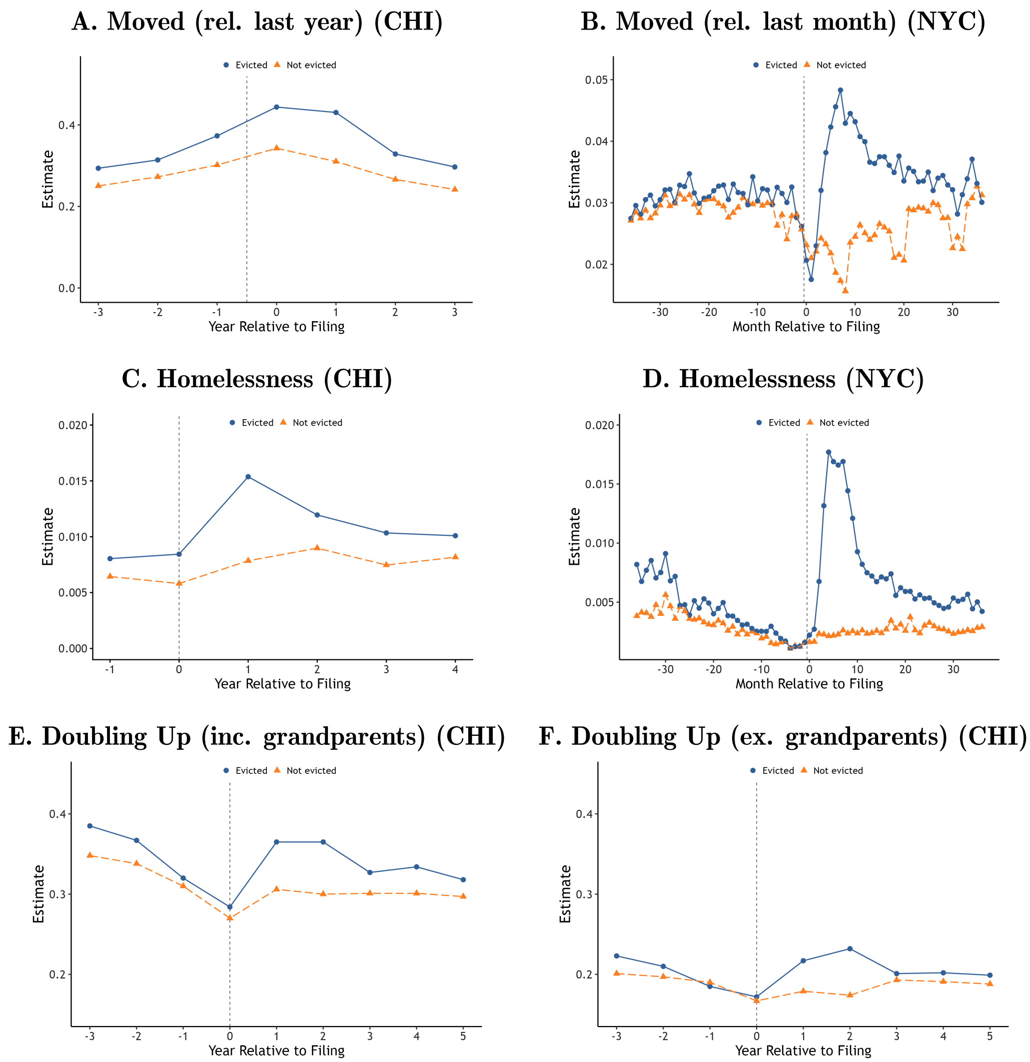
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns (1)-(2) report results for Chicago, (3)-(4) report results for New York City, and (5)-(7) report combined results as described in Section 5.4. In Panel A, "Graduation" is an indicator for if the student graduated conditional on seeing the student through at least age 18, and having a non-missing graduation status. "Graduation status not observed" is an indicator for a student who reaches age 18 by the end of our panel but whose graduation outcome is not observed, predominantly because the student moved out of the district. Columns (1), (3), and (5) report the nonevicted mean, column (6) reports the coefficient on an eviction indicator from an OLS regression, and columns (2), (4), and (7) report 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 F.5 provides cell-specific observation counts, and Appendix C checks for robustness to excluding all controls other than the fixed effects. Appendix Table C.22 reports the Panel A estimates with Benjamini–Hochberg-adjusted p-values. In Panel B, we present estimated bounds and 90% confidence intervals for the LATE-AO of graduation as developed in Section 6.5.1 for different 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.

Table 9: Education Results by Gender

	Girls		Boys		
	$\mathbb{E}[Y \mid E=0]$ (1)	IV (2)	$\mathbb{E}[Y \mid E=0]$ (3)	IV (4)	P-value of (2) = (4)
Post-filing school year 1:					
Not at pre-case address	0.222	0.145**	0.223	0.128**	0.839
	(0.415)	(0.061)	(0.417)	(0.058)	
Not at pre-case school	0.272	-0.015	0.283	0.159***	0.028
	(0.445)	(0.055)	(0.450)	(0.057)	
Percent absent	0.130	0.014	0.137	0.033**	0.331
	(0.161)	(0.014)	(0.166)	(0.014)	
Chronic absent	0.408	0.019	0.430	0.158***	0.061
	(0.492)	(0.049)	(0.495)	(0.055)	
Retained	0.112	0.038	0.142	0.010	0.561
	(0.315)	(0.035)	(0.349)	(0.033)	
Test score	-0.242	-0.065	-0.433	0.108	0.300
	(0.784)	(0.123)	(0.832)	(0.114)	
Credits earned	0.874	-0.099	0.788	-0.223**	0.326
	(0.408)	(0.082)	(0.433)	(0.096)	
Joint test†:					0.017
Observations	69,490	109,883	71,357	113,021	
Graduation	0.729	-0.049	0.621	-0.216***	0.099
	(0.444)	(0.064)	(0.485)	(0.078)	
Observations	45,421	69,282	44,412	67,234	

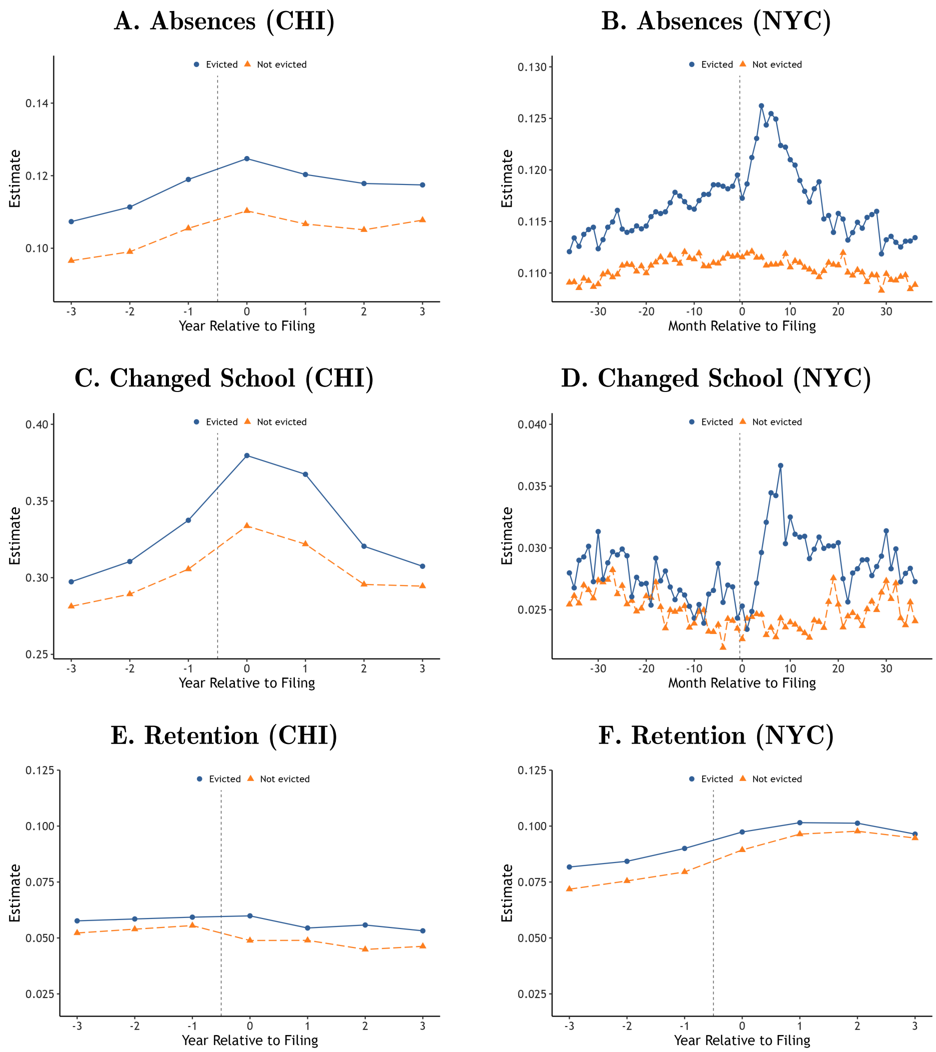
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Post-filing school year 1" is the first complete school year after the case was filed. The graduation outcome time window is defined in Table 8. Columns (1)-(2) report Chicago and New York combined results for girls, (3)-(4) report combined results for boys. Combined results are constructed as described in Section 5.4. The first column reports the nonevicted mean, and the second reports the TSLS estimate for eviction. Means are accompanied by standard deviations in parentheses, while TSLS estimates are accompanied by standard errors in parentheses. "Not at pre-case address" is an indicator for not being at the same address as the pre-case school year. "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 10% or more 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 ($r = -1$). "Test score" is defined as mean of reading and math scores 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 of one within each grade-school year for all students enrolled in that grade and school year in the district who took the test. "Credits earned" is the number of credits completed divided by the standard number of credits attempted in each district, which is 7 in Chicago and typically 14 in New York. Standard errors are clustered at the judge×case-year level. The regression specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes. †The joint test reports the p-value from a test of the null hypothesis that the difference in IV coefficients between boys and girls is jointly equal to zero across all outcomes in a given year. Appendix section A.6.3 contains detailed results by gender.

Figure 1: Housing Environment Relative to Time of Eviction Filing



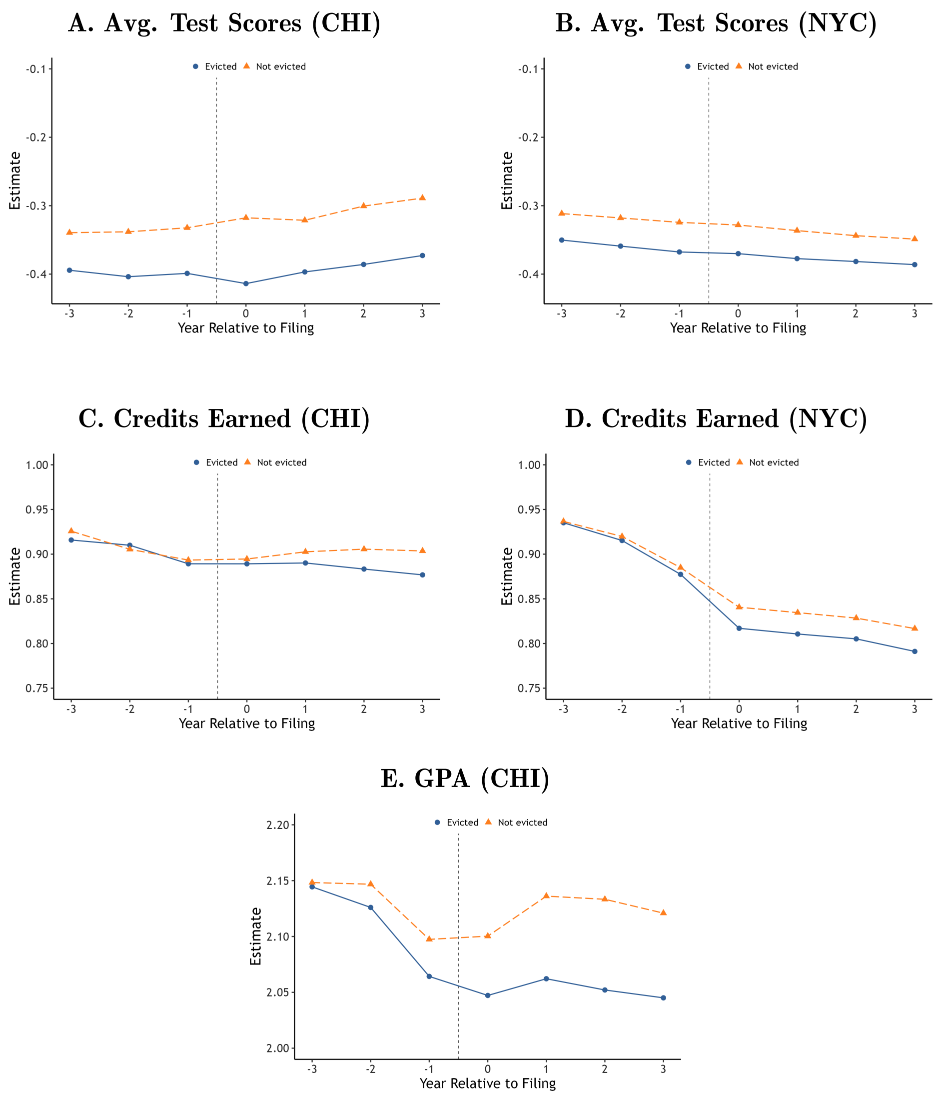
Notes: Each panel displays trends in outcomes relative to eviction filing. Panel A (B) plots trends for the Chicago education (New York education) sample for an indicator for being observed at an address other than the address of residence in the prior year (month). Panel C (D) plots trends for the Census (New York education) sample for a measure of homelessness system interaction (shelter application) in the year (month), as defined in Section 3. 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 or the adult children 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 numbers CBDRB-FY24-P2476-R11514 and CBDRB-FY26-P2476-R12909.

Figure 2: Schooling Environment Relative to Time of Eviction Filing



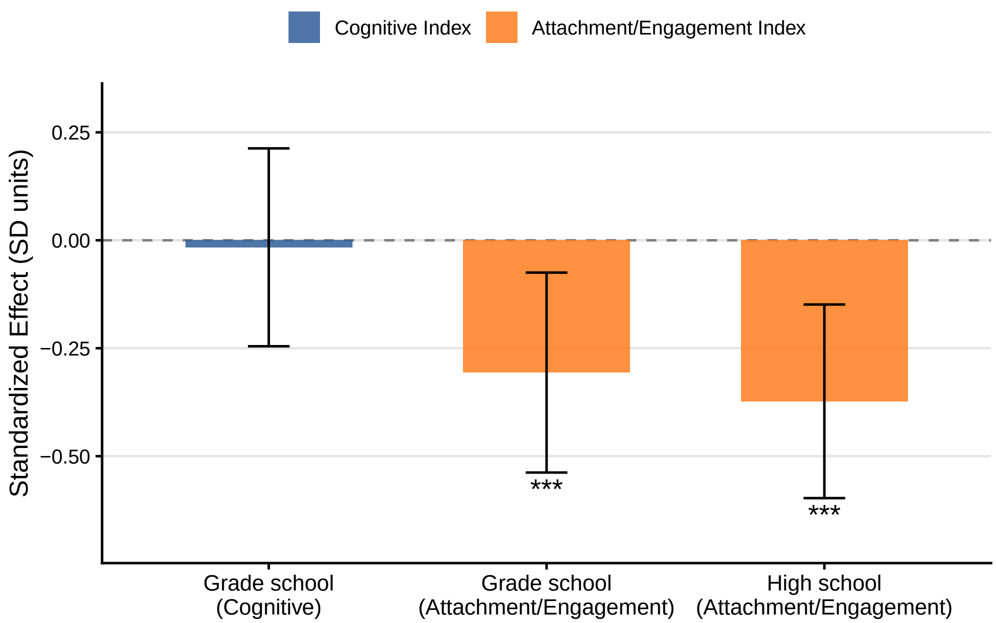
Notes: Each panel displays trends in outcomes relative to eviction filing. Panel A (B) plots trends for the Chicago (New York) education sample for the proportion of days absent in the year (month). Panel C (D) plots trends for the Chicago (New York) education sample for an indicator for being observed 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 mean of standardized reading and math test scores administered between 3rd and 8th grade, where scores have been standardized to have a mean of zero and standard deviation of one 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 number of credits completed divided by the standard number of credits attempted in each city, which is 7 in Chicago and typically 14 in New York. Panel E 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.

Figure 4: Summary Indices of Impacts on Schooling Outcomes



Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The cognitive index is based on math and reading test scores. The attachment/engagement index before high school is based on the percent of days absent, an indicator for changing schools, and an indicator for missing a standardized test. For high school, the engagement/attachment index includes the percent of days absent, an indicator for changing schools, credits earned, and (for Chicago) GPA. For Chicago, we allow for GPA or credits to be missing (but not both); otherwise, we require all outcomes. Regressions include the controls described in the notes to Table 3: court-year (court-year-quarter in New York) and child age-at-filing fixed effects, court variables, rent claim amount, indicators for legal representation, demographic controls, and outcome-specific lags averaged over relative years -3 to -1. The effects are presented for combined results, constructed as described in Section 5.4. Outcomes are averaged over year 1 and year 2 after the filing and are normalized to have a mean of zero with a standard deviation of one. If an outcome is missing for a given year, a single year is used. Error bars show 95% confidence intervals.

A Additional Tables and Figures

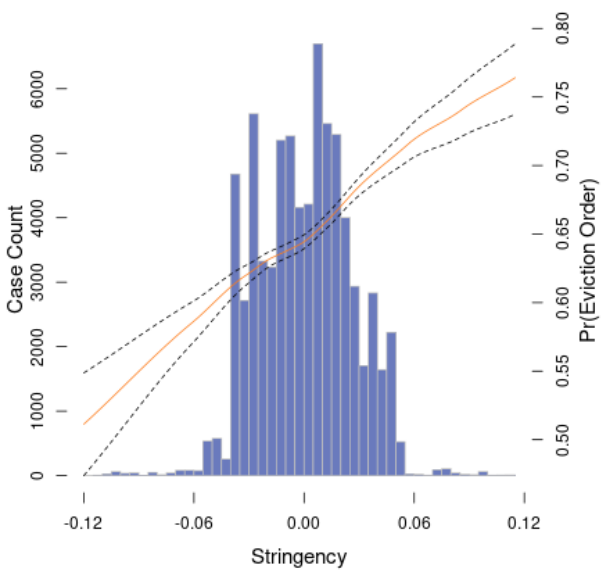
A.1 Validity of the empirical design

A.1.1 First stage.

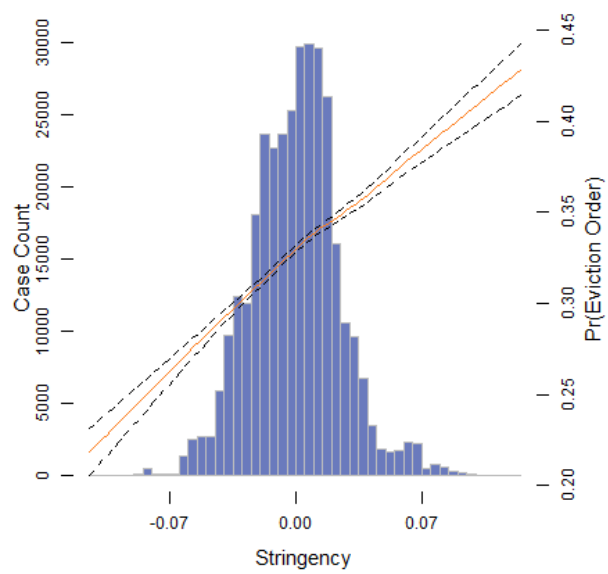
Figure A.1 shows the distribution of judge stringency (residualized by court-year-quarter). Table A.1 presents first stage estimates for our three samples.

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

(a) Chicago



(b) New York



Notes: For each location, this figure shows a histogram of the distribution of judge stringency, $Z_{j(i)}$, with the number of cases indicated along the left vertical axis. The stringencies are residualized by court-year-quarter. Each panel also depicts fitted values of the first-stage regression of eviction on judge stringency and court-year fixed effects (solid line, plotted along the right vertical axis). Dotted lines indicate 95% confidence intervals. The probability of eviction is plotted on the right y-axis against the year-specific judge stringencies. The sample restrictions are those described in Appendix B.

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

	Chicago Public Schools		New York		Census	
	(1)	(2)	(3)	(4)	(5)	(6)
Judge Stringency	1.043***	1.038***	0.842***	0.836***	0.939***	0.931***
	(0.085)	(0.084)	(0.044)	(0.044)	(0.096)	(0.095)
Controls	No	Yes	No	Yes	No	Yes
Observations	74,536	74,536	254,218	254,218	49,000	49,000
F-statistic	149.133	153.358	361.833	362.726	94.870	96.240

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports results from the first stage regression of eviction on judge stringency for Chicago and New York using the linked student sample and the Census analysis sample. Columns (1) and (3) include our judge stringency measure with court-year and age at filing date fixed effects. Columns (2) and (4) add controls (age, race, gender, free/reduced price lunch, and IEP status), three-year average lagged outcomes (percent absent, math and reading scores, indicator for retained), and dummies for missing controls. Column (5) represents a regression at the child-case level of an eviction indicator on judge stringency, and controls only for court-year fixed effects. Column (6) adds the main IV controls. Standard errors are shown in parentheses and are clustered at the judge-year level. Approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2476-R10965 and CBDRB-FY26-P2476-R12909.

A.1.2 Balance.

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

Table A.2: Balance Test (Education Sample)

	Chicago		New York	
	Evicted (1)	Stringency ($\times$ 100) (2)	Evicted (3)	Stringency ($\times$ 100) (4)
Amount Owed	0.013***	0.000	0.061***	0.004
	(0.001)	(0.005)	(0.002)	(0.008)
Tenant has Lawyer	-0.232***	0.031	-0.156***	0.015
	(0.014)	(0.064)	(0.014)	(0.136)
Female	0.003	-0.003	0.000	-0.001
	(0.003)	(0.021)	(0.002)	(0.009)
Black	0.011	0.002	-0.025***	-0.024
	(0.011)	(0.062)	(0.006)	(0.026)
Hispanic	0.030***	0.094	-0.004	-0.000
	(0.011)	(0.068)	(0.007)	(0.029)
Age	-0.003***	-0.001	-0.005***	-0.002
	(0.001)	(0.003)	(0.000)	(0.002)
FRP Lunch	0.011	-0.062	0.007**	0.015
	(0.007)	(0.079)	(0.003)	(0.014)
Learning IEP/Disabled	-0.007	-0.004	0.001	-0.005
	(0.009)	(0.053)	(0.003)	(0.017)
Speech IEP/Impairment	-0.002	-0.025	-0.006	-0.006
	(0.013)	(0.084)	(0.004)	(0.021)
IEP			-0.012***	0.004
			(0.004)	(0.018)
Born in NYC			-0.041***	-0.003
			(0.003)	(0.015)
Pct Absent	0.231***	0.082	0.236***	0.012
	(0.028)	(0.163)	(0.012)	(0.061)
Reading Score	-0.013***	0.009	-0.006***	0.007
	(0.005)	(0.026)	(0.002)	(0.012)
Math Score	-0.008*	-0.047	-0.001	-0.004
	(0.005)	(0.029)	(0.002)	(0.011)
Retained	0.014	0.082	-0.007	-0.011
	(0.012)	(0.064)	(0.005)	(0.024)
Observations	74,536	74,536	254,220	254,220
Joint F-statistic	42.835	0.925	64.847	0.831
P-Value	0.000	0.547	0.000	0.691

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

Table A.3: Balance Test (Census Sample)

	Evicted (1)	Judge stringency ($\times$ 100) (2)
Age at case	-0.000	0.004
	(0.001)	(0.006)
Female	-0.003	-0.009
	(0.004)	(0.027)
Black	-0.003	-0.056
	(0.012)	(0.068)
White	0.020	0.042
	(0.014)	(0.078)
Hispanic	0.009	-0.059
	(0.010)	(0.061)
Single mom (2000)	-0.016	-0.029
	(0.011)	(0.067)
Single dad (2000)	-0.004	0.010
	(0.016)	(0.101)
Two parent (2000)	-0.010	-0.097
	(0.014)	(0.078)
Grandchild of h.h. head (2000)	-0.010	-0.033
	(0.013)	(0.069)
Doubling up (2000)	0.025***	-0.035
	(0.008)	(0.035)
Neighborhood poverty rate	-0.152***	-0.200
	(0.029)	(0.166)
Neighborhood fraction Hispanic	0.229***	0.198
	(0.028)	(0.190)
Neighborhood fraction White	0.111**	0.069
	(0.044)	(0.243)
Neighborhood fraction Black	0.249***	0.185
	(0.043)	(0.260)
Missing neighborhood char.	0.582***	0.386
	(0.153)	(0.840)
Observations	53,000	49,000
F-statistic	13.780	0.603
P-value	p<.01	0.870

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table above reports tests of balance for our Census analysis sample. The left column presents a regression where the dependent variable is the eviction indicator, which is regressed on our main controls and court-year of the case fixed effects. The right column presents the same regression except the dependent variable is judge stringency. We perform an F-test on the null hypothesis that all coefficients are equal to zero and report the p-value. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

A.1.3 Monotonicity checks.

