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Essays on Low-Income Housing Policies

A dissertation submitted in partial satisfaction of the
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in

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by

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Professor Lane Kenworthy

2023

The dissertation of Ellen Wen-Kai Liaw is approved, and it is acceptable in quality and form for publication on microfilm and electronically.

University of California San Diego

2023

DEDICATION

To my father Y.H., my mother Shirley, and my sister Lauren,

for their unconditional love and support

To Joci for his company through this journey

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Chapter 1, “The Impact of Place-Based Housing Access on Academic Performance,” is currently being prepared for submission for publication of the material. The dissertation author was the sole author of the chapter.

Chapter 2, “The Effect of Low-Income Housing Tax Credit on Homelessness,” is currently being prepared for submission for publication of the material. The dissertation author was the sole author of the chapter.

Chapter 3, “The Impact of Right to Counsel to the Poor: Evidence from New York City Housing Courts,” is currently being prepared for submission for publication of the material. The dissertation author was the sole author of the chapter.

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ABSTRACT OF THE DISSERTATION

Essays on Low-Income Housing Policies

by

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Doctor of Philosophy in Economics

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Professor Julie Cullen, Chair

This dissertation comprises three chapters on topics related to low-income housing policies in the United States. Each chapter uses econometric methods to analyze data from administrative sources, aiming to establish causal relationships between variables of interest.

In the first chapter, I provide evidence on how the Low-Income Housing Tax Credit policy impacts children's short- and medium-run human capital formation. I construct a novel dataset using the San Diego Unified School District administrative data and the California LIHTC database. Combining propensity score matching and difference-in-differences, I find an 0.28 percentage points decrease in the absenteeism rate, a 0.049 standard deviation increase in standardized English scores, and a 0.048 standard deviation increase in standardized math scores

for students who moved into LIHTC during the study period. I also find positive effects on high school completion, college enrollment, and college completion.

In the second chapter, I explore the impact of LIHTC on homelessness. Although an increase in affordable housing supply is observed, the effect on households at risk of homelessness is unclear. Combining point-in-time homeless counts and the LIHTC database from the Department of Housing and Urban Development, I construct a panel dataset that allows me to examine changes in homelessness on the Continuum of Care level from 2009 to 2019. With a first-differenced model, I find that one additional LIHTC unit is associated with a decrease of 1.1 homeless people. I also find that sheltered families and children are the primary beneficiaries of new LIHTC units.

In the third chapter, I estimate the impact of the right-to-counsel policy in housing courts. New York City introduced a novel policy in 2018 to provide the right to counsel in housing courts for income-eligible tenants facing eviction. This policy allows low-income households better access to the formal justice system. Taking advantage of the staggered roll-out schedule on the zip code level, I estimate the causal effect of the policy change using a difference-in-differences approach. Despite no statistically significant impact on eviction filings, I find a 16.9 percent decrease in quarterly evictions. The results demonstrate positive impacts of tenant representation on evictions in the short run.

Chapter 1

The Impact of Place-Based Housing Access on Academic Performance

1.1 Introduction

Housing is a basic necessity for individuals to develop and become more prosperous. Recent research shows that housing insecurity leads to higher homeless rate (Glynn et al., 2021), lower employment security (Desmond and Gershenson, 2016), poorer health outcomes (Cutts et al., 2011), and higher crime rates (Jacobs and Gottlieb, 2020). However, access to affordable housing is lacking and is an ongoing policy debate in the United States. A key consideration that has been overlooked in empirical work is the extent to which housing affects human capital formation. Housing is particularly essential for children as childhood experiences often have downstream effects and shape adulthood outcomes. In this paper, I study the impacts of the largest place-based housing subsidy on school-age children's academic achievement and attainment.

Housing can affect human capital formation via several channels – affordability, quality, stability, and neighborhood location (Cunningham and MacDonald, 2012). It is challenging for income-constrained households to seek affordable housing that is also high-quality, which is why it is imperative for government to intervene and increase the supply of such housing. The Low Income Housing Tax Credit (LIHTC) is one of the housing policies that incentivizes the creation of high-quality, affordable rental housing. This policy is ideal for examining the effects of various housing channels on human capital formation.

This paper analyzes the effects of LIHTC on students' short- and medium-run academic outcomes using administrative data from the San Diego Unified School District (SDUSD). The main hypothesis to be tested is whether moving into housing units funded by LIHTC has positive schooling outcomes for these students. Specifically, whether attendance, standardized English and math test scores, high school completion, and post-secondary education enrollment and completion improved for children who moved into LIHTC developments relative to the counterfactual. I further explore the mechanisms that drive any gains. By leveraging duration in residences, school changes, and characteristics of former and new residences, I attempt to understand which housing channel – housing affordability, residential stability, school mobility, and neighborhood location – is most critical to improving children's academic performance. While identifying causal effects, I also provide descriptive statistics on LIHTC tenants in SDUSD, as there is little information documenting the composition of tenants.

The Low-Income Housing Tax Credit is the largest place-based housing subsidy in the United States and is a crucial tool for producing and preserving affordable housing. Housing developers are encouraged to construct rental developments for low-income households in exchange for tax credits. Developers go through a yearly bidding process through the state's housing finance agency. The competitiveness of the process guarantees a certain quality of these developments. Furthermore, developments are inspected every three years after they have been placed in service to ensure that quality is maintained over time. Household income must be below a certain threshold to qualify for a unit in LIHTC developments. However, once moved in, tenants can remain in the units even when household income surpasses the income threshold. This statute allows residential stability, permitting the tenants to stay in the same residence and neighborhood longer.

The affordability, quality, and stability of LIHTC developments would likely improve children's academic performance. These developments also allow parents to access alternative school options. On the other hand, LIHTC developments are likely to be located in lower-income neighborhoods. There could also be disruption effects from housing mobility and school mobility. It is an empirical question to identify how residing in LIHTC impacts school-aged children's educational outcomes. To the best of my knowledge, this is the first paper to explore the short-run student-level effects of LIHTC on academic performance.

I use San Diego Unified School District administrative data from the 2001-2002 to 2012-2013 academic years. SDUSD is the largest school district in San Diego County and the second largest in California. With its diverse ethnic and socioeconomic backgrounds, SDUSD is an excellent setting to conduct education research. The annual California Standards Test provides consistent standardized test scores in English-language arts and mathematics for each student. I normalize and geocode students' residential addresses and link them to the California LIHTC database to identify the treated students in the study period.

Next, I implement a propensity score matching procedure to identify comparable control groups. Specifically, I look at two sets of alternative control groups. The first control group includes the five-most similar never-treated students in the year prior to the move for each treated student. The assumption is that the student who moves into LIHTC would otherwise have made the same transitions on average as the control students. That is, they would not necessarily have moved if the LIHTC unit were not available. In other words, this control group contains the *non-movers* and *movers* who have never resided in LIHTC during the study period. Under this comparison, the impacts capture differential disruption effects. The second set of control groups includes the five-most similar never-treated students for each treated student in the year prior to the move who make the same type of transition, i.e. *change residences and remain at the same school* and *change residences and schools*. That is, this set of control groups only contains the *movers*. In these cases, I assume that the treated students would have made the same type of transition they indeed make, and the only difference is the type of housing they end up in.

For short-run outcomes where I observe both pre- and post-periods, I implement a difference-in-differences (DID) approach to remove time-invariant fixed effects. Comparing students who moved into LIHTC to the control group with both *non-movers* and *movers*, I find a 0.28 percentage point decrease in absenteeism rate for students in grade levels 2 to 11, a 0.049 standard deviation increase in English scores for students in grade levels 2 to 11, and a 0.048 standard deviation increase in math scores for students in grade levels 2 to 7 one year after moving into a LIHTC unit. Comparing students who moved into LIHTC but attending the same school to the *movers* in the control group who are also attending the same school, I do not find any statistically significant changes in absenteeism rate, but I observe a 0.094 standard deviation increase in English scores and a 0.096 standard deviation increase in math scores. Since the students in this set of comparisons did not change schools, the only differential effects come from the difference in housing. The results indicate that, on average, students who moved into LIHTC developments perform better than those who moved into other housing types. Nonetheless, comparing students who moved into LIHTC and changed schools to the *movers* in the control group who also changed schools, I find a 0.61 percentage point decrease in absenteeism rate, a 0.049 standard deviation decrease in English scores, and a 0.123 standard deviation decrease in math scores.

For medium-run outcomes, I implement a logistic regression model to estimate the difference between the treatment and control groups after matching. For students who moved into a LIHTC development, I find that the high school completion rate is 0.8 percentage points higher, the college enrollment rate is 0.6 percentage points higher, and the college completion rate is 0.5 percentage points higher. Overall, residing in LIHTC demonstrates positive impacts on students.

This study primarily contributes to two existing bodies of literature: (1) research related to the Low-Income Housing Tax Credit and (2) research on housing and human capital formation.

The merit of place-based subsidized housing is frequently debated amongst policymakers and homeowners. The "not in my backyard" mentality hinders new constructions of apartment homes, yet the shortage of housing further burdens existing societal issues. Quantifying the impacts of subsidized housing in a neighborhood is an ongoing research area that demands more empirical evidence. Schwartz et al. (2006) find significant and long-term external benefits associated with subsidized rental properties in New York City, such as increased safety and demand for retail services. Positive spillovers to nearby neighborhoods increase with project size and decrease with distance to the project site. The authors attribute some of these benefits to the replacement of existing disamenities. Similarly, Baum-Snow and Marion (2009) find that new LIHTC properties lead to higher housing values in low-income neighborhoods but have little to no effects in gentrifying areas. Diamond and McQuade (2019) use a structural model to estimate the costs and benefits of LIHTC properties. The authors find a 6.5 percent increase in housing prices and a decrease in crime rates in low-income neighborhoods but a 2.5 percent decrease in housing prices in higher-income neighborhoods. According to the authors' estimates, annual aggregate welfare benefits from LIHTC developments amount to around $116 million. This study broadly makes an empirical contribution to the Low Income Housing Tax Credit literature.

This study also expands on the research on housing and human capital formation. Most existing studies focus on the lack of housing and its adverse effects on children's educational attainment (Dworsky, 2008; Zhang et al., 2013; Oakley and Burchfield, 2009; Cutuli et al., 2013). The causal impact of housing on schooling outcomes is difficult to estimate due to limitations in methodology and data. To fill the gap in the literature, Cunningham and MacDonald (2012) propose a conceptual framework using housing as a platform to improve educational outcomes for children. The authors suggest unbundling housing into four dimensions: housing quality, residential stability, affordability, and neighborhood location. I utilize this framework to understand which aspects of LIHTC developments are essential for students' academic success.

A distinctive feature of LIHTC is that it grants additional tax credits for developments in higher-poverty areas. This diverges from the ongoing research on the Moving to Opportunity (MTO) project. Administered by the Department of Housing and Urban Development (HUD), MTO is designed to help low-income families move from high-poverty neighborhoods to low-poverty areas with better schools, jobs, and transportation. Via a lottery system, families selected were given vouchers that they could use to rent housing in more affluent areas.[1]

Drawing comparisons with education research on the Moving to Opportunity project, MTO studies do not find consistent and significant short-run schooling effects for students who moved into low-poverty neighborhoods. Sanbonmatsu et al. (2006) do not find any evidence of improvements in reading, math, and behavior scores for MTO children of any age group. Ludwig et al. (2001) find improvements in reading and math test scores for young children aged 5 to 12 when families move from high- to low-poverty neighborhoods, but adverse effects for older children due to disruption. However, I find positive short-run effects on schooling when students move into high-quality housing in high-poverty neighborhoods. Nonetheless, positive medium-run effects exist for the MTO project and LIHTC policy. Chetty et al. (2016) find that children who moved before they reached adolescence (age 13) had higher college attendance rates and incomes later in life. Similarly, I find increased odds of graduating from high school and attending and completing college in my setting.

This study is closely related to two papers. Di and Murdoch (2013) investigate the effect of LIHTC developments on nearby school performance. Using elementary school data in Texas from the 2003-2004 through 2008-2009 academic years, the authors conclude that school ratings and standardized test passing rates do not change when a new LIHTC development is placed in the neighborhood. The unit of analysis in this study is school, so the authors cannot observe student-level performance directly. As a result, it is not possible to differentiate academic performance between new students from LIHTC households and existing students at the receiving schools. The second paper is Derby (2021). The author examines post-secondary education enrollment

[1] See Sanbonmatsu et al. (2011) for more details on Moving to Opportunity.

and earnings of individuals who grew up in LIHTC units. Using administrative tax records, the author finds a 4.3 percent increase in the likelihood of attending a four-year college with each additional year spent in LIHTC housing. The author also finds a 5.7 percent increase in future earnings for every additional year spent in LIHTC. I build on the findings in these two papers by leveraging student-level data where I observe residential addresses and annual standardized test scores over time. My data allows me to identify short- and medium-run schooling effects of residing in LIHTC dynamically, which adds to the literature on access to place-based housing and academic performance.

The paper is organized as follows. Section 1.2 contextualizes the institutional background of Low Income Housing Tax Credit and San Diego Unified School District. Section 1.3 describes the data. Section 1.4 describes the empirical strategy. Section 1.5 presents the results. Section 1.6 concludes.

1.2 Low Income Housing Tax Credit

In this section, I highlight the characteristics of the Low-Income Housing Tax Credit policy.[2] LIHTC was created by the Tax Reform Act of 1986 under the Reagan administration and is managed by the Internal Revenue Service (IRS).[3] The main objective is to encourage private housing developers to supply affordable rental housing by providing subsidies in the form of tax credits. Housing units developed under the LIHTC accounted for nearly one-third of all multifamily rental housing constructed between 1987 and 2006 (Khadduri et al., 2012).

Each year, the federal LIHTC allocation authority distributes credits to each state based on population with a small state minimum; see Figure 1.A.1 for historical LIHTC credit amounts

[2]For a comprehensive description of LIHTC, see Cummings and DiPasquale (1999), Khadduri et al. (2012), and Keightley (2021).

[3]The purpose of the Tax Reform Act of 1986 was to simplify the tax code, remove numerous loopholes and shelters that favored special interests, and lower marginal tax rates. In particular, LIHTC was established as a replacement for the existing tax code that encouraged the provision of low-income housing.

since policy enactment. Once the housing finance agency (HFA) in each state receives the credits from the federal authority, each HFA prepares and publishes a Qualified Allocation Plan (QAP) that complies with federal requirements. The QAP outlines each state's eligibility priorities and housing needs and provides a detailed scoring system.[4] Credits are awarded to developments with the highest scores until the annual funding is exhausted. The competitive process ensures the quality of housing provided to income-eligible tenants.

Tax credits are distributed to developers over a ten-year period once the development is placed in service. Each project is assigned a qualifying basis, which is the fraction of the total cost (less land cost) of the housing project reserved for low-income tenants. Over seventy percent of developers reserve 100 percent of rental units to qualifying tenants to obtain the maximum qualifying basis. The annual credit claimed by a developer is equal to the project's qualified basis multiplied by a credit percentage. Generally, new constructions receive nine percent of tax credits, and rehabilitated developments receive four percent of tax credits.[5] For example, if the qualifying basis is $1 million for a new construction receiving a 9 percent credit, the developer will receive $90,000 a year in tax credits for the next ten years once the property is placed in service.

There would still be affordable housing without LIHTC. Eriksen and Rosenthal (2010) find substantial crowding out effects for LIHTC developments. The authors estimate that nearly one hundred percent of the developments are offset by reductions in newly built unsubsidized rental units. However, the authors find that the units are estimated to be twenty percent more expensive per square foot than the industry average, which they attribute to higher quality and

[4]The scoring system varies from state to state. Some examples of positive points include access to public transportation or amenities upgrades such as a community room, an in-unit washer and dryer, and a playground. Properties targeting special needs, including the elderly, disabled, and homeless populations, also receive additional points. On the other hand, points are deducted for negative neighborhood services, such as within three blocks of an airport, prison, or railroad. For previously adopted QAPs in California, see www.treasurer.ca.gov/ctcac/programreg/pastregs.asp.

[5]In the statute, the credit percentages are specified as 70 percent net present value (NPV) of the qualified basis for the 9 percent credit and 30 percent NPV for the 4 percent credit. Initially, the Internal Revenue Service periodically reset the specified credit percentages to maintain the present value of the 10-year stream of tax credits at 70 percent or 30 percent of the qualified basis. However, since 2008, Congress has specified the minimum credit rate for the 70% (30%) present value credit to be at least 9% (4%), regardless of interest rates.

less efficient production. LIHTC could provide assurance in both affordability and quality, which are generally substitutes in the rental market.

An additional thirty percent of credits are awarded to developments in concentrated poverty or areas with higher development costs. These areas are defined as Qualified Census Tracts (QCTs) and Difficult Development Areas (DDAs), respectively. QCTs are census tracts where at least fifty percent of households have income below sixty percent of the Area Median Gross Income (AMGI) or have a poverty rate of 25 percent or more, whereas DDAs are areas with high land, high construction, and utility costs relative to the AMGI. Baum-Snow and Marion (2009) find QCTs receive around six more low-income housing units on a base of seven units per tract. This key policy feature enables me to study the impact of children living in high-quality housing in low-income neighborhoods on schooling outcomes.

Developments approved after 1990 need to be in compliance for thirty years. During the compliance period, LIHTC developments must satisfy two rules; otherwise, the tax credits are recaptured by the IRS. The first rule is the income test, which ensures the rental units are targeted towards income-eligible tenants.[6] The second rule is the gross rent test to guarantee the affordability of the rental units. The gross rent test requires rents not to exceed thirty percent of either fifty or sixty percent of AMGI, depending on the share or units reserved for low-income households. Unlike most housing policies where the monthly rent depends on household income, LIHTC developments charge a fixed rent for all tenants in reserved low-income units. Nonetheless, Section 8 Housing Choice Vouchers can be used to help cover the cost of rent in LIHTC properties. Every three years, the state's housing finance agency staff will conduct a site visit to examine tenant files and rent rolls to ensure compliance. During the site visits, the physical conditions of the development and promised amenities are also inspected. The meticulous inspection process

[6]There are three ways to meet the income test. The first is the 20-50 test, where at least twenty percent of the project's units are occupied by tenants with an income of fifty percent or less of AMGI adjusted for family size. The second is the 40-60 test, where at least forty percent of the project's units are occupied by tenants with an income of sixty percent or less of AMGI adjusted for family size. The third test, initiated in 2018, requires at least forty percent of units to be occupied by tenants with income averaging no more than sixty percent of AMGI, and no units are occupied by tenants with income greater than eighty percent of AMGI.

further assures the quality of LIHTC developments.

LIHTC is often compared to Public Housing, a government-owned affordable housing program for income-eligible households. Established by the United States Housing Act of 1937, the goal of the program is to provide low-income families with safe and sanitary housing. As of 2022, there are a total of 1,850 local Public Housing Agencies (PHAs) administering more than one million units across the country. The main difference between LIHTC developments and public housing is that the former is owned and managed by private developers, whereas the latter is overseen by government agencies. Another key difference is that LIHTC tenants pay a fixed rent (thirty percent of either fifty or sixty percent of AMGI), and public housing tenants generally pay thirty percent of their adjusted gross income, which typically results in a lower rent for public housing. Although it will be a valuable exercise to analyze education outcomes for students residing in LIHTC versus public housing, there do not exist public housing developments in my sample and is left for future work.[7]

Unlike most housing policies overseen by the Department of Housing and Urban Development, LIHTC is administered by the Internal Revenue Service. Data on tenants and rents in LIHTC properties were not collected until 2008 under the Housing and Economic Recovery Act (HERA), and little is known about LIHTC participants. In Section 1.3.2, I document descriptive statistics of families residing in LIHTC developments in my sample.