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

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

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

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

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

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

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

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

Sample	Coefficient	Standard Errors	Observations
All	0.931***	0.095	49,000
All (out-of-sample instrument)	0.893***	0.112	53,000
Male	1.037***	0.106	24,500
Male (out-of-sample instrument)	0.996***	0.134	26,500
Female	0.830***	0.113	24,500
Female (out-of-sample instrument)	0.793***	0.118	26,500
Age K-12	0.915***	0.092	37,500
Age K-12 (out-of-sample instrument)	0.875***	0.114	41,000
Non-Black	0.733***	0.143	11,000
Non-Black (out-of-sample instrument)	0.684***	0.171	12,500
Black	0.986***	0.111	38,000
Black (out-of-sample instrument)	0.947***	0.128	41,000
First Case	0.710***	0.139	15,500
First Case (out-of-sample instrument)	0.715***	0.150	17,000

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table above reports monotonicity tests of the Census analysis sample. The first row reports the first-stage regression for all children. The second row reports the first-stage regression for all children, using all other cases to construct the judge stringency instrument. The next rows perform the same exercise for the subgroup of male children, female children, children aged 6-18 (the K-12 subgroup), non-Black children, Black children, and first-case only children. Approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2476-R10965 and CBDRB-FY26-P2476-R12909.

A.1.4 Exclusion.

Table A.7: Correlation Between Various Judge Stringency Measures (Cook County)

	Eviction Order	Continuance	Amount	Stays
Stringency (eviction order)	1	−0.065	0.101	0.081
Stringency (continuance)	−0.065	1	0.025	0.015
Stringency (judgment amount)	0.101	0.025	1	0.097
Stringency (stays)	0.081	0.015	0.097	1

Notes: This table reports the correlation between judges' eviction order stringency and three other stringency measures in Cook County: stringency in granting continuances, stringency in judgment amount, and stringency in granting stays. Stringency measures are judge-year leave-one out averages. Judgment amount stringency is the leave-one-out average of the difference between judgment amounts and ad damnum amounts for cases ending in an eviction. We also calculate stringency related to granting stays of the eviction order among the cases ending in an eviction order.

Table A.8: Correlation Between Various Judge Stringency Measures (New York)

	Stringency	Stays
Stringency (eviction order)	1	0.0457
Stringency (stays)	0.0457	1

Notes: This table reports the correlation between judge's eviction stringency and stringency in granting stays. All stringencies are judge-year leave-one-out averages.

A.2 Additional background and details on trends around eviction filing

A.2.1 Estimating proportion of households facing eviction with children

Table A.9: Census Sample: Estimating the Proportion of Households Facing Eviction With Children

	Households in eviction court		All households
	2000 Decennial (1)	2010 Decennial (2)	2000 Decennial (3)
Share of households with children			
Option 1: Randomized	0.630	0.558	0.35
Option 2: Household head	0.597	0.534	
Option 3: Female first	0.631	0.560	
Number of children per household (among those with children)			
Option 1 (random)	2.472	2.342	2.095

Notes: For columns (1) and (2), the table above merges tenants in eviction court to the Decennial Census year indicated in the column heading. In cases with more than one tenant listed, we select one tenant according to three different rules (indicated in the row): (i) randomly choosing the tenant, (ii) choosing the Census household head, (iii) choosing the female first and if there are multiple female adults we choose one at random. We then report the proportion of these tenants with children aged 0-18 in the household, and, in the last row, the number of children per household (for those with children). Column (3) includes information sourced from IPUMS on all households in Cook County present in the 2000 Decennial and therefore is not calculated using any of the options listed. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2476-R10965.

Table A.10: Number of cases

Number of cases (1)	Share of cases (2)
One	0.683
Two	0.199
More than two	0.117

Notes: The table above reports the proportion of children in the Cook County sample with one case only, two cases only, and more than two cases. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

A.2.2 Additional details and results on trends around eviction filing

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

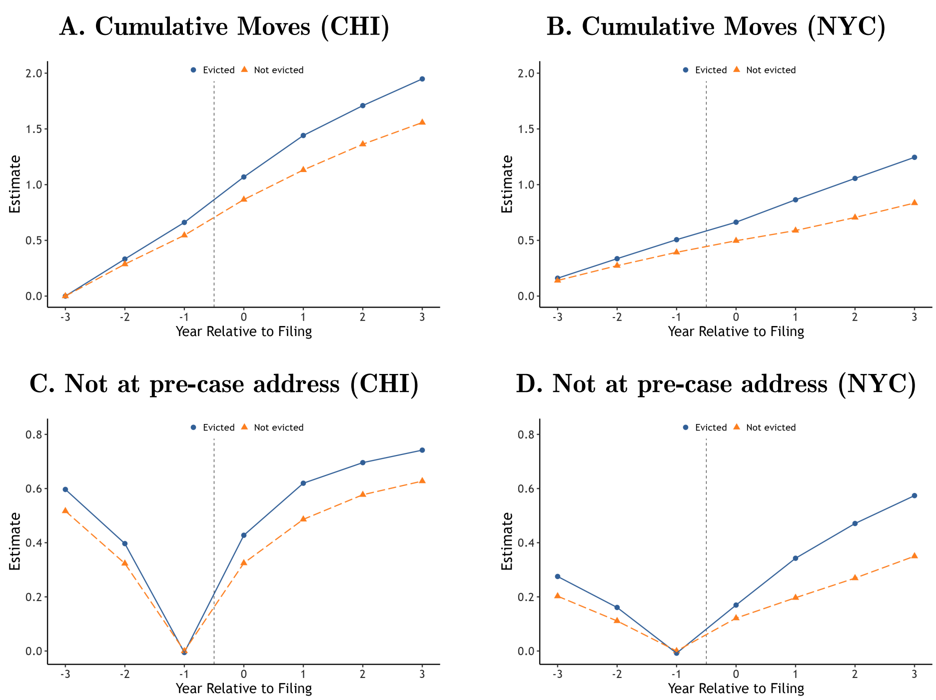
$$\mathbb{E}[Y_{i,r}|E_i = e, X_{i,-1}] = \alpha + \beta_r \mathbb{1}[r \neq -1] + \delta_r E_i + \gamma X_{i,-1}.$$

The mean of $\mathbb{E}[Y_{i,r}|E_i = e, X_{i,-1}]$ re-weighted to the characteristics of the nonevicted group in the omitted period is $\mathbb{E}\left[\mathbb{E}[Y_{i,r}|E_i = e, X_{i,-1}]\middle|E_i = 0\right]$. Using the above equation, this value equals

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

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

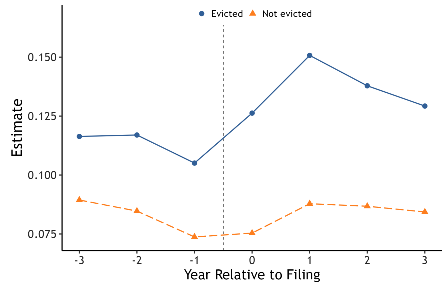
Figure A.2: Education Sample: Residential Address Changes



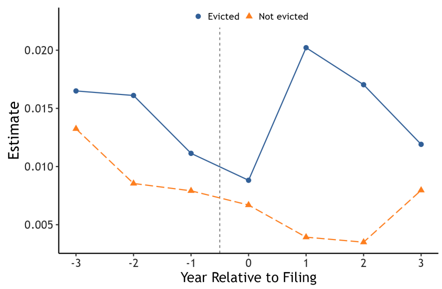
Notes: Each panel displays trends in outcomes relative to eviction filing. Panel A (B) plots the number of moves recorded between relative years -3 and 3 for the Chicago (New York) education sample. Panel C (D) plots the proportion of children not residing at their t=-1 address according to the Chicago (New York) education records by school year relative to eviction filing year.

Figure A.3: Chicago Education Sample: McKinney-Vento

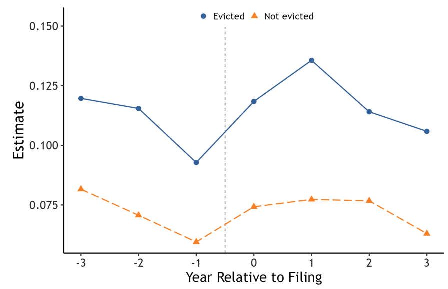
A. McKinney-Vento Flag (CHI)



B. McKinney-Vento Shelter (CHI)



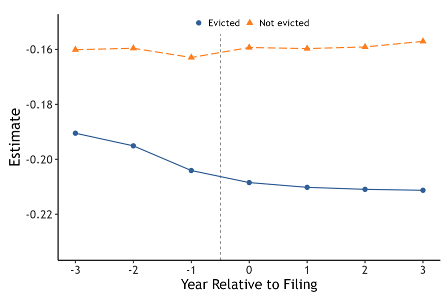
C. McKinney-Vento Doubling Up (CHI)



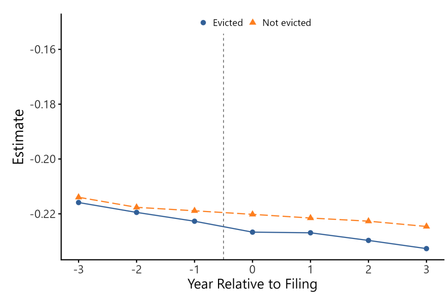
Notes: Each panel displays annual trends from -3 to 3 years relative to eviction filing for children in the Chicago education sample using the panel structure of the data. Panels A plots trends for a McKinney-Vento flag for student homelessness, which primarily indicates living doubled-up or at a homeless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

Figure A.4: Education Sample: School Average Test Scores

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

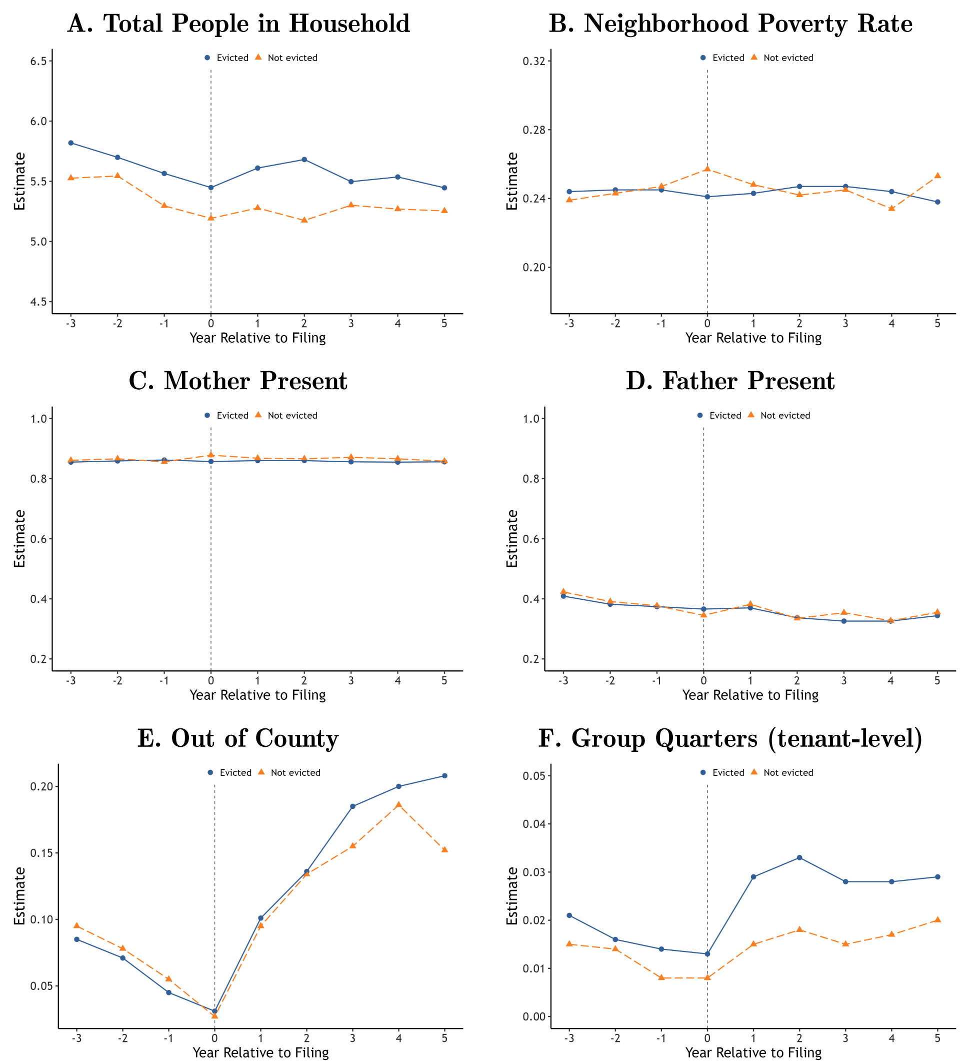


B. School's Avg. Test Scores (NYC)



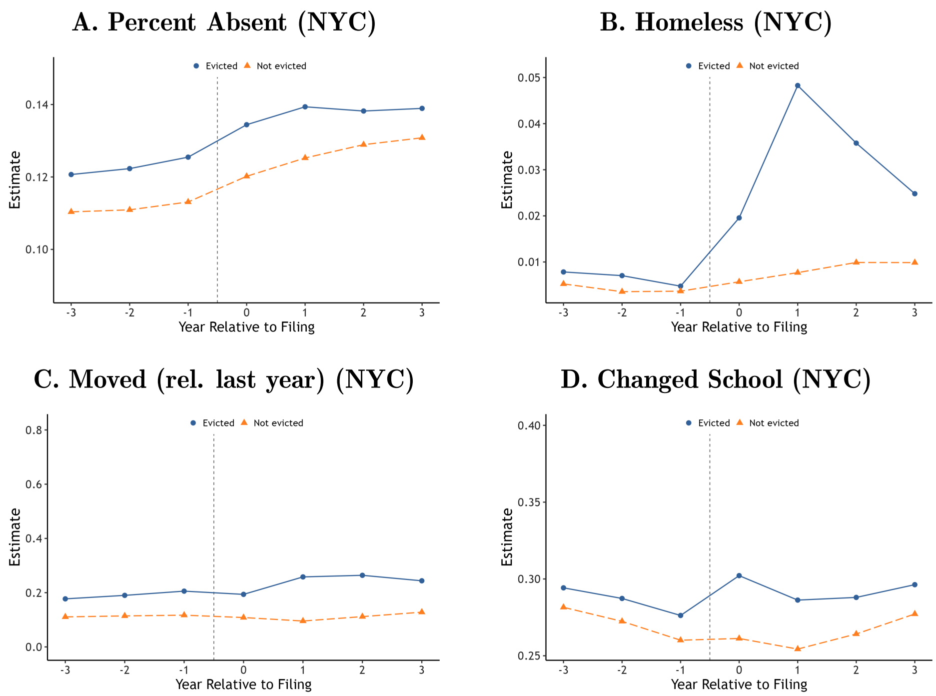
Notes: Each panel displays trends in outcomes relative to eviction filing. Panel A (B) plots trends for the Chicago (New York) education sample for the average achievement of the school attended as measured by average test scores and is defined among actively enrolled students. See equation 4.1 and related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

Figure A.5: Census Sample: Additional Trends



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

Figure A.6: New York Education Sample: Yearly Trends



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

A.3 Additional IV/OLS results

Table A.11: School Average Test Scores (Education Sample)

	Chicago		New York		Combined		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	$\mathbb{E}[Y\|E=0]$ (5)	OLS (6)	IV (7)
Post-filing school year 1:							
School's Avg. Test Scores	-0.157	-0.004	-0.230	0.063*	-0.221	-0.007***	0.049
	(0.455)	(0.049)	(0.378)	(0.036)	(0.388)	(0.001)	(0.030)
Observations	14,755	42,607	114,214	152,106	128,969	194,713	194,713
Post-filing school year 2:							
School's Avg. Test Scores	-0.152	0.046	-0.230	0.056	-0.220	-0.011***	0.054
	(0.459)	(0.057)	(0.388)	(0.048)	(0.398)	(0.002)	(0.039)
Observations	12,128	35,363	89,188	118,029	101,316	153,392	153,392

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "Post-filing school year 1" is the first complete school year after the case was filed. "Post-filing school year 2" is the second complete school year after the case was filed. "School average test score" is the average achievement of school attended as measured by average test scores and is defined among actively enrolled students. Columns (1)-(2) report results for Chicago, (3)-(4) report results for New York City, and (5)-(7) report combined results as described in Section 5.4. Within each pair of city columns, the first reports the nonevicted mean (with standard deviations in parentheses) and the second reports the TSLS estimate for eviction. Columns (5) to (7) report the combined nonevicted mean, the OLS estimate, and the TSLS estimate. Means include standard deviations in parentheses, while OLS and TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3.

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

	Chicago		New York		Combined		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	$\mathbb{E}[Y\|E=0]$ (5)	OLS (6)	IV (7)
Graduated on time	0.711	-0.070	0.568	-0.044	0.578	-0.037***	-0.047
	(0.453)	(0.088)	(0.495)	(0.067)	(0.494)	(0.003)	(0.060)
Dropped out	0.233	0.086	0.212	0.134**	0.213	0.029***	0.128**
	(0.423)	(0.087)	(0.409)	(0.056)	(0.410)	(0.003)	(0.051)
Observations	6,034	16,242	83,384	119,605	89,418	135,846	135,846

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "Graduated on time" is an indicator for students who graduate within four years of first entering 9th grade and is defined for students who are actively enrolled in 9th grade at some point in our panels. "Dropped out" is an indicator for if the student dropped out of high school, conditional on seeing the student through at least age 18. Dropping out is not the exact reverse of graduating because some students may appear as "still enrolled" in the education records. Columns (1)-(2) report results for Chicago, (3)-(4) report results for New York City, and (5)-(7) report combined results as described in Section 5.4. Within each pair of city columns, the first reports the nonevicted mean (with standard deviations in parentheses) and the second reports the TSLS estimate for eviction. Columns (5) to (7) report the combined nonevicted mean, the OLS estimate, and the TSLS estimate. Means include standard deviations in parentheses, while OLS and TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3. For each column, the final row reports the average sample size across outcomes.

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

	Pr[Lower poverty neighborhood]	Pr[Lower poverty neighborhood ∩ multi-gen]	Pr[Lower poverty neighborhood ∩ no-multi-gen]
	(1)	(2)	(3)
IV: Evicted	0.099	0.062	0.037
	(0.084)	(0.038)	(0.087)
Dependent variable mean	0.371	0.037	0.333

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. This table estimates the impact of eviction on living in a lower-poverty Census tract in 2010 relative to the case address (column 1), using our IV specification of Table 4. We interact this outcome with an indicator for the 2010 household being multigenerational and estimate the impact on this interacted outcome in column 2; we also interact this outcome with an indicator for the 2010 household not being multigenerational and estimate the impact on this interacted outcome in column 3. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

A.3.1 Attrition out of sample

Table A.14: Effects of Eviction on Transferring Out of District

	Chicago		New York		Combined		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Transferred out of school system	0.126	-0.073	0.031	0.008	0.042	0.010***	-0.007
	(0.332)	(0.051)	(0.174)	(0.015)	(0.201)	(0.001)	(0.016)
Observations	24,744	71,343	197,182	291,736	221,926	363,079	363,079
Post-filing school year 2:							
Transferred out of school system	0.162	0.001	0.029	0.026	0.043	0.008***	0.021
	(0.369)	(0.051)	(0.168)	(0.016)	(0.203)	(0.001)	(0.016)
Observations	23,565	68,474	197,182	291,736	220,747	360,210	360,210

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "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. "Transferred out of school system" is an indicator for whether a student transferred out of the school district during the relevant period. Columns (1)-(2) report results for Chicago, (3)-(4) report results for New York City, and (5)-(7) report combined results as described in Section 5.4. Within each pair of city columns, the first reports the nonevicted mean (with standard deviations in parentheses) and the second reports the TSLS estimate for eviction. Columns (5) to (7) report the combined nonevicted mean, the OLS estimate, and the TSLS estimate. 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.

A.4 Extended Results

Table A.15: Living Arrangements, Household Structure, and Geography (Census Sample; All Outcomes)

	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	IV (3)
Living Arrangements			
Total household size	4.841	0.133***	0.686*
		(0.024)	(0.400)
Doubling up (incl. grandparents)	0.219	0.023***	0.169**
		(0.005)	(0.077)
Doubling up (excl. grandparents)	0.137	0.007	0.102
		(0.004)	(0.066)
Multigenerational household	0.097	0.015***	0.132**
		(0.003)	(0.054)
Grandparent household head	0.086	0.017***	0.097**
		(0.003)	(0.049)
Household Structure			
Mother present	0.859	-0.024***	0.018
		(0.004)	(0.052)
Father present	0.308	0.006	0.003
		(0.006)	(0.082)
Single mother	0.572	-0.026***	-0.008
		(0.006)	(0.085)
Non-relative household head	0.015	0.002	-0.000
		(0.002)	(0.025)
Geography			
Neighborhood poverty rate	0.235	-0.003	-0.051*
		(0.002)	(0.029)
Out of county	0.234	0.021***	-0.028
		(0.005)	(0.067)
Out of state	0.139	0.021***	-0.015
		(0.005)	(0.060)
Observations		48,000	48,000

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. This table reproduces Table 4 with additional outcomes. The 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 nonevicted 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 clustered at the judge×case-year level. 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-FY26-P2476-R12909.

A.4.1 Education sample case filing year IV/OLS results

This appendix section reports IV/OLS results for the "case filing year," to complement the "first post-filing year" and "second post-filing year" results reported in the main text. For "first post-filing year," outcomes are measured in the first academic school year after the case is filed. In contrast, "case filing year" measures more immediate outcomes, but the outcomes often combine pre- and post-filing behavior (such as the percent of days in the school year absent). For example, if a case is filed in September, most of that school year will be after the filing, but if a case is filed in June, most of that school year will be before the filing. For summer cases, we define "case filing year" as the school year that starts immediately after summer. Note that, for summer cases, the same school year contributes to the "case filing year" results reported here and the "first post-filing year" results reported in the paper. See Section B.2.3 for additional details on the timing of outcomes.

Table A.16: Home Environment (Education Sample)

	Chicago			New York			Combined
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Case filing year:							
Not at pre-case address	0.344	0.095***	0.292***	0.110	0.095***	0.087**	0.123***
	(0.475)	(0.007)	(0.093)	(0.313)	(0.003)	(0.037)	(0.035)
Neighborhood poverty	0.322	0.000	-0.004	0.301	-0.004***	0.009	0.007
	(0.148)	(0.002)	(0.030)	(0.119)	(0.001)	(0.014)	(0.013)
McKinney-Vento	0.082	0.039***	0.073				
	(0.275)	(0.004)	(0.063)				
Observations	17,761	50,029	50,029	148,582	219,812	219,812	272,936

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "Case filing year" is the school year in which the case was filed and the upcoming year for cases filed in the summer. "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. "McKinney-Vento" is an indicator of the student being in an unstable living situation. 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) reports combined results as described in Section 5.4. Columns (1) and (4) report the nonevicted mean, columns (2) and (5) report the coefficient on an eviction indicator from an OLS regression, and columns (3), (6), and (7) report 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. The regression and sample specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes.

Table A.17: School Attachment and Engagement (Education Sample)

	Chicago			New York			Combined
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Case filing year:							
Not at pre-case school	0.262	0.049***	0.325***	0.153	0.014***	0.018	0.068**
	(0.440)	(0.005)	(0.079)	(0.360)	(0.002)	(0.032)	(0.029)
Percent absent	0.113	0.008***	-0.014	0.126	0.004***	0.012	0.008
	(0.127)	(0.001)	(0.020)	(0.149)	(0.000)	(0.009)	(0.008)
Chronic absent	0.381	0.043***	0.088	0.414	0.019***	0.052	0.057
	(0.486)	(0.005)	(0.085)	(0.493)	(0.002)	(0.043)	(0.039)
Observations	15,896	44,530	44,530	185,357	274,594	274,594	319,123

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Case filing year" is the school year in which the case was filed and the upcoming year for cases filed in the summer. "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 10% or more 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 ($r = -1$). Columns (1)-(3) report results for Chicago, (4)-(6) report results for New York City, and (7) reports combined results as described in Section 5.4. Columns (1) and (4) report the nonevicted mean, columns (2) and (5) report the coefficient on an eviction indicator from an OLS regression, and columns (3), (6), and (7) report 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 A.18: 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	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Case filing year:							
Test score	-0.324	-0.040***	0.039	-0.341	-0.014***	0.029	0.031
	(0.846)	(0.008)	(0.109)	(0.811)	(0.004)	(0.072)	(0.063)
Missed test	0.058	0.005	0.002	0.052	0.009***	0.033	0.028
	(0.234)	(0.003)	(0.049)	(0.222)	(0.001)	(0.022)	(0.020)
Observations	9,666	28,069	28,069	96,191	143,286	143,286	171,354

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Case filing year" is the school year in which the case was filed and the upcoming year for cases filed in the summer. "Test score" is defined as mean of reading and math scores 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 of one within each grade-school year for all students enrolled in that grade and school year in the district who took the test. "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) reports combined results as described in Section 5.4. Columns (1) and (4) report the nonevicted mean, columns (2) and (5) report the coefficient on an eviction indicator from an OLS regression, and columns (3), (6), and (7) report 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 A.19: High School Credit Accumulation and GPA (Education Sample)

	Chicago			New York			Combined
	$\mathbb{E}[Y\|E=0]$	OLS	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Case filing year:							
Credits earned	0.898	-0.003	-0.159	0.850	-0.018***	-0.081	-0.085
	(0.226)	(0.007)	(0.115)	(0.417)	(0.003)	(0.056)	(0.054)
GPA	2.118	-0.049***	-0.323				
	(1.020)	(0.013)	(0.255)				
Observations	2,329	6,158	6,158	47,481	68,604	68,604	71,783

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Case filing year" is the school year in which the case was filed and the upcoming year for cases filed in the summer. "Credits earned" is the number of credits completed divided by the standard number of credits attempted 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) report combined results as described in Section 5.4. Columns (1) and (4) report the nonevicted mean, columns (2) and (5) report the coefficient on an eviction indicator from an OLS regression, and columns (3), (6), and (7) report 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.

A.5 What predicts graduation?

Table A.20 studies what intermediate outcomes predict high school graduation, focusing on outcomes measured in grades 6-8 (middle school), as well as 9th grade, where we observe additional outcomes, such as credits earned and GPA. The regression is not restricted to children facing eviction, but it is restricted to individuals for whom graduation and the intermediate outcomes are observed, as described in the table notes. The first column considers only math and reading test scores (averaged across grades 6-8). When considering only test scores, we find that they explain 6 percent of the variation in the graduation outcome in Chicago and 13 percent in New York and have sizable coefficients (with a one standard deviation increase in test scores predicting a 3 to 13 percentage point increase in the probability of graduating). When only considering the proportion of days absent, a chronic absenteeism indicator, and changing residential address in middle school, they explain 11 percent of the variation in graduation rates in Chicago and 23 percent in New York. When including all these measures in a single regression, they explain 14 and 27 percent of the variation in the outcome, respectively. These three regressions imply that absenteeism and residential mobility better predict graduation than test scores, and adding middle school absenteeism and mobility to the regression with test scores notably increases the R-squared value.

The fourth and fifth columns of Table A.20 additionally add 9th grade credits earned, GPA, proportion of days absent, and changing residential address. Column 4 omits test scores yet has large R-squared values of 0.36 for Chicago and 0.53 for New York. Column 5 adds middle-school test scores to the prior regression and only marginally increases its predictive power, with the R-squared rising by 0.0005 in Chicago and 0.005 in New York. Similarly, the coefficients on test scores are substantially smaller than those discussed in the prior paragraph, ranging from -0.0024 (and statistically insignificant) for math scores in Chicago to 0.026 for reading scores in New York. Overall, these results suggest that the outcomes we measure in middle school and 9th grade are quite predictive of graduation.

One question is how our IV estimates on intermediate outcomes compare to our estimates on high school graduation. Using Table A.20 and our IV estimates for the intermediate schooling and housing outcomes, we can construct a back-of-the-envelope estimate of how impacts on intermediate outcomes may affect graduation. Consider a student who is evicted before the start of 8th grade. First, we will consider the regression in column (3) of Table A.20 that includes middle school math and reading scores, percent of days absent, a chronic absenteeism indicator, and an indicator for changing residential addresses. We can multiply the combined IV estimates for each of these outcomes in post-filing school year 1 by the weighted average of the regression coefficients in column (3) for Chicago and New York.[58] Doing so yields a predicted change in graduation of -5.0 percentage points, though this does not include intermediate

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

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

Second, we calculate the predicted change in graduation using the more complete set of predictors in column (5) of Table A.20. As this column uses predictors from middle school and high school, we will need to use IV estimates from both post-filing year 1 and post-filing year 2. Specifically, as we are considering a student evicted before the start of 8th grade, we use the IV estimates for post-filing year 1 for the middle school outcomes (i.e., those in Panel A), as this is when the student would be in 8th grade, and post-filing year 2 for the 9th grade outcomes (i.e., those in Panel B) as this is when the student would be in 9th grade. We then multiply these combined IV estimates by the coefficients in column (5) of Table A.20, where doing so yields a predicted change in graduation of -12.0 percentage points, which is similar to the -12.6 percentage point reduction in high school graduation we estimate. Overall, this exercise suggests that the impact on intermediate outcomes we estimate can broadly rationalize our estimated change in graduation rates.

Table A.20: Middle and High School Predictors of Graduation

	Chicago					New York				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Intercept	0.8637	0.9774	0.9531	0.31	0.3125	0.7881	0.9732	0.9422	0.2431	0.269
	(0.002)	(0.0033)	(0.0034)	(0.0127)	(0.0127)	(0.0006)	(0.0008)	(0.0008)	(0.0035)	(0.0035)
Panel A. Grades 6-8 variables										
Math test score	0.0566***		0.0285***		-0.0024	0.1324***		0.0564***		0.0085***
	(0.0036)		(0.0035)		(0.0032)	(0.0009)		(0.0009)		(0.0007)
Reading test score	0.031***		0.0303***		0.0139***	0.0374***		0.0498***		0.0259***
	(0.0037)		(0.0035)		(0.0031)	(0.0009)		(0.0008)		(0.0007)
Percent absent		-1.9082***	-1.5759***	-0.176**	-0.1833**		-2.1211***	-1.764***	-0.2858***	-0.2227***
		(0.0828)	(0.0828)	(0.0752)	(0.0754)		(0.0184)	(0.0173)	(0.0137)	(0.0137)
Chronic absent		-0.1191***	-0.1174***	-0.063***	-0.0599***		-0.1515***	-0.1362***	-0.0346***	-0.0354***
		(0.0146)	(0.0144)	(0.0125)	(0.0125)		(0.0038)	(0.0036)	(0.0028)	(0.0028)
Changed address		-0.0518***	-0.0276***	-0.0109	-0.0082		-0.0269***	-0.0085***	-0.0106***	-0.0048***
		(0.0092)	(0.0091)	(0.0079)	(0.0079)		(0.0022)	(0.0022)	(0.0018)	(0.0017)
Panel B. Grade 9 variables										
Credits				0.5627***	0.5785***				0.761***	0.7247***
				(0.0136)	(0.014)				(0.0034)	(0.0034)
GPA				0.0374***	0.0307***					
				(0.002)	(0.0025)					
Percent absent				-0.4981***	-0.4951***				-0.1762***	-0.204***
				(0.0289)	(0.0289)				(0.0053)	(0.0053)
Chronic absent				-0.0388***	-0.0391***				-0.0849***	-0.0791***
				(0.0059)	(0.0059)				(0.0019)	(0.0019)
Changed address				0.003	0.0033				-0.009***	-0.0066***
				(0.0043)	(0.0043)				(0.0014)	(0.0014)
R squared	0.0621	0.1104	0.1356	0.3552	0.356	0.1317	0.2286	0.2721	0.5273	0.5319
Observations	25,546	25,558	25,546	25,558	25,546	462,787	462,787	462,787	462,787	462,787

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

A.6 Heterogeneity and Subgroup Analyses

A.6.1 Characterizing compliers

Table A.21: Characterizing Compliers

	Chicago			New York			Combined		
	Evicted	Non-evicted	Complier	Evicted	Non-evicted	Complier	Evicted	Non-evicted	Complier
Panel A. Characteristics									
Female	0.491	0.491	0.505	0.490	0.492	0.479	0.490	0.492	0.483
			(0.030)			(0.039)			(0.033)
Black	0.765	0.769	0.762	0.396	0.429	0.432	0.451	0.480	0.482
			(0.048)			(0.050)			(0.043)
Ad damnum	2,108	1,740	1,763	4,407	3,764	2,415	4,063	3,461	2,317
			(269)			(1,223)			(1,041)
Ad damnum below median	0.467	0.564	0.626	0.421	0.542	0.525	0.428	0.545	0.540
			(0.057)			(0.049)			(0.043)
Panel B. Pre-case outcomes									
Not at pre-case address (in -2)	0.379	0.302	0.313	0.212	0.110	0.096	0.237	0.138	0.128
			(0.050)			(0.025)			(0.022)
Retained (in -1)	0.058	0.052	0.032	0.117	0.122	0.088	0.108	0.112	0.080
			(0.019)			(0.030)			(0.026)
Chronic absent (in -1)	0.414	0.358	0.308	0.445	0.406	0.410	0.441	0.399	0.395
			(0.063)			(0.043)			(0.038)
Percent absent (in -1)	0.117	0.105	0.096	0.113	0.105	0.108	0.114	0.105	0.106
			(0.015)			(0.008)			(0.007)
Test score (in -1)	-0.387	-0.328	-0.215	-0.229	-0.231	-0.263	-0.253	-0.246	-0.255
			(0.082)			(0.028)			(0.027)
Credits earned (in -1)	0.888	0.893	0.978	0.887	0.907	0.920	0.887	0.905	0.929
			(0.165)			(0.055)			(0.053)
GPA (in -1)	2.046	2.098	2.334						
			(0.331)						
Panel C. Case school year outcomes if not evicted									
Not at pre-case address		0.344	0.319		0.110	0.079		0.145	0.115
			(0.055)			(0.026)			(0.024)
Chronic absent		0.381	0.357		0.414	0.444		0.409	0.431
			(0.056)			(0.039)			(0.034)
Percent absent		0.113	0.122		0.126	0.128		0.124	0.127
			(0.014)			(0.011)			(0.010)
Test Score		-0.324	-0.288		-0.341	-0.431		-0.339	-0.410
			(0.095)			(0.076)			(0.066)
Credits earned		0.898	1.024		0.850	0.910		0.857	0.927
			(0.062)			(0.061)			(0.053)
GPA		2.118	2.316						
			(0.263)						

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

A.6.2 Grade at filing IV splits

Appendix Table A.22 shows pooled (across Chicago and New York) education results separately for younger students with case filings during grades 1-5 (capturing students with filings during elementary school) and for older students with case filings during grades 6-12 (capturing students with filings during middle and high school).[59] This is similar to the gender split reported in Table 9 and includes a joint-test across all five outcomes. We only present home environment and schooling attachment and engagement results because the other main tables present outcomes that are primarily observed for only one of these two groups (e.g., credits are only observed for students in grades 9-12). We focus on the combined estimate due to limited sample sizes in the site-by-grade results. Appendix Tables A.23 and A.24 show the full set of combined home environment and schooling attachment and engagement outcomes.

Table A.22: Results by Grade Split (Education Sample)

	Grades 1-5 at Filing		Grades 6-12 at Filing		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	P-value of
	(1)	(2)	(3)	(4)	(2)= (4)
Post-filing school year 1:					
Not at pre-case address	0.238	0.205***	0.200	0.048	0.090
	(0.426)	(0.060)	(0.400)	(0.070)	
Not at pre-case school	0.261	0.017	0.286	0.129**	0.156
	(0.439)	(0.059)	(0.452)	(0.052)	
Percent absent	0.087	0.014*	0.172	0.033**	0.312
	(0.077)	(0.008)	(0.202)	(0.017)	
Chronic absent	0.327	0.043	0.493	0.134***	0.226
	(0.469)	(0.056)	(0.500)	(0.050)	
Retained	0.089	0.080**	0.153	-0.006	0.076
	(0.284)	(0.031)	(0.360)	(0.037)	
Joint test†:					0.011
Observations	76,687	125,162	86,432	132,100	

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "Post-filing school year 1" is the first 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. "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 10% or more 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 ($r = -1$). All outcomes are defined among actively enrolled students. Columns (1)-(2) report Chicago and New York combined results for students with case filings in grades 1-5, and (3)-(4) report combined results for students with case filings in grades 6-12. The final column reports the p-value of the test of equal IV effects across groups. Note: these estimates do not include kindergartners, who are included in the main sample. Combined results are constructed as described in Section 5.4. The first column reports the nonevicted mean, and the second reports the TSLS estimate for eviction. Means are accompanied by standard deviations in parentheses, while TSLS estimates are accompanied by standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes. †The joint test reports the p-value from a test of the null hypothesis that the difference in IV coefficients between students in grades 1-5 and 6-12 at filing is jointly equal to zero across all outcomes in a given year.

[59] We exclude kindergartners from this analysis, though they are included in our main analysis.

Table A.23: Home Environment by Grade at Filing (Education Sample)

	Grades 1-5 at Filing		Grades 6-12 at Filing		
	$\mathbb{E}[Y \mid E=0]$ (1)	IV (2)	$\mathbb{E}[Y \mid E=0]$ (3)	IV (4)	P-value of (2) = (4)
Post-filing school year 1:					
Not at pre-case address	0.238	0.205***	0.200	0.048	0.090
	(0.426)	(0.060)	(0.400)	(0.070)	
Number of moves	0.360	0.374***	0.295	0.021	0.024
	(0.682)	(0.107)	(0.626)	(0.114)	
Neighborhood poverty	0.305	0.020	0.300	0.017	0.894
	(0.123)	(0.018)	(0.123)	(0.018)	
Observations	67,977	111,715	58,702	92,854	
Post-filing school year 2:					
Not at pre-case address	0.315	0.177**	0.276	0.201**	0.852
	(0.465)	(0.084)	(0.447)	(0.097)	
Number of moves	0.636	0.608***	0.528	0.218	0.237
	(1.038)	(0.207)	(0.959)	(0.256)	
Neighborhood poverty	0.302	0.038*	0.300	-0.016	0.083
	(0.124)	(0.022)	(0.124)	(0.022)	
Observations	58,679	95,615	38,542	61,457	

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

Table A.24: Schooling Attachment and Engagement by Grade at Filing (Education Sample)

	Grades 1-5 at Filing		Grades 6-12 at Filing		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	P-value of
	(1)	(2)	(3)	(4)	(2) = (4)
Post-filing school year 1:					
Not at pre-case school	0.261	0.017	0.286	0.129**	0.156
	(0.439)	(0.059)	(0.452)	(0.052)	
Percent absent	0.087	0.014*	0.172	0.033**	0.312
	(0.077)	(0.008)	(0.202)	(0.017)	
Chronic absent	0.327	0.043	0.493	0.134***	0.226
	(0.469)	(0.056)	(0.500)	(0.050)	
Retained	0.089	0.080**	0.153	-0.006	0.076
	(0.284)	(0.031)	(0.360)	(0.037)	
Observations	79,398	129,419	93,827	142,682	
Post-filing school year 2:					
Not at pre-case school	0.348	0.029	0.456	0.152**	0.234
	(0.476)	(0.073)	(0.498)	(0.073)	
Percent absent	0.088	0.011	0.190	0.035	0.346
	(0.082)	(0.009)	(0.217)	(0.024)	
Chronic absent	0.328	0.001	0.521	0.114**	0.165
	(0.470)	(0.062)	(0.500)	(0.052)	
Retained	0.117	0.070*	0.187	0.053	0.779
	(0.321)	(0.038)	(0.390)	(0.047)	
Observations	72,714	118,190	71,605	108,660	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "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 10% or more 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 ($r = -1$). Columns (1)-(2) report Chicago and New York combined results for students with case filings in grades 1-5, and (3)-(4) report combined results for students with case filings in grades 6-12. The final column reports the p-value of the test of equal IV effects across groups. Note: these estimates do not include kindergartners, who are included in the main sample. Combined results are constructed as described in Section 5.4. The first column reports the nonevicted mean, and the second reports the TSLS estimate for eviction. Means include standard deviations in parentheses, 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.

A.6.3 Gender IV splits

Building on Table 9, Appendix Tables A.25and Tables A.27-A.30 show pooled (across Chicago and New York) education results separately by child gender for the full set of outcomes. We focus on the combined estimate due to limited sample sizes in the site-by-gender results. Appendix Table A.26 reports analogous results separately by gender for the Cook County census sample.

Table A.25: Home Environment by Gender (Education Sample)

	Girls		Boys		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	P-value of (2) = (4)
Post-filing school year 1:					
Not at pre-case address	0.222	0.145**	0.223	0.128**	0.839
	(0.415)	(0.061)	(0.417)	(0.058)	
Number of moves	0.331	0.275***	0.333	0.158*	0.415
	(0.658)	(0.106)	(0.659)	(0.096)	
Neighborhood poverty	0.304	0.025	0.302	0.011	0.577
	(0.124)	(0.019)	(0.123)	(0.017)	
Observations	64,929	105,502	66,694	108,657	
Post-filing school year 2:					
Not at pre-case address	0.303	0.247***	0.301	0.114	0.264
	(0.460)	(0.084)	(0.459)	(0.084)	
Number of moves	0.598	0.760***	0.594	0.090	0.022
	(1.008)	(0.213)	(1.010)	(0.202)	
Neighborhood poverty	0.303	0.022	0.301	0.016	0.844
	(0.125)	(0.025)	(0.124)	(0.020)	
Observations	50,028	81,380	51,489	83,957	

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

Table A.26: Living Arrangements, Household Structure, and Geography by Gender (Census Sample)

	Girls		Boys		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	P-value of (2) = (4)
Living Arrangements:					
Total household size	4.820	1.255**	4.860	0.233	0.195
		(0.629)		(0.475)	
Doubling up (incl. grandparents)	0.214	0.283**	0.225	0.084	0.199
		(0.131)		(0.083)	
Doubling up (excl. grandparents)	0.130	0.204*	0.143	0.026	0.198
		(0.118)		(0.072)	
Multigenerational household	0.096	0.183**	0.098	0.091	0.347
		(0.073)		(0.065)	
Grandparent household head	0.085	0.159**	0.086	0.048	0.214
		(0.072)		(0.053)	
Household Structure:					
Mother present	0.870	0.101	0.847	-0.049	0.160
		(0.084)		(0.066)	
Father present	0.298	-0.102	0.317	0.084	0.199
		(0.115)		(0.088)	
Single mother	0.583	0.040	0.560	-0.052	0.542
		(0.111)		(0.102)	
Non-relative household head	0.016	-0.011	0.015	0.007	0.673
		(0.033)		(0.027)	
Geography:					
Neighborhood poverty rate	0.237	-0.089**	0.232	-0.021	0.189
		(0.039)		(0.034)	
Out of county	0.234	-0.087	0.234	0.007	0.461
		(0.101)		(0.078)	
Out of state	0.138	-0.037	0.140	-0.008	0.799
		(0.085)		(0.076)	
Joint test†:					0.289
Observations		24,000		24,000	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports results for the Census sample (Cook County) of two-stage least squares (IV) regressions as in Table 4 except it presents estimates separately by the child's gender. The final column reports the p-value of the test of equal IV effects across groups. Observation counts reflect the modal observation counts. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909. †The joint test reports the p-value from a test of the null hypothesis that the difference in IV coefficients between boys and girls is jointly equal to zero across all outcomes presented under "Living Arrangements".

Table A.27: Schooling Attachment and Engagement by Gender (Education Sample)

	Girls		Boys		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	P-value of
	(1)	(2)	(3)	(4)	(2) = (4)
Post-filing school year 1:					
Not at pre-case school	0.272	-0.015	0.283	0.159***	0.028
	(0.445)	(0.055)	(0.450)	(0.057)	
Percent absent	0.130	0.014	0.137	0.033**	0.331
	(0.161)	(0.014)	(0.166)	(0.014)	
Chronic absent	0.408	0.019	0.430	0.158***	0.061
	(0.492)	(0.049)	(0.495)	(0.055)	
Retained	0.112	0.038	0.142	0.010	0.561
	(0.315)	(0.035)	(0.349)	(0.033)	
Observations	87,914	138,838	90,645	143,321	
Post-filing school year 2:					
Not at pre-case school	0.383	-0.037	0.404	0.174**	0.032
	(0.486)	(0.067)	(0.491)	(0.072)	
Percent absent	0.136	0.006	0.143	0.041**	0.168
	(0.170)	(0.017)	(0.175)	(0.019)	
Chronic absent	0.413	0.024	0.438	0.078	0.510
	(0.492)	(0.057)	(0.496)	(0.059)	
Retained	0.138	0.051	0.174	0.059	0.884
	(0.345)	(0.039)	(0.379)	(0.040)	
Observations	73,381	116,090	76,098	120,523	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "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 10% or more 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 ($r = -1$). Columns (1)-(2) report Chicago and New York combined results for girls, (3)-(4) report combined results for boys. The final column reports the p-value of the test of equal IV effects across groups. Combined results are constructed as described in Section 5.4. The first column reports the nonevicted mean, and the second reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes.

Table A.28: Elementary and Middle School Test Scores by Gender (Education Sample)

	Girls		Boys		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	P-value of (2) = (4)
Post-filing school year 1:					
Test score	-0.242	-0.065	-0.433	0.108	0.300
	(0.784)	(0.123)	(0.832)	(0.114)	
Missed test	0.098	0.050*	0.119	0.081**	0.466
	(0.298)	(0.027)	(0.324)	(0.032)	
Observations	47,787	77,068	49,462	79,995	
Post-filing school year 2:					
Test score	-0.238	0.232*	-0.431	0.030	0.283
	(0.783)	(0.129)	(0.833)	(0.137)	
Missed test	0.158	0.071	0.185	0.074*	0.965
	(0.365)	(0.044)	(0.388)	(0.039)	
Observations	43,502	70,820	44,510	72,938	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "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. "Test score" is defined as mean of reading and math scores 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 of one within each grade-school year for all students enrolled in that grade and school year in the district who took the test. "Missed test" is defined as an indicator that is equal to one if a student was actively enrolled in grades 3-8 but does not have one or both test scores. All outcomes are defined among actively enrolled students. Columns (1)-(2) report Chicago and New York combined results for girls, (3)-(4) report combined results for boys. The final column reports the p-value of the test of equal IV effects across groups. Combined estimates are constructed as described in Section 5.4. The first column reports the nonevicted mean, and the second reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes.