1.3 Data

This study mainly draws information from two data sources: administrative data from the San Diego Unified School District and the Low-Income Housing Tax Credit data from the California Tax Credit Allocation Committee. I also utilize census data to obtain neighborhood

[7]There are 121 public housing units in San Diego County, including four different sites in Chula Vista and 38 farm worker housing units in San Marcos. My study area, San Diego Unified School District, is located in the City of San Diego and does not contain any public housing developments in the study period.

characteristics. In this section, I describe the datasets and key variables used to estimate the effects of LIHTC on academic performance in the San Diego Unified School District.

1.3.1 SDUSD Administrative Data

I use student-level administrative data from the San Diego Unified School District for 2001-2002 to 2012-2013 academic years. SDUSD is the second largest school district in California, and one of the top large urban school districts in the US. The school district covers 51 zip codes and 3,405 census block groups (see Figure 1.A.2), and consists of 226 educational facilities with student enrollment close to 120,000.

For each student, I obtain information on individual background characteristics, including birth date, gender, ethnicity, home language spoken, and parental education. I also obtain yearly information, including school, grade, registered residential address, resident status, English learner status, and Special Education status. Table 1.3 includes a student-year level full sample summary statistics. Diverse ethnic and socioeconomic backgrounds are observed among the students. From Panel B in the table, 45.0 % are Hispanic, 24.6 % are White, 15.4 % are Asian, and 13.5 % are African American. Furthermore, 14.3 % of parents did not receive a high school degree, 18.8 % of parents received a high school degree, and 31.5 % of parents have at least a college degree.[8] I include data from American Community Surveys (ACS) to obtain census block poverty rates based on residential addresses.

Due to School Choice and Open Enrollment programs in the district, only 61.9 % of students are *residents*, i.e. attending the neighborhood school. To control for the differences between residents and non-residents, I calculate the distance from the residential address to their school for each student-year and include the variable as a covariate in my analysis. See Figure 1.A.3 for the density plot of distance to school for residents and non-residents.

[8] It is essential to note that parental education level could be missing or inaccurate prior to 2010 as the data was directly provided by students. I supplement previous years' data from 2010 onwards if the data exists.

One caveat of the data is that registered residential addresses were only collected once a year on census day, usually in the first week of October. Therefore, I do not observe residential movements within a given year. Furthermore, there might be misreporting of residential addresses either unintentionally or intentionally. Unintentional misreporting includes data entry errors and missing data, whereas intentional misreporting includes using relatives' addresses to gain access to schools in the district. Although I cannot detect and mitigate unintentional misreporting, I can examine whether intentional misreporting exists. I link each registered residential address to a census tract and compare the number of students in each census tract based on the district data to the number based on official 2010 census data (Figure 1.1). In the figure, each dot represents a census tract, and the line represents the 45-degree line. There exists some noise in the figure, but overall there is no obvious misreporting of addresses in the sample.

The primary short-run outcomes are standardized test scores in English-language arts and mathematics on the California Standards Test (CST).[9] Test scores are available for public school students from 2001-2002 to 2012-2013 academic years, which constitutes my main sample. English test was conducted each spring for students in grade levels two to eleven. However, consistent math test scores are only available from grades two to seven as students enter different mathematics tracks after grade seven. I exclude grades eight to eleven for math outcomes from the analysis. The raw test scores ranged from 150 to 600 points. I standardize raw scores by academic year and grade level across all observations in the school district to obtain the standardized test score with mean 0 and standard deviation 1 (see Table 1.3 Panel E).

In addition to standardized test scores, I examine students' absenteeism rates. The absenteeism rate is defined as the number of absent days divided by the number of available school days in an academic year. I provide the descriptive statistics of absenteeism by grade level

[9] California Standards Test (CST) is a component of the Standardized Testing and Reporting (STAR) program administered by the California Department of Education from 1998 through 2013. Each spring, public school students in grade levels two through eleven were required to take standardized tests that assessed the California content standards. The objective of the STAR program was to help schools improve the academic achievement of all students. STAR was superseded by the California Assessment of Student Performance and Progress (CAASPP) System in October 2013.

in Table 1.4. The overall mean absenteeism rate is 4.84 percent. Using the year-round school calendars of 180 days of instruction in California, students miss 8.71 school days on average. I also report the chronically absent rate, which is an important tracking metric for school districts. Students are classified as chronically absent if their absenteeism rate is 10 percent or more. In the SDUSD data, roughly 12 percent of students are chronically absent.

Literature has consistently shown positive correlations between school attendance and academic performance (Chen and Lin, 2008; Arulampalam et al., 2012; Andrietti, 2014; Ansari and Pianta, 2019). Undoubtedly, confounding factors such as health problems could drive the correlations. Hsu et al. (2016) and Allison et al. (2019) deduce asthma as a leading cause of chronic school absenteeism. Although I do not have health data in my sample, I assume there do not exist systematic differences in attendance patterns of LIHTC students and non-LIHTC students, at least in the pre-treatment periods. I outline two potential channels of why moving into LIHTC could affect school absenteeism. First, the distance to school might change after the move. Figure 1.A.4 shows the density plots of distance to school by LIHTC and non-LIHTC addresses. The median distance to school is around one mile for both sets of addresses, but LIHTC addresses, on average, have a greater distance to school (3 miles) compared to non-LIHTC addresses (2.5 miles). Although LIHTC addresses are further away from school, public transportation or school bus access might be superior. Transit amenities receive high points in California's QAP, so LIHTC developments generally have easy access to public transportation.[10] Second, LIHTC developments might provide better housing conditions hence leading to better health (Krieger and Higgins, 2002; Bonnefoy, 2007). Since the developments are inspected every three years by the state's housing finance agency staff, overall standards of the physical conditions of the dwellings are expected. Nonetheless, there is currently no empirical evidence linking LIHTC developments to better health, and this is an important question for future work.

[10]Based on California QAP, a project receives a maximum point if the development site is located within 1/3 miles of a bus rapid transit station, light rail station, commuter rail station, ferry terminal, bus station, or public bus stop with service at least every thirty minutes during peak hours. See https://www.treasurer.ca.gov/ctcac/programreg/2022/20220720/2022-Regulations.pdf for more details on transit requirements for LIHTC developments.

For medium-run outcomes, I focus on high school completion, college enrollment, and college completion.[11] A student is considered a high school graduate if the student has completed one of the following: received a traditional diploma, received a Joint Adult Education High School Diploma (JDP), or passed the California High School Proficiency Exam (CHSPE). In my sample, 88 percent of students graduated from high school.[12]

College enrollment and completion data are obtained from the National Student Clearinghouse database. I evaluate two types of post-secondary institutions: 2-year colleges and 4-year colleges. A 2-year college mainly serves students seeking a technical certificate, a two-year associate's degree, or credits to transfer to a 4-year college, while a 4-year college offers students bachelor's degrees upon completion. Attending 2-year and 4-year colleges are not mutually exclusive as students could attend a 2-year college to obtain credits and transfer to a 4-year college. Therefore, I also include 2- or 4-year college as a category. For each category (2-year, 4-year, 2- or 4-year), I take the probability of enrolling and graduating as outcome variables.

Only a subsample of the data is used for the medium-run analysis. When a student completes high school, a degree type (traditional diploma, JDP, or CHSPE) and degree date are recorded in the data. If a student officially drops out of school, the drop year is recorded in the data. However, this only constitutes 44 percent of the sample. Of the remaining 56 percent of the sample, I exclude two types of students: (1) students who were not old enough to graduate, and (2) students whose medium-run outcomes are not observed in the sample. For the first type, there are no systematic biases as all students in these cohorts are excluded. For the second type of students, there might exist attrition bias. There are two circumstances why medium-run outcomes are missing for a student. First, if the student is no longer attending a public or charter school in

[11]Note that "medium-run" is a blanket term to describe high school completion and post-secondary education outcomes. High school completion is still considered a medium-run outcome if a treated student moves into a LIHTC unit during high school. Conversely, short-run outcomes describe year-to-year changes in standardized test scores.

[12]The national average public high school graduation rate was 79 percent in the academic year 2010–2011, and the number has been rising in recent years. San Diego Unified School District has generally outperformed the national average. Statistics are retrieved from the National Center for Education Statistics on the Institute of Education Sciences (IES) website.

San Diego Unified School District, either moving to another school district or attending a private school, the medium-run outcomes were not backfilled. I assume these moves as natural attrition and exclude them from the medium-run analysis. Second, suppose students repeated grades 10, 11, or 12 for multiple years and were old enough to graduate. In that case, I assume the students informally dropped out and labeled them as *drop out* in the analysis.

1.3.2 LIHTC Data

I use the LIHTC database compiled by the California Tax Credit Allocation Committee, which contains information on approved LIHTC developments since the inception of the policy in California. The dataset contains 5,493 properties consisting of 437,621 housing units from 1987 to 2020. I restrict the LIHTC sample to the 217 properties in San Diego County and placed in service before 2013, which is the last year of the study period in this paper.

For each property, available data include the name of the property, credit type, allocated annual federal award, property street address, application cycle year, placed-in-service date, property status (i.e. active or inconclusive), number of units, number of low-income units, developer type, construction type, and targeted tenant type. Figure 1.2 shows the rent limit in LIHTC developments and median rent in San Diego County in my study period. It is clear that LIHTC guarantees lower rental prices than the market rate.

Table 1.1 include summary statistics of LIHTC developments in placed in service by 2013. Of the 217 developments in San Diego County, 73 are within the San Diego Unified School District boundary (Figure 1.A.5). See Figure 1.A.6 for the stocks of LIHTC in the SDUSD boundary since policy inception. On average, each LIHTC property has 103 units, of which 98 units are reserved for income-eligible households. In my analysis, I assume all addresses matched to the LIHTC property addresses are income-eligible households.

Figure 1.3 shows the cumulative distribution function (CDF) of number of developments by poverty rate in Panel (a) and CDF of number of units by poverty rate in Panel (b). Roughly five percent of developments are in low-poverty neighborhoods, 45 percent of developments are in medium-poverty neighborhoods, and fifty percent of developments are in high-poverty neighborhoods.[13] The figures verify that LIHTC developments are more likely to be constructed in areas of concentrated poverty.

Table 1.2 records the types of LIHTC units in San Diego County and in SDUSD. In San Diego County, more than 60 percent of developments receive four percent credits, and 65 percent of developments are new constructions. Furthermore, around 58 percent of developments target large families, which aligns with the objective of my research question. I assess heterogeneity based on LIHTC characteristics in my analysis.

1.3.3 Identifying Treated Students

I define treatment as living in any LIHTC development, either rehabilitated or newly constructed, after the development is placed in service. I link the LIHTC data to the SDUSD data to identify treated students. I geocode the addresses in the two data sets following Koumarelas et al. (2018), then cross-reference the students' registered residential addresses to the LIHTC database each year. A total of 11,496 student-year observations are identified as LIHTC addresses, corresponding to 4,546 unique students.

The "Treat" column in Table 1.5 shows key characteristics of students who moved into a LIHTC development. Roughly 63 percent of LIHTC residents in SDUSD are Hispanic, and 31 percent are African American. For parental education level, 22 percent completed less than high school, 30 percent completed high school, and 20 percent completed some college.

First, I provide insights on when the moves into LIHTC occur and whether there exists a pattern. Figure 1.4 provides a summary of residential and school mobility across the grade

[13]Based on Census definition, low-poverty neighborhoods are areas with 5 percent or less of residents in poverty, medium-poverty neighborhoods are those where 5.01 to 24.99 percent of residents in poverty, and high-poverty neighborhoods are those with 25 percent or more of resident in poverty.

level.[14] The blue bars (left) represent the percentage of all residential moves by grade level, and the green bars (right) represent the percentage of all LIHTC moves by grade level. Within each bar, the lighter shade represents the proportion of residential moves where students attended the same schools. The darker shade represents the proportion of moves where students attended new schools.[15] There is a higher proportion of residential and school changes occurring in grades 9-12 for LIHTC moves. This observation could suggest that parents are concerned with college placements for their children and choose to move to neighborhoods with better schooling options where the housing remains affordable.

Second, I compare the duration of stays in any residential addresses to LIHTC addresses. Figure 1.5 Panel (a) shows the histogram of the length of stay in a residential address for all students, and Panel (b) shows the histogram of the length of stay in LIHTC addresses for treated students.[16] Note that I only have data from 2001 to 2013. The full duration is not captured if a family moved into a housing unit before 2001 or stayed after 2013. In other words, the figures capture the lower bound of duration in the population. On average, families stayed in non-LIHTC addresses for 2.9 years but only stayed in LIHTC addresses for 1.78 years. Since families are not required to move out of LIHTC even if their household incomes surpass the income limit (see footnote 6), it is surprising that LIHTC does not provide more stability for families residing in the developments.

Third, I show the density plot of distance moved from the prior residence in Figure 1.6 by LIHTC status and whether the student changed schools. The median distance moved for non-LIHTC addresses are 1.3 miles, while the median distance moved for LIHTC addresses are 1.6 miles. Furthermore, households are willing to move further into LIHTC developments

[14]Figure 1.A.7 provides the same figure but excludes grade levels 6 and 9, which are the common years to attend a new school. Figure 1.A.8 shows the same data in raw counts.

[15]For example, of all the residential changes in the sample, 12 percent of which happened when students entered grade 2. Roughly one-fifth of the 12 percent switched schools when they moved, and the remaining four-fifths attended the same school. Similarly, 12 percent of all residential moves into LIHTC happened in grade 2, but more students switched schools in the same year.

[16]Figure 1.A.9 provides a breakdown of Figure 1.5 Panel (a) by race.

when the parents are sending children to new schools (orange versus red lines). Nonetheless, significant differences do not exist between households who moved into LIHTC but attended the same school and all other moves. Note that the data plotted only includes observations in the SDUSD boundary. Hence, the figures do not capture further moves outside the boundary (and enrolling in a new school district).

1.4 Empirical Strategy

The main challenge to estimating the causal effect of moving into LIHTC developments on academic outcomes is the possible endogeneity of the household's decision to move. It is not likely to be random which households decided to move into LIHTC. These households might exhibit characteristics that are systematically different from households that decided to stay put (the *non-movers*) or households that decided to move into other types of housing (the *movers*). Thus, leading to biased estimates of outcome variables. In order to control for selection bias, I implement a propensity score matching (PSM) method combined with a difference-in-differences (DID) model for the short-run analyses (Stuart et al., 2014).

The goal of the matching procedure is to find a control group as similar as possible to the treatment group in terms of key characteristics. The matching procedure is as follows. Let $LIHTC_i \in \{0,1\}$ be a treatment indicator equal to 1 if student i ever lived in LIHTC in the study period and 0 otherwise. Let $y_{i,t+k}(1)$ denote the outcome at time $t+k$ for a student i who moved into LIHTC at time t, and $y_{i,t+k}(0)$ as the outcome if the student did not move into LIHTC. As the potential outcomes framework stated, only one of the two states can be observed at a given time, i.e. either $y_{i,t+k}(1)$ or $y_{i,t+k}(0)$ is missing for each student i. However, under the conditional independence assumption (CIA), the average treatment effect on the treated (ATT) can be identified as $\mathbb{E}\left[y_{t+k}(1) - y_{t+k}(0) | LIHTC = 1\right] = \mathbb{E}\left[y_{t+k}(1) | LIHTC = 1\right] - \mathbb{E}\left[y_{t+k}(0) | LIHTC = 1\right]$. Clearly, the second term is not observed. I use a propensity score matching method to approximate the counterfactual event. The conditional independence assumption states that, after conditioning on a set of observable characteristics, treatment assignment is independent of potential outcomes, i.e. $(y(0), y(1)) \perp LIHTC|X$. In other words, treatment is as good as random conditional on a set of observed covariates X.

Denote a cohort by Ω_{gy}, which contains all students attending grade g in academic year y. Denote event-year t as the difference in years between an academic year y and the academic year a student first moved into a LIHTC development. First, for each treated student, I identify the cohort in event-year -1, i.e. the year before the student moved into a LIHTC unit. Second, I identify all the never-treated students within this cohort. Third, I estimate the probability of a student moving into a LIHTC unit (the propensity score) using a logistic regression model with a set of observable characteristics. Finally, I implement a one-to-five propensity score matching with replacement to establish a control group.

For the short run, I define two sets of alternative control groups. In the first control group, I include all the never-treated students who are most similar to the treated students. That is, students who did not move (the *non-movers*) and students who moved to non-LIHTC residence (the *movers*) in event-year 0. It is not possible to identify what the treated family would have done had they not moved into a LIHTC development, so I assume the baseline rate of moving is the same for the students who moved into LIHTC and those who did not. In other words, the control group picks up the overall effects of what the LIHTC students would have done had they not moved into LIHTC.

In the second set of control groups, I match the treated students to the never-treated students who made the same transition in event-year 0. This set of control groups only contains the *movers*. Additionally, I separate the treated and control students by schooling decisions – I match the students who moved into LIHTC and remained in the same school to students who moved into non-LIHTC residences and remained in the same school, and I match the students who moved into LIHTC and changed school to students who moved into non-LIHTC residences and changed school. I denote the first type as *NRSS* (new residence, same school) and the second type as *NRNS* (new residence, new school).

NRSS allows me to elicit the effect of housing mobility. Conditioning on similar socioeconomic backgrounds, it compares the academic performance of students who moved into LIHTC developments versus that of students who moved into other housing developments. In other words, since both the treated and control students attend the same school as before the residential move, the only difference between treatment and control is the type of housing they ended up moving into. Note that in addition to the transitions for the treatment group, the transitions for the control groups are now potentially endogenous (i.e. the family moved because income changed). However, I show that there is no Ashenfelter dip prior to the move for both the treatment and control groups. On the other hand, NRNS captures the compound effect of residential and school mobility. However, note that decisions on housing and schooling are often endogenous, and it is not possible to disentangle the two effects in the current setting.

I select variables that affect both schooling outcomes and parents' decision to move. It would be ideal if I could match on household income, as LIHTC units are reserved for households below a predefined income threshold. However, I do not have data on household income. Instead, I use the residential census block poverty rate, parental education level, and the interaction between the two variables as proxies to capture the socioeconomic status of each household.[17] Treatment and control groups are also matched along the following dimensions: gender, race, birth quarter, home language, special education status, English Learner status, and past school performance. To control for school characteristics, I match on free lunch percentage and average academic performance at the school level. Table 1.5 demonstrates the short-run balance table after the matching is performed for the first alternative control group. The matched variables are balanced across the treatment and control groups. I also conduct an F-test to test for joint orthogonality and do not find any joint significance (p-val = 0.999).

[17] See Figure 1.A.10 for correlations between education level and household income using 2010 Census data.