Table A.29: High School Credit Accumulation by Gender (Education Sample)

	Girls		Boys		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	P-value of (2) = (4)
Post-filing school year 1:					
Credits earned	0.874	-0.099	0.788	-0.223**	0.326
	(0.408)	(0.082)	(0.433)	(0.096)	
Observations	26,804	39,227	26,177	38,195	
Post-filing school year 2:					
Credits earned	0.870	-0.068	0.782	-0.275**	0.213
	(0.412)	(0.104)	(0.434)	(0.129)	
Observations	25,801	37,751	25,579	37,393	

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "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 earned" is the number of credits completed divided by the standard number of credits attempted in each district, which is 7 in Chicago and typically 14 in New York. "Credits earned" is only defined in high school, i.e., grades 9-12, and outcomes are defined among actively enrolled students. Columns (1)-(2) report Chicago and New York combined results for girls, (3)-(4) report combined results for boys. The final column reports the p-value of the test of equal IV effects across groups. Combined estimates are constructed as described in Section 5.4. The first column reports the nonevicted mean, and the second reports the TSLS estimate for eviction. Means include standard deviations in parentheses, while TSLS estimates include standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression and sample specifications are as described in the notes of Table 3.

Table A.30: High School Graduation by Gender (Education Sample; Filing in Grades 6 to 12)

	Girls		Boys		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	P-value of (2) = (4)
Graduation	0.729	-0.049	0.621	-0.216***	0.099
	(0.444)	(0.064)	(0.485)	(0.078)	
Graduation status not observed	0.122	0.075	0.140	0.053	0.758
	(0.327)	(0.047)	(0.347)	(0.051)	
Observations	48,854	75,661	48,440	74,737	

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

A.6.4 Heterogeneity by Student Race

Table A.31: Heterogeneity by Race (Education Sample)

	All		Black		Not Black		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	$\mathbb{E}[Y\|E=0]$ (5)	IV (6)	P-value of (4) = (6)
Post-filing school year 1:							
Not at pre-case address	0.223	0.134***	0.242	0.149**	0.205	0.133**	0.866
	(0.416)	(0.047)	(0.428)	(0.067)	(0.404)	(0.062)	
Not at pre-case school	0.278	0.077**	0.311	0.051	0.247	0.100*	0.557
	(0.448)	(0.039)	(0.463)	(0.062)	(0.432)	(0.055)	
Percent absent	0.133	0.024**	0.128	0.015	0.138	0.033**	0.433
	(0.163)	(0.010)	(0.159)	(0.017)	(0.167)	(0.013)	
Chronic absent	0.419	0.088**	0.398	0.113*	0.436	0.069	0.605
	(0.493)	(0.042)	(0.490)	(0.062)	(0.496)	(0.058)	
Retained	0.127	0.022	0.127	0.021	0.127	0.029	0.880
	(0.333)	(0.024)	(0.333)	(0.040)	(0.333)	(0.033)	
Test score	-0.338	0.059	-0.342	0.203	-0.335	-0.073	0.110
	(0.810)	(0.081)	(0.808)	(0.127)	(0.820)	(0.116)	
Credits earned	0.832	-0.142**	0.838	-0.176	0.826	-0.129*	0.731
	(0.423)	(0.062)	(0.413)	(0.115)	(0.431)	(0.073)	
Observations	138,986	220,073	65,078	105,243	75,763	117,658	
Post-filing school year 2:							
Not at pre-case address	0.302	0.176***	0.326	0.221**	0.280	0.144*	0.551
	(0.459)	(0.065)	(0.469)	(0.094)	(0.449)	(0.087)	
Not at pre-case school	0.394	0.075	0.435	0.081	0.356	0.061	0.857
	(0.489)	(0.051)	(0.496)	(0.087)	(0.479)	(0.066)	
Percent absent	0.139	0.024*	0.133	0.048**	0.144	0.007	0.137
	(0.172)	(0.014)	(0.167)	(0.020)	(0.176)	(0.019)	
Chronic absent	0.426	0.055	0.405	0.084	0.443	0.051	0.705
	(0.494)	(0.042)	(0.491)	(0.063)	(0.497)	(0.060)	
Retained	0.156	0.052*	0.159	0.062	0.154	0.045	0.766
	(0.363)	(0.028)	(0.365)	(0.045)	(0.361)	(0.038)	
Test score	-0.331	0.068	-0.342	0.332**	-0.329	-0.055	0.031
	(0.803)	(0.089)	(0.805)	(0.129)	(0.823)	(0.124)	
Credits earned	0.826	-0.144*	0.832	-0.020	0.822	-0.221**	0.241
	(0.425)	(0.080)	(0.416)	(0.138)	(0.433)	(0.101)	
Observations	115,078	182,771	54,555	88,656	63,654	98,906	
Graduation	0.676	-0.126**	0.696	-0.127	0.657	-0.146**	0.871
	(0.468)	(0.051)	(0.460)	(0.095)	(0.475)	(0.066)	
Observations	89,889	136,589	43,020	66,368	46,818	70,149	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "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. "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 10% or more 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 ($r = -1$). "Test score" is defined as mean of reading and math scores 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 of one within each grade-school yearfor all students enrolled in that grade and school year in the district who took the test. "Credits earned" is the number of credits completed divided by the standard number of credits attempted in each district, which is 7 in Chicago and typically 14 in New York. All outcomes are defined among actively enrolled students. Columns (1)-(2) report combined results for all students, and (3)-(4) report combined results for Black students, and (5)-(6) report results for not Black students. The final column reports the p-value of the test of equal IV effects across groups. For each category, the first sub-column reports the nonevicted mean, and the second reports the TSLS estimate for eviction. Means are accompanied by standard deviations in parentheses, while TSLS estimates are accompanied by standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes.

Table A.32: Living Arrangements, Household Structure, and Geography by Race (Census Sample)

	All		Black		Not Black		
	$\mathbb{E}[Y\|E=0]$ (1)	IV (2)	$\mathbb{E}[Y\|E=0]$ (3)	IV (4)	$\mathbb{E}[Y\|E=0]$ (5)	IV (6)	P-value of (4) = (6)
Living Arrangements							
Total household size	4.841	0.686*	4.910	0.845*	4.600	-0.072	0.396
		(0.400)		(0.443)		(0.985)	
Doubling up (incl. grandparents)	0.219	0.169**	0.227	0.161*	0.192	0.184	0.930
		(0.077)		(0.095)		(0.244)	
Doubling up (excl. grandparents)	0.137	0.102	0.143	0.099	0.116	0.118	0.929
		(0.066)		(0.086)		(0.194)	
Multigenerational household	0.097	0.132**	0.101	0.137**	0.085	0.076	0.691
		(0.054)		(0.058)		(0.142)	
Grandparent household head	0.086	0.097**	0.091	0.089	0.067	0.120	0.841
		(0.049)		(0.057)		(0.144)	
Household Structure							
Mother present	0.859	0.018	0.866	0.117**	0.834	-0.489**	0.006
		(0.052)		(0.052)		(0.213)	
Father present	0.308	0.003	0.248	-0.021	0.521	0.122	0.629
		(0.082)		(0.093)		(0.281)	
Single mother	0.572	-0.008	0.628	0.056	0.370	-0.337	0.209
		(0.085)		(0.086)		(0.301)	
Non-relative household head	0.015	-0.000	0.013	-0.018	0.024	0.106	0.110
		(0.025)		(0.023)		(0.074)	
Geography							
Neighborhood poverty rate	0.235	-0.051*	0.262	-0.075**	0.144	0.046	0.029
		(0.029)		(0.031)		(0.046)	
Out of county	0.234	-0.028	0.193	0.040	0.368	-0.260	0.179
		(0.067)		(0.067)		(0.213)	
Out of state	0.139	-0.015	0.128	-0.003	0.176	-0.045	0.813
		(0.060)		(0.065)		(0.165)	
Joint test†:							0.970
Observations		48,000		38,000		10,000	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns (1)-(2) report combined results for all children, and (3)-(4) report combined results for Black children, and (5)-(6) report results for not Black children. The final column reports the p-value of the test of equal IV effects across groups. For each category, the first sub-column reports the nonevicted mean, and the second reports the TSLS estimate for eviction. Means are accompanied by standard deviations in parentheses, while TSLS estimates are accompanied by standard errors in parentheses. Standard errors are clustered at the judge×case-year level. †The joint test reports the p-value from a test of the null hypothesis that the difference in IV coefficients between Black and non-Black children is jointly equal to zero across all outcomes presented under "Living Arrangements." The final row reports the modal sample size. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

A.6.5 Heterogeneity by First Eviction Case

Table A.33: First Case Only (Education Sample)

	All		First Case Only	
	$\mathbb{E}[Y \mid E=0]$	IV	$\mathbb{E}[Y \mid E=0]$	IV
	(1)	(2)	(3)	(4)
Post-filing school year 1:				
Not at pre-case address	0.223	0.134***	0.265	0.096
	(0.416)	(0.047)	(0.441)	(0.071)
Not at pre-case school	0.278	0.077**	0.301	0.047
	(0.448)	(0.039)	(0.459)	(0.049)
Percent absent	0.133	0.024**	0.131	0.027**
	(0.163)	(0.010)	(0.159)	(0.012)
Chronic absent	0.419	0.088**	0.417	0.057
	(0.493)	(0.042)	(0.493)	(0.057)
Retained	0.127	0.022	0.131	-0.015
	(0.333)	(0.024)	(0.337)	(0.030)
Test score	-0.338	0.059	-0.325	-0.216*
	(0.810)	(0.081)	(0.821)	(0.127)
Credits earned	0.832	-0.142**	0.834	-0.153*
	(0.423)	(0.062)	(0.423)	(0.080)
Observations	138,986	220,073	81,230	136,040
Post-filing school year 2:				
Not at pre-case address	0.302	0.176***	0.348	0.084
	(0.459)	(0.065)	(0.476)	(0.090)
Not at pre-case school	0.394	0.075	0.407	0.018
	(0.489)	(0.051)	(0.491)	(0.064)
Percent absent	0.139	0.024*	0.135	0.031*
	(0.172)	(0.014)	(0.167)	(0.016)
Chronic absent	0.426	0.055	0.418	0.040
	(0.494)	(0.042)	(0.493)	(0.059)
Retained	0.156	0.052*	0.162	0.041
	(0.363)	(0.028)	(0.368)	(0.041)
Test score	-0.331	0.068	-0.316	-0.103
	(0.803)	(0.089)	(0.825)	(0.140)
Credits earned	0.826	-0.144*	0.819	-0.257**
	(0.425)	(0.080)	(0.427)	(0.108)
Observations	115,078	182,771	68,640	115,449
Graduation	0.676	-0.126**	0.669	-0.146*
	(0.468)	(0.051)	(0.471)	(0.076)
Observations	89,889	136,589	45,720	74,329

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "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. "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 10% or more 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 ($r = -1$). "Test score" is defined as mean of reading and math scores 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 of one within each grade-school year for all students enrolled in that grade and school year in the district who took the test. All outcomes are defined among actively enrolled students. The sample for graduation is described in Table 8. Columns (1)-(2) report combined results for all students, and (3)-(4) report combined results for first case students. For each category, the first sub-column reports the nonevicted mean, and the second reports the TSLS estimate for eviction. Means are accompanied by standard deviations in parentheses, while TSLS estimates are accompanied by standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes.

Table A.34: Living Arrangements, Household Structure, and Geography by First Case Only (Census Sample)

	All		First Case Only	
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV
	(1)	(2)	(3)	(4)
Living Arrangements				
Total household size	4.841	0.686*	4.750	0.646
		(0.400)		(0.970)
Doubling up (incl. grandparents)	0.219	0.169**	0.220	0.520***
		(0.077)		(0.163)
Doubling up (excl. grandparents)	0.137	0.102	0.133	0.347**
		(0.066)		(0.168)
Multigenerational household	0.097	0.132**	0.104	0.273**
		(0.054)		(0.130)
Grandparent household head	0.086	0.097**	0.091	0.260**
		(0.049)		(0.130)
Household Structure				
Mother present	0.859	0.018	0.853	-0.023
		(0.052)		(0.140)
Father present	0.308	0.003	0.343	-0.177
		(0.082)		(0.220)
Single mother	0.572	-0.008	0.538	0.112
		(0.085)		(0.264)
Non-relative household head	0.015	-0.000	0.012	-0.023
		(0.025)		(0.052)
Geography				
Neighborhood poverty rate	0.235	-0.051*	0.219	0.004
		(0.029)		(0.075)
Out of county	0.234	-0.028	0.225	0.034
		(0.067)		(0.199)
Out of state	0.139	-0.015	0.127	0.077
		(0.060)		(0.172)
Observations		48,000		15,000

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns (1)-(2) report results for all child-cases in the Census sample, and (3)-(4) report results for first observed case for each child in the Census sample. For each category, the first sub-column reports the nonevicted mean, and the second reports the TSLS estimate for eviction. Means are accompanied by standard deviations in parentheses, while TSLS estimates are accompanied by standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The final row reports the modal sample size. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

A.6.6 Baseline Test Scores IV Splits

Table A.35: Test Score and Missed Test by Baseline Test Score (Education Sample)

	Below Median Score						Above Median Score						
	Chicago		New York		Combined		Chicago		New York		Combined		
	$\mathbb{E}[Y \mid E=0]$	IV	$\mathbb{E}[Y \mid E=0]$	IV	$\mathbb{E}[Y \mid E=0]$	IV	$\mathbb{E}[Y \mid E=0]$	IV	$\mathbb{E}[Y \mid E=0]$	IV	$\mathbb{E}[Y \mid E=0]$	IV	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(6) = (12)
Post-filing school year 1:													
Test score	-0.890	-0.219	-0.766	0.190	-0.774	0.141	0.185	-0.006	0.059	-0.151	0.067	-0.135	0.091
	(0.587)	(0.157)	(0.720)	(0.125)	(0.712)	(0.112)	(0.690)	(0.176)	(0.678)	(0.132)	(0.679)	(0.119)	
Missed test	0.054	0.003	0.136	0.128***	0.131	0.114***	0.032	-0.016	0.092	0.010	0.088	0.007	0.016
	(0.226)	(0.060)	(0.343)	(0.040)	(0.338)	(0.036)	(0.176)	(0.064)	(0.289)	(0.029)	(0.284)	(0.026)	
Observations	2,881	8,750	46,372	69,739	46,014	73,411	3,088	8,360	47,535	70,113	48,383	75,177	
Post-filing school year 2:													
Test score	-0.861	0.134	-0.734	0.173	-0.741	0.169	0.155	0.154	-0.005	0.062	0.004	0.071	0.611
	(0.604)	(0.186)	(0.744)	(0.158)	(0.737)	(0.142)	(0.682)	(0.237)	(0.709)	(0.142)	(0.709)	(0.130)	
Missed test	0.058	-0.048	0.212	0.104**	0.204	0.089*	0.036	0.198*	0.157	0.035	0.152	0.049	0.538
	(0.233)	(0.087)	(0.409)	(0.052)	(0.403)	(0.048)	(0.186)	(0.117)	(0.364)	(0.048)	(0.359)	(0.045)	
Observations	2,116	6,531	41,277	62,493	38,958	62,246	2,269	6,290	46,515	68,856	45,085	69,703	

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "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. Columns (1)-(6) report results for below-median score students, and (7)-(12) report results for above median score students. For each category, the first sub-column reports the Chicago nonevicted mean and the TSLS estimate for eviction, the second reports the New York nonevicted mean and the TSLS estimate for eviction. The third reports the combined nonevicted mean and the TSLS estimate for eviction. The combined results are constructed as described in Section 5.4. Means are accompanied by standard deviations in parentheses, while TSLS estimates are accompanied by standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes. "Test score" is defined as mean of reading and math scores 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 of one within each grade-school year for all students enrolled in that grade and school year in the district who took the test. "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.

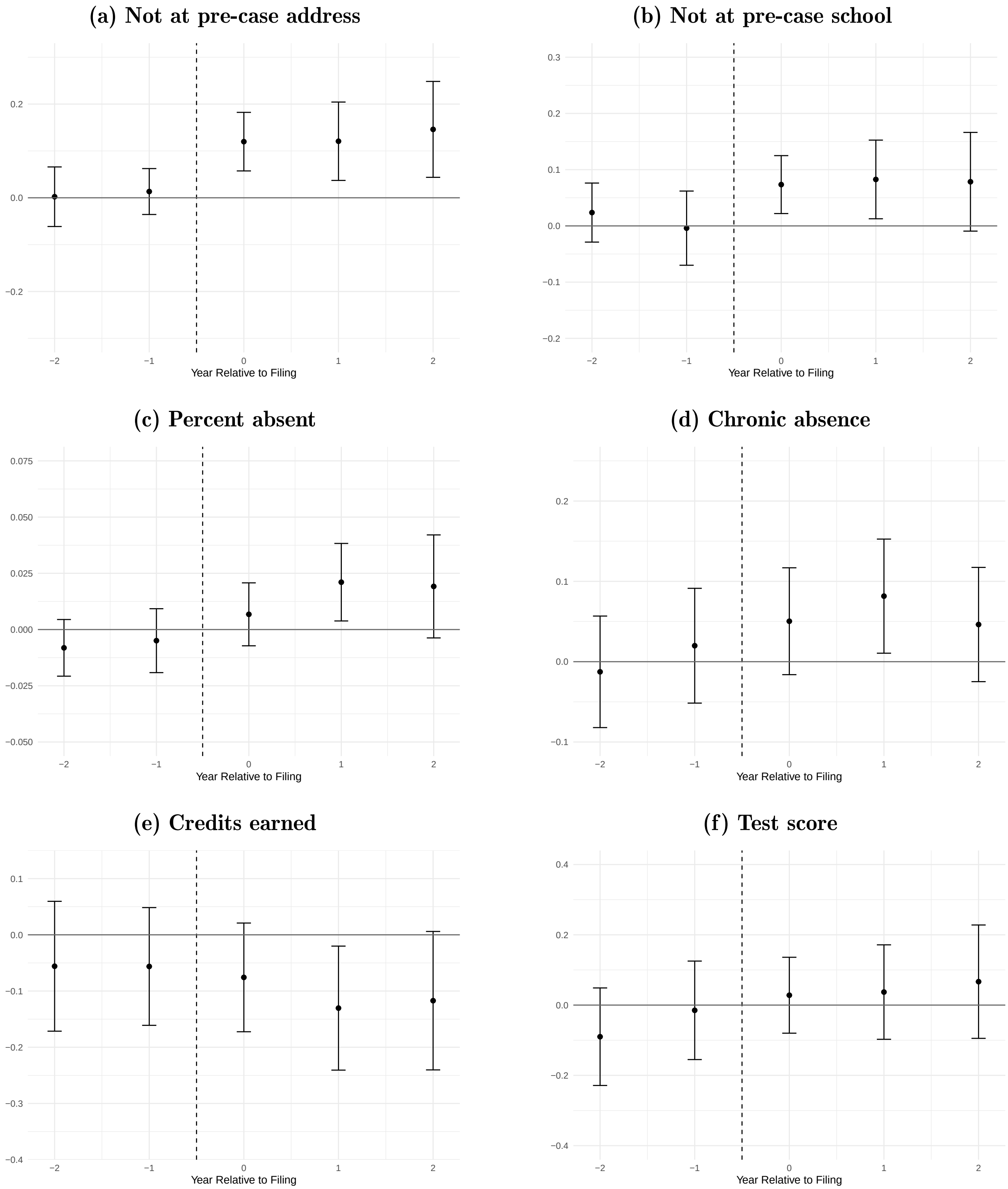
Table A.36: School Attendance by Baseline Test Score (Education Sample)

	Below Median Score						Above Median Score						
	Chicago		New York		Combined		Chicago		New York		Combined		
	$\mathbb{E}[Y \mid E=0]$	IV	$\mathbb{E}[Y \mid E=0]$	IV	$\mathbb{E}[Y \mid E=0]$	IV	$\mathbb{E}[Y \mid E=0]$	IV	$\mathbb{E}[Y \mid E=0]$	IV	$\mathbb{E}[Y \mid E=0]$	IV	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(6) = (12)
Post-filing school year 1:													
Percent absent	0.142	0.070*	0.172	0.028	0.170	0.031*	0.106	0.029	0.101	0.013	0.101	0.014	0.465
	(0.152)	(0.036)	(0.190)	(0.018)	(0.189)	(0.017)	(0.125)	(0.026)	(0.130)	(0.015)	(0.130)	(0.014)	
Observations	3,365	9,773	84,182	125,105	87,547	134,878	4,010	10,252	88,431	129,078	92,441	139,330	
Post-filing school year 2:													
Percent absent	0.150	0.030	0.177	0.038*	0.176	0.037*	0.112	-0.034	0.108	0.016	0.108	0.012	0.341
	(0.157)	(0.037)	(0.197)	(0.022)	(0.196)	(0.020)	(0.130)	(0.040)	(0.144)	(0.017)	(0.143)	(0.016)	
Observations	2,919	8,590	74,731	110,955	77,650	119,545	3,471	9,062	76,975	112,371	80,446	121,433	

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "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. Columns (1)-(6) report results for below-median score students, and (7)-(12) report results for above median score students. For each category, the first sub-column reports the Chicago nonevicted mean and the TSLS estimate for eviction, the second reports the New York nonevicted mean and the TSLS estimate for eviction. The third reports the combined nonevicted mean and the TSLS estimate for eviction. The combined results are constructed as described in Section 5.4. Means are accompanied by standard deviations in parentheses, while TSLS estimates are accompanied by standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes. "Percent absent" is the proportion of days absent. All outcomes are defined among actively enrolled students.

A.7 Reduced-Form Effects Over Time

Figure A.7: Reduced-Form Effects Over Time



Notes: Each panel of the figure plots the combined reduced-form estimate from a regression of the outcome listed in the panel header on the instrument by year relative to filing. The whiskers on the point estimates are 95% confidence intervals.

B Data and Linkage

B.1 Court data

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

B.1.1 Overview of the data

We use the near-universes of court records for Chicago (for the period 2000-2019) and for New York (for the period 2007-2017). For both cities, court records include the residential address of the disputed housing unit and the names of tenant(s). Other key elements we observe in the court records include case type, filing date, courtroom and date assignment, name of the landlord, attorneys' names, the amount claimed by the landlord (ad damnum amount), and whether an eviction order was granted. Below, we describe the court variables that are most relevant for our analyses.

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

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

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

B.1.2 Baseline sample restrictions

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

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

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

B.2 Education data

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

B.2.1 Overview of the data

The calendar year can be divided into the academic term, which includes the fall and spring seasons, and the summer. In both Chicago and New York, the academic term has typically run from early September to late June, so we use September 1 to June 30 to define the academic term and July 1 to August 31 to define summer. We index the school year based on the year of the spring term, with the previous summer also belonging to that school year. For example, the 2010 school year includes the summer of 2009 and concludes with the spring term of 2010.

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

New York. We use administrative schooling records from New York City's Department of Education (NYCDOE). The NYCDOE data contains student-year level records from 2005-2018 for grades K-12 (though some outcomes are only captured through 2017). We observe panel data for 2,859,983 unique students.

B.2.2 Linkage and sample restrictions

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

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

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

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

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

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

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

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

Table B.1: Linked to CPS records

	(1)	(2)	(3)	(4)
Evicted	0.0010	0.0018		
	(0.0013)	(0.0013)		
Stringency			0.0293	0.0293
			(0.0212)	(0.0211)
No attorney		-0.0263^{***}		-0.0261^{***}
		(0.0035)		(0.0035)
Ad Damnum		-0.0020^{***}		-0.0020^{***}
		(0.0001)		(0.0001)
Court controls	No	Yes	No	Yes
Observations	431,044	431,044	431,044	431,044

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

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

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

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

The record linkage then proceeded in the following manner:

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

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

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

To examine whether linkage is correlated with the instrument, Table B.2 reports the estimates of regressions of the indicator for the case being linked to a student separately on eviction status and on the instrument. All regressions include court-year fixed effects, and we consider specifications with and without the court controls included in our IV/OLS results (indicator for the tenant being without an attorney and the ad damnum amount). In both specifications, the coefficient on the instrument is statistically insignificant.

Table B.2: Linked to DOE records

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

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. The table reports regression coefficients from a regression in which the left-hand-side variable is linked to the New York Department of Education data, and the right-hand side includes case characteristics. The Court sample is the full court records after applying the restrictions described in B.1.2. All regressions include court-year fixed effects. Standard errors are clustered at the judge-year level.

B.2.3 Variable construction and time indexing

Most outcomes in the schooling data are defined over the entire school year, while eviction filings occur throughout the calendar year. We therefore need to map school outcomes into years relative to the eviction filing. As described in Appendix B.2.1, we use September 1 to June 30 to define the academic term and July 1 to August 31 to define summer. We index school years by the calendar year in which the academic term ends, with each summer belonging to the following school year.

Relative year 0 ($r = 0$) is the school year that contains the filing date. For cases filed during the academic term, this is the school year in which the case is filed. For cases filed during the summer, it is the school year that begins that fall. Lagged relative years (i.e., $r = -1$, $r = -2$, etc.) and lead relative years (i.e., $r = 1$, $r = 2$, etc.) are defined relative to $r = 0$. For example, a case filed in the fall of 2010 or the spring of 2011 occurs during the 2011 school year, and thus $r = -1$ is 2010, $r = 0$ is 2011, and $r = 1$ is 2012. A case filed in the summer of 2011 occurs during the 2012 school year, and thus $r = -1$ is 2011 (the school year that ended before the case filing), $r = 0$ is 2012 (the school year that begins after the case filing), and $r = 1$ is 2013.

For our IV/OLS analysis, we focus on periods fully after the case is filed. We therefore define the *first post-filing school year* as the first school year in which the academic term begins entirely after the filing date. Thus, the first post-filing school year is $r = 1$ for cases filed during the academic term and $r = 0$ for cases filed during the summer. We similarly define *second post-filing school year* as the school year that follows the first: $r = 2$ for cases filed during the academic term and $r = 1$ for cases filed during the summer. September filings are treated as occurring during the academic term even when they precede the first day of classes (which is not always September 1st), so their first post-filing school year is the school year that begins the following September.

In Appendix A.4.1, we additionally report IV/OLS results for the *case filing year*, which is the school year in which the case was filed. This is $r = 0$ for both cases filed during the academic term and the prior summer. This timing may capture more immediate effects, but the outcomes are often a mixture of pre- and post-filing behavior. For example, a case filed in September will have outcomes measured almost entirely post-filing, while cases filed in June will have outcomes measured almost entirely pre-filing. Note that for cases filed during the summer, the first post-filing school year is the case filing year itself ($r = 0$), so these cases contribute the same school-year observation to the main "first post-filing school year" results and the "case filing year" results in Appendix A.4.1.

For homelessness outcomes derived from HMIS records and, in New York, transfers out of the school system, we know the specific dates the outcome occurs, so we do not use the school calendar. Instead, we define relative year 1 ($r = 1$) as the first 365 days after the case filing and $r = 2$ as the next 365 days; lagged relative years (i.e., $r = -1$, $r = -2$, etc.) are the 365-day windows before the filing. Because no window straddles the filing date, these outcomes have no $r = 0$. In the IV/OLS analyses, year 1 and year 2 correspond to $r = 1$ and $r = 2$, respectively.

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

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

- *Gender* is an indicator for the student's reported gender being female.
- *Race and ethnicity* are the reported race and ethnicity of the student. For Chicago it takes one of the following values: Black, Hispanic, White, or other[60]. Similarly, for New York, race and ethnicity are consolidated, but may also take the value "Asian" or "Native American."
- *Free or reduced lunch* for Chicago and New York is an indicator for the student qualifying for free or reduced lunch in a school year prior to the one that contains the case filing date.[61]

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

[61] For precise definitions of school years relative to the case filing date, see the school year indexing at the start of

- *Individualized Education Program (IEP) and disability status* takes values for overall IEP status and for corresponding learning accommodations. In Chicago, IEP codes are used to distinguish students with speech-related accommodations, emotional or behavioral disorders, or other learning considerations such as dyslexia or autism. For New York, we consider an indicator for IEP, and indicators for the following disability statuses: learning disability, emotional disability, and speech impediment.
- *Language status* is observed for New York and is used to construct indicators for Spanish-speaking and for other-language-speaking.
- *Born in New York City* is observed for New York and is an indicator for being born in New York City.

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

For cases filed during the summer, we define $r = 0$ as the school year with the academic term that begins right after the summer. Lagged relative years (i.e., $r = -1$,$r = -2$,etc.) and lead relative years (i.e., $r = 1$, $r = 2$, etc.) are defined relative to $r = 0$. For instance, consider a case filed in the summer of 2011. Such a case occurs during the 2012 school year, and thus $r = -1$ is 2011 (the school year that ended before the case filing), $r = 0$ is 2012 (the school year that begins after the case filing), and $r = 1$ is 2013.

For homelessness outcomes derived from HMIS records (and transferring out of New York), we know the specific dates the outcome occurs, so we define $r = 1$ as the first 365 days after the case filing. Lagged relative years (i.e., $r = -1$, $r = -2$ etc.) and lead relative years (i.e., $r = 2$, etc.) are defined by 365-day windows around the exact filing date.

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

this section.

Based on characteristics data:

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

Based on enrollment data:

- *Grade (in r)* is the grade (K-12) in r.
- *Predicted grade in r* is the grade in r implied by a normal progression from their grade in $r = -1$. This is equal to the grade in $r = -1$ plus $(r + 1)$.
- *Not at pre-case school (in r)* is an indicator that is one if the school identifier in $r = -1$ is different from the school identifier in r, not counting mechanical school changes due to progressing to a grade that is not available at the prior school.
- *Number of school changes (in r)* is the sum of year-on-year school changes starting from $r = -1$ and ending with r, where a school change is defined as in the previous bullet. Thus, not at pre-case school in $r = 0$ and number of school changes in $r = 0$ are equal.
- *Transferred out of school system (in r)* is an indicator that is one if the student is marked as having transferred out of the school system in r. This variable does not condition on being enrolled in r. For New York, transfers are recorded with exact dates and indexed by the 365-day relative years described above.

Based on address data:

- *Not at pre-case address (in r)* for Chicago is an indicator that is one if the distance between the latitude and longitude coordinates in $r = -1$ and r exceeds a minimum threshold, which we set to 100m and which is not sensitive to the choice of threshold.[62] We consider the address measured in the spring season of the school year. For New York, it is an indicator that is one if the census block in $r = -1$ is different from the census block in r. The addresses are recorded in June of the respective school year.
- *Number of moves (in r)* is the sum of year-on-year address changes starting from $r = -1$ and ending with r, where an address change is defined as in the previous bullet. Thus, not at pre-case address in $r = 0$ and number of moves in $r = 0$ are equal.
- *Neighborhood poverty (in r)* for Chicago is the tract-level percent of households below the federal poverty line obtained from American Community Survey (ACS) 5-year aggregates. Because each estimate spans 5 years, we match each school year with the 5-year estimate for which it is the midpoint. For instance, if r has school year 2014, we pull the poverty

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

rate from the 2012-2016 5-year ACS.[63] For New York, we rely on ACS 5-year estimates for the period 2006-2010.

Based on McKinney-Vento data:

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

Based on HMIS data:

- *Homelessness (in r)* for New York is an indicator for the child being listed on a family application for a shelter bed as recorded by the Department of Homeless Services. HMIS data is not linked to CPS data, and is thus not directly observed for Chicago. However, as described in Appendix B.3, we do measure it for a different sample of children. Homelessness is recorded with exact dates and indexed by the 365-day relative years, as described above.

Based on school attendance data:

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

[63] For school years that do not have corresponding ACS data from this strategy, we use the most recently-available ACS data at the time of writing. For instance, for school year 2017, we used ACS 5-year data from 2014-2017.

Based on test scores data: Standardized state reading/English and Language Arts (ELA) and math tests are administered in grades 3-8 for both Chicago and New York.[64] In both cities, tests are required to be completed over certain time windows of the year.

For Chicago, the different standardized test regimes that span the panel are the Illinois Standards Achievement Test (ISAT), Partnership for Assessment of Readiness for College and Careers (PARCC), and the Illinois Assessment of Readiness (IAR), and their testing windows are determined by CPS administration of Illinois State Board of Education guidance.[65] For New York, all standardized test scores come from the New York State Assessment. Since 2005-06, the New York State Education Department applies the ELA and mathematics testing programs to Grades 3-8. Previously, state tests were administered in Grades 4 and 8 and citywide tests were administered in Grades 3, 5, 6, and 7.

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

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

Based on credits data:

- *Credits (in r)* is the number of completed credits in r divided by the standard number of

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

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

credits attempted in each district. This normalization is done to make the Chicago and New York results comparable. We implement this by dividing the number of completed credits by the modal number of attempted credits in the student's grade (computed using all students in the school system in the considered grade). In Chicago, this standard number of credits is always 7. In New York, this number is typically 14. Credits are observed for grades 9-12 in Chicago and for grades 6-12 in New York. To ensure comparability across cities, we restrict New York credits data to grades 9-12.

Based on GPA data:

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

High school completion measures. We measure high school completion by using the last observed enrollment status of high school students as recorded in our panels. This final observed enrollment status for a high school student can take one of the following values: still enrolled, dropped out, graduated, or transferred out of the school system. Because we do not observe whether a student who transfers out is still enrolled in another school, dropped out, or graduated from a school outside the district, we restrict attention to the sample of students who are in grade 6 or higher at case filing, and do not transfer out of the system (see Appendix D for details on the bounding approach we use to address transfers and attrition). Lastly, to focus on students who are expected to graduate by the end of our panels, we additionally condition on students who are at least 18 years old by the end of our panels. We define the following outcomes:

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

Table B.3: Data sources and availability

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

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

B.3 Census data

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

B.3.1 Overview of the data

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

B.3.2 Linkage and sample restrictions

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

Because all Census analysis samples are derived from PIK'd eviction court records, we characterize the population of PIK'd individuals. Appendix Table B.5 below reports estimates from a regression of an indicator for an individual being successfully assigned a PIK on case characteristics. We find that cases ending in an eviction are less likely to be assigned a PIK. Males, Hispanics, and residents of high-poverty neighborhoods are also less likely to receive PIKs. The second column of Table B.5 re-estimates the regression but replaces the eviction order right-hand side variable with judge stringency. This analysis shows that stringency is somewhat correlated with being assigned a PIK. While this relationship is a potential threat to internal validity, we argue that it is unlikely to meaningfully affect our estimates of the impact of eviction for two reasons. First, the relationship is quantitatively small. The estimates in column (2) show that moving from the 10th to the 90th percentile in stringency (a difference of 0.07) is associated with a decrease in the likelihood of being assigned a PIK by 0.38 percentage points (-0.054×0.07), which is a 0.73 percent decrease relative to the overall PIK rate of 52 percent. Second, stringency remains uncorrelated with characteristics in the PIK'd sample, as we show in Appendix Table A.3.

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

To construct the Census pseudo-event study sample (used in Section 4.4), excluding the HMIS analysis, we link the PIK'd tenant-case records at the individual level to tenants' 2010 Decennial records. We use case years between 2005 and 2013 to study children beginning 3 years prior to and continuing until 5 years after the 2010 Decennial. We restrict to tenants who are not in group quarters in the 2010 Decennial, because the household relationship variable is missing for those in group quarters.[66] Next, we construct a child-case level dataset using any children

[66]Panel F of Appendix Figure A.5 uses the tenant-level linkage and shows a pseudo-event study depicting the proportion of tenants in group quarters by year relative to filing. This figure shows that evicted tenants are about 1

in the tenant's household aged 0-18 regardless of their relationship to the tenant.[67]

To construct the IV/OLS Census sample (used in Section 6.1) for all household and neighborhood outcomes excluding the HMIS analysis, we link tenants in PIK'd court records at the individual level to their 2000 Decennial records. We restrict the sample to tenants with case filings between 2000 and 2009 so that the 2000 Decennial precedes the court case and the 2010 Decennial follows the court case. We restrict tenants to those not in group quarters. We then collect all the PIKs of children in these households. We restrict children to those who are 18 or under as of the case year, and 18 or under as of the Decennial 2010. The purpose of this restriction is to study the impact of eviction on children who are still children as of the outcome period. We make a few other minor sample restrictions: (1) requiring an age discrepancy of no more than 1 year between the child's age in the 2000 Decennial plus 10 years, and the child's age in the 2010 Decennial; (2) requiring at least one of the linked tenants in the household to be over 18 as of the case year; and (3) the court case restrictions described in Section 3 such as dropping bulk filings, condominiums, etc. The analysis sample is a child-case dataset. The household relationship outcomes (i.e., doubling up, multigenerational household, mother/father present, etc.) are missing for children in group quarters as of the 2010 Decennial, while the neighborhood outcomes (neighborhood poverty rate, out-of-county indicator, out-of-state indicator) are measured regardless of whether the child is living in group quarters.[68]

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

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percentage point more likely to be in group quarters in the 1-5 years after eviction filing.

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

[68]Only 1.4 percent of the nonevicted group are in group quarters in 2010, and there is no statistically significant impact of being evicted on the likelihood of being in group quarters: our main IV specification produces an insignificant point estimate of -1.2 percentage points, and our main OLS specification produces an insignificant point estimate of 0.1 percentage point, as shown in Table B.6.

Table B.4: Decennial Census Linkage Details

A. Match rates and baseline relationships	Mean				
Match rate (tenant): Court records to Decennial 2000	0.67				
Match rate (child): Decennial 2000 to Decennial 2010	0.68				
Child linked through:					
Mother	0.62				
Father	0.19				
Grandparent	0.07				
Aunt or Uncle	0.02				
Sibling	0.08				
B. Eviction and match rates		Matched to 2000		Matched to 2010	
		(1)	(2)	(3)	(4)
Eviction		-0.033***		-0.006	
		(0.002)		(0.005)	
Stringency			-0.039		-0.122
			(0.042)		(0.112)
Observations		203,000	187,000	78,000	72,000

Notes: Panel A. provides summary statistics of the data linkage procedure that creates the Census sample. We present the overall match rate of the tenant sample to the Decennial 2000 court records, and the match rate of the children in the Decennial 2000 to the Decennial 2010. We also provide the proportion of the child sample linked through each relative. Panel B. estimates a regression of an indicator of the tenant matching to the Decennial 2000 records on the eviction indicator (column 1) and on the stringency measure (column 2). We then use the sample of children in the Decennial 2000 and regress an indicator for those children linking to the Decennial 2010, on eviction (column 3) and on stringency (column 4). Approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2476-R10965 and CBDRB-FY26-P2476-R12909.

Table B.5: Linked to Census records—Cook County

	Has PIK	
	(1)	(2)
Intercept	0.5514***	0.5632***
	(0.00903)	(0.01984)
Evicted	-0.05073***	
	(0.00239)	
Judge stringency		-0.05394**
		(0.02658)
Joint action	0.02719***	0.02618***
	(0.00315)	(0.00322)
Female (predicted)	0.06049***	0.06097***
	(0.00171)	(0.00171)
White (predicted)	0.1284***	0.1248***
	(0.00671)	(0.00676)
Black (predicted)	0.1232***	0.1173***
	(0.00623)	(0.00621)
Hispanic (predicted)	-0.03052***	-0.0366***
	(0.0066)	(0.00668)
Tenant without attorney	-0.05457***	-0.06266***
	(0.00433)	(0.00432)
Ad damnum (1000s)	-0.04006***	-0.04249***
	(0.00314)	(0.00317)
Neighborhood poverty rate	-0.07974***	-0.0808***
	(0.00924)	(0.00929)
Neighborhood median rent	-0.03832***	-0.03267***
	(0.0062)	(0.00626)
Number of Observations	457,000	457,000

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. This table reproduces Table D.1 of Collinson et al. (2024). This table shows results in column (1) from the regression of an indicator for if the individual was linked to Census records in Cook County regressed on covariates and eviction order, and column (2) shows the results from a regression on covariates and the stringency instrument. Race is imputed using a Bayesian procedure using last names and racial composition of census tracts as proposed in Imai and Khanna (2016). Gender is predicted using first names as described in Blevins and Mullen (2015). The regressions also include controls for court and year, and indicators for missing covariates (not reported). Standard errors are in parentheses. Cook County observation counts are rounded in accordance with Census Bureau disclosure requirements. Cook County results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-072.

Table B.6: Group Quarters (Census Sample)

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

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. This table reports results for the Census sample (Cook County) of OLS and two-stage least squares (IV) regressions to estimate the impact of being evicted on the likelihood of being in group quarters. The first column reports the nonevicted mean, the second reports the coefficient on an eviction indicator from an OLS regression, and the third reports the TSLS estimate for eviction. The regression and sample specifications are as described in the notes of Table 4. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

B.3.3 Variable construction and time indexing

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

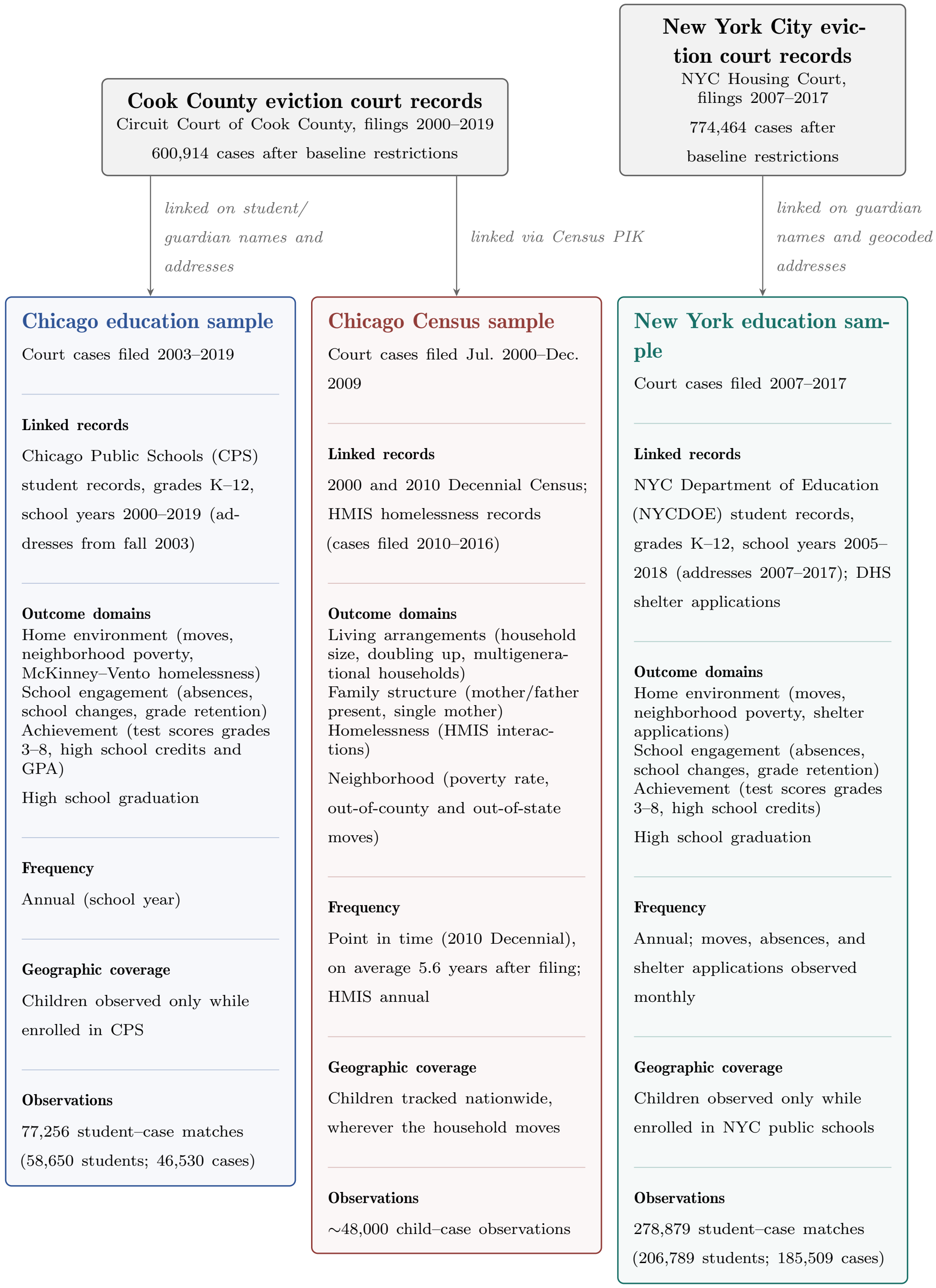
- *Single mother*. Takes a value of one if the reference child is the child of a female household head and there is no spouse present.
- *Single mother (without cohabiting partner)*. Takes a value of one if the reference child is the child of a female household head and there is no spouse present and there is no cohabiting partner present.
- *Single father.* Takes a value of one if the reference child is the child of a male household head and there is no spouse present.
- *Single father (without cohabiting partner)*. Takes a value of one if the reference child is the child of a male household head and there is no spouse present and there is no cohabiting partner present.
- *Multigenerational household.* Takes a value of one if any of the conditions are met: (i) the reference child is the child of the household head and the household head has a parent, parent-in-law, or grandparent present, (ii) the reference child is the grandchild of the household head and a child (biological, adopted, or step), parent, or parent-in-law of the household head is present.
- *Doubling-up (including grandparents)*. Takes a value of one if there is any adult (19 and older) in the household who is not the household head, or the child, spouse, or cohabiting partner of the household head.
- *Doubling-up (excluding grandparents)*. This measure excludes parents or parents-in-law of the household head or adult children of the household head.
- *Mother present.* This measure takes a value of one if (i) the reference child is the child of a female household head, (ii) the reference child is the child of a male household head and

the female spouse is present (note this would include step-mothers), (iii) the reference child is the grandchild of the household head and the female child of the household head is present (note this condition may erroneously include aunts), (iv) the reference child is the brother or sister of the household head and the female parent of the household head is present (note this may include step-mothers).

- *Father present.* This measure is analogous to *mother present*.
- *Out of county.* An indicator equal to 1 if the child is living outside of Cook County, IL.
- *Out of state.* An indicator equal to 1 if the child is living outside of Illinois.

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

Figure B.1: Data sources and the three analysis samples



Notes: This figure summarizes the construction of the three analysis samples. Gray boxes are the underlying eviction court records; both Chicago samples are built from the same Cook County court records. Each colored panel describes one final analysis sample. Arrows denote record linkages and are labeled with the variables used for matching. CPS = Chicago Public Schools; NYCDOE = New York City Department of Education; DHS = New York City Department of Homeless Services; HMIS = Homeless Management Information System; GPA = grade point average. PIK = Protected Identification Key, an anonymized person identifier assigned by the U.S. Census Bureau on the basis of tenants' names and addresses; only cases in which the tenant receives a PIK can be linked to Census records. A student–case (child–case) observation is a unique pairing of a child and an eviction case; children linked to multiple cases appear once per case. Census observation counts are rounded according to Census Bureau disclosure rules. Details of the linkage procedures and sample restrictions appear in Appendix B.