In the balance table, 32.1 percent of Black students and 62.7 percent of Hispanic students are observed in the treatment group, compared to 13.5 percent of Black students and 45 percent of Hispanic students in the full sample. In addition, a higher proportion of less than some college education is observed in parental education level. The table identifies the main components of targeted households, at least in the context of the San Diego Unified School District boundary.

I reperform the matching procedure for the medium run, only including samples with medium-run outcomes. Table 1.6 shows the medium-run balance table after the matching procedure. The matched variables are balanced across the treatment and control groups. The F-test does not demonstrate any joint significance (p-val = 0.782).

Once the matching procedure is completed, I examine the dynamic effects of LIHTC using a difference-in-differences approach for the short run. DID uses the difference between treatment and control group outcomes during the pre-treatment period to control for existing differences across groups in unobservables that affect outcomes. Once the pre-treatment differences across groups are accounted for, the post-treatment difference in group means is attributed to the treatment.

My main estimating equation takes the following general form:

$$y_{igt} = \alpha + \beta LIHTC_i + \gamma Post_t + \delta LIHTC_i \times Post_t + \lambda \mathbf{X}_{igt} + \varepsilon_{igt}, \tag{1.1}$$

where y_{igt} is the outcome variable for student i in grade g in academic year t, $LIHTC_i$ is an indicator equals to 1 if student i ever lived in LIHTC in the study period, $Post_t$ is an indicator equals to 1 if student i moved into LIHTC in t' and $t > t'$, X_{igt} is a vector of student control variables. The variable ε_{igt} is the error term, which I cluster by pre-treatment school and cohort across all specifications. The parameter of interest is δ, the effect of moving into LIHTC on the outcome variable y_{igt}. I use ordinary least squares regressions for short-run continuous outcome variables.

The panel data is not balanced. There are several reasons why data is missing. First, data is only observed if a student attended a school in the San Diego Unified School District during the study period. If a student attended school in a different school district or attended a private school in an academic year, the student's data is not available for that year. Second, comparable test scores data is only available from the 2001-2002 to 2012-2013 academic years. For instance, if a student attended third grade in the 2012-2013 academic year, I do not observe any data beyond third grade for this student. Third, if the student opted out of the CST in an academic year, I do not observe the test scores. In order to retain as many treated students as possible, I match on variables in event-year -1.

To ensure the parallel trends assumption holds, I utilize event study style analysis to detect the presence of pre-trends in the outcome variable y_{igt}. I estimate the following specification:

$$y_{igt} = \sum_{j} \alpha_j \cdot Event_{j=t} + \beta LIHTC_i + \sum_{j \neq -1} \delta_j \cdot Event_{j=t} \cdot LIHTC_i + \lambda \mathbf{X}_{igt} + \varepsilon_{igt}. \tag{1.2}$$

The right-hand side includes indicators for each event-year, an indicator for the treatment group, the interaction indicators between event-year and treatment group, and a vector of control variables. The interaction term omits the event-year just before implementation, which is denoted by $j \neq 1$. The DID coefficient δ_j can be interpreted as the extensive margin effect in event-year t relative to the pre-treatment year.

For the medium-run, I cannot use the difference-in-differences approach, as I do not have a pre-period. Instead, I estimate the following equation:

$$y_i = \beta LIHTC_i + \gamma \mathbf{X}_i + \varepsilon_i. \tag{1.3}$$

The parameter of interest is β, the effect of residing in LIHTC on the outcome variable y_i. Since all outcomes are binary variables, I use logistic regressions to perform the treatment-control comparison.

1.5 Results

1.5.1 Main Results

Table 1.7 presents the DID estimates of the short-run outcomes for the first alternative control group by estimating Equation 1.1, with clustered robust standard errors by school and cohort in parenthesis. In this set of results, the matched control group consists of students who never resided in LIHTC developments in the study period. The estimated coefficient in column (1) implies a 0.28 percentage point decrease in absenteeism rate one year after moving into LIHTC. Using a base of 180 instructional days per year, the treatment effect is equivalent to 0.5 days of additional schooling. The effect may not be significant enough to impact the overall absenteeism rate, but it could still be seen as a positive step in the right direction. For standardized test scores, there is a 0.049 standard deviation increase in English scores statistically significant at the 0.05 level (column (2)), and a 0.048 standard deviation increase in math scores statistically significant at the 0.10 level (column (3)).

Figures 1.7, 1.8, and 1.9 show DID estimates and 95 percent confidence intervals by estimating Equation 1.2. Each point estimate is the difference between the treatment group mean outcome and the matched control mean outcome at each pre- and post-event year, minus the mean difference at event-year -1. These figures have no observable pre-trends, which supports the parallel trends assumption.

Figure 1.7 demonstrates a decrease in absenteeism rate post-event years for students who moved into LIHTC compared to the control group. There is a general association between a reduction in absences and a statistically significant increase in academic achievements (Gottfried, 2009, 2011; Aucejo and Romano, 2016; Gershenson et al., 2017). Specifically, Aucejo and Romano (2016) show a reduction in absences would lead to a 5.5 percent increase in math and a 2.9 percent in reading. This aligns with my findings in standardized test scores. Figure 1.8 shows an immediate increase in standardized English test scores after moving into LIHTC, with a steady increase even two years after moving in. Figure 1.9 shows an upward trend in standardized math test scores over time, but the point estimates are not always statistically significant, likely due to lack of power.

Table 1.8 presents the DID estimates of the short-run outcomes for the NRSS control group. Although there is no statistically significant effect on absenteeism rate, English test scores increase by 0.094 standard deviations and are statistically significant at the 10 percent level, and math test scores increase by 0.096 standard deviations and are statistically significant at the 0.05 level. The results indicate that students who moved into LIHTC developments, on average, attain higher academic achievements than their comparable peers who moved into other types of housing. This could indicate that LIHTC developments provide better learning environments for school-aged children.

Figure 1.10 Panel (a) shows the event-year figure of English test scores, and Panel (b) shows the figure of math test scores. Neither figures demonstrate observable pre-trends in pre-treatment periods. Students who moved into LIHTC developments achieved higher test scores than those who moved into other housing types.

Table 1.9 shows the short-run results for the NRNS control group. Compared to non-treated students who moved into a new residence and changed schools, students who moved into LIHTC and changed schools have a 0.61 percentage point decrease in absenteeism rate, statistically significant at the five percent level. The decrease in absenteeism rate is equivalent to an additional day of schooling in an academic year. This could imply that the new coupled housing and schooling choices provide easier access to attend school. Despite a decreased absenteeism rate, I find adverse effects on standardized test scores. The effect for English test scores is -0.049 standard deviation and statistically insignificant, but the effect for math test scores is -0.123 standard deviation and statistically significant. Note that the pre-event mean for the NRNS control group is less than that of the NRSS control group, indicating that students who change residences and schools concurrently are the lower-performing students in class.

Figure 1.11 shows the event-year figures for the NRNS group. Similarly, there are no observable pre-trends. Although negative effects are observed for both English and math in event-year 0, upward trends can be observed in the next two years after moving in.

Table 1.10 shows medium-run results for estimating Equation 1.1 using a logistic model. The coefficients represent margins.[18] The effect on high school completion in column (1) is positive and statistically significant. The high school completion rate for students who have resided in LIHTC is 0.8 percentage points higher than those of comparable non-LIHTC students. Columns (2) to (4) show the results for 2-year, 4-year, and 2- or 4-year college enrollment, respectively. Residing in LIHTC increases the probability of attending college. Columns (5) to (7) show the results for college completion. The coefficients show positive effects of residing in LIHTC on college completion. The 2-year college completion rate is 0.2 percentage points higher for treated students, and the 4-year college completion rate is 0.3 percentage points higher. Overall, the policy demonstrates positive effects in both the short- and medium-run schooling outcomes.

1.5.2 Heterogeneity

I conduct exploratory analyses of treatment effects by gender, age moved, construction type, and neighborhood type transition.

Gender heterogeneity is often explored in the education literature, as girls and boys have different experiences in schools. Voyer and Voyer (2014) perform a meta-analysis and found existing female advantages in academic achievement. Similarly, Sanbonmatsu et al. (2006) show that the intent-to-treat of the combined reading and math scores for females was 0.051 standard deviation (with a standard error of 0.038), whereas the estimate for males was -0.008 standard

[18]Odds ratios are reported in Table 1.A.1. As stated in Aldrich et al. (1984) and Wooldridge (2010), a linear probability model (LPM) often performs a good job estimating a binary response model for partial effects of the explanatory variables. In addition to the logistic model, I also provide estimation using LPM in Table 1.A.2. The LPM results are comparable in magnitude to the logistic model estimates.

deviation (with a standard error of 0.042) for the MTO project. I test for gender heterogeneity to examine how moving into LIHTC affects males and females, respectively.

Table 1.11 shows the DID estimates of the short-run outcomes by gender. In columns (1) and (4), negative coefficients of absenteeism rate are observed for both females and males. The absenteeism rate decreases by 0.22 percentage points for females, although not statistically significant. The absenteeism rate decreases by 0.34 percentage points for males, with a statistical significance of 0.05. One possible explanation is the gender differences in independent mobility. Brown et al. (2008) provide empirical evidence that boys have a broader spatial terrain and fewer parental restrictions than girls of a similar age. Medeiros et al. (2021) show that if the distance from home to school is further, girls have less independent mobility compared to boys of a similar age.

The coefficients in columns (2) and (3) show the effects of moving into LIHTC on standardized test scores for females. There is a 0.074 standard deviation increase in English scores and a 0.044 standard deviation increase in math scores for girls. However, there are no positive and statistically significant effects for boys (columns (5) and (6)). These findings align with existing literature where female students demonstrate positive schooling effects from the treatment, whereas male students do not exhibit any treatment effects.

In addition to gender heterogeneity, I examine whether heterogeneity exists in the age when students first moved into a LIHTC unit. Moving causes disruption effects for all students. However, moving during adolescence could be especially challenging for students since it is a period of significant physical, emotional, and social development (Coleman, 1988; South et al., 2007; Chetty et al., 2016). I split the treated students into two age groups: children younger than adolescence (students who moved before 13 years old) and adolescent children (students who moved between 13 to 18 years old).

Table 1.12 reports the DID estimates for short-run outcomes by age group. There is no significant decrease in absenteeism rate for the younger children (column (1)), but standardized

English and math test scores increase by 0.072 and 0.049 standard deviations, respectively (columns (2) and (3)). On the other hand, the absenteeism rate for older children decreases by 0.45 percentage points, equivalent to 0.8 more school days in an academic year. However, there do not exist statistically significant effects on English test scores.[19]

Another crucial context of LIHTC developments is whether the development is newly constructed or rehabilitated. Generally, new constructions receive 9 percent credit, and rehabilitation projects receive 4 percent credit. On average, a new construction receives $961,244 federal credits annually, and a rehabilitated project receives $534,882 annually (Figure 1.A.11). Translating to per housing unit cost, a newly constructed unit receives $3,964 per year in tax credits, and a rehabilitated unit receives $1,042 per year on average. I test for heterogeneity by construction types to determine whether newly constructed units provide better schooling outcomes for children.

Table 1.13 presents the DID estimates of the short-run standardized test scores by construction types. There is a 0.99 percentage point decrease in absenteeism rate for students who moved into new constructions (column (1)), equivalent to 1.8 additional days of schooling. There are also positive and statistically significant effects on standardized test scores for students who moved into newly constructed developments (columns (2) and (3)). A 0.135 standard deviation increase in English scores and a 0.095 standard deviation increase in math scores are observed. Both are of higher magnitudes compared to the main estimation in Table 1.7. On the other hand, I do not find statistically significant results for rehabilitated developments. The estimates for standardized test scores in columns (5) and (6) are negative, although not statistically significant. Note that the pre-event mean for the control groups is comparable for new construction and rehabilitation units. However, the students residing in newly constructed units achieve higher education outcomes in the short run than those residing in rehabilitated units.

[19]Adolescent children are typically in middle school or high school, which are grade levels 6-8 and 9-12, respectively. Students split into different mathematics tracks after grade level seven, so I do not include math outcomes for adolescent children.

I provide event study figures by construction type to verify there do not exist pre-trends. Figure 1.12 shows the DID event study figures for new constructions, and Figure 1.13 shows the figures for rehabilitated developments. There are no observable pre-trends in these figures. In Figure 1.12 Panel (a), a significant increase in English scores can be observed even two years after moving into a LIHTC development. Figure 1.12 Panel (b) shows similar patterns for math test scores. Although the results are slightly nosier, an increase in math scores can also be observed two years after moving into a LIHTC development. Figure 1.13 shows that there do not exist significant effects on standardized test scores for children residing in rehabilitated LIHTC developments. Nonetheless, no statistically significant trends are observed.

I explore the underlying mechanisms to further understand the results in Table 1.13. The absenteeism rate for students who moved into new constructions decreases, whereas the absenteeism rate for students who moved into rehabilitated constructions does not decrease. One possible mechanism is that students can attend school easier when residing in newly constructed units. In Table 1.14 columns (1) and (5), I provide DID estimates of the distance to school in miles from the residential address. Although students in new constructions live further away from their schools than students in rehabilitated constructions, new constructions may have easier access to public transportation. Another possible mechanism is that new constructions provide higher-quality dwellings, which improves general health outcomes.

To understand why students only benefit from moving into new constructions and not rehabilitated units, I estimate Equation 1.1 with the following dependent variables: census block poverty rate, school-level standardized English test score, and school-level standardized math test score. The results are presented in Table 1.14. Compared to rehabilitated developments, new constructions tend to be in neighborhoods with lower poverty rates and better overall school performance. The findings allude to the effectiveness of rehabilitating outdated buildings. Whether it is worthwhile for housing agencies to fund lower-cost rehabilitation projects instead of funding higher-cost new constructions is a question that requires more data and structural modeling techniques. However, rehabilitated projects do not seem to have positive schooling effects on school-aged children in the short run.

Lastly, I test for heterogeneity in test scores by neighborhood types to determine what type of transition provides the best short-run impacts. Specifically, I study the transition from high-income neighborhood to high-income neighborhood (HI $\rightarrow$ HI), low-income neighborhood to high-income neighborhood (LI $\rightarrow$ HI), high-income neighborhood to low-income neighborhood (HI $\rightarrow$ LI), and low-income neighborhood to low-income neighborhood (LI $\rightarrow$ LI), where *neighborhood* is a census block and the cutoff value between the two neighborhood types is 25 percent poverty rate.

Table 1.15 shows the DID estimates of the short-run standardized test scores by the four neighborhood transition types. Interestingly, transitioning into high-income neighborhoods does not benefit children's short-run academic achievements. Columns (1), (2), (3), and (4) demonstrate negative effects for children remaining or moving into high-income neighborhoods, although not statistically significant likely due to the noise of the data. On the other hand, when children moved into LIHTC developments in low-income neighborhoods, their test scores seemed to improve compared to the control group.

The findings in this paper align with the finding in the MTO project. Chetty et al. (2016) find that moving to a lower-poverty neighborhood negatively affects older children's outcomes. These findings fit into the theory of relative deprivation (Wood, 1989; Marsh, 1987; Sanbonmatsu et al., 2006). First introduced in Stouffer et al. (1949), relative deprivation theory posits that people compare their circumstances to those of others and feel deprived when they believe their conditions are inferior. In the context of this paper, children in low-income families may fare better in low-income neighborhoods. In high-income neighborhoods, these children may face discrimination and experience resentment in schools. It is worthwhile for policymakers to evaluate these trade-offs when designing the optimal housing policy for low-income households.

1.6 Conclusion

Understanding how access to place-based housing subsidy policy affects human capital formation is a first-order empirical question for policymakers. There exist tradeoffs to residing in LIHTC developments – the housing developments are affordable and high-quality, but they are also likely to be in high-poverty neighborhoods. There is little existing evidence of how LIHTC could affect human capital formation. My study measures the educational outcomes of children residing in LIHTC developments using data obtained from administrative school records in San Diego. Specific outcome measures include student performance on standardized academic tests and school absenteeism for the short run, and high school completion, college attendance, and college completion for the medium run.

To examine the effects, I geocode and link data from San Diego Unified School District to the California LIHTC database. I first implement a propensity score matching method to obtain a comparable control group, then use a difference-in-differences model to estimate the impacts. Albeit a small decrease of 0.28 percentage points in absenteeism rate is estimated, I found an increase of 0.049 standard deviations in standardized English test scores and an increase of 0.048 standard deviations in standardized math test scores in the short run. The effects are more significant for newly constructed developments, where I estimate a 0.135 standard deviation increase in English scores and a 0.095 standard deviation increase in math scores. I provide event study style figures to show that there do not exist observable pre-trends in my specifications. For the medium-run, I find that students residing in LIHTC developments are 0.8 percentage points more likely to complete high school than its matched control group. Furthermore, students are 0.6 percentage points more likely to enroll in a 2- or 4-year college and 0.5 percentage points more likely to complete a post-secondary education.

Overall I find positive and statistically significant effects of LIHTC housing for both short- and medium-run schooling outcomes. In future work, cost-benefit analyses to compare funding newly constructed developments versus rehabilitated developments are essential to understand the optimal strategy to disseminate the tax credits. The marginal value of public funds (MVPF) framework introduced in Hendren and Sprung-Keyser (2020) can be used to estimate the welfare value of LIHTC. Additionally, more data on the characteristics of LIHTC units and tenants are needed to study the mechanisms of LIHTC. Future work could also explore and compare the differences between LIHTC developments and Public Housing.

1.7 Acknowledgements

Chapter 1, "The Impact of Place-Based Housing Access on Academic Performance," is currently being prepared for submission for publication of the material. The dissertation author was the sole author of the chapter. The researcher's own analyses were calculated based in part on data from San Diego Unified School District administrative database and the California Low-Income Housing Tax Credit database. The conclusions drawn from the San Diego Unified School District data are those of the researcher and do not reflect the views of the San Diego Unified School District. San Diego Unified School District is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

1.8 Figures and Tables

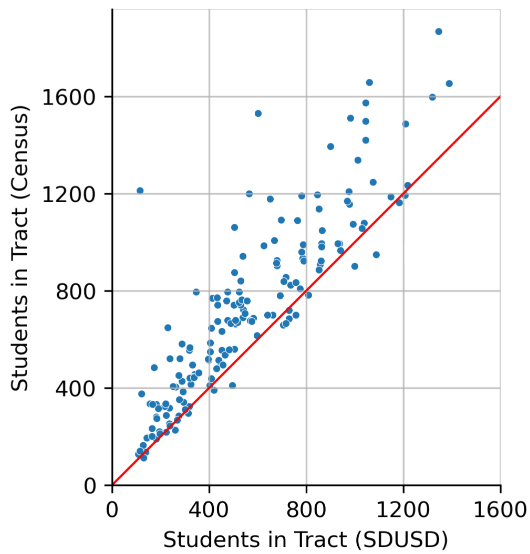


Figure 1.1. Scatter Plot of Students in the SDUSD Data vs the Census Data.

Notes: Figure shows the number of students in K-12 in each census tract based on the SDUSD data on the x-axis and the number of students aged 5-18 in each census tract based on the 2010 census data. Data shown only includes census tract with more than 200 students.

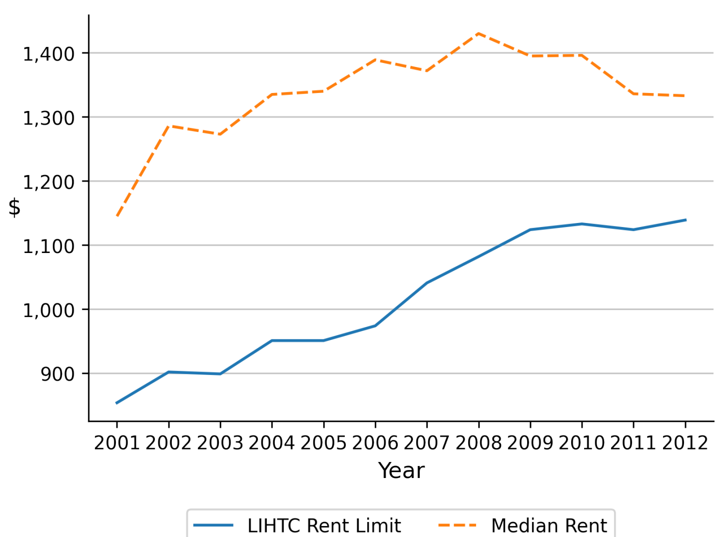
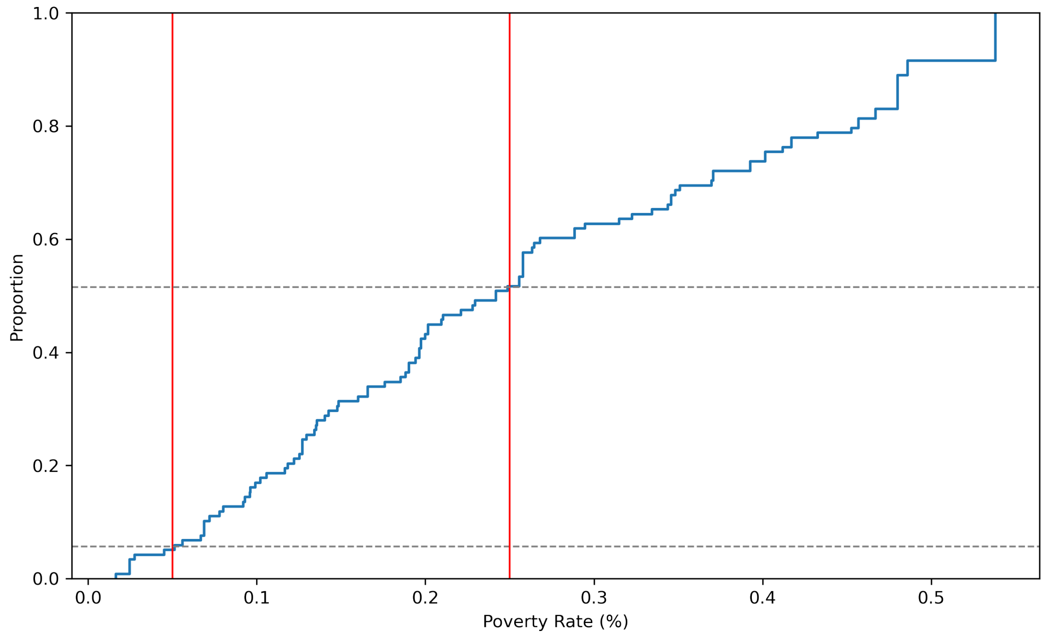


Figure 1.2. LIHTC Rent vs Median Rent.

Notes: Figure depicts the median rent and LIHTC income rent limits in San Diego County from 2001 to 2012.

(a) Developments.



(b) Units.

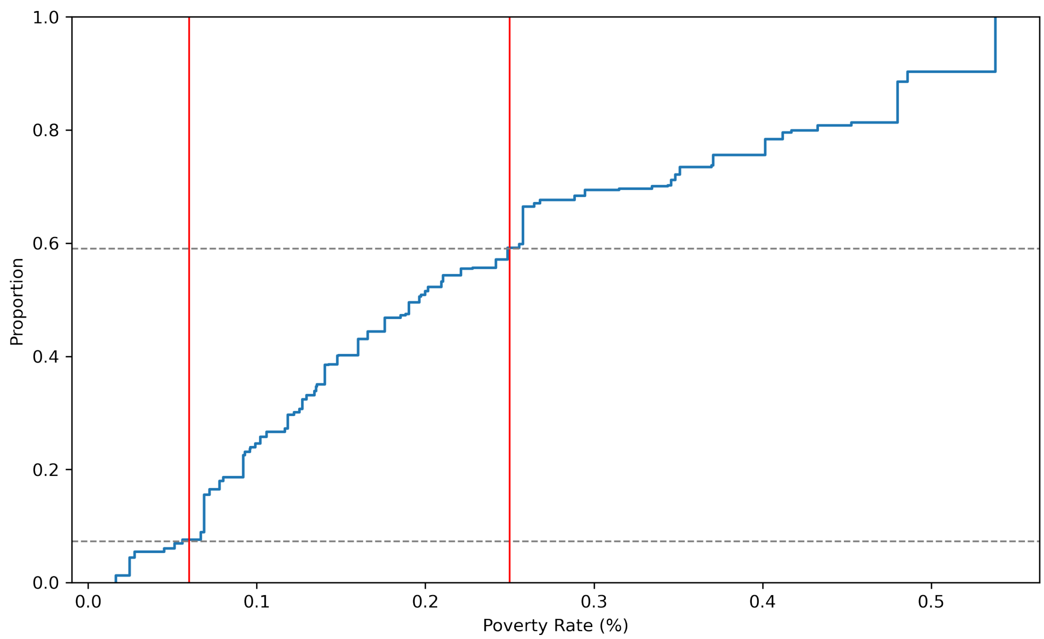


Figure 1.3. CDFs of LIHTC Developments and Units by Poverty Rate.

Notes: Figures depict the cumulative distribution functions of LIHTC developments (Panel (a)) and units (Panel (b)) by census block poverty rate. The vertical red lines depict 5 percent and 25 percent poverty rate respectively, which are the Census cutoffs for low-poverty, medium-poverty, and high-poverty neighborhoods.

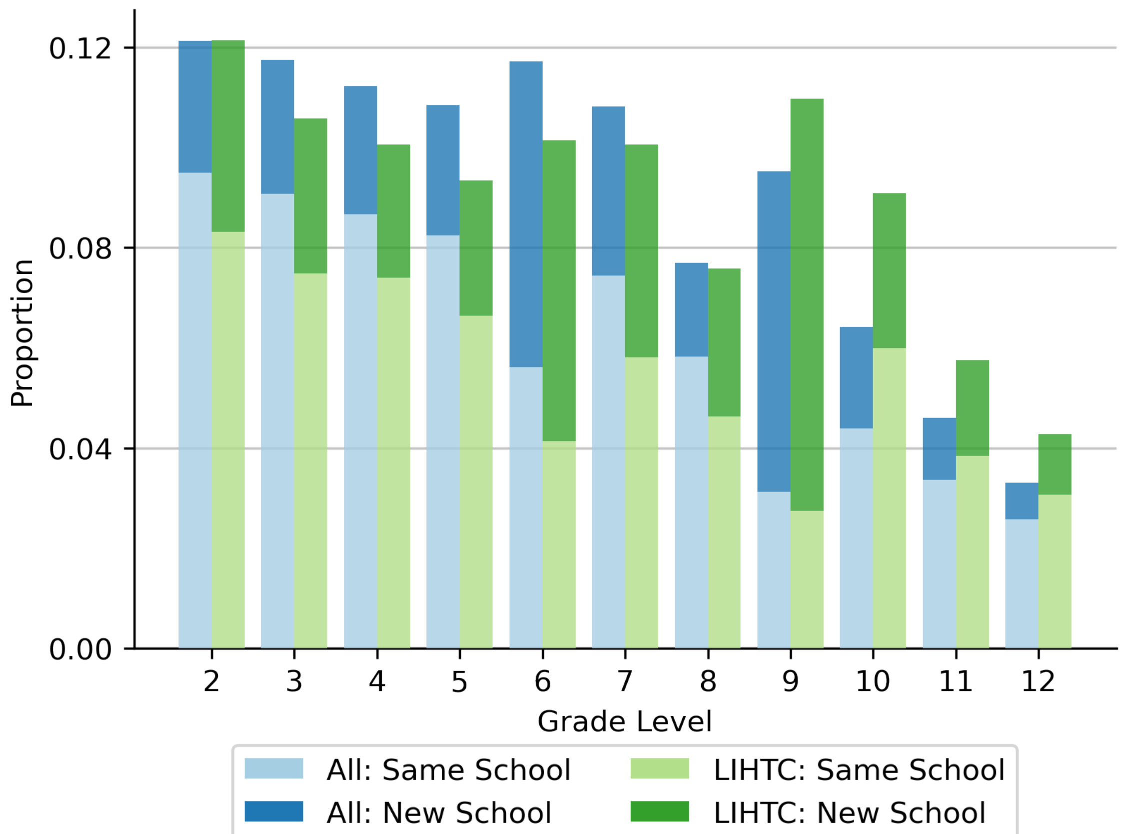
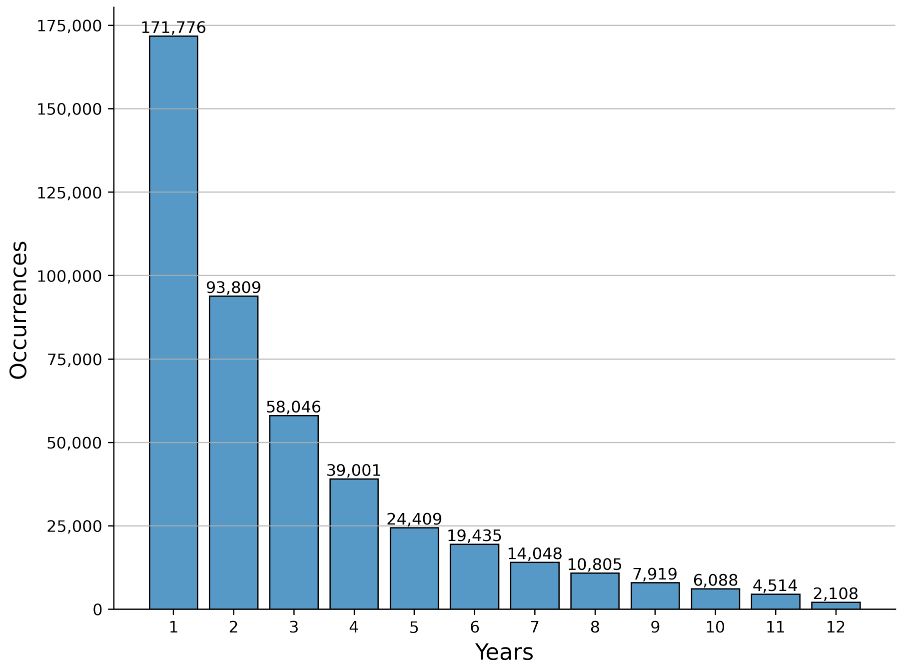


Figure 1.4. Fraction of Moves by Grade Level.

Notes: Figure depicts fractions of all moves by grade level. A "move" is counted when the residential address changes from one grade level to the next. The x-axis denotes the grade level, and the y-axis denotes the proportion. See Figure 1.A.8 for raw counts.

(a) All.



(b) LIHTC.

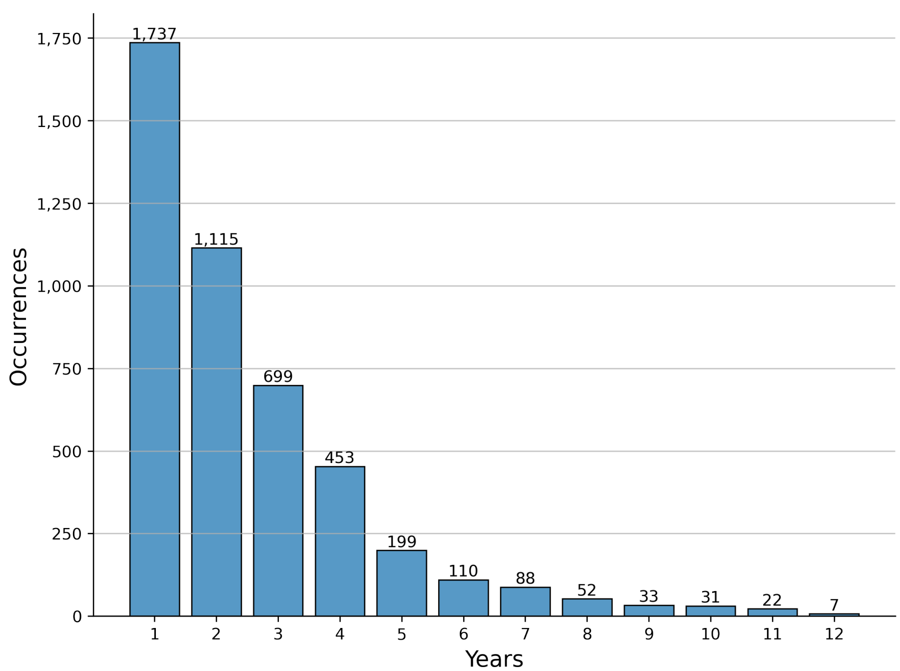


Figure 1.5. Years in Same Residence.

Notes: Figures depict the number of years in the same residence for all addresses (Panel (a)) and LIHTC addresses (Panel (b)). The x-axis denotes the number of years and the y-axis denotes the number of occurrences.

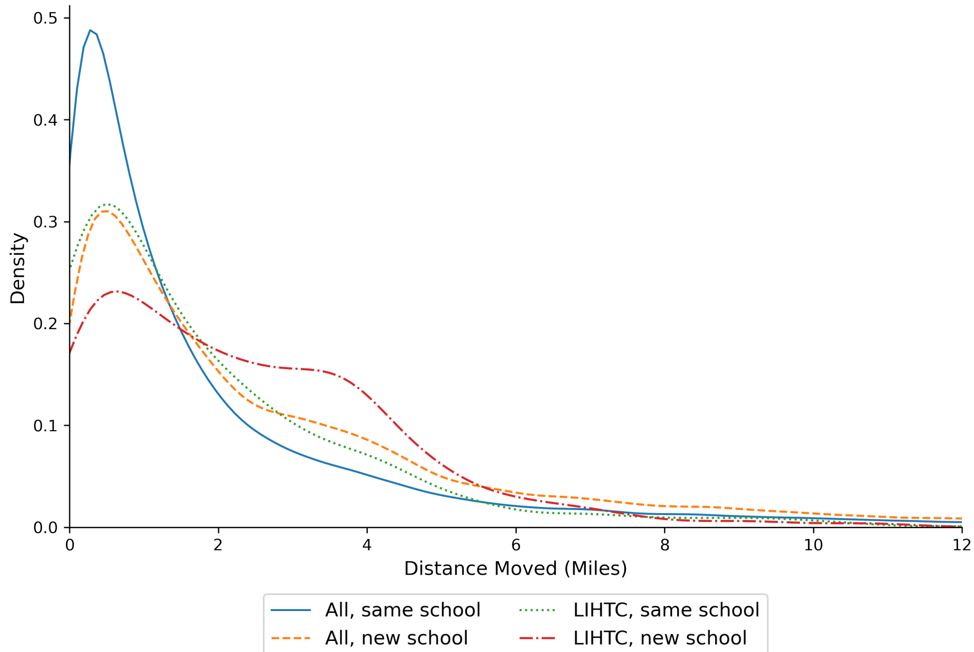


Figure 1.6. Distance Moved From Prior Residence by LIHTC and School-Change Status.

Notes: Figure depicts the density plots of distance moved from prior residence by whether the current residential address is a LIHTC unit and whether the students change schools.

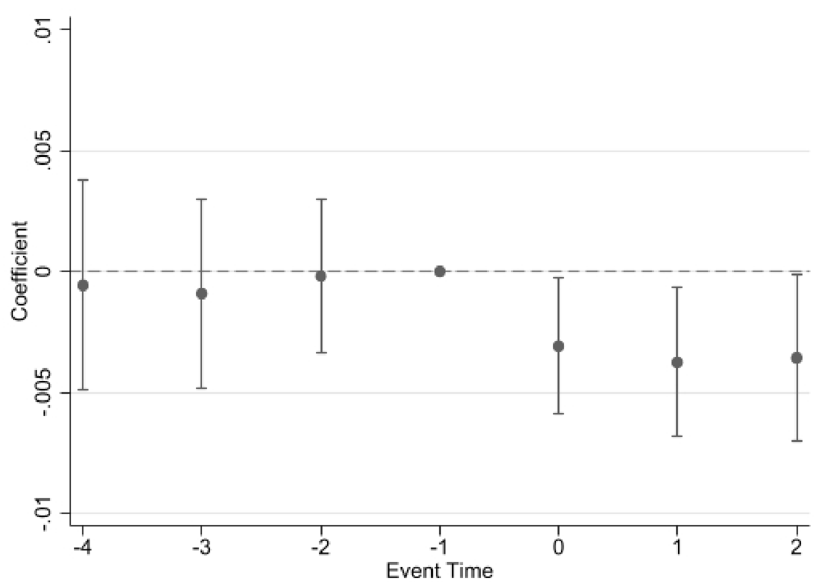


Figure 1.7. Absenteeism Rates by Event-Year.

Notes: Figure shows the DID event study for absenteeism rate by estimating Equation 1.2. Each dot is the difference between the treatment group mean outcome and the matched control mean outcome at each pre and post event-year, minus the mean difference at event-year $= -1$. The vertical bars provide the 95% confidence intervals based on robust standard errors clustered at the school and cohort level.

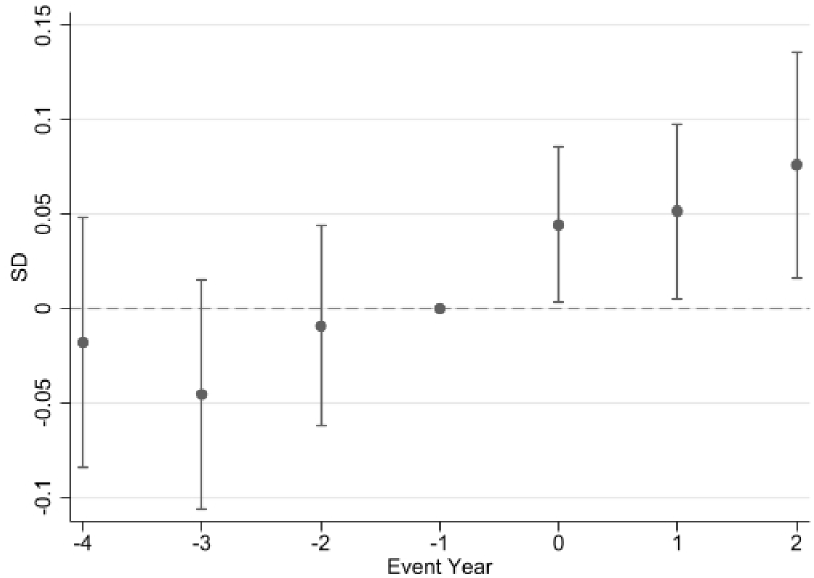


Figure 1.8. Standardized English Test Scores by Event-Year.

Notes: Figure shows the DID event study for CST English scores by estimating Equation 1.2. See notes to Figure 1.7.