C Specification Details and Robustness

C.1 Robustness to Controls

This appendix presents robustness results to our main specification. Tables C.1-C.5 present IV results for our education samples under three different specifications in addition to our main specification. In the first specification ('No X'), we include no controls (but keep the fixed effects). In the second specification ('Base X'), we include court and education covariates, but no lagged controls. Apart from these differences in how we incorporate covariates, these specifications are like our main specification described in Section 5. The third specification ('Main') is our main specification that we include for ease of comparison.[69] The fourth specification ('W/Stringencies') includes additional judge stringencies as controls. We include stringencies in granting stays in Chicago and New York, and judgment amount stringency in Chicago (not available in the New York data). Overall, we find that the estimates and standard errors across all four specifications are quite similar.

Table C.6 presents IV results for our Chicago Census sample under a 'No X' specification that includes no controls (but keeps the fixed effects). Apart from this difference, this specification is like our main specification described in Section 5. The second specification ('Main') is our main specification that we include for ease of comparison. We again find that the estimates and standard errors across both specifications are quite similar.

Lastly, Tables C.13-C.17 also present reduced-form estimates under our main specification ('RF'). These estimates are similar to our IV estimates, consistent with our first stage being close to unity.

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

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

	Chicago				New York				Combined			
	No X	Base X	Main	W/ Stringencies	No X	Base X	Main	W/ Stringencies	No X	Base X	Main	W/ Stringencies
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-filing school year 1:												
Not at pre-case address	0.140	0.134	0.134	0.080	0.126**	0.121**	0.135**	0.130**	0.129***	0.123***	0.134***	0.121***
	(0.088)	(0.087)	(0.087)	(0.087)	(0.052)	(0.053)	(0.053)	(0.052)	(0.045)	(0.046)	(0.047)	(0.045)
Number of moves	0.112	0.100	0.100	-0.021	0.218**	0.208**	0.233**	0.227**	0.199**	0.189**	0.210***	0.184**
	(0.160)	(0.157)	(0.157)	(0.164)	(0.089)	(0.091)	(0.092)	(0.089)	(0.078)	(0.080)	(0.081)	(0.079)
Neighborhood poverty	-0.026	-0.011	-0.011	-0.008	0.024	0.024	0.024	0.021	0.013	0.016	0.017	0.015
	(0.024)	(0.022)	(0.022)	(0.023)	(0.017)	(0.017)	(0.017)	(0.016)	(0.014)	(0.014)	(0.014)	(0.014)
Homelessness†	0.043*		0.047*		0.023	0.024	0.031	0.032	0.025		0.032*	
	(0.026)		(0.027)		(0.021)	(0.021)	(0.020)	(0.020)	(0.019)		(0.019)	
McKinney-Vento	0.104	0.118*	0.090	0.073								
	(0.072)	(0.066)	(0.065)	(0.064)								
Observations	38,503	42,503	38,503	42,263	198,956	198,956	198,956	198,956	235,334	214,209	235,334	213,983
Post-filing school year 2:												
Not at pre-case address	0.152*	0.145*	0.136	0.117	0.171**	0.170**	0.186**	0.151**	0.167***	0.165***	0.176***	0.145**
	(0.089)	(0.086)	(0.085)	(0.080)	(0.074)	(0.076)	(0.078)	(0.075)	(0.062)	(0.063)	(0.065)	(0.062)
Number of moves	0.214	0.193	0.193	0.099	0.410**	0.404**	0.448**	0.365**	0.378**	0.370**	0.407**	0.322**
	(0.198)	(0.188)	(0.188)	(0.194)	(0.181)	(0.183)	(0.190)	(0.178)	(0.155)	(0.157)	(0.163)	(0.153)
Neighborhood poverty	-0.018	-0.014	-0.014	-0.011	0.031	0.027	0.028	0.021	0.020	0.018	0.018	0.013
	(0.028)	(0.024)	(0.024)	(0.024)	(0.021)	(0.021)	(0.021)	(0.021)	(0.017)	(0.017)	(0.017)	(0.017)
Homelessness†	0.063*		0.066*		0.043	0.045*	0.051**	0.056**	0.045*		0.052**	
	(0.035)		(0.035)		(0.027)	(0.027)	(0.026)	(0.026)	(0.024)		(0.023)	
McKinney-Vento	0.087	0.101	0.077	0.078								
	(0.077)	(0.073)	(0.069)	(0.067)								
Observations	34,950	36,812	34,950	36,578	163,387	163,387	163,387	163,387	194,610	165,357	194,610	165,149

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Table shows robustness of IV results to specification choice. "No X" includes only court-by-year fixed effects, "Base X" includes a limited set of demographic controls, and "Main" replicates our main specification. "W/ Stringencies" includes judgment amounts and rate of granting stays in Chicago and rate of granting stays in New York (see Section 6.7 for details). The † indicates that homelessness results for Chicago are from the Census sample as HMIS records are not linked to the CPS education sample. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

Table C.2: Robustness to Controls: School Attachment and Engagement (Education Sample)

	Chicago				New York				Combined			
	No X	Base X	Main	W/ Stringencies	No X	Base X	Main	W/ Stringencies	No X	Base X	Main	W/ Stringencies
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-filing school year 1:												
Not at pre-case school	0.270***	0.274***	0.271***	0.248***	0.044	0.042	0.038	0.040	0.082**	0.081**	0.077**	0.074*
	(0.082)	(0.085)	(0.083)	(0.078)	(0.044)	(0.044)	(0.044)	(0.044)	(0.039)	(0.039)	(0.039)	(0.039)
Percent absent	0.043*	0.041*	0.030*	0.027	0.025*	0.022	0.023**	0.023**	0.027**	0.025**	0.024**	0.023**
	(0.022)	(0.021)	(0.018)	(0.018)	(0.014)	(0.014)	(0.011)	(0.011)	(0.013)	(0.013)	(0.010)	(0.010)
Chronic absent	0.306***	0.295***	0.213***	0.224***	0.071	0.062	0.069	0.070	0.101**	0.092**	0.088**	0.090**
	(0.085)	(0.084)	(0.076)	(0.072)	(0.050)	(0.050)	(0.047)	(0.047)	(0.045)	(0.045)	(0.042)	(0.042)
Retained	0.002	-0.001	-0.005	0.009	0.028	0.027	0.027	0.030	0.024	0.023	0.022	0.027
	(0.044)	(0.044)	(0.045)	(0.044)	(0.027)	(0.027)	(0.027)	(0.026)	(0.024)	(0.024)	(0.024)	(0.023)
Observations	40,469	40,469	40,469	40,253	241,728	241,728	241,728	241,728	282,198	282,198	282,198	281,981
Post-filing school year 2:												
Not at pre-case school	0.245***	0.253***	0.249***	0.221**	0.047	0.038	0.037	0.038	0.083*	0.076	0.075	0.071
	(0.094)	(0.096)	(0.093)	(0.091)	(0.058)	(0.058)	(0.058)	(0.058)	(0.050)	(0.051)	(0.051)	(0.050)
Percent absent	0.004	0.003	-0.008	-0.002	0.035**	0.032*	0.028*	0.029*	0.031**	0.028*	0.024*	0.025*
	(0.023)	(0.022)	(0.021)	(0.021)	(0.017)	(0.017)	(0.015)	(0.015)	(0.015)	(0.015)	(0.014)	(0.014)
Chronic absent	0.072	0.070	0.011	0.033	0.090*	0.077	0.062	0.063	0.087**	0.076*	0.055	0.059
	(0.084)	(0.083)	(0.080)	(0.080)	(0.047)	(0.049)	(0.047)	(0.047)	(0.042)	(0.044)	(0.042)	(0.043)
Retained	0.060	0.059	0.051	0.066	0.055*	0.052	0.052	0.053*	0.055**	0.053*	0.052*	0.055**
	(0.053)	(0.053)	(0.053)	(0.053)	(0.032)	(0.032)	(0.032)	(0.032)	(0.028)	(0.028)	(0.028)	(0.028)
Observations	34,363	34,363	34,363	34,168	202,292	202,292	202,292	202,292	236,655	236,655	236,655	236,460

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Table shows robustness of IV results to specification choice. "No X" includes only court-by-year fixed effects, "Base X" includes a limited set of demographic controls, and "Main" replicates our main specification. "W/ Stringencies" includes additional leave-one-out judge stringencies for judgment amounts and rate of granting stays in Chicago and rate of granting stays in New York (see Section 6.7 for details).

Table C.3: Robustness to Controls: Elementary and Middle School Test Scores (Education Sample)

	Chicago				New York				Combined			
	No X (1)	Base X (2)	Main (3)	W/ Stringencies (4)	No X (5)	Base X (6)	Main (7)	W/ Stringencies (8)	No X (9)	Base X (10)	Main (11)	W/ Stringencies (12)
Post-filing school year 1:												
Test score	-0.245	-0.305*	-0.133	-0.114	0.080	0.069	0.104	0.097	0.018	-0.002	0.059	0.057
	(0.166)	(0.168)	(0.127)	(0.127)	(0.110)	(0.115)	(0.095)	(0.095)	(0.095)	(0.098)	(0.081)	(0.080)
Missed test	0.010	-0.003	-0.015	-0.020	0.072***	0.077***	0.077***	0.077***	0.062***	0.065***	0.063***	0.062***
	(0.048)	(0.049)	(0.047)	(0.047)	(0.026)	(0.025)	(0.025)	(0.025)	(0.023)	(0.023)	(0.022)	(0.023)
Observations	25,184	25,184	25,184	25,044	121,876	121,876	121,876	121,876	147,060	147,060	147,060	146,919
Post-filing school year 2:												
Test score	-0.009	-0.003	0.114	0.120	0.049	0.044	0.053	0.045	0.035	0.033	0.068	0.063
	(0.177)	(0.174)	(0.144)	(0.139)	(0.147)	(0.131)	(0.109)	(0.109)	(0.120)	(0.108)	(0.089)	(0.089)
Missed test	0.048	0.043	0.036	0.043	0.075*	0.080*	0.080*	0.080*	0.070**	0.074**	0.073**	0.074**
	(0.050)	(0.048)	(0.047)	(0.046)	(0.039)	(0.041)	(0.041)	(0.042)	(0.034)	(0.036)	(0.036)	(0.036)
Observations	24,341	24,341	24,341	24,215	102,560	102,560	102,560	102,560	126,900	126,900	126,900	126,774

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Table shows robustness of IV results to specification choice. "No X" includes only court-by-year fixed effects, "Base X" includes a limited set of demographic controls, and "Main" replicates our main specification. "W/ Stringencies" includes additional leave-one-out judge stringencies for judgment amounts and rate of granting stays in Chicago and rate of granting stays in New York (see Section 6.7 for details).

Table C.4: Robustness to Controls: High School Credit Accumulation and GPA (Education Sample)

	Chicago				New York				Combined			
	No X	Base X	Main	W/ Stringencies	No X	Base X	Main	W/ Stringencies	No X	Base X	Main	W/ Stringencies
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-filing school year 1:												
Credits earned	-0.217**	-0.253**	-0.236**	-0.197**	-0.121*	-0.120*	-0.138**	-0.137**	-0.125*	-0.125*	-0.142**	-0.140**
	(0.102)	(0.102)	(0.098)	(0.082)	(0.068)	(0.068)	(0.064)	(0.064)	(0.066)	(0.065)	(0.062)	(0.062)
GPA	-0.327	-0.348	-0.276	-0.145								
	(0.393)	(0.358)	(0.316)	(0.319)								
Observations	6,068	6,068	6,068	6,031	74,312	74,312	74,312	74,312	77,459	77,459	77,459	77,443
Post-filing school year 2:												
Credits earned	-0.153*	-0.170**	-0.165*	-0.182**	-0.140*	-0.142*	-0.143*	-0.104	-0.141*	-0.143*	-0.144*	-0.107
	(0.085)	(0.085)	(0.084)	(0.085)	(0.085)	(0.084)	(0.084)	(0.081)	(0.081)	(0.081)	(0.080)	(0.077)
GPA	-0.388	-0.395	-0.281	-0.214								
	(0.331)	(0.311)	(0.299)	(0.317)								
Observations	6,263	6,263	6,263	6,223	71,766	71,766	71,766	71,766	75,173	75,173	75,173	75,154

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Table shows robustness of IV results to specification choice. "No X" includes only court-by-year fixed effects, "Base X" includes a limited set of demographic controls, and "Main" replicates our main specification. "W/ Stringencies" includes additional leave-one-out judge stringencies for judgment amounts and rate of granting stays in Chicago and rate of granting stays in New York (see Section 6.7 for details).

Table C.5: Robustness to Controls: Graduation (Education Sample; Grade 6 to 12)

	Chicago			New York			Combined		
	No X	Main	W/ Stringencies	No X	Main	W/ Stringencies	No X	Main	W/ Stringencies
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Graduation	-0.119	-0.113	-0.073	-0.137**	-0.128**	-0.123**	-0.135**	-0.126**	-0.117**
	(0.093)	(0.092)	(0.097)	(0.062)	(0.056)	(0.056)	(0.056)	(0.051)	(0.051)
Graduation status not observed	0.083	0.080	0.056	0.071*	0.063	0.063	0.073**	0.066*	0.062*
	(0.085)	(0.085)	(0.087)	(0.041)	(0.039)	(0.039)	(0.037)	(0.036)	(0.036)
Observations	21,014	21,014	20,911	129,452	129,452	129,452	150,467	150,467	150,364

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Table shows robustness of IV results to specification choice. "No X" includes only court-by-year fixed effects and "Main" replicates our main specification. "W/ Stringencies" includes additional leave-one-out judge stringencies for judgment amounts and rate of granting stays in Chicago and rate of granting stays in New York (see Section 6.7 for details).

Table C.6: Robustness to Controls: Living Arrangements, Household Structure, and Geography (Census Sample)

	No X	Main	W/ Stringencies	Main RF
	(1)	(2)	(3)	(4)
Living Arrangements				
Total household size	0.601	0.686*	0.754*	0.640*
	(0.391)	(0.400)	(0.409)	(0.386)
Doubling up (incl. grandparents)	0.166**	0.169**	0.181**	0.158**
	(0.079)	(0.077)	(0.078)	(0.068)
Doubling up (excl. grandparents)	0.102	0.102	0.105	0.095
	(0.066)	(0.066)	(0.066)	(0.060)
Multigenerational household	0.127**	0.132**	0.136**	0.123**
	(0.056)	(0.054)	(0.056)	(0.049)
Grandparent household head	0.098*	0.097**	0.103**	0.091**
	(0.052)	(0.049)	(0.051)	(0.045)
Household Structure				
Mother present	0.005	0.018	0.011	0.017
	(0.052)	(0.052)	(0.053)	(0.048)
Father present	-0.009	0.003	0.009	0.003
	(0.082)	(0.082)	(0.083)	(0.077)
Single mother	-0.005	-0.008	-0.018	-0.008
	(0.092)	(0.085)	(0.085)	(0.080)
Non-relative household head	0.004	-0.000	0.003	-0.000
	(0.025)	(0.025)	(0.025)	(0.024)
Geography				
Neighborhood poverty rate	-0.061**	-0.051*	-0.063**	-0.048*
	(0.031)	(0.029)	(0.029)	(0.026)
Out of county	-0.014	-0.028	-0.016	-0.026
	(0.064)	(0.067)	(0.069)	(0.062)
Out of state	-0.011	-0.015	-0.003	-0.014
	(0.059)	(0.060)	(0.063)	(0.056)
Observations	48,000	48,000	48,000	48,000

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Table shows robustness of IV results to specification choice. The "No X" column specification includes only court-year fixed effects as controls, and the "Main" reproduces our main specification. "W/ Stringencies" includes additional leave-one-out judge stringencies for judgment amounts and the rate of stay grants. The "Main RF" column shows the estimate of a regression of the outcome on the instrument with the main set of controls. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

C.2 Robustness to Minimum Number of Cases

This appendix reports the main IV estimates under different minimum numbers of cases a judge must see to be included in the sample. We focus on Chicago because, in New York City, all courtrooms hear more than 500 cases annually after excluding courtrooms that involve non-random case assignment, so setting a lower threshold does not affect the results.

For the outcomes from Chicago Public Schools Data, we report the main estimates "Main IV", which requires each included judge to see 100 cases that year, as well as results that relax this threshold to 25 or 50 observations. We also include estimates that raise the requirement to 200 cases. Lowering the threshold to 25 or 50 observations raises the risk of measurement error. We correct for potential measurement error in judge stringency using Empirical Bayes methods (Morris, 1983). We assume that judge stringencies are drawn from a Beta distribution, and the individual stringencies follow a Bernoulli distribution (Humphries et al., 2025). This is similar to the adjustments used in the literature on teacher value-added, where empirical Bayes is used assuming that test scores and measurement error are normally distributed (Chetty et al., 2014; Kane and Staiger, 2008).

Tables C.7 and C.9–C.12 report our robustness estimates for outcomes using the Chicago Public Schools data. Estimates are broadly similar, with all statistically significant results remaining significant except for credits earned two years after filing, which shifts from marginally significant to insignificant, and percent absent in year 1, which shifts from marginally significant to insignificant in some specifications. Similarly, some outcomes gain statistical significance in some alternative specifications, but these changes all stem from small shifts in the point estimates or standard errors. Table C.8 shows similar robustness results for the Census sample, when relaxing the minimum number of cases to 50. Using the lower minimum, estimates are somewhat larger and Doubling up (excl. grandparents) becomes statistically significant, and the neighborhood poverty estimate loses marginal significance.

Table C.7: Robustness to Minimum Number of Cases: Home Environment (Education Sample; Chicago)

	Chicago						
	Main IV	$N = 25$	$N = 25$ (EB)	$N = 50$	$N = 50$ (EB)	$N = 100$ (EB)	$N = 200$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Not at pre-case address	0.134	0.101	0.101	0.096	0.107	0.139	0.097
	(0.087)	(0.095)	(0.089)	(0.094)	(0.089)	(0.085)	(0.088)
Number of moves	0.100	-0.009	0.016	0.032	0.052	0.109	0.048
	(0.157)	(0.166)	(0.159)	(0.169)	(0.161)	(0.155)	(0.160)
Neighborhood poverty	-0.011	0.011	0.001	-0.000	-0.005	-0.011	-0.008
	(0.022)	(0.027)	(0.023)	(0.024)	(0.023)	(0.022)	(0.022)
McKinney-Vento	0.090	0.109	0.092	0.087	0.077	0.087	0.069
	(0.065)	(0.068)	(0.068)	(0.073)	(0.070)	(0.065)	(0.067)
Observations	42,503	43,177	43,177	42,912	42,912	42,503	41,933
Post-filing school year 2:							
Not at pre-case address	0.136	0.153	0.135	0.131	0.130	0.135	0.103
	(0.085)	(0.095)	(0.087)	(0.093)	(0.087)	(0.083)	(0.085)
Number of moves	0.193	0.111	0.122	0.158	0.164	0.195	0.132
	(0.188)	(0.208)	(0.194)	(0.208)	(0.195)	(0.183)	(0.189)
Neighborhood poverty	-0.014	0.003	-0.006	-0.009	-0.012	-0.014	-0.007
	(0.024)	(0.028)	(0.025)	(0.026)	(0.025)	(0.024)	(0.025)
McKinney-Vento	0.077	0.121*	0.096	0.087	0.076	0.076	0.065
	(0.069)	(0.072)	(0.070)	(0.077)	(0.072)	(0.068)	(0.069)
Observations	36,813	37,356	37,356	37,169	37,169	36,813	36,259

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Table shows robustness of the IV estimates to variation in the minimum case threshold used to select judges in the Cook County sample. "Main IV" reproduces the baseline specification, which restricts the sample to judges hearing at least 100 cases in the calendar year. "$N = 25$," "$N = 50$," "$N = 100$," and "$N = 200$" correspond to thresholds of 25, 50, 100, and 200 cases per judge-year, respectively. The columns containing "(EB)" are adjusted for measurement error using Empirical Bayes, assuming that judge stringencies are drawn from a Beta distribution, and the individual stringencies follow a Bernoulli distribution.

Table C.8: **Robustness to Minimum Number of Cases: Living Arrangements, Household Structure, and Geography (Census Sample)**

	Main IV	$N = 50$
	(1)	(2)
Living Arrangements		
Total household size	0.686*	0.959**
	(0.400)	(0.444)
Doubling up (incl. grandparents)	0.169**	0.216**
	(0.077)	(0.086)
Doubling up (excl. grandparents)	0.102	0.145**
	(0.066)	(0.073)
Multigenerational household	0.132**	0.152**
	(0.054)	(0.059)
Household Structure		
Mother present	0.018	-0.010
	(0.052)	(0.055)
Father present	0.003	-0.006
	(0.082)	(0.087)
Single mother	-0.008	-0.021
	(0.085)	(0.090)
Geography		
Neighborhood poverty rate	-0.051*	-0.038
	(0.029)	(0.030)
Observations	48,000	48,500

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Table shows robustness of the IV estimates to variation in the minimum case threshold used to select judges in the Cook County sample. "Main IV" reproduces the baseline specification, which restricts the sample to judges hearing at least 100 cases in the calendar year. "n50" corresponds to a threshold of 50 cases. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

Table C.9: Robustness to Minimum Number of Cases: School attachment and engagement (Education Sample; Chicago)

	Chicago						
	Main IV	$N = 25$	$N = 25$ (EB)	$N = 50$	$N = 50$ (EB)	$N = 100$ (EB)	$N = 200$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Not at pre-case school	0.271***	0.264***	0.262***	0.278***	0.273***	0.266***	0.245***
	(0.083)	(0.087)	(0.084)	(0.088)	(0.085)	(0.081)	(0.081)
Percent absent	0.030*	0.033*	0.031*	0.032	0.031*	0.029	0.031*
	(0.018)	(0.018)	(0.018)	(0.020)	(0.019)	(0.018)	(0.019)
Chronic absent	0.213***	0.217***	0.214***	0.200**	0.210***	0.219***	0.242***
	(0.076)	(0.082)	(0.079)	(0.088)	(0.081)	(0.074)	(0.075)
Retained	-0.005	-0.007	-0.005	-0.003	-0.000	-0.001	-0.004
	(0.045)	(0.047)	(0.046)	(0.047)	(0.046)	(0.044)	(0.045)
Observations	40,469	41,066	41,066	40,835	40,835	40,469	39,944
Post-filing school year 2:							
Not at pre-case school	0.249***	0.284***	0.261***	0.266***	0.254***	0.242***	0.233***
	(0.093)	(0.099)	(0.093)	(0.099)	(0.093)	(0.089)	(0.087)
Percent absent	-0.008	-0.011	-0.013	-0.021	-0.016	-0.007	-0.012
	(0.021)	(0.022)	(0.022)	(0.024)	(0.023)	(0.021)	(0.023)
Chronic absent	0.011	-0.000	-0.008	-0.023	-0.013	0.010	-0.011
	(0.080)	(0.081)	(0.081)	(0.089)	(0.085)	(0.080)	(0.083)
Retained	0.051	0.054	0.052	0.055	0.054	0.051	0.048
	(0.053)	(0.056)	(0.054)	(0.057)	(0.054)	(0.052)	(0.054)
Observations	34,363	34,837	34,837	34,665	34,665	34,363	33,891

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Table shows robustness of the IV estimates to variation in the minimum case threshold used to select judges in the Cook County sample. "Main IV" reproduces the baseline specification, which restricts the sample to judges hearing at least 100 cases in the calendar year. "$N = 25$," "$N = 50$," "$N = 100$," and "$N = 200$" correspond to thresholds of 25, 50, 100, and 200 cases per judge-year, respectively. The columns containing "(EB)" are adjusted for measurement error using Empirical Bayes, assuming that judge stringencies are drawn from a Beta distribution, and the individual stringencies follow a Bernoulli distribution.

Table C.10: Robustness to Minimum Number of Cases: Elementary and Middle School Test Scores (Education Sample; Chicago)

	Chicago						
	Main IV	$N = 25$	$N = 25$ (EB)	$N = 50$	$N = 50$ (EB)	$N = 100$ (EB)	$N = 200$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Test score	-0.133	-0.097	-0.097	-0.145	-0.125	-0.117	-0.075
	(0.127)	(0.127)	(0.128)	(0.135)	(0.132)	(0.127)	(0.128)
Missed test	-0.015	-0.016	-0.012	-0.014	-0.007	-0.011	-0.005
	(0.047)	(0.050)	(0.049)	(0.052)	(0.050)	(0.047)	(0.049)
Observations	25,185	25,540	25,540	25,386	25,386	25,185	24,847
Post-filing school year 2:							
Test score	0.114	0.151	0.160	0.122	0.146	0.140	0.213
	(0.144)	(0.152)	(0.148)	(0.155)	(0.150)	(0.144)	(0.148)
Missed test	0.036	0.018	0.026	0.039	0.039	0.037	0.035
	(0.047)	(0.052)	(0.049)	(0.051)	(0.049)	(0.047)	(0.047)
Observations	24,341	24,644	24,644	24,523	24,523	24,341	24,020

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Table shows robustness of the IV estimates to variation in the minimum case threshold used to select judges in the Cook County sample. "Main IV" reproduces the baseline specification, which restricts the sample to judges hearing at least 100 cases in the calendar year. "$N = 25$," "$N = 50$," "$N = 100$," and "$N = 200$" correspond to thresholds of 25, 50, 100, and 200 cases per judge-year, respectively. The columns containing "(EB)" are adjusted for measurement error using Empirical Bayes, assuming that judge stringencies are drawn from a Beta distribution, and the individual stringencies follow a Bernoulli distribution.

Table C.11: Robustness to Minimum Number of Cases: High School Credit Accumulation and GPA (Education Sample; Chicago)

	Chicago						
	Main IV	$N = 25$	$N = 25$ (EB)	$N = 50$	$N = 50$ (EB)	$N = 100$ (EB)	$N = 200$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Credits earned	-0.236**	-0.269***	-0.250***	-0.261***	-0.247***	-0.235**	-0.237**
	(0.098)	(0.103)	(0.094)	(0.099)	(0.091)	(0.094)	(0.096)
GPA	-0.276	-0.400	-0.331	-0.342	-0.311	-0.256	-0.167
	(0.316)	(0.318)	(0.303)	(0.326)	(0.307)	(0.305)	(0.294)
Observations	6,068	6,172	6,172	6,136	6,136	6,068	5,987
Post-filing school year 2:							
Credits earned	-0.165*	-0.138	-0.134	-0.131	-0.135	-0.162*	-0.133
	(0.084)	(0.089)	(0.086)	(0.089)	(0.086)	(0.084)	(0.085)
GPA	-0.281	-0.330	-0.284	-0.256	-0.250	-0.277	-0.180
	(0.299)	(0.296)	(0.295)	(0.318)	(0.306)	(0.297)	(0.289)
Observations	6,263	6,358	6,358	6,325	6,325	6,263	6,178

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Table shows robustness of the IV estimates to variation in the minimum case threshold used to select judges in the Cook County sample. "Main IV" reproduces the baseline specification, which restricts the sample to judges hearing at least 100 cases in the calendar year. "$N = 25$," "$N = 50$," "$N = 100$," and "$N = 200$" correspond to thresholds of 25, 50, 100, and 200 cases per judge-year, respectively. The columns containing "(EB)" are adjusted for measurement error using Empirical Bayes, assuming that judge stringencies are drawn from a Beta distribution, and the individual stringencies follow a Bernoulli distribution.

Table C.12: Robustness to Minimum Number of Cases: High School Graduation (Education Sample; Chicago)

	Chicago						
	Main IV	$N = 25$	$N = 25$ (EB)	$N = 50$	$N = 50$ (EB)	$N = 100$ (EB)	$N = 200$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Graduation	-0.113	-0.113	-0.106	-0.130	-0.118	-0.109	-0.086
	(0.092)	(0.094)	(0.091)	(0.098)	(0.093)	(0.090)	(0.090)
Graduation status not observed	0.080	0.093	0.089	0.099	0.090	0.078	0.070
	(0.085)	(0.087)	(0.085)	(0.091)	(0.087)	(0.084)	(0.082)
Observations	21,015	21,264	21,264	21,158	21,158	21,015	20,791

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Table shows robustness of the IV estimates to variation in the minimum case threshold used to select judges in the Cook County sample. "Main IV" reproduces the baseline specification, which restricts the sample to judges hearing at least 100 cases in the calendar year. "$N = 25$," "$N = 50$," "$N = 100$," and "$N = 200$" correspond to thresholds of 25, 50, 100, and 200 cases per judge-year, respectively. The columns containing "(EB)" are adjusted for measurement error using Empirical Bayes, assuming that judge stringencies are drawn from a Beta distribution, and the individual stringencies follow a Bernoulli distribution.

C.3 City Specific OLS and RF Results

Table C.13: Home Environment (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$	OLS	RF	$\mathbb{E}[Y\|E=0]$	OLS	RF	$\mathbb{E}[Y\|E=0]$	OLS	RF
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post-filing school year 1:									
Not at pre-case address	0.486	0.112***	0.164	0.191	0.148***	0.111**	0.223	0.141***	0.121***
	(0.500)	(0.007)	(0.108)	(0.393)	(0.004)	(0.046)	(0.416)	(0.003)	(0.043)
Number of moves	0.580	0.173***	0.122	0.304	0.247***	0.192**	0.332	0.234***	0.180**
	(0.649)	(0.009)	(0.192)	(0.654)	(0.007)	(0.078)	(0.659)	(0.006)	(0.073)
Neighborhood poverty	0.318	0.002	-0.013	0.301	-0.005***	0.019	0.303	-0.003***	0.012
	(0.147)	(0.002)	(0.025)	(0.120)	(0.001)	(0.013)	(0.124)	(0.001)	(0.012)
Homelessness†	0.008	0.007***	0.048*	0.017	0.062***	0.026	0.016	0.056***	0.028*
		(0.002)	(0.029)	(0.129)	(0.002)	(0.017)		(0.002)	(0.016)
McKinney-Vento	0.118	0.059***	0.094						
	(0.323)	(0.004)	(0.069)						
Observations	17,640	40,103	38,503	133,959	198,957	198,957	151,786	237,334	235,334
Post-filing school year 2:									
Not at pre-case address	0.602	0.113***	0.171	0.264	0.168***	0.140**	0.302	0.157***	0.146***
	(0.490)	(0.008)	(0.107)	(0.441)	(0.004)	(0.059)	(0.459)	(0.004)	(0.052)
Number of moves	0.815	0.240***	0.242	0.573	0.424***	0.337**	0.596	0.394***	0.322**
	(0.789)	(0.015)	(0.239)	(1.026)	(0.010)	(0.147)	(1.009)	(0.009)	(0.129)
Neighborhood poverty	0.315	0.001	-0.017	0.300	-0.005***	0.020	0.302	-0.003***	0.012
	(0.145)	(0.002)	(0.029)	(0.121)	(0.001)	(0.015)	(0.124)	(0.001)	(0.014)
Homelessness†	0.010	0.003	0.057**	0.023	0.021***	0.041**	0.021	0.019***	0.042**
		(0.002)	(0.028)	(0.148)	(0.001)	(0.021)		(0.001)	(0.019)
McKinney-Vento	0.150	0.064***	0.080						
	(0.357)	(0.005)	(0.072)						
Observations	16,038	36,850	34,950	110,233	163,387	163,387	125,813	196,985	194,610

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "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 in Chicago and an indicator for being listed on a family application for a shelter bed in New York. 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-FY26-P2476-R12909. "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. Columns (1), (4), and (7) report the nonevicted mean, columns (2), (5), (8) report the coefficient on an eviction indicator from an OLS regression, and columns (3), (6), and (9) present reduced-form estimates under our main specification. Means include standard deviations in parentheses, while OLS and RF 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 C.14: School Attachment and Engagement (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$	OLS	RF	$\mathbb{E}[Y\|E=0]$	OLS	RF	$\mathbb{E}[Y\|E=0]$	OLS	RF
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post-filing school year 1:									
Not at pre-case school	0.391	0.060***	0.330***	0.266	0.041***	0.033	0.278	0.044***	0.083**
	(0.488)	(0.007)	(0.098)	(0.442)	(0.003)	(0.038)	(0.448)	(0.002)	(0.036)
Percent absent	0.109	0.009***	0.034*	0.135	0.007***	0.019**	0.133	0.007***	0.021**
	(0.124)	(0.001)	(0.021)	(0.166)	(0.001)	(0.010)	(0.163)	(0.001)	(0.009)
Chronic absent	0.361	0.046***	0.242***	0.424	0.027***	0.058	0.419	0.030***	0.082**
	(0.480)	(0.005)	(0.085)	(0.494)	(0.002)	(0.040)	(0.493)	(0.002)	(0.036)
Retained	0.105	0.012***	-0.006	0.129	0.004**	0.023	0.127	0.005***	0.018
	(0.307)	(0.003)	(0.054)	(0.335)	(0.001)	(0.023)	(0.333)	(0.001)	(0.021)
Observations	14,459	40,469	40,469	164,125	241,728	241,728	178,584	282,198	282,198
Post-filing school year 2:									
Not at pre-case school	0.499	0.065***	0.295***	0.382	0.057***	0.031	0.394	0.058***	0.078*
	(0.500)	(0.008)	(0.107)	(0.486)	(0.003)	(0.049)	(0.489)	(0.003)	(0.045)
Percent absent	0.105	0.009***	-0.009	0.142	0.005***	0.023*	0.139	0.005***	0.019
	(0.123)	(0.001)	(0.025)	(0.175)	(0.001)	(0.013)	(0.172)	(0.001)	(0.012)
Chronic absent	0.343	0.038***	0.013	0.432	0.018***	0.051	0.426	0.021***	0.046
	(0.475)	(0.005)	(0.092)	(0.495)	(0.002)	(0.039)	(0.494)	(0.002)	(0.036)
Retained	0.144	0.015***	0.064	0.157	0.006***	0.043	0.156	0.007***	0.046*
	(0.351)	(0.004)	(0.066)	(0.364)	(0.002)	(0.026)	(0.363)	(0.002)	(0.024)
Observations	12,132	34,363	34,363	137,378	202,292	202,292	149,510	236,655	236,655

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "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 10% or more 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 ($r = -1$). 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. Columns (1), (4), and (7) report the nonevicted mean, columns (2), (5), (8) report the coefficient on an eviction indicator from an OLS regression, and columns (3), (6), and (9) present reduced-form estimates under our main specification. Means include standard deviations in parentheses, while OLS and RF 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 C.15: Elementary and Middle School Test Scores (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$	OLS	RF	$\mathbb{E}[Y\|E=0]$	OLS	RF	$\mathbb{E}[Y\|E=0]$	OLS	RF
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post-filing school year 1:									
Test score	-0.337	-0.041***	-0.161	-0.339	-0.018***	0.084	-0.338	-0.022***	0.037
	(0.853)	(0.009)	(0.155)	(0.805)	(0.004)	(0.076)	(0.810)	(0.004)	(0.069)
Missed test	0.057	0.006*	-0.019	0.114	0.015***	0.064***	0.109	0.014***	0.051**
	(0.232)	(0.003)	(0.060)	(0.318)	(0.001)	(0.021)	(0.312)	(0.001)	(0.020)
Observations	8,704	25,185	25,185	81,974	121,876	121,876	90,678	147,060	147,060
Post-filing school year 2:									
Test score	-0.330	-0.051***	0.129	-0.331	-0.019***	0.047	-0.331	-0.027***	0.067
	(0.846)	(0.011)	(0.163)	(0.796)	(0.005)	(0.095)	(0.803)	(0.005)	(0.082)
Missed test	0.057	0.001	0.042	0.183	0.007***	0.068*	0.172	0.006***	0.063**
	(0.231)	(0.003)	(0.055)	(0.387)	(0.001)	(0.035)	(0.377)	(0.001)	(0.031)
Observations	8,252	24,341	24,341	68,729	102,560	102,560	76,981	126,901	126,901

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "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. "Test score" is defined as mean of reading and math scores 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 of one within each grade-school year for all students enrolled in that grade and school year in the district who took the test. "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. Columns (1), (4), and (7) report the nonevicted mean, columns (2), (5), (8) report the coefficient on an eviction indicator from an OLS regression, and columns (3), (6), and (9) present reduced-form estimates under our main specification. Means include standard deviations in parentheses, while OLS and RF 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 C.16: High School Credit Accumulation and GPA (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$	OLS	RF	$\mathbb{E}[Y\|E=0]$	OLS	RF	$\mathbb{E}[Y\|E=0]$	OLS	RF
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post-filing school year 1:									
Credits earned	0.900	-0.001	-0.292***	0.830	-0.015***	-0.124**	0.832	-0.015***	-0.130**
	(0.225)	(0.008)	(0.108)	(0.426)	(0.003)	(0.058)	(0.423)	(0.003)	(0.056)
GPA	2.139	-0.057***	-0.325						
	(1.037)	(0.020)	(0.370)						
Observations	2,286	6,068	6,068	51,807	74,312	74,312	53,003	77,459	77,459
Post-filing school year 2:									
Credits earned	0.910	-0.025***	-0.227**	0.824	-0.020***	-0.112*	0.826	-0.020***	-0.117*
	(0.221)	(0.009)	(0.106)	(0.429)	(0.003)	(0.066)	(0.425)	(0.003)	(0.063)
GPA	2.143	-0.079***	-0.383						
	(1.033)	(0.024)	(0.405)						
Observations	2,294	6,263	6,263	50,141	71,766	71,766	51,401	75,173	75,173

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. "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 earned" is the number of credits completed divided by the standard number of credits attempted 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. Columns (1), (4), and (7) report the nonevicted mean, columns (2), (5), (8) report the coefficient on an eviction indicator from an OLS regression, and columns (3), (6), and (9) present reduced-form estimates under our main specification. Means include standard deviations in parentheses, while OLS and RF 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 C.17: High School Graduation (Education Sample)

	Chicago			New York			Combined		
	$\mathbb{E}[Y\|E=0]$	OLS	RF	$\mathbb{E}[Y\|E=0]$	OLS	RF	$\mathbb{E}[Y\|E=0]$	OLS	RF
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Graduation	0.753	-0.041***	-0.158	0.670	-0.039***	-0.107**	0.676	-0.039***	-0.113**
	(0.431)	(0.007)	(0.128)	(0.470)	(0.003)	(0.049)	(0.468)	(0.003)	(0.046)
Graduation status not observed	0.207	0.020***	0.099	0.123	0.029***	0.054	0.131	0.027***	0.061*
	(0.405)	(0.006)	(0.102)	(0.329)	(0.002)	(0.034)	(0.337)	(0.002)	(0.033)
Observations	7,646	21,015	21,015	89,700	129,453	129,453	97,347	150,467	150,467

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. "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. Columns (1), (4), and (7) report the nonevicted mean, columns (2), (5), (8) report the coefficient on an eviction indicator from an OLS regression, and columns (3), (6), and (9) present reduced-form estimates under our main specification. Means include standard deviations in parentheses, while OLS and RF 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, the final row reports the average sample size across outcomes.

C.4 Adjusting for Multiple Hypothesis Testing

Because we examine the effects of eviction on many outcomes, we assess whether our statistically significant estimates are robust to corrections for multiple hypothesis testing. Tables C.18–C.22 report our main combined IV estimates alongside their standard errors, unadjusted p-values, and p-values adjusted using the Benjamini–Hochberg (BH) procedure (Benjamini and Hochberg, 1995), which controls the false discovery rate (FDR): the expected share of false rejections among all rejected hypotheses. We group outcomes into table-specific families corresponding to each outcome domain and apply the correction separately to the year-1 and year-2 estimates within each family. We report adjusted p-values only for domains where more than one main outcome is present for both cities.

The adjusted p-values indicate that our central findings are not driven by multiple testing. The effects on residential mobility (not being at the pre-case address and the number of moves), on high school graduation, and on school switching and absenteeism in the first post-filing year remain statistically significant after adjustment, with the first two at the five percent level and the first-year school-engagement outcomes at the ten percent level. Several Census living-arrangement outcomes (doubling up, living in a multigenerational household, and living in a grandparent-headed household) move from unadjusted p-values below 0.05 to adjusted p-values just below 0.10. A few marginal estimates, including first-year homelessness and the Census neighborhood-poverty effect, are no longer significant at conventional levels once adjusted.

Table C.18: Home Environment (Education Sample, adjusted for multiple hypothesis testing)

	Estimate (1)	SE (2)	p-val (raw) (3)	p-val (BH) (4)
Post-filing school year 1:				
Not at pre-case address	0.134	0.047	0.004	0.015
Number of moves	0.210	0.081	0.009	0.018
Neighborhood poverty	0.017	0.014	0.240	0.240
Homelessness†	0.032	0.019	0.089	0.118
Post-filing school year 2:				
Not at pre-case address	0.176	0.065	0.006	0.025
Number of moves	0.407	0.163	0.012	0.025
Neighborhood poverty	0.018	0.017	0.286	0.286
Homelessness†	0.052	0.023	0.026	0.034

Notes: Columns (1) and (2) report the combined IV estimate and standard error, respectively. Column (3) reports the unadjusted p-value. Column (4) reports the p-value after applying the Benjamini–Hochberg (BH) false discovery rate correction, computed separately within each post-filing school year. The † indicates that homelessness results for Chicago are from the Census sample as HMIS records are not linked to the CPS education sample. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

Table C.19: Living Arrangements, Household Structure, and Geography (Census Sample, adjusted for multiple hypothesis testing)

	Estimate (1)	SE (2)	p-val (raw) (3)	p-val (BH) (4)
Living Arrangements				
Total household size	0.686	0.400	0.086	0.107
Doubling up (incl. grandparents)	0.169	0.077	0.029	0.071
Doubling up (excl. grandparents)	0.102	0.066	0.123	0.123
Multigenerational household	0.132	0.054	0.014	0.071
Grandparent household head	0.097	0.049	0.048	0.081
Household Structure				
Mother present	0.018	0.052	0.733	0.985
Father present	0.003	0.082	0.973	0.985
Single mother	-0.008	0.085	0.922	0.985
Non-relative household head	-0.000	0.025	0.985	0.985
Geography				
Neighborhood poverty rate	-0.051	0.029	0.074	0.222
Out of county	-0.028	0.067	0.675	0.805
Out of state	-0.015	0.060	0.805	0.805

Notes: Columns (1) and (2) report the IV estimate and standard error, respectively. Column (3) reports the unadjusted p-value. Column (4) reports the p-value after applying the Benjamini–Hochberg (BH) false discovery rate correction. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

Table C.20: School Environment (Education Sample, adjusted for multiple hypothesis testing)

	Estimate (1)	SE (2)	p-val (raw) (3)	p-val (BH) (4)
Post-filing school year 1:				
Not at pre-case school	0.077	0.039	0.050	0.067
Percent absent	0.024	0.010	0.019	0.067
Chronic absent	0.088	0.042	0.036	0.067
Retained	0.022	0.024	0.350	0.350
Post-filing school year 2:				
Not at pre-case school	0.075	0.051	0.137	0.182
Percent absent	0.024	0.014	0.086	0.172
Chronic absent	0.055	0.042	0.194	0.194
Retained	0.052	0.028	0.066	0.172

Notes: Columns (1) and (2) report combined IV estimate and standard error, respectively. Column (3) reports the unadjusted p-value. Column (4) reports the p-value after applying the Benjamini–Hochberg (BH) false discovery rate correction, computed separately within each post-filing school year.

Table C.21: Elementary and Middle School Test Scores (Education Sample, adjusted for multiple hypothesis testing)

	Estimate (1)	SE (2)	p-val (raw) (3)	p-val (BH) (4)
Post-filing school year 1:				
Test score	0.059	0.081	0.465	0.465
Missed test	0.063	0.022	0.005	0.010
Post-filing school year 2:				
Test score	0.068	0.089	0.449	0.449
Missed test	0.073	0.036	0.040	0.080

Notes: Columns (1) and (2) report the combined IV estimate and standard error, respectively. Column (3) reports the unadjusted p-value. Column (4) reports the p-value after applying the Benjamini–Hochberg (BH) false discovery rate correction.

Table C.22: High School Graduation (Education Sample, adjusted for multiple hypothesis testing)

	Estimate (1)	SE (2)	p-val (raw) (3)	p-val (BH) (4)
Graduation	-0.126	0.051	0.013	0.026
Graduation status not observed	0.066	0.036	0.066	0.066

Notes: Columns (1) and (2) report the combined IV estimate and standard error, respectively. Column (3) reports the unadjusted p-value. Column (4) reports the p-value after applying the Benjamini–Hochberg (BH) false discovery rate correction.

C.5 Balanced Panel

Table C.23: Balanced Panel (Education Sample)

	All		Balanced	
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV
	(1)	(2)	(3)	(4)
Post-filing school year 1:				
Not at pre-case address	0.223	0.134***	0.216	0.131***
	(0.416)	(0.047)	(0.411)	(0.051)
Not at pre-case school	0.278	0.077**	0.272	0.077*
	(0.448)	(0.039)	(0.445)	(0.046)
Percent absent	0.133	0.024**	0.118	0.022**
	(0.163)	(0.010)	(0.137)	(0.010)
Chronic absent	0.419	0.088**	0.394	0.084*
	(0.493)	(0.042)	(0.489)	(0.043)
Retained	0.127	0.022	0.113	0.023
	(0.333)	(0.024)	(0.316)	(0.025)
Test score	-0.338	0.059	-0.357	0.037
	(0.810)	(0.081)	(0.811)	(0.093)
Credits earned	0.832	-0.142**	0.863	-0.130*
	(0.423)	(0.062)	(0.408)	(0.073)
Observations	138,986	220,073	119,388	187,407
Post-filing school year 2:				
Not at pre-case address	0.302	0.176***	0.301	0.169**
	(0.459)	(0.065)	(0.458)	(0.071)
Not at pre-case school	0.394	0.075	0.380	0.059
	(0.489)	(0.051)	(0.485)	(0.053)
Percent absent	0.139	0.024*	0.139	0.024*
	(0.172)	(0.014)	(0.171)	(0.014)
Chronic absent	0.426	0.055	0.424	0.062
	(0.494)	(0.042)	(0.494)	(0.043)
Retained	0.156	0.052*	0.150	0.046
	(0.363)	(0.028)	(0.357)	(0.029)
Test score	-0.331	0.068	-0.356	0.115
	(0.803)	(0.089)	(0.809)	(0.097)
Credits earned	0.826	-0.144*	0.833	-0.131
	(0.425)	(0.080)	(0.422)	(0.083)
Observations	115,078	182,771	112,831	177,785

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "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. "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 10% or more 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 ($r = -1$). "Test score" is defined as mean of reading and math scores 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 of one within each grade-school year for all students enrolled in that grade and school year in the district who took the test. "Missed test" is defined as an indicator that is equal to one if a student was actively enrolled in grades 3-8 but does not have one or both test scores. All outcomes are defined among actively enrolled students.
Columns (1)-(2) report combined results for all students, and (3)-(4) report combined results for students that appear in all-post-filing school years that we study. For each category, the first sub-column reports the nonevicted mean, and the second reports the TSLS estimate for eviction. Means are accompanied by standard deviations in parentheses, while TSLS estimates are accompanied by standard errors in parentheses. Standard errors are clustered at the judge×case-year level. The regression specifications are as described in the notes of Table 3. For each column and time period, the final row reports the average sample size across outcomes.