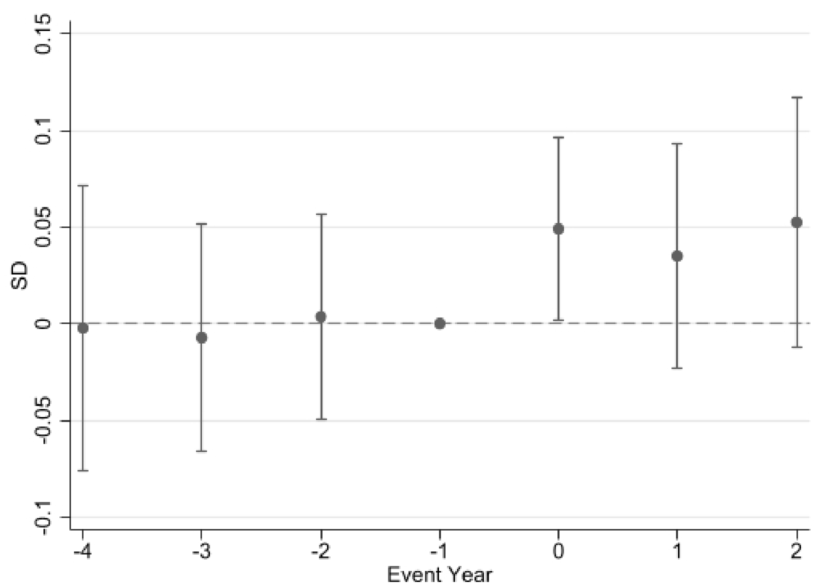
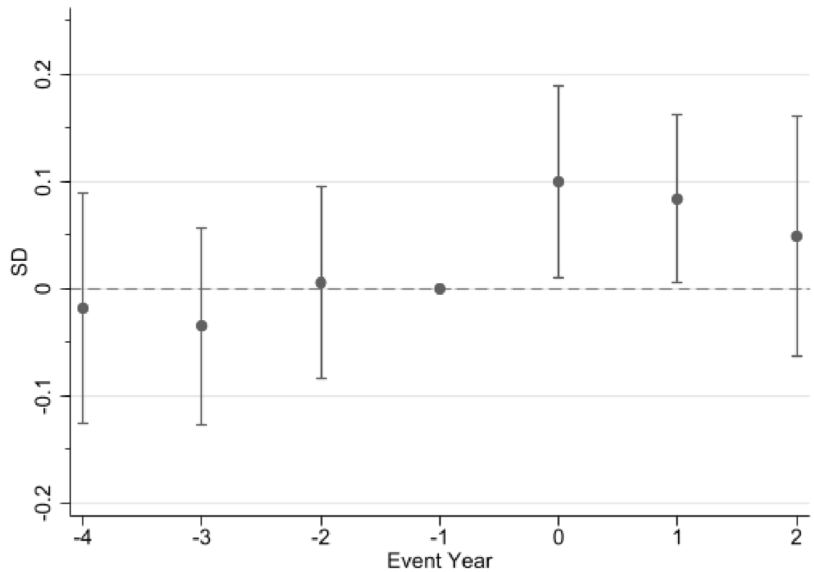


Figure 1.9. Standardized Math Test Scores by Event-Year.

Notes: Figure shows the DID event study for CST math scores by estimating Equation 1.2. See notes to Figure 1.7.

(a) English.



(b) Math.

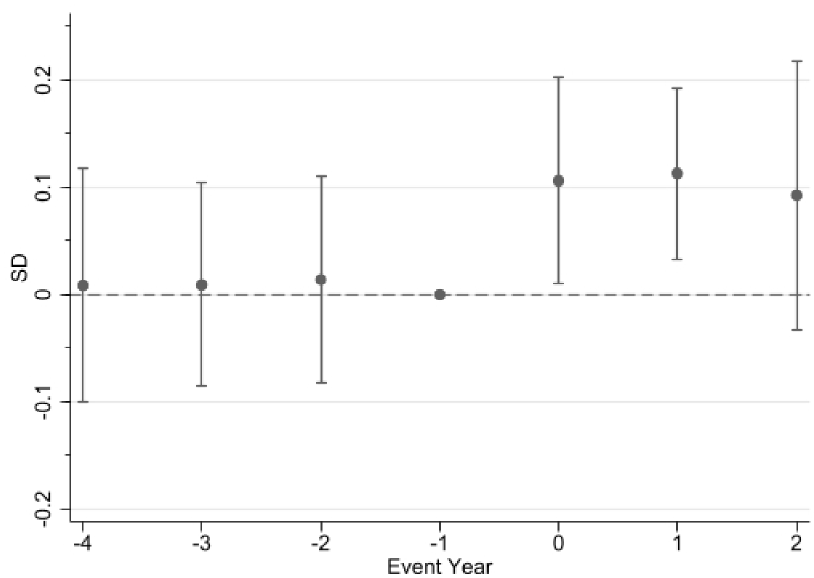
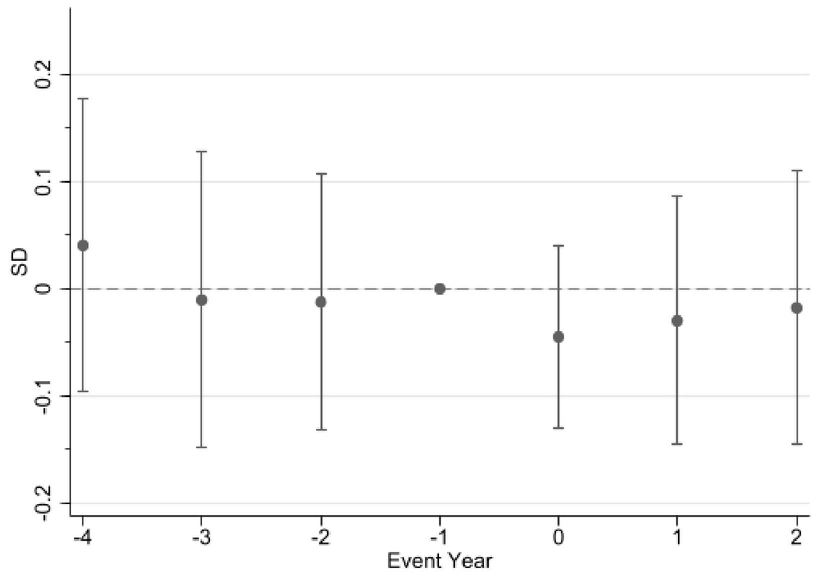


Figure 1.10. Standardized Test Scores by Event-Year for NRSS.

Notes: Figures depict number of years in the same residence for all addresses (Panel (a)) and LIHTC addresses (Panel (b)). The x-axis denotes number of years and the y-axis denotes number of occurrences.

(a) English.



(b) Math.

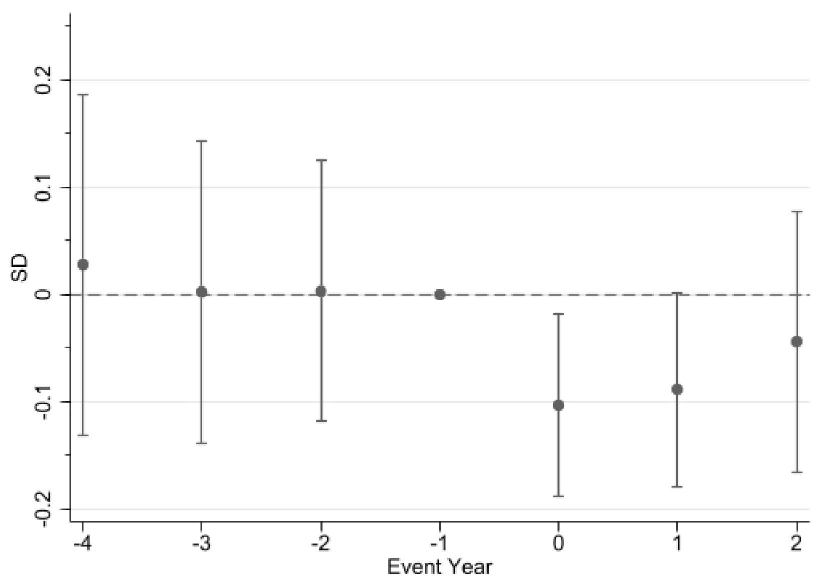
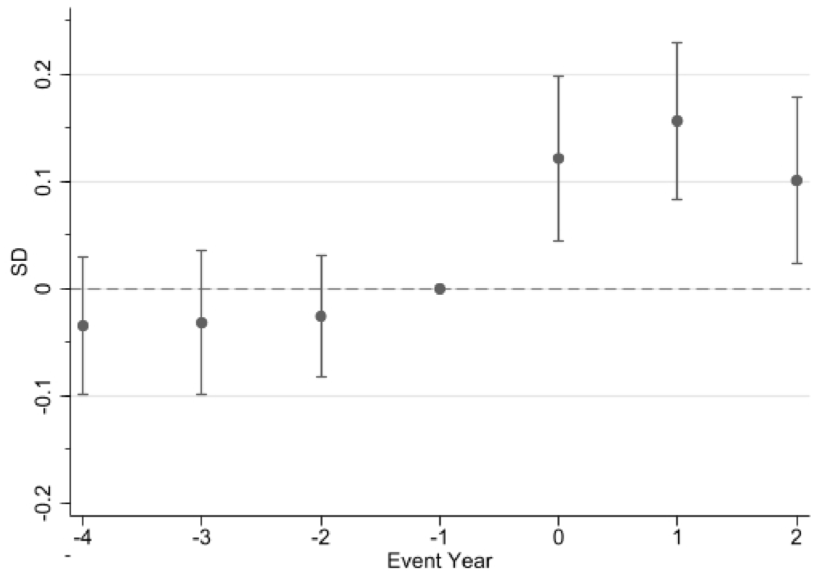


Figure 1.11. Standardized Test Scores by Event-Year for NRNS.

Notes: Figures depict number of years in the same residence for all addresses (Panel (a)) and LIHTC addresses (Panel (b)). The x-axis denotes number of years and the y-axis denotes number of occurrences.

(a) English.



(b) Math.

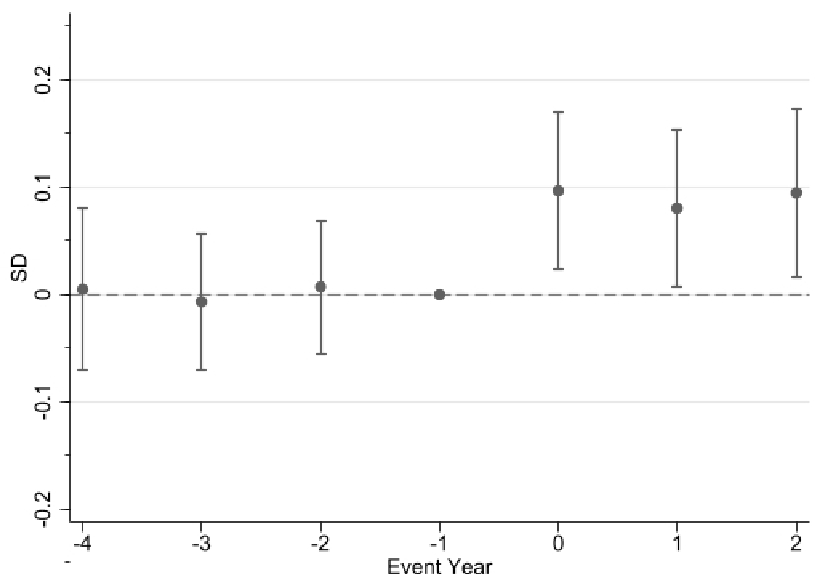
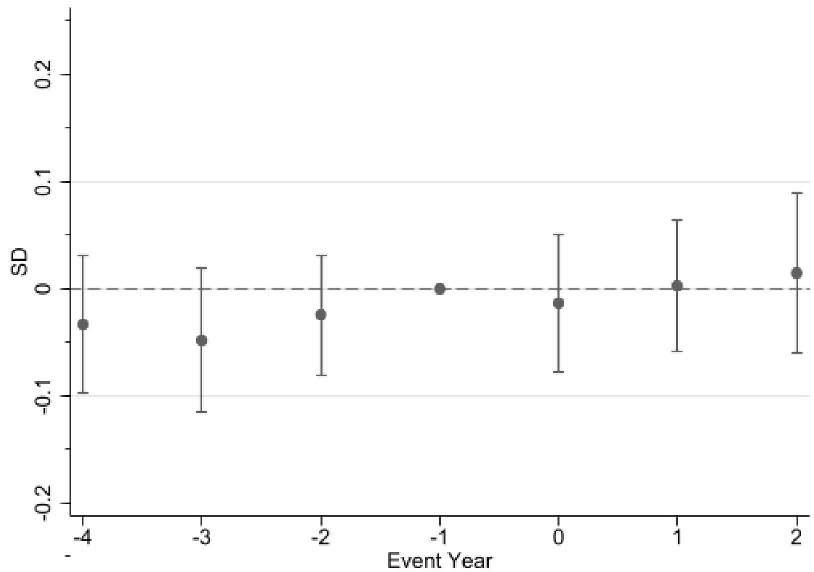


Figure 1.12. Standardized Test Scores by Event-Year for New Construction.

Notes: Figure shows the DID event study for short-run test scores by estimating Equation 1.2 for new constructions. See notes to Figure 1.7.

(a) English.



(b) Math.

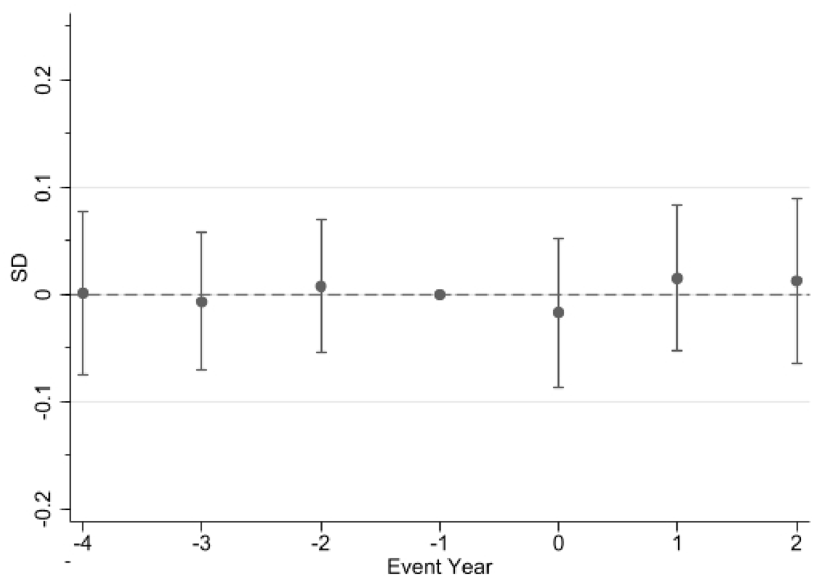


Figure 1.13. Standardized Test Scores by Event-Year for Rehabilitated Units.

Notes: Figure shows the DID event study for short-run test scores by estimating Equation 1.2 for rehabilitated constructions. See notes to Figure 1.7.

Table 1.1. Summary Statistics of LIHTC in San Diego County and SDUSD.

	San Diego County					SDUSD				
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max
Approved Year	217	2,003.17	5.50	1989	2013	73	2,002.81	6.15	1989	2013
PIS Year	217	2,004.44	5.63	1989	2013	73	2,004.10	6.40	1989	2013
Total Units	217	103.09	82.70	10	504	73	105.52	80.65	14	448
Low Income Units	217	98.18	75.21	10	443	73	102.21	77.49	14	443
Annual Federal Award ($)	216	737,472	569,347	100	3,189,999	73	769,620	654,711	100	2,548,069
Funded by State	217	0.12	0.33	0	1	73	0.16	0.37	0	1
QCT	217	0.19	0.39	0	1	73	0.32	0.47	0	1

Notes: This table includes summary statistics of LIHTC properties in San Diego County and in SDUSD boundaries for the full sample at the property level.

Table 1.2. LIHTC Types of Properties in San Diego County and SDUSD.

	San Diego County		SDUSD	
	Properties	Units	Properties	Units
A. Tax Credit Type				
4 percent	131 (60.37%)	15,989 (71.47%)	37 (50.68%)	4,816 (62.62%)
9 percent	86 (39.63%)	6,382 (28.53%)	36 (49.32%)	2,887 (37.48%)
B. Construction Type				
New Construction	138 (65.09%)	12,327 (55.82%)	37 (54.41%)	3,547 (47.83%)
Acquisition & Rehabilitation	64 (30.19%)	8,728 (39.52%)	25 (36.76%)	3,330 (44.90%)
Rehabilitation	10 (4.72%)	1,029 (4.66%)	6 (8.82%)	539 (7.27%)
C. Targeted Tenant Type				
Large Family	126 (58.33%)	11,043(49.46%)	32 (43.84%)	2,666 (34.61%)
At Risk	5 (2.31%)	734 (3.29%)	2 (2.74%)	488 (6.34%)
SRO	7 (3.24%)	1,119 (5.01%)	7 (9.59%)	1,119 (14.53%)
Senior	29 (13.43%)	2,551 (11.43%)	13 (17.81%)	1,141 (14.81%)
Special Needs	4 (1.85%)	241 (1.08%)	3 (4.11%)	220 (2.86%)
Non Targeted	45 (20.83%)	6,639 (29.74%)	16 (21.92%)	2,069 (26.86%)

Notes: This table provides the breakdown of tax credit type, construction type, and targeted tenant type of LIHTC properties in San Diego County and in SDUSD boundaries for the full sample.

Table 1.3. SDUSD Full Sample Summary Statistics.

	N	Mean	SD	Min	Max
A. Demographics					
Year	1,131,503	2,006.794	3.305	2,001	2,012
Grade Level	1,131,503	6.184	2.772	2	11
Female	1,131,503	0.487	0.500	0	1
Special Ed. Status	1,131,503	0.124	0.329	0	1
English Learner Status	1,131,503	0.251	0.433	0	1
Resident	1,130,145	0.619	0.486	0	1
B. Race					
White	1,131,503	0.246	0.431	0	1
Black	1,131,503	0.135	0.342	0	1
Asian	1,131,503	0.154	0.361	0	1
Hispanic	1,131,503	0.450	0.497	0	1
C. Home Language					
English	1,131,154	0.458	0.498	0	1
Spanish	1,131,154	0.362	0.481	0	1
Other	1,131,154	0.180	0.384	0	1
D. Parental Education Level					
Less Than HS	1,108,001	0.143	0.350	0	1
Completed HS	1,108,001	0.188	0.391	0	1
Some College	1,108,001	0.185	0.388	0	1
College	1,108,001	0.176	0.381	0	1
Post Baccalaureate	1,108,001	0.115	0.319	0	1
E. Short-Run Outcomes					
CST English	1,027,420	0.000	1.000	-3.916	5.843
CST Math	682,619	0.000	1.000	-3.311	4.972
F. Medium-Run Outcomes					
Completed HS	107,943	0.880	0.325	0	1
Enrolled 2-Year College	120,815	0.583	0.493	0	1
Enrolled 4-Year College	114,700	0.429	0.495	0	1
Completed 2-Year College	120,815	0.086	0.281	0	1
Completed 4-Year College	114,700	0.257	0.437	0	1

Notes: This table includes the full sample summary statistics of the SDUSD data at the student-year level.

Table 1.4. Descriptive Statistics of Absenteeism by Grade Level.

Grade	Absenteeism Rate (%)	Days Missed	Chronically Absent (%)
1	4.78 (5.76)	8.60 (10.37)	11.07
2	4.27 (5.46)	7.68 (9.83)	8.79
3	4.07 (5.33)	7.33 (9.60)	8.21
4	4.11 (5.35)	7.40 (9.62)	8.62
5	4.07 (5.27)	7.33 (9.49)	8.54
6	4.34 (5.72)	7.81 (10.29)	10.33
7	4.76 (6.48)	8.56 (11.67)	12.53
8	5.11 (7.34)	9.20 (13.22)	13.86
9	6.00 (9.93)	10.80 (17.88)	17.04
10	5.54 (9.37)	9.97 (16.87)	15.48
11	4.99 (8.47)	8.98 (15.24)	13.54
12	5.09 (8.91)	9.16 (16.03)	14.21
All	4.84 (7.43)	8.71 (13.37)	12.08

Notes: This table provides the descriptive statistics of absenteeism by grade level across the full sample. Standard deviations are in parentheses. *Absenteeism rate* is the mean absenteeism rate of each grade level, where the variable is defined as the number of absent days divided by the number of available school days in an academic year. *Days missed* is calculated using the year-round school calendars of 180 days of instruction in California. *Chronically absent* is the percentage of students who missed at least ten percent of the instructional days they were enrolled to attend a school.

Table 1.5. Short-Run Covariate Balance Table.

	All		Treat		Matched Control		Treat v.s. Control	
	Mean	SD	Mean	SD	Mean	SD	Diff	p-val
A. Demographics								
Female	0.486	0.499	0.519	0.500	0.519	0.500	0.000	1.000
Special Ed. Status	0.125	0.330	0.093	0.290	0.093	0.290	0.000	1.000
English Learner Status	0.265	0.441	0.397	0.489	0.397	0.489	0.000	1.000
B. Ethnicity								
White	0.246	0.431	0.024	0.154	0.024	0.154	0.000	1.000
Black	0.135	0.342	0.321	0.467	0.321	0.467	0.000	1.000
Asian	0.154	0.361	0.022	0.146	0.022	0.146	0.000	1.000
Hispanic	0.450	0.497	0.627	0.484	0.627	0.484	0.000	1.000
C. Home Language								
English	0.458	0.498	0.343	0.475	0.351	0.477	-0.008	0.622
Spanish	0.362	0.481	0.563	0.496	0.552	0.497	0.011	0.489
Other	0.180	0.384	0.094	0.292	0.098	0.297	-0.010	0.713
D. Parental Education Level								
Less Than HS	0.143	0.350	0.215	0.411	0.215	0.411	0.000	1.000
Completed HS	0.188	0.391	0.300	0.459	0.300	0.458	0.000	1.000
Some College	0.185	0.388	0.196	0.397	0.196	0.397	0.000	1.000
College	0.176	0.381	0.067	0.249	0.067	0.249	0.000	1.000
Post Baccalaureate	0.115	0.319	0.024	0.152	0.024	0.152	0.000	1.000
Missing	0.193	0.450	0.199	0.399	0.199	0.399	0.000	1.000
E. Other								
Poverty Rate	0.260	0.141	0.301	0.133	0.301	0.133	0.000	0.843
Observations	1,131,503		1,232		6,160			
Sum of Weights			1,232		1,232			
Joint F-test								0.999

Notes: This table provides the covariate balance table after performing the short-run matching procedure.

Table 1.6. Medium-Run Covariate Balance Table.

	All		Treat		Matched Control		Treat v.s. Control	
	Mean	SD	Mean	SD	Mean	SD	Diff	*p*-val
A. Demographics								
Female	0.491	0.500	0.501	0.500	0.505	0.500	-0.004	0.740
Special Ed. Status	0.115	0.319	0.138	0.345	0.146	0.353	-0.017	0.317
English Learner Status	0.232	0.422	0.352	0.478	0.341	0.474	0.012	0.309
B. Ethnicity								
White	0.262	0.440	0.030	0.171	0.025	0.156	0.050	0.128
Black	0.128	0.334	0.393	0.489	0.403	0.490	-0.010	0.396
Asian	0.180	0.384	0.039	0.195	0.037	0.189	0.015	0.607
Hispanic	0.422	0.494	0.534	0.499	0.531	0.499	0.003	0.778
Other	0.009	0.093	0.004	0.060	0.005	0.069	-0.068	0.449
C. Home Language								
English	0.502	0.500	0.349	0.477	0.360	0.480	-0.012	0.307
Spanish	0.333	0.471	0.484	0.500	0.485	0.500	-0.001	0.921
Other	0.164	0.371	0.167	0.373	0.154	0.361	0.023	0.151
D. Parental Education Level								
Less Than HS	0.104	0.305	0.173	0.379	0.178	0.382	-0.008	0.604
Completed HS	0.144	0.351	0.198	0.398	0.199	0.399	-0.002	0.902
Some College	0.151	0.358	0.125	0.330	0.125	0.331	-0.002	0.923
College	0.175	0.380	0.053	0.224	0.052	0.221	0.007	0.785
Post Baccalaureate	0.119	0.324	0.023	0.151	0.021	0.142	0.033	0.378
Missing	0.307	0.461	0.428	0.495	0.426	0.494	0.002	0.847
E. Other								
Poverty Rate	0.178	0.135	0.297	0.107	0.296	0.123	0.001	0.501
Academic Performance	0.063	0.951	-0.501	0.766	-0.498	0.789	-0.003	0.889
Observations	131,370		1,232		6,160			
Sum of Weights			1,232		1,232			
Joint F-test								0.782

Notes: This table provides the covariate balance table after performing the medium-run matching procedure.

Table 1.7. Main Results for Short-Run Outcomes.

	(1)	(2)	(3)
Dependent variable:	Absenteeism	English	Math
DID	-0.0028**	0.049**	0.048*
	(0.0013)	(0.024)	(0.029)
Observations	4,968	4,968	3,180
Pre-event mean, control	0.0456	-0.370	-0.392

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the results for short-run outcomes by estimating Equation 1.1. The control group includes both *non-movers* and *movers*. The dependent variable in column (1) is the absenteeism rate. The dependent variables in columns (2) and (3) are the standardized English and math test scores.

Table 1.8. New Residence Same School (NRSS) Short-Run Results.

	(1)	(2)	(3)
Dependent variable:	Absenteeism	English	Math
DID	-0.0017	0.094*	0.096**
	(0.0021)	(0.053)	(0.057)
Observations	2,800	2,800	2,014
Pre-event mean, control	0.0426	-0.405	-0.431

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the results for short-run outcomes by estimating Equation 1.1 for the new residence same school (NRSS) control group.

Table 1.9. New Residence New School (NRNS) Short-Run Results.

	(1)	(2)	(3)
Dependent variable:	Absenteeism	English	Math
DID	-0.0061**	-0.049	-0.123*
	(0.0028)	(0.057)	(0.066)
Observations	2,168	2,168	1,166
Pre-event mean, control	0.0520	-0.563	-0.606

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the results for short-run outcomes by estimating Equation 1.1 for the new residence new school (NRNS) control group.

Table 1.10. Main Results for Medium-Run Outcomes.

	HS Completion	College Enrollment			College Completion		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	2-Year	4-Year	2- or 4-Year	2-Year	4-Year	2- or 4-Year
LIHTC	0.008***	0.005**	0.003	0.006**	0.002**	0.003*	0.005***
	(0.0027)	(0.0025)	(0.0027)	(0.0026)	(0.0008)	(0.0013)	(0.0015)
Observations	4,968	4,968	4,968	4,968	3,872	4,172	4,496
Control group rate	0.810	0.493	0.236	0.599	0.073	0.136	0.183

Coefficients reported are margins from logistic regressions. Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results for medium-run outcomes by estimating Equation 1.1.

Table 1.11. Short-Run Outcomes by Gender.

Gender	Female			Male		
	(1)	(2)	(3)	(4)	(5)	(6)
	Absenteeism	English	Math	Absenteeism	English	Math
DID	-0.0022	0.074**	0.044*	-0.0034**	0.020	-0.016
	(0.0018)	(0.036)	(0.025)	(0.0015)	(0.029)	(0.043)
Observations	2,488	2,488	2,488	2,480	2,480	2,480
Pre-event mean, control	0.0482	-0.498	-0.407	0.0430	-0.251	-0.378

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the regression estimates of Equation 1.1 for short-run outcomes by gender.

Table 1.12. Short-Run Outcomes by Age Moved.

Age Group	Younger than Adolescence			Adolescent Children	
	(1) Absenteeism	(2) English	(3) Math	(4) Absenteeism	(5) English
DID	-0.0013	0.072**	0.049**	-0.0045**	0.033
	(0.0019)	(0.030)	(0.022)	(0.0022)	(0.041)
Observations	3,068	3,068	3,068	1,900	1,900
Pre-event mean, control	0.0398	-0.370	-0.375	0.0425	-0.369

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the results for short-run outcomes by age moved into LIHTC. *Younger than adolescence* include students who moved into a LIHTC unit before the age of 13, and *adolescent children* include students who moved between the age of 13 to 18.

Table 1.13. Short-Run Outcomes by Construction Type.

Construction Type	New Construction			Rehabilitation		
	(1) Absenteeism	(2) English	(3) Math	(4) Absenteeism	(5) English	(6) Math
DID	-0.0099***	0.135***	0.095***	-0.0015	-0.008	-0.010
	(0.114)	(0.033)	(0.036)	(0.0023)	(0.030)	(0.038)
Observations	1,900	1,900	1,168	2,932	2,932	1,952
Pre-event mean, control	0.0452	-0.368	-0.390	0.0459	-0.370	-0.392

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the results for short-run outcomes by construction types.

Table 1.14. Short-Run Mechanisms by Construction Type.

Construction Type	New Construction				Rehabilitation			
	(1) Distance	(2) Poverty	(3) English	(4) Math	(5) Distance	(6) Poverty	(7) English	(8) Math
DID	0.384*	-0.063***	0.008	0.001	0.199	0.039***	-0.026***	-0.042***
	(0.215)	(0.008)	(0.012)	(0.014)	(0.160)	(0.006)	(0.010)	(0.012)
Observations	1,900	1,900	1,900	1,168	2,932	2,932	2,932	1,952
Pre-event mean, control	2.276	0.287	-0.139	-0.134	2.281	0.287	-0.170	-0.167

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table explores the mechanisms for the short-run outcomes by construction types. I estimate Equation 1.1 with the following dependent variables: distance to school (in miles), neighborhood poverty rate, school-level standardized English test score, and school-level standardized math test score.

Table 1.15. Short-Run Outcomes by Neighborhood Type Transition.

Neighborhood Transition	HI → HI		LI → HI		HI → LI		LI → LI	
	(1) English	(2) Math	(3) English	(4) Math	(5) English	(6) Math	(7) English	(8) Math
DID	-0.026	-0.040	-0.069	-0.044	0.190**	0.041	0.059*	0.058*
	(0.052)	(0.077)	(0.101)	(0.131)	(0.075)	(0.096)	(0.032)	(0.031)
Observations	756	536	644	420	1,204	812	2,324	1,628
Control group mean	-0.268	-0.309	-0.399	-0.425	-0.326	-0.397	-0.417	-0.409

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the results for short-run outcomes by neighborhood type transition. High-income neighborhood is denoted as HI and low-income neighborhood is denoted as LI, where neighborhood is a census block and the cutoff value between the two neighborhood types is 25 percent poverty rate.

1.9 Appendix Figures and Tables

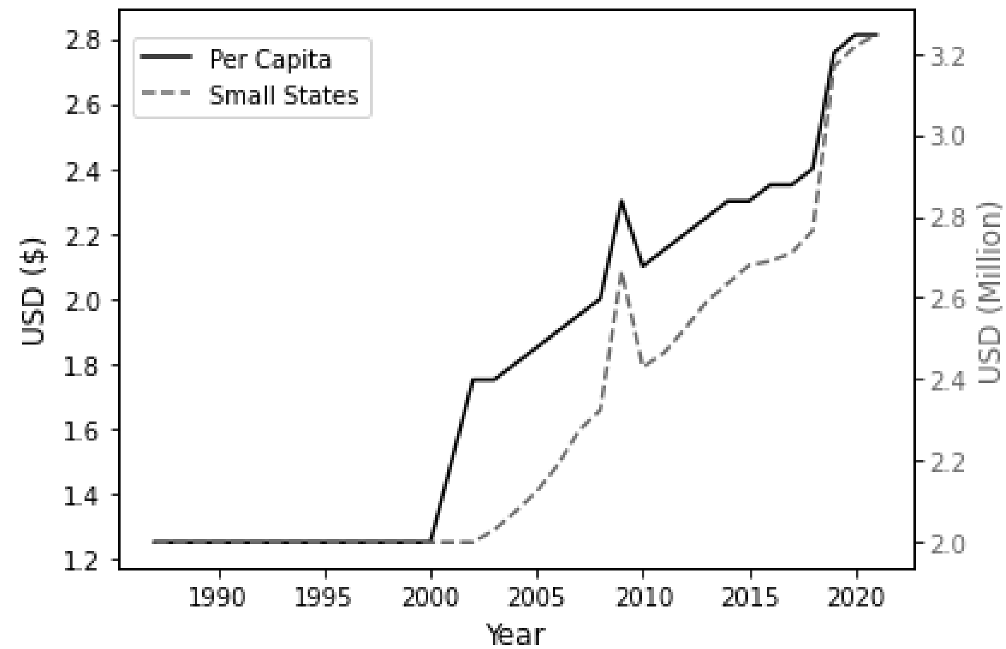


Figure 1.A.1. Historical LIHTC Credit Allocation.

Notes: Figure depicts historical LIHTC credits allocation rule. The black line depicts credit per capita for larger states from 1987 to the present, with the credits amount on the left axis. The dotted gray line depicts small state credits with the credits amount on the right axis.

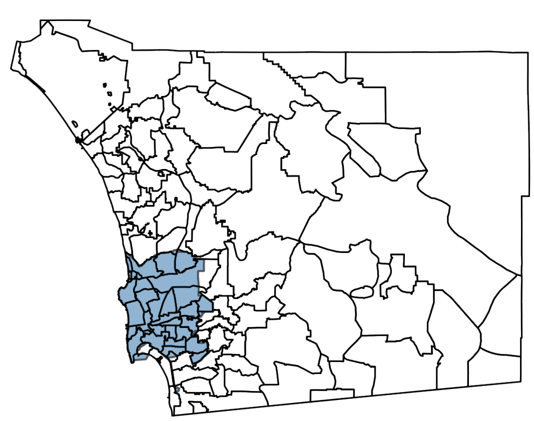


Figure 1.A.2. SDUSD Boundaries.

Notes: Figure depicts the zip code tabulation areas comprising San Diego County. The shaded area denotes the San Diego Unified School District boundaries.

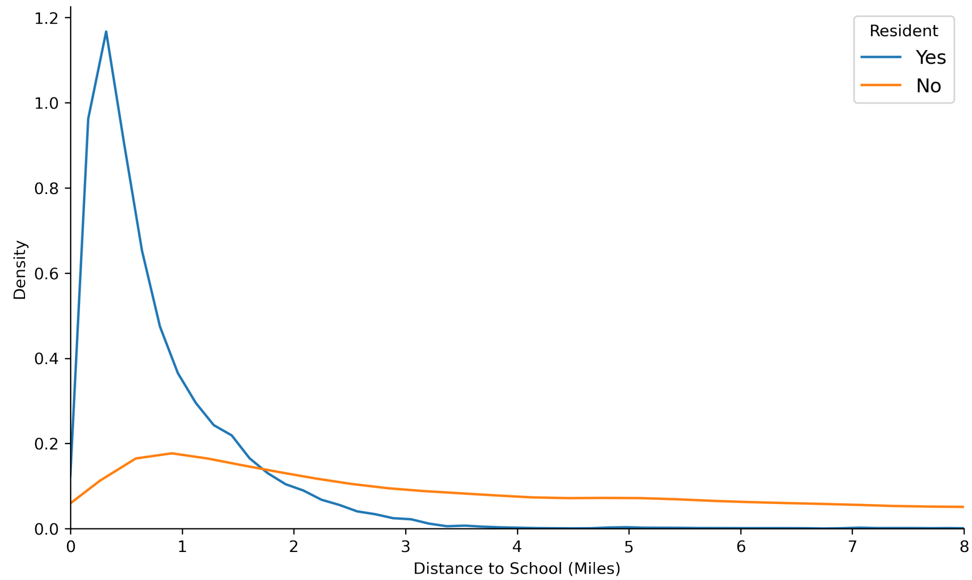


Figure 1.A.3. Distance to School by Resident Status.

Notes: Figure depicts the density plots of distance to school by resident status.

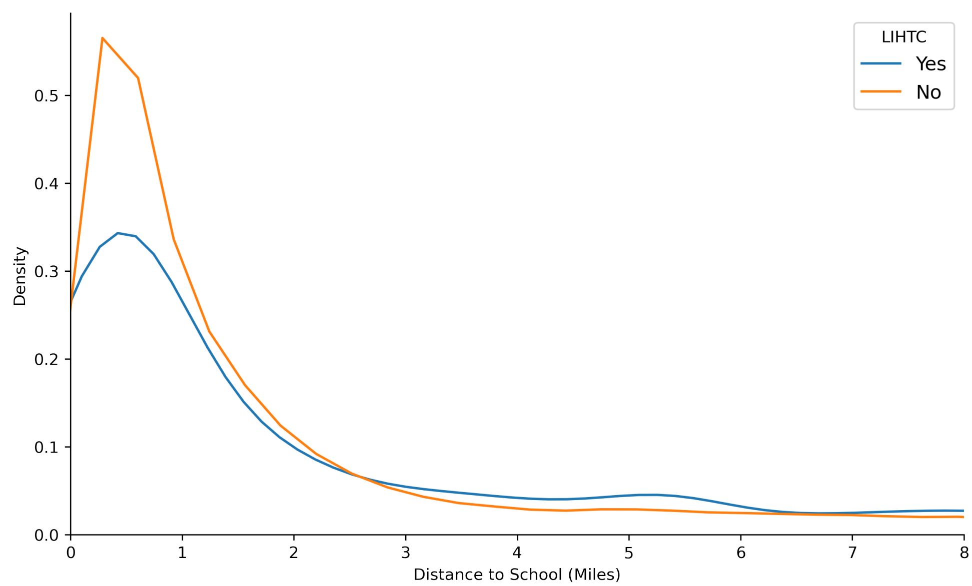


Figure 1.A.4. Distance to School by LIHTC and Non-LIHTC.

Notes: Figure depicts the density plots of distance to school by whether the residential address is a LIHTC unit or not.

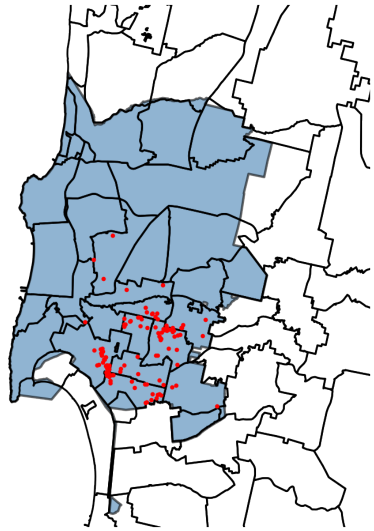


Figure 1.A.5. LIHTC in SDUSD.