C.6 Robustness to Alternative Clustering

We cluster standard errors at the judge-by-year level, which is the level at which the instrument is constructed. Clustering at the level of the variation follows standard practice and addresses the downward bias that arises when a regressor is common to every observation in a cluster (Moulton, 1990; Colin Cameron and Miller, 2015). In New York we do not observe judge identities and construct stringency at the courtroom-year level. Judges rotate across courtrooms annually, so clustering on courtroom would group cases decided by different judges. The tables below report standard errors under both judge and judge-by-year clustering. The two sets are close for most outcomes, and the judge-clustered standard error is the smaller of the two in roughly seven of every ten populated cells.

Table C.24: IV Estimates and Alternative Clustering

	Chicago			New York			Combined		
	IV (1)	SE (Judge×Year) (2)	SE (Judge) (3)	IV (4)	SE (Judge×Year) (5)	SE (Judge) (6)	IV (7)	SE (Judge×Year) (8)	SE (Judge) (9)
Not at pre-case address (Post-filing school year 1)	0.134	(0.087)	(0.075)	0.135	(0.053)	(0.054)	0.134	(0.047)	(0.046)
Not at pre-case address (Post-filing school year 2)	0.136	(0.085)	(0.080)	0.186	(0.078)	(0.077)	0.176	(0.065)	(0.064)
Number of moves (Post-filing school year 1)	0.100	(0.157)	(0.141)	0.233	(0.092)	(0.094)	0.210	(0.081)	(0.081)
Number of moves (Post-filing school year 2)	0.193	(0.188)	(0.158)	0.448	(0.190)	(0.181)	0.407	(0.163)	(0.154)
Neighborhood poverty (Post-filing school year 1)	-0.011	(0.022)	(0.023)	0.024	(0.017)	(0.018)	0.017	(0.014)	(0.015)
Neighborhood poverty (Post-filing school year 2)	-0.014	(0.024)	(0.023)	0.028	(0.021)	(0.018)	0.018	(0.017)	(0.015)
McKinney-Vento (Post-filing school year 1)	0.090	(0.065)	(0.070)						
McKinney-Vento (Post-filing school year 2)	0.077	(0.069)	(0.062)						
Not at pre-case school (Post-filing school year 1)	0.271	(0.083)	(0.082)	0.038	(0.044)	(0.041)	0.077	(0.039)	(0.036)
Not at pre-case school (Post-filing school year 2)	0.249	(0.093)	(0.090)	0.037	(0.058)	(0.057)	0.075	(0.051)	(0.050)
Percent absent (Post-filing school year 1)	0.030	(0.018)	(0.019)	0.023	(0.011)	(0.010)	0.024	(0.010)	(0.009)
Percent absent (Post-filing school year 2)	-0.008	(0.021)	(0.018)	0.028	(0.015)	(0.013)	0.024	(0.014)	(0.012)
Chronic absent (Post-filing school year 1)	0.213	(0.076)	(0.081)	0.069	(0.047)	(0.051)	0.088	(0.042)	(0.046)
Chronic absent (Post-filing school year 2)	0.011	(0.080)	(0.078)	0.062	(0.047)	(0.035)	0.055	(0.042)	(0.032)
Retained (Post-filing school year 1)	-0.005	(0.045)	(0.043)	0.027	(0.027)	(0.028)	0.022	(0.024)	(0.025)
Retained (Post-filing school year 2)	0.051	(0.053)	(0.051)	0.052	(0.032)	(0.028)	0.052	(0.028)	(0.025)
Test score (Post-filing school year 1)	-0.133	(0.127)	(0.142)	0.104	(0.095)	(0.090)	0.059	(0.081)	(0.077)
Test score (Post-filing school year 2)	0.114	(0.144)	(0.159)	0.053	(0.109)	(0.074)	0.068	(0.089)	(0.068)
Missed test (Post-filing school year 1)	-0.015	(0.047)	(0.032)	0.077	(0.025)	(0.021)	0.063	(0.022)	(0.019)
Missed test (Post-filing school year 2)	0.036	(0.047)	(0.050)	0.080	(0.041)	(0.043)	0.073	(0.036)	(0.037)
Credits earned (Post-filing school year 1)	-0.236	(0.098)	(0.103)	-0.138	(0.064)	(0.064)	-0.142	(0.062)	(0.061)
Credits earned (Post-filing school year 2)	-0.165	(0.084)	(0.058)	-0.143	(0.084)	(0.080)	-0.144	(0.080)	(0.076)
GPA (Post-filing school year 1)	-0.276	(0.316)	(0.289)						
GPA (Post-filing school year 2)	-0.281	(0.299)	(0.290)						
Graduation	-0.113	(0.092)	(0.083)	-0.128	(0.056)	(0.045)	-0.126	(0.051)	(0.041)
Graduation status not observed	0.080	(0.085)	(0.072)	0.063	(0.039)	(0.048)	0.066	(0.036)	(0.042)
Observations	30,090			161,283			191,618		

Notes: Columns (1), (4), and (7) report main IV estimates for Chicago, New York, and the combined sample, respectively. For each sample, two sets of standard errors are reported: SE (Judge× Year) and SE (Judge). Judge×Year clusters at the judge-by-year level and is reported in columns (2), (5), and (8), and Judge clusters at the judge level and is reported in columns (3), (6), and (9).

Table C.25: Census Estimates and Alternative Clustering

	IV (1)	SE (Judge×Year) (2)	SE (Judge) (3)
Living Arrangements			
Total household size	0.686	(0.400)	(0.480)
Doubling up (incl. grandparents)	0.169	(0.077)	(0.077)
Doubling up (excl. grandparents)	0.102	(0.066)	(0.072)
Multigenerational household	0.132	(0.054)	(0.054)
Grandparent household head	0.097	(0.049)	(0.048)
Household Structure			
Mother present	0.018	(0.052)	(0.058)
Father present	0.003	(0.082)	(0.101)
Single mother	-0.008	(0.085)	(0.099)
Non-relative household head	-0.000	(0.025)	(0.027)
Geography			
Neighborhood poverty rate	-0.051	(0.029)	(0.025)
Out of county	-0.028	(0.067)	(0.070)
Out of state	-0.015	(0.060)	(0.055)
Observations	48,000		

Notes: Column (1) reports main IV estimates for the Census sample. Two sets of standard errors are reported: SE (Judge× Year) and SE (Judge). Judge×Year clusters at the judge-by-year level and is reported in column (2) and Judge clusters at the judge level and is reported in column (3). Approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2476-R10965 and CBDRB-FY26-P2476-R12909.

C.7 Census Sample Robustness Tables

Table C.26: Living Arrangements, Household Structure, and Geography (Census Sample; ages 6-18)

	$\mathbb{E}[Y\|E=0]$ (1)	OLS (2)	IV (3)
Living Arrangements			
Total household size	4.809	0.139***	0.808*
		(0.025)	(0.462)
Doubling up (incl. grandparents)	0.212	0.028***	0.254***
		(0.005)	(0.093)
Doubling up (excl. grandparents)	0.134	0.010**	0.192**
		(0.005)	(0.077)
Multigenerational household	0.090	0.017***	0.146**
		(0.003)	(0.062)
Grandparent household head	0.082	0.017***	0.074
		(0.003)	(0.063)
Household Structure			
Mother present	0.855	-0.025***	0.006
		(0.004)	(0.064)
Father present	0.302	0.004	-0.004
		(0.006)	(0.090)
Single mother	0.577	-0.029***	0.006
		(0.006)	(0.100)
Non-relative household head	0.015	0.003*	-0.004
		(0.002)	(0.026)
Geography			
Neighborhood poverty rate	0.234	-0.002	-0.054*
		(0.002)	(0.032)
Out of county	0.224	0.016***	-0.078
		(0.005)	(0.075)
Out of state	0.133	0.016***	-0.069
		(0.005)	(0.067)
Observations		40,000	37,000

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reproduces Table 4 except restricts the sample to age 6-18, to allow for comparison with the education K-12 analysis. Observation counts reflect the modal observation counts. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

Table C.27: Robustness to Group Quarters: Living Arrangements, Household Structure, and Geography (Census Sample)

	Main IV	GQ = 0	GQ = 1
	(1)	(2)	(3)
Doubling up (incl. grandparents)	0.169**	0.172**	0.160**
	(0.077)	(0.077)	(0.075)
Doubling up (excl. grandparents)	0.102	0.103	0.092
	(0.066)	(0.066)	(0.066)
Multigenerational household	0.132**	0.133**	0.121**
	(0.054)	(0.053)	(0.057)
Grandparent household head	0.097**	0.099**	0.087*
	(0.049)	(0.048)	(0.050)
Observations	48,000	49,000	49,000

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Column (1) reproduces the baseline specification, which drops children in group quarters. Column (2) codes all group quarters observations as 0 for each outcome. Column (3) codes all group quarters observations as 1. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

D Details on the Bounding Approach

D.1 Partial identification approach

We follow the notation and approach developed in Section 6.5.1. Let Y denote an outcome (e.g., graduation), S denote being observed in the sample, E denote eviction status (treatment), and Z denote the instrument. To develop our approach, we implicitly condition on covariates and suppose that individuals are randomly assigned to instrument $Z = z_1$ or $Z = z_0$. We discuss implementation using covariates and the full range of instrument values Z in Appendix D.2.

The researcher observes the distribution (YS, S, E, Z), where $YS = Y$ when $S = 1$ and $YS = 0$ when $S = 0$ captures that the outcome is only observed for those in the sample. The observed data relates to potential variables via $Y = Y(E)$, $S = S(E)$, and $E = E(Z)$. Throughout, we will maintain the IV assumptions that Z is independent of potential variables $(Y(0), Y(1), S(0), S(1), E(z_1), E(z_0))$ with the monotonicity direction $E(z_1) \geq E(z_0)$ for everyone. Additionally, we will impose the sample selection monotonicity assumption of Lee (2009) that $S(1) \leq S(0)$ for everyone. In Appendix D.4, we show how to adapt our results to the case where $S(1) \geq S(0)$ for everyone.

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

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

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

and

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

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

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

Combining the above, we then have that

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

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

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

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

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

Again, see Appendix D.5 for the derivation.

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

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

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

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

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

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

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

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

D.2 Estimation and inference

D.2.1 Estimation

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

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

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

D.2.2 Inference

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

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

To construct a $1-\alpha$ (with $\alpha < 1/2$) confidence interval of $\theta^\star$, we use the ideas of Imbens and Manski (2004); Stoye (2009). Let $z_{1-\alpha}$ be the $1-\alpha$ quantile of the standard normal distribution. Define $CI_{n,1-\alpha}$ as

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

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

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

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

To construct these confidence intervals, we additionally obtain $\hat{\sigma}_L$ and $\hat{\sigma}_U$ by a bootstrap that resamples judge-by-year clusters, matching the clustering used for the point estimates.

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

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

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

As a last robustness check to our inference results, we implement the confidence interval procedure proposed in Lemma 4 of Imbens and Manski (2004). Although this does not require $\pi \geq \underline{\pi} > 0$, it does require a super-efficiency condition that likely fails (since the bound endpoints need not be well-ordered with probability one as $\hat{\pi}$ need not be positive with probability one).

Using this approach, we continue to reject that the LATE-AO for graduation equals zero at the 10% level even as we take $[\delta_L, \delta_U] = [-0.35, 0.35]$.

D.3 Combining bounds across cities

To estimate bounds that combine across cities, we proceed as in Section 5.4. Let ω be the observation weight for New York City and $1-\omega$ be the weight for Chicago. Letting $\theta_j^\star$ denote the parameter of interest in city j and letting $\theta_{L,j}$ and $\theta_{U,j}$ denote the bounds for city j, it follows that

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

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

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

with an analogous expression for $\hat\sigma_{U,\text{comb}}$. The rest proceeds as above.

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

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

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

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

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

with

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

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

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

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

and that

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

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

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

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

D.5 Derivations

D.5.1 Identification

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

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

$$= \mathbb{E}[Y(1)|T=c, S(1)=1]\,\mathbb{P}[T=c, S(1)=1]. \tag{D.25}$$

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

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

Dividing the two, we have

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

$$= \mathbb{E}[Y(1)|T=c,S(1)=1], \tag{D.28}$$

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

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

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

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

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

$$\mathbb{E}[Y(0)|T=c,S(0)=1] \tag{D.31}$$

$$= \mathbb{E}[Y(0)|T=c,S(0)=1,S(1)=0]\pi \tag{D.32}$$

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

$$= \mathbb{E}[Y(0)|T=c,S(0)=1,S(1)=1] \tag{D.34}$$

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

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

$$\mu = \mathbb{E}[Y(1)-Y(0)|T=c,S(0)=1,S(1)=1] \tag{D.36}$$

$$- \Big(\mathbb{E}[Y(0)|T=c,S(0)=1,S(1)=0] - \mathbb{E}[Y(0)|T=c,S(0)=1,S(1)=1] \Big)\pi, \tag{D.37}$$

as required.

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

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

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

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

$$\mathbb{E}[S(1-E)|Z=z_1] - \mathbb{E}[S(1-E)|Z=z_0] = -\,\mathbb{E}[S(0)(E(z_1)-E(z_0))] \tag{D.40}$$

$$= -\,\mathbb{P}[T=c, S(0)=1]. \tag{D.41}$$

Dividing the two yields

$$\frac{\mathbb{E}[S|Z=z_1] - \mathbb{E}[S|Z=z_0]}{\mathbb{E}[S(1-E)|Z=z_1] - \mathbb{E}[S(1-E)|Z=z_0]} = \frac{\mathbb{P}[T=c, S(0)=1, S(1)=0]}{\mathbb{P}[T=c, S(0)=1]} \tag{D.42}$$

$$= \mathbb{P}[S(1)=0|T=c, S(0)=1], \tag{D.43}$$

as required.

D.5.2 Inference

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

$$\lim_{n\to\infty} \mathbb{P}[\theta^\star \in CI_{n,1-\alpha}] \tag{D.44}$$

$$= \lim_{n\to\infty} \mathbb{P}[\hat{\theta}_L - z_{1-\alpha}\frac{\hat{\sigma}_L}{\sqrt{n}} \leq \theta^\star \leq \hat{\theta}_U + z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}}] \tag{D.45}$$

$$\geq \lim_{n\to\infty} \mathbb{P}[\hat{\theta}_L - z_{1-\alpha}\frac{\hat{\sigma}_L}{\sqrt{n}} \leq \theta^\star] \tag{D.46}$$

$$+ \mathbb{P}[\theta^\star \leq \hat{\theta}_U + z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}}] \tag{D.47}$$

$$-1. \tag{D.48}$$

We know $\theta^\star \in [\theta_L, \theta_U]$. We have three cases: $\theta^\star = \theta_L$, $\theta^\star = \theta_U$, or $\theta^\star \in (\theta_L, \theta_U)$. If $\theta^\star = \theta_L$, then (D.46) becomes

$$\mathbb{P}\left[-z_{1-\alpha} \leq \frac{\sqrt{n}(\theta_L - \hat{\theta}_L)}{\hat{\sigma}_L}\right] \to 1-\alpha, \tag{D.49}$$

while (D.47) becomes 1 since

$$1 \geq \mathbb{P}[\theta_L \leq \hat{\theta}_U + z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}}] \geq \mathbb{P}[\theta_L \leq \hat{\theta}_U] \to 1, \tag{D.50}$$

where we use that $\hat{\theta}_U \to \theta_U$ and $\theta_U - \theta_L = (\delta_U - \delta_L)\pi > 0$ by assumption. It follows that $\lim_{n\to\infty} \mathbb{P}[\theta^\star \in CI_{n,1-\alpha}] \geq 1-\alpha$. The case of $\theta^\star = \theta_U$ proceeds analogously, and the above arguments also immediately imply that when $\theta^\star \in (\theta_L, \theta_U)$, then $\lim_{n\to\infty} \mathbb{P}[\theta^\star \in CI_{n,1-\alpha}] = 1$, as required.

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

$$\pi \geq \underline{\pi} \text{ for all } F \tag{D.51}$$

$$\hat{\theta}_L \xrightarrow{p} \theta_L \text{ and } \hat{\theta}_U \xrightarrow{p} \theta_U \text{ uniformly over } \mathcal{F} \tag{D.52}$$

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

$$\lim_{n\to\infty} \inf_{F\in\mathcal{F}} \mathbb{P}\left[-z_{1-\alpha} \leq \frac{\sqrt{n}(\theta_L - \hat{\theta}_L)}{\hat{\sigma}_L}\right] = 1 - \alpha. \tag{D.54}$$

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

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

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

$$-1. \tag{D.57}$$

We again have three cases for $\theta^\star$. Considering the case where $\theta^\star = \theta_L$, the point-wise derivations and (D.54) imply

$$\lim_{n\to\infty} \inf_{F\in\mathcal{F}} \mathbb{P}\left[-z_{1-\alpha} \leq \frac{\sqrt{n}(\theta_L - \hat{\theta}_L)}{\hat{\sigma}_L}\right] = 1 - \alpha. \tag{D.58}$$

As in the point-wise derivations, we can also show that

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

$$\geq \lim_{n\to\infty} \inf_{F\in\mathcal{F}} \mathbb{P}\left[(\theta_U - \hat{\theta}_U) - z_{1-\alpha}\frac{\hat{\sigma}_U}{\sqrt{n}} \leq (\delta_U - \delta_L)\underline{\pi}\right] \to 1, \tag{D.60}$$

where the inequality used (D.51) and the convergence used (D.52). The case of $\theta^\star = \theta_U$ proceeds analogously, and these arguments also immediately imply that when $\theta^\star \in (\theta_L, \theta_U)$, we obtain at least the same coverage. Thus, (D.13) converges at least to $1 - \alpha$, as required.

E Signing the bias in noisy move outcomes

This appendix shows that, in an instrumental-variables design, one-sided outcome misclassification that worsens under treatment biases the Wald (LATE) estimand downward. Under the assumptions laid out below, the non-negative impacts we estimate on moving are a lower bound. This proof is derived for a binary instrument. For a multi-valued instrument, the 2SLS estimand is a positively weighted average of the binary-Z Wald estimands under correctly specified controls, so the inequality shown here is preserved.

Setup. Let $Y^* \in \{0,1\}$ be the true outcome (e.g., an indicator for moving) and $Y \in \{0,1\}$ its observed measurement (e.g., a move recorded in administrative data). Let $Z \in \{0,1\}$ be an instrument and $E \in \{0,1\}$ treatment. Maintain the standard LATE assumptions: random assignment of Z, exclusion (Z affects Y^* only through E), monotonicity, relevance, and correctly measured E.[70] Misclassification is one-sided: there are no false positives, $P(Y = 1 \mid Y^* = 0, E) = 0$, and the arm-specific false-negative rate is $\alpha_e \equiv P(Y = 0 \mid Y^* = 1, E = e) \in [0, 1)$. Let $p_e^c \equiv \mathbb{E}[Y^*(e) \mid \text{complier}]$, so the true LATE is $\tau^* = p_1^c - p_0^c$.

Assume $\tau^* \geq 0$ and $\alpha_1^c \geq \alpha_0^c$, where α_e^c is the false-negative rate for $Y^*(e)$ among compliers. In the moving example, this ordering holds if treated compliers are more likely than control compliers to move in ways that escape administrative measurement.

Claim. Under these assumptions, the Wald estimand $\tau_{\text{IV}}^{\text{obs}}$ satisfies $\tau_{\text{IV}}^{\text{obs}} \leq \tau^*$.

Proof. Under the maintained assumptions, always-takers and never-takers cancel from both the reduced form and the first stage, leaving only compliers; the Wald estimand is therefore the complier contrast of the observed outcome. One-sided misclassification makes the complier mean of the observed outcome under treatment e equal to $(1-\alpha_e^c)\, p_e^c$—a true positive is recorded with probability $1 - \alpha_e^c$, and there are no false positives—so

$$\tau_{\text{IV}}^{\text{obs}} = \frac{\mathbb{E}[Y \mid Z = 1] - \mathbb{E}[Y \mid Z = 0]}{\mathbb{E}[E \mid Z = 1] - \mathbb{E}[E \mid Z = 0]} = (1 - \alpha_1^c)\, p_1^c - (1 - \alpha_0^c)\, p_0^c.$$

Subtracting from $\tau^* = p_1^c - p_0^c$,

$$\tau^* - \tau_{\text{IV}}^{\text{obs}} = \alpha_1^c p_1^c - \alpha_0^c p_0^c = \alpha_0^c \tau^* + (\alpha_1^c - \alpha_0^c)\, p_1^c.$$

With $\tau^* \geq 0$, $\alpha_0^c \geq 0$, $\alpha_1^c \geq \alpha_0^c$, and $p_1^c \geq 0$, the last expression is nonnegative, so $\tau_{\text{IV}}^{\text{obs}} \leq \tau^*$.

[70] Note that we must also assume that mismeasurement of Y can depend on E, but not directly on Z, which is a small extension of the standard exclusion assumption.

Note that, if $\alpha_1^c = \alpha_0^c \geq 0$, then we can relax $\tau^* \geq 0$ and τ_{IV}^{obs} is weakly biased towards 0 regardless of the sign of τ^*.

F Observation Counts for Education IV/OLS Results

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

	Chicago		New York		Combined		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Not at pre-case address	13,536	37,770	113,571	168,258	127,107	206,028	206,028
Number of moves	12,614	35,185	113,571	168,258	126,185	203,443	203,443
Neighborhood poverty	17,660	50,054	124,015	183,102	141,675	233,156	233,156
Homelessness†	27,500	22,500	184,677	276,208	212,177	306,708	298,708
McKinney-Vento	16,891	47,004					
Post-filing school year 2:							
Not at pre-case address	11,118	31,057	87,349	129,205	98,467	160,262	160,262
Number of moves	8,990	24,650	87,349	129,205	96,339	153,855	153,855
Neighborhood poverty	14,711	41,685	95,061	140,269	109,772	181,954	181,954
Homelessness†	27,500	27,500	171,174	254,870	198,674	291,870	282,370
McKinney-Vento	17,869	49,858					

Notes: This table provides the observation counts for the "Home Environment" main IV/OLS results, as presented in Table 3. For each column and time period, the main table observation count reports the average of the counts reported in this table. †Observation counts for HMIS records are rounded in accordance with U.S. Census Bureau disclosure requirements and were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P2476-R12909.

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

	Chicago		New York		Combined		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Not at pre-case school	14,097	40,047	134,775	198,810	148,872	238,857	238,857
Percent absent	13,472	37,279	172,639	254,219	186,111	291,498	291,498
Chronic absent	13,472	37,279	172,639	254,219	186,111	291,498	291,498
Retained	16,796	47,272	176,446	259,665	193,242	306,937	306,937
Post-filing school year 2:							
Not at pre-case school	10,333	29,931	92,695	136,893	103,028	166,824	166,824
Percent absent	12,009	33,604	151,735	223,371	163,744	256,975	256,975
Chronic absent	12,009	33,604	151,735	223,371	163,744	256,975	256,975
Retained	14,177	40,314	153,345	225,533	167,522	265,847	265,847

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

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

	Chicago		New York		Combined		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Test score	8,447	24,389	70,007	103,844	78,454	128,233	128,233
Missed test	8,960	25,980	93,941	139,907	102,901	165,887	165,887
Post-filing school year 2:							
Test score	8,012	23,625	49,626	73,716	57,638	97,341	97,341
Missed test	8,493	25,057	87,831	131,403	96,324	156,460	156,460

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

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

	Chicago		New York		Combined		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-filing school year 1:							
Credits earned	1,196	3,147	51,807	74,312	53,003	77,459	77,459
GPA	3,376	8,989					
Post-filing school year 2:							
Credits earned	1,260	3,407	50,141	71,766	51,401	75,173	75,173
GPA	3,329	9,119					

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

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

	Chicago		New York		Combined		
	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	IV	$\mathbb{E}[Y\|E=0]$	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Graduation	6,086	16,393	83,803	120,196	89,889	136,589	136,589
Graduation status not observed	9,207	25,636	95,597	138,709	104,804	164,345	164,345

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