Notes: Figure depicts the LIHTC developments in the San Diego Unified School District boundary (shaded area). Each red dot represents a development. The base map is a partial zip code map of San Diego County.

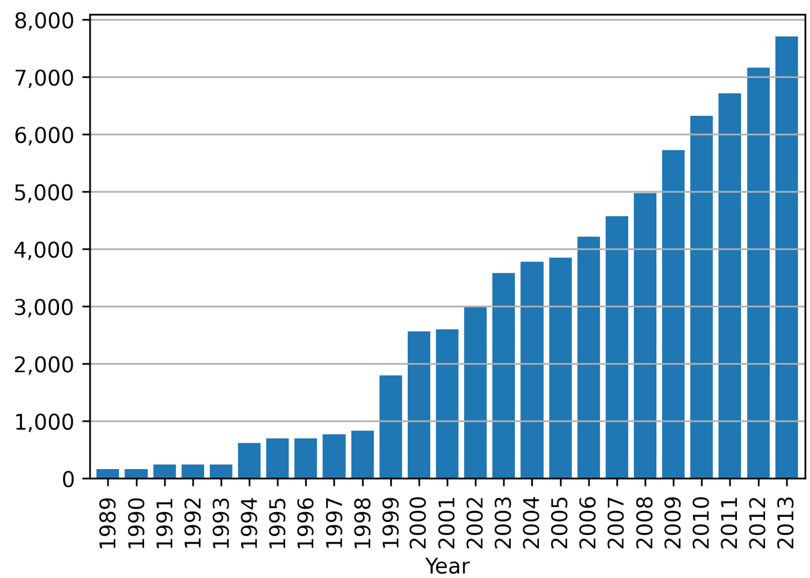


Figure 1.A.6. Stock of LIHTC Units in SDUSD Boundary.

Notes: Figure depicts the histogram of LIHTC stocks in San Diego Unified School District boundary since policy inception.

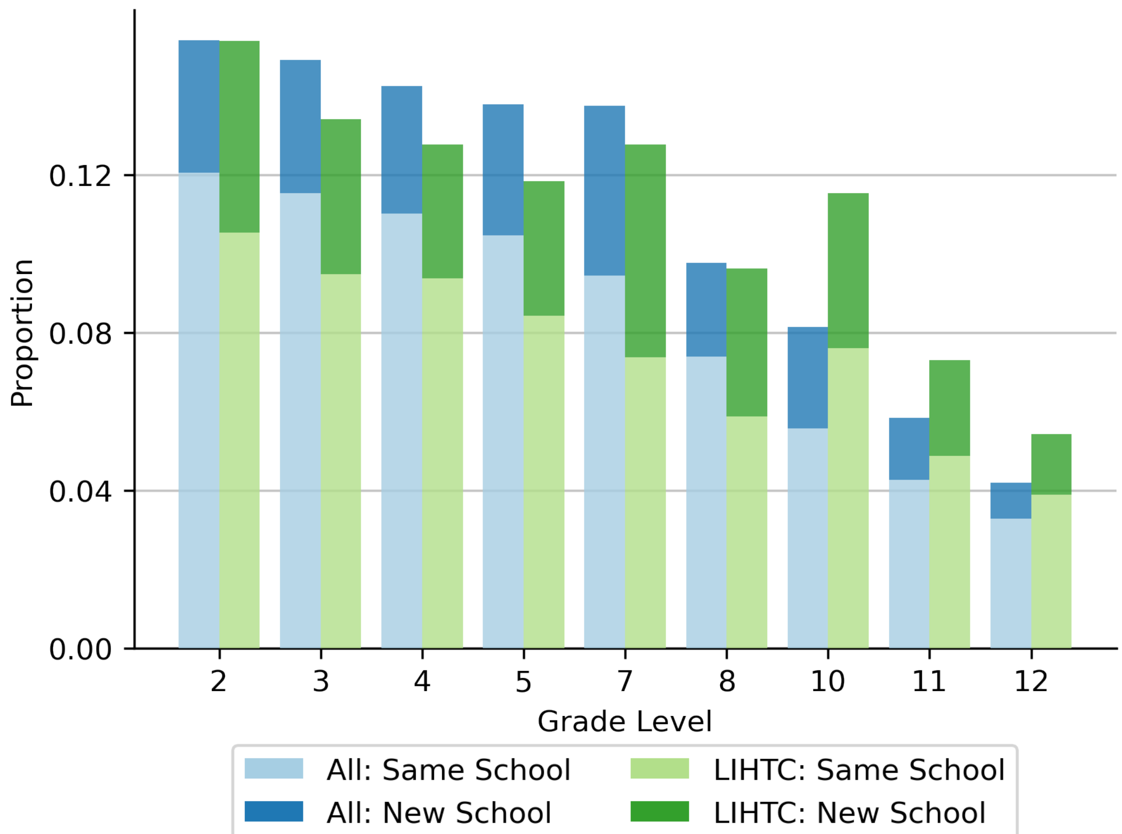
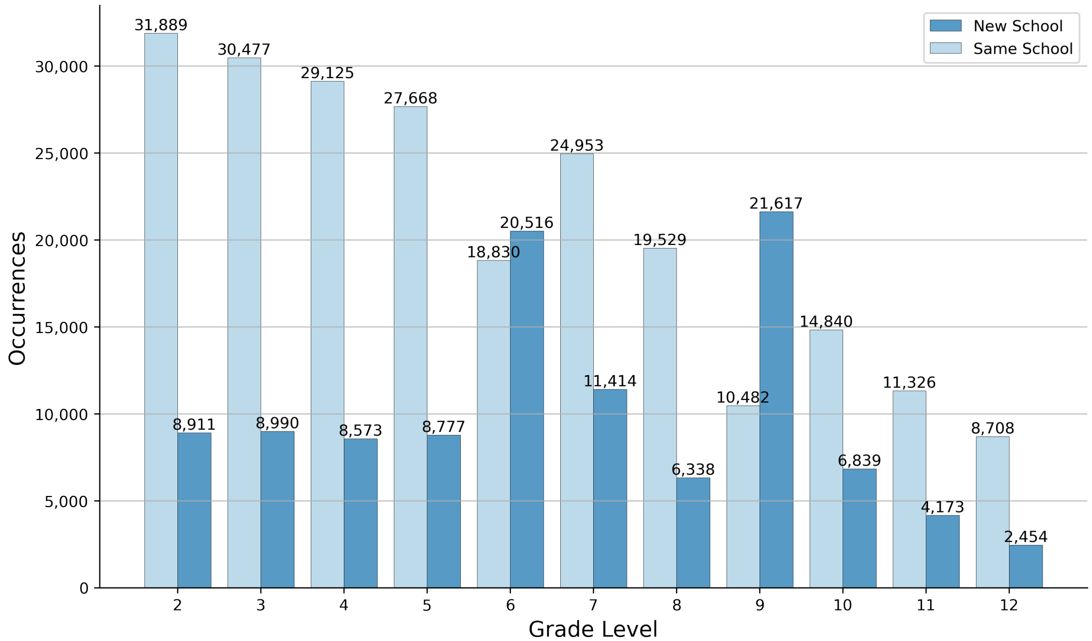


Figure 1.A.7. Fraction of Moves by Grade Level.

Notes: Figure depicts fractions of all moves by grade level, excluding grade levels 6 and 9. A "move" is counted when the residential address changes from one grade level to the next. The x-axis denotes the grade level, and the y-axis denotes the proportion. See Figure 1.A.8 for raw counts.

(a) All.



(b) LIHTC.

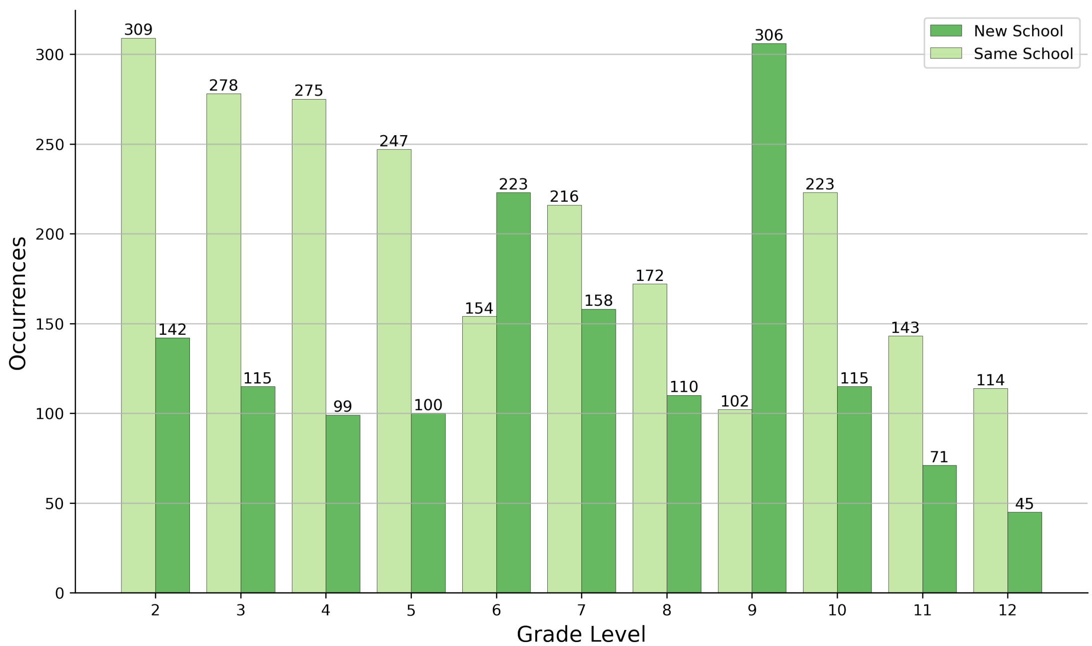


Figure 1.A.8. Counts of Residential Moves by Grade Level.

Notes: Figures depict number of residential moves by grade level. Panel (a) shows the count of moves into any type of residence and Panel (b) shows the count of moves into LIHTC residences. The x-axis denotes the number of years, and the y-axis denotes the number of occurrences.

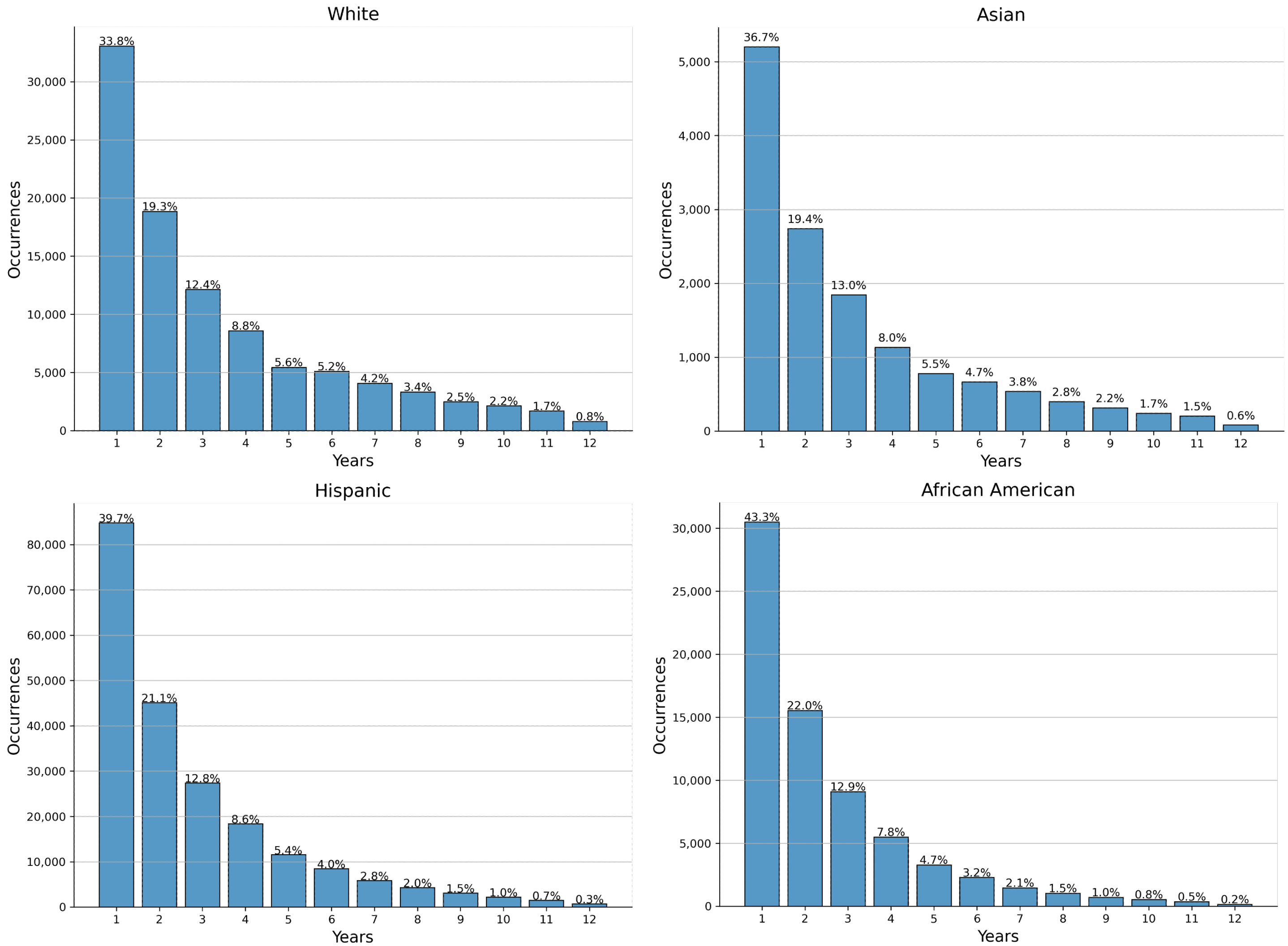


Figure 1.A.9. Years in Same Residence by Race.

Notes: Figures depict the number of years in the same residence for all addresses and LIHTC addresses available in the SDUSD data from 2001-2012 by race. The x-axis denotes the number of years, and the y-axis denotes the number of occurrences.

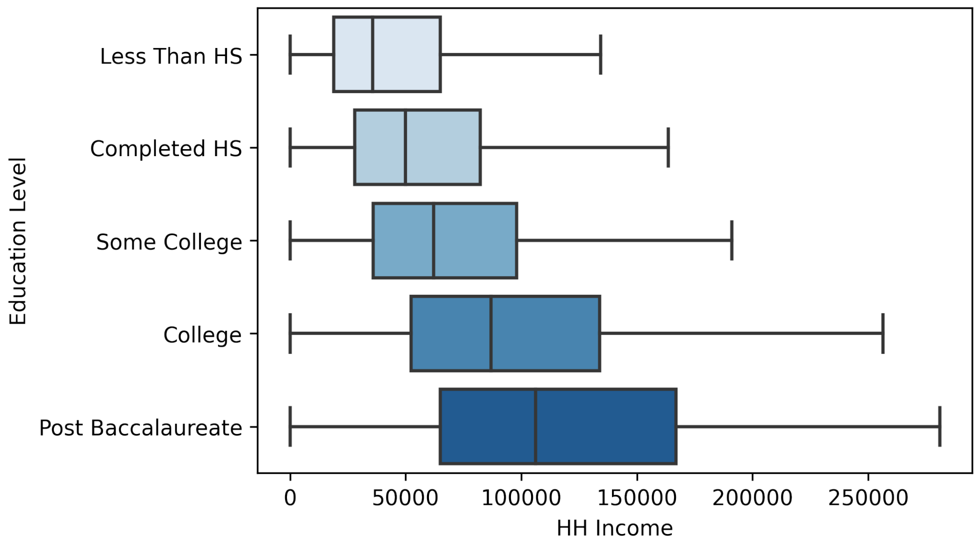


Figure 1.A.10. Correlation Between Income and Education Level.

Notes: Figure depicts the correlation between income and education level. The data in the figure is from the 2010 Census.

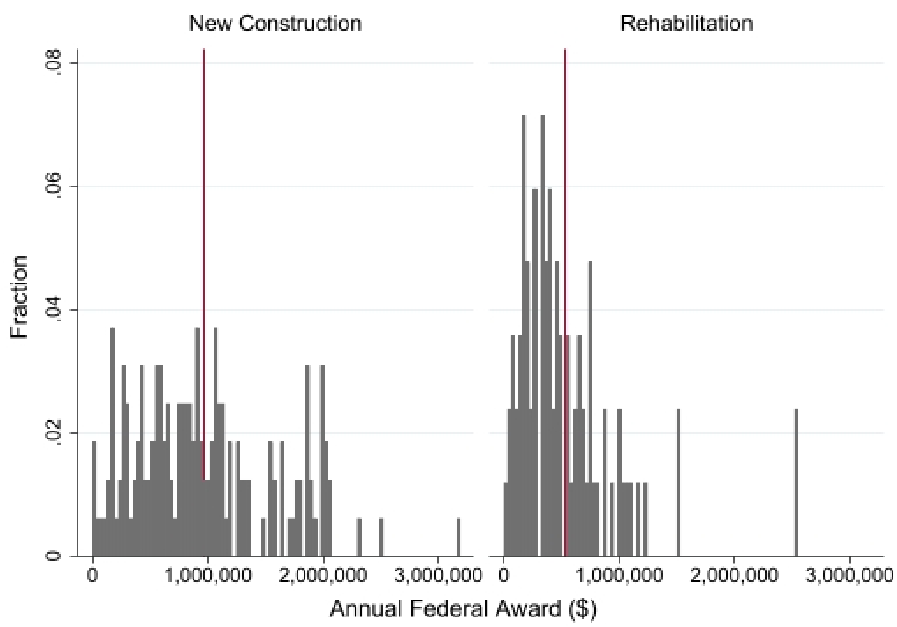


Figure 1.A.11. Annual Federal Award by Construction Type.

Notes: Figure depicts the distribution of annual federal credits for LIHTC developments. The left panel includes awards for new construction developments, and the right panel includes developments for rehabilitation developments. The vertical red line depicts the mean amount awarded.

Table 1.A.1. Medium-Run Results Using a Logistic Model.

	HS Completion	College Enrollment			College Completion		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	2-Year	4-Year	2- or 4-Year	2-Year	4-Year	2- or 4-Year
LIHTC	1.066^{**}	1.025^{***}	1.021	1.026^{**}	1.047^{**}	1.049^{*}	1.051^{***}
	(0.023)	(0.012)	(0.020)	(0.012)	(0.023)	(0.026)	(0.021)
Observations	4,968	4,968	4,968	4,968	3,872	4,172	4,496
Control group rate	0.810	0.493	0.236	0.599	0.073	0.136	0.183

Coefficients reported are odds ratios from logistic regressions. Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results for medium-run outcomes by estimating Equation 1.1 with a logistic regression model. The coefficients presented in the table are odds-ratios.

Table 1.A.2. Medium-Run Results Using a Linear Probability Model.

	HS Completion	College Enrollment			College Completion		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	2-Year	4-Year	2- or 4-Year	2-Year	4-Year	2- or 4-Year
LIHTC DID	0.007^{**}	0.005^{**}	0.003	0.005^{**}	0.002^{**}	0.003^{**}	0.004^{***}
	(0.003)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)
Observations	4,968	4,968	4,968	4,968	3,872	4,172	4,496
Control group rate	0.810	0.493	0.236	0.599	0.073	0.136	0.183

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results for medium-run outcomes by estimating Equation 1.1 using a linear probability model (LPM).

Chapter 2

The Effect of Low-Income Housing Tax Credit on Homelessness

2.1 Introduction

With limited housing supply and soaring housing prices, housing has been an ongoing concern in the United States in recent decades. In 2018, roughly fifty percent of renter households were rent-burdened or severely rent-burdened, exacerbating housing instability for low-income households and putting them at risk of becoming homeless.[1] According to the Department of Housing and Urban Development (HUD) annual Point-in-Time (PIT) count, roughly 553,000 people were experiencing homelessness in the United States on a single night in 2018. Furthermore, over 1.44 million people experienced sheltered homelessness sometime in 2018 (Henry et al., 2020). To alleviate housing instability and homelessness, federal and state governments have enacted various housing policies over the years (Collinson et al., 2016; Kingsley, 2017).

Aside from tenant-based rental housing policies such as the Housing Choice Voucher

[1] See Figure 2.A.1. Defined by the US Department of Housing and Urban Development (HUD), a household is considered *rent-burdened* if it spends more than 30 percent of household income on housing and *severely rent-burdened* if it spends more than 50 percent of household income for housing.

Program (formerly Section 8) and HOME Investment Partnerships Program (HOME), the place-based Low-Income Housing Tax Credit (LIHTC) program has gained traction in recent years. The program incentivizes housing developers to create multi-family rental housing reserved for low-income renters. During the compliance period, a percentage of units are reserved for households below a certain income threshold, and the rent cannot exceed thirty percent of said income threshold. With increased housing supply through the tax credit program, more households should be able to access and retain affordable housing. However, it is unclear whether the policy is effective for marginalized households at-risk of homelessness. In this paper, I examine how LIHTC affects rates of homelessness in the US.

The Low-Income Housing Tax Credit is currently the largest place-based subsidy for low-income housing in the United States. Created by the Tax Reform Act of 1986, the main objective of the policy is to increase new construction and rehabilitation of low-income rental housing. Every year, the federal government issues tax credits to state governments, and state housing agencies award the credits to private developers through a competitive process. By providing a dollar-for-dollar tax credit to rental housing developers, more than 110,000 affordable rental units are created annually, funding over 21 percent of all multi-family developments since 1987 (Khadduri et al., 2012). LIHTC has been the fastest-growing housing program over the last two decades, assisting over 2.57 million households each year (Kingsley, 2017).

Whether LIHTC alleviates homelessness is an important policy question and has been previously explored. Jackson and Kawano (2015) examine the impact of LIHTC on decreasing homelessness using the homeless counts in the 2000 Decennial Census. Utilizing a discontinuous increase in tax credits in selected high-poverty census tracts, the authors find no significant impact on neighborhood homelessness but a significant decrease in county-level homelessness. The authors conclude that the null effect on the neighborhood level is due to the cancellation of housing-driven and mobility-driven responses. This paper builds on the results in the preceding paper and extends the analysis using a panel of homeless counts released by HUD. The annual

Point-in-Time (PIT) reports provide estimated counts of sheltered and unsheltered individuals for each Continuum of Care, a sub-state geographical unit typically consisting of several counties in a given state. Instead of looking at a snapshot of LIHTC on homelessness using the cross-sectional census data, the new panel data allows me to study the dynamic effect of changes in the stock of subsidized multi-family rental housing on changes in homelessness from 2009 to 2019.

I implement a first-differenced model to estimate the effect of new LIHTC units on homelessness on the Continuum of Care level. I find that one additional LIHTC unit is associated with an expected decrease in 1.1 homeless individuals. The effect is particularly significant for sheltered homeless, but there is no statistically significant effect on unsheltered homeless. Furthermore, the effect of new LIHTC units on reducing homelessness among children under 18 years old is more substantial than that on the overall homeless population, suggesting that LIHTC units are more likely to benefit families with children. I also provide robustness tests to check whether the models are correctly specified.

This paper falls into three strands of literature. The first is the literature on place-based subsidized housing and how it affects nearby neighborhoods. Schwartz et al. (2006) find significant and long-term external benefits from subsidized rental properties in New York City, such as increased safety or demand for retail services. Positive spillovers to nearby neighborhoods increase with project size and decrease with distance to the project site. The authors conclude that some benefits are due to the replacement of existing disamenities. Similarly, Baum-Snow and Marion (2009) find that new LIHTC properties lead to higher housing values in low-income neighborhoods but have little to no effects in gentrifying areas. Diamond and McQuade (2019) use a structural model to estimate the costs and benefits of LIHTC properties. The authors find a 6.5 percent increase in housing prices and a decrease in crime rates in low-income neighborhoods, but find a 2.5 percent decrease in housing prices in higher-income neighborhoods. The authors estimate aggregate welfare benefits of $116 million a year from LIHTC developments.

The second strand is the literature on racial and poverty concentration of low-income housing. One of the biggest concerns of place-based housing subsidies is that such policies aggravate poverty concentration in low-income neighborhoods. Carter et al. (1998) argue that public housing concentrates poverty over the years as most large-scale public housing developments were exclusively dedicated to very-low-income tenants and were placed in very-low-income neighborhoods. On the contrary, little evidence has shown that LIHTC developments exacerbate the concentration of poverty (Ellen et al., 2009; Freedman and McGavock, 2015; Ellen et al., 2016). Furthermore, Horn and O'Regan (2011) find that an increase in the use of LIHTC is associated with declines in racial segregation at the metropolitan level.

The third strand is the literature on homelessness and housing subsidies. Quigley et al. (2001) and Raphael (2010, p.110–140) provide cross-sectional evidence on how housing supply, due to zoning restrictions and rent levels, affects homelessness. O'Flaherty (2019) provides a synopsis of recent research on homelessness and concludes that housing subsidies are the most attractive policy to reduce homelessness in the US. Nonetheless, the optimal housing subsidy policies remain unclear. Evans et al. (2019) review randomized controlled trials and quasi-experimental evidence in recent studies. One critical outstanding research question outlined by the authors is the effectiveness of supply-side interventions on homelessness. This paper contributes to the ongoing research on homelessness and housing subsidies, hoping to provide a clearer picture of the effect of place-based housing subsidies on homelessness in the US.

The paper is organized as follows. Section 2.2 contextualizes the institutional background of the Low-Income Housing Tax Credit. Section 2.3 describes the data. Section 2.4 describes the empirical strategy. Section 2.5 presents the results. Section 2.6 concludes.

2.2 Institutional Background

The Low-Income Housing Tax Credit is a federal housing subsidy program enacted in 1986 as part of the Tax Reform Act. The program is designed to incentivize private investment in constructing and rehabilitating affordable rental housing for low-income households. LIHTC provides an upfront capital boost through tax credits to offset construction costs, making the investments more attractive and financially feasible. According to the Department of Housing and Urban Development (HUD), database, more than 3.44 million units were placed in service between 1987 and 2020. The program has successfully provided affordable housing to low-income families and individuals and continues to be a vital tool in addressing the affordable housing crisis in the United States.

The allocation of federal funding to each state is determined annually by the Internal Revenue Service (IRS) based on a formula established by the IRS. The formula takes into account the population in each state, as well as the number of housing units that are considered to be substandard or overcrowded (see Figure 2.1). Once the allocation is determined, each state's housing finance agency (HFA) is responsible for distributing the LIHTC to developers through a competitive application process. Each HFA creates and maintains a Qualified Allocation Plan (QAP), a document outlining guidelines and priorities for allocating the tax credit in a particular state. Detailed scoring criteria are included in a QAP to evaluate applications, such as the targeted population, amenities and quality of the development, developer experience, and project financial feasibility. QAPs vary widely from state to state and directly affect allocations of credits. For example, 58 percent of LIHTC units have three or more bedrooms targeting large families in Cincinnati. In contrast, Los Angeles mostly has single-room occupancy (SRO) projects serving as transitional housing for the homeless population (Cummings and DiPasquale, 1999). Ellen and Horn (2018) examines whether the state allocation plans serve as an effective tool for allotting credits to higher-opportunity neighborhoods. Documenting changes in QAPs in twenty states from 2002 to 2010, the authors observe how these changes alter poverty rates and racial compositions of neighborhoods that received credits and conclude that QAPs can shape LIHTC siting patterns.

After the release of the updated QAP, developers can submit a proposal outlining their proposed development and demonstrating how it meets the HFA's guidelines. The HFA will review the proposals and select the projects with the highest scores based on the scoring criteria. The number of proposals often exceeds the available funding, and most proposals that won the bidding achieved the highest score available. The competitive nature of the funding allocation process ensures the quality of the housing constructed and rehabilitated via the policy.

Two types of credits are available: the 9 percent credit and the 4 percent credit. Generally, the 9 percent credit is reserved for new construction or substantial rehabilitation projects, whereas the 4 percent credit is allocated to properties acquired for rehabilitation or projects financed with tax-exempt bonds. Once the development is built and open to the public (i.e. placed-in-service), tax credits are disbursed to housing developers over the next ten years. Each development is assigned a qualifying basis, calculated from the total cost (less land cost) incurred for units reserved for low-income tenants. More than seventy percent of developments reserve all units to low-income tenants to maximize the qualifying basis. The annual tax credits claimed by a housing developer are the credit type multiplied by the qualifying basis. For instance, if a new construction is approved for the 9 percent credit and has a qualifying basis of $1 million, the developer will receive $90,000 a year in tax credits for the next ten years when the property is placed in service. In reality, most developers sell tax credits to private investors at a discount to receive upfront capital.

According to federal guidelines, state housing finance agencies may allocate an additional thirty percent of tax credits to areas of concentrated poverty (Qualified Census Tracts, or QCTs) or areas with higher development costs (Difficult Development Areas, or DDAs). Qualified Census Tracts are census tracts with 50 percent of households with income below 60 percent of the Area Median Gross Income (AMGI) or have a poverty rate of 25 percent or more[2]; Difficult Development Areas are areas with high land and high construction and utility costs relative to the AMGI. Ellen et al. (2018) find that siting of LIHTC developments is affected by QCT and DDA

[2]Not all census tracts satisfying the requirements are designated as QCTs. All designated QCTs in a single metropolitan or non-metropolitan area may not contain more than twenty percent of the population of that metropolitan or non-metropolitan area. Baum-Snow and Marion (2009) show that the twenty percent cap is not binding.

status. Compared to other rental properties, LIHTC units are more likely to be in neighborhoods with higher poverty rates, weaker labor markets, and more polluted environments. Baum-Snow and Marion (2009) conclude that QCTs receive six more low-income housing units on a base of seven units per tract. Figure 2.A.2 shows the density of all LIHTC properties by county. The map shows that LIHTC properties are more common in densely populated areas, particularly along the east and west coasts, as well as in urban centers like Chicago, Detroit, and Atlanta. LIHTC properties are less common in rural areas and states with smaller populations, such as Wyoming, Montana, and North Dakota.

Properties allocated before 1990 have a 15-year compliance period, and properties allocated after 1990 have an additional 15-year restricted-use period for a total of a 30-year compliance period. The credits must be repaid to the IRS if the conditions are not satisfied for the required compliance years. Once the compliance period is over, developers can remain affordable, recapitalize for new credits, or reposition as market-rate rentals (see Khadduri et al. (2012) for more details).

During the compliance period, LIHTC properties must satisfy two conditions for their tenants: the income test and the gross rent test. The income test requires a proportion of rental units reserved for income-eligible households. More specifically, three rules could be used to meet the income test. The first rule is the 20-50 test, which requires at least 20 percent of the rental units to be reserved for tenants with household incomes below 50 percent of the Area Median Gross Income. The second way to satisfy the income test is the 40-50 test, which requires at least 40 percent of the rental units to be reserved for tenants with household income below 60 percent of the AMGI. The third rule requires at least 40 percent of the rental units to be occupied by tenants with income below 80 percent of the AMGI, and the average income of all tenants must be below 60 percent of the AMGI. In addition to the income test, the gross rent test requires that the rents not exceed 30 percent of either 50 or 60 percent of the AMGI. Unlike the Housing Choice Voucher Program or the Public Housing Program, where rent depends on household income, LIHTC developments charge fixed rent for the tenants in low-income units.

Since LIHTC is administered by the IRS instead of HUD, data on tenants and rents in LIHTC properties were not collected until the Housing and Economic Recovery Act (HERA) in 2008. It is still being determined who the primary beneficiaries are from the LIHTC policy. O'Regan and Horn (2013) collect tenant-level data from eighteen states that represent almost forty percent of all LIHTC units and find 45 percent of tenants have extremely low income.[3] However, whether LIHTC directly targets and affects households at risk of homelessness is unclear and needs to be addressed empirically.

2.3 Data

I describe various data sources I use to estimate the effect of LIHTC on homelessness.

2.3.1 LIHTC Data

HUD compiles a list of properties receiving LIHTC since the policy's inception in 1987. For each property, available data include the name of the property, credit type, allocated credit amount, property street address, application cycle year, placed-in-service year, property status (i.e. active or inconclusive), number of units, number of low-income units, developer type, construction type, and target tenant type. The LIHTC database is aggregated from data provided by each state's housing authority which inevitably includes entry errors and missing data problems. I verify and incorporate state administrative data whenever possible.[4] A total of 35,132 properties have received some form of the tax credit from the LIHTC policy since 1987, resulting in nearly

[3] *Extremely-low-income* household is defined as a household earning 30 percent or less of Area Median Gross Income; *very-low-income* household earning 30 to 50 percent of AMGI; *low-income* household earning 50 to 80 percent of AMGI; *moderate-income* household earning 80 to 120 percent of AMGI; *middle-income* household earning 120 to 165 percent of AMGI.

[4] Some state housing authorities publicly share historical LIHTC inventories on their websites. For states that do not have the inventory listed publicly, I submitted Freedom of Information Act (FOIA) requests to the agencies' FOIA Offices.

3.44 million newly constructed or rehabilitated rental units, of which 3.12 million are reserved for income-eligible households. Figure 2.2 shows annual counts in total funded developments and units.

Table 2.1 provides summary statistics of the LIHTC properties. The table is split into two columns – the first column includes all properties allocated tax credits (i.e. both active and inconclusive properties), and the second column includes only active properties. There are mainly two reasons why a building could be inconclusive. The first reason is if the compliance period of the property has passed, and the second reason is if the end date of the compliance period is missing. Around 95 percent of properties are currently active.

Around 61 percent of properties were newly constructed, and 36 percent were acquired or rehabilitated. Of the active LIHTC developments, 54 percent receive the 9 percent credit, 33 percent receive the 4 percent credit, and 13 percent receive both types. Over half of the developers are for-profit, and 24 percent of the developers are non-profit organizations. Most target tenant types are income-eligible families, elderly, and disabled households.

2.3.2 Homeless Data

There is a wide range of definitions for homelessness across different government agencies (Evans et al., 2019). In this paper, I use the definition from HUD. Under the Homeless Emergency Assistance and Rapid Transition to Housing (HEARTH) Act of 2009, HUD defines homelessness as individuals or households who are: a) living in a place that is not meant for habitation, which includes emergency shelters and transitional housing; b) expecting to lose their residence within fourteen days, including people living in doubled-up arrangements; c) families with children that are unstably housed; d) people fleeing domestic violence.

Under the McKinney-Vento Act in 1987, the Continuum of Care (CoC) Program was established to promote community-wide commitment to ending homelessness. Regions are divided into Continuums of Care, which are local planning bodies coordinating housing and services funding for homeless families and individuals. There are roughly 390 CoCs across all US states and territories (see Figure 2.3). The function of the CoC Program is to provide stable dwelling and job training programs to individuals and families experiencing homelessness. Each CoC submits a grant proposal annually to compete for federal grants. Once each CoC receives its allocated grants, the CoC board of directors then allocates the funding to different homeless programs within the CoC.

Since 2007, HUD has required each CoC to estimate the sheltered and unsheltered homeless population yearly to better understand and serve households experiencing homelessness. Three reports are compiled and published annually: the Point-in-Time (PIT) Report, the Housing Inventory Count (HIC) Report, and the CoC Homeless Populations and Subpopulations Reports. The PIT Report is a panel dataset that records counts of sheltered and unsheltered homeless individuals and the basic demographics of the population. Unsheltered counts are conducted annually on a single night in January, where surveyors visit every high-density public space and a statistically valid sample of medium and low-density areas. The HIC Report documents the inventory of sponsored programs annually. In particular, the report records counts of beds and units available for homeless individuals. The CoC Homeless Populations and Subpopulations Reports are detailed demographic synopses for each CoC. The reports provide summary statistics of household types, ethnicity, gender, and race. Counts of severely mentally ill, chronic substance abuse, veterans, HIV/AIDS, victims of domestic violence, unaccompanied youth, parenting youth, and children of parenting youth are also recorded in the report.

The main outcome variables in this paper are from the data in the PIT Reports. Each CoC is required to provide an accurate count of homeless individuals according to the HUD standard based on the Point-in-Time Count Methodology Guide. Available data in the PIT Reports include overall homeless counts, sheltered homeless counts, unsheltered homeless counts, sheltered/unsheltered homeless individuals, sheltered/unsheltered homeless people in a family, chronically homeless, homeless veterans, and homeless children (under 18). Trends of homeless counts are shown in Figure 2.4, and trends by race are shown in Figure 2.A.3.[5] There is a general downward trend in people experiencing homelessness. However, the annual counts are still above 500,000 individuals each year.

Jurisdictional boundaries of CoCs are relatively fixed, but boundaries still change due to local strategic planning and funding decisions over the years. Each county can only be claimed by one CoC in a given year to avoid double-funding (HUD, 2009). To ensure the panel data are comparable from year to year over the studying period, I create *Super CoCs*, where I group CoCs sharing the same counties across years into one unit of analysis. Figure 2.5 illustrates an example of Super CoCs grouping for Washington State. Panel (a) shows the jurisdictional CoC boundaries in 2017, and Panel (b) shows the boundaries in 2018. In 2017, Yakima City and County was assigned to CoC WA-507. However, in 2018, the county was grouped into WA-501, the Washington Balance of State Continuum of Care. To mitigate the discrepancy across years, I include Yakima County in WA-501 for all years and exclude WA-507 from the analysis. After aggregating, there are 351 Super CoCs across all years, which I loosely call "CoCs" in the remainder of the paper.

2.3.3 ACS and Census Data

I acquire county-level characteristics data from the 2010 Census and the American Community Survey (ACS) five-year estimates. Variables of interest include demographics such as population counts, population density, poverty rate, unemployment rate, race composition, gender composition, and marriage rate for each county. Households and housing measures such as the number of households, average family size, number of occupied housing units, median rent, and homeownership rate in each county are also of interest. The ACS five-year estimates are only available starting in 2009, so I limit the analysis to starting from 2009. All county-level

[5] An individual is considered chronically homeless if the individual has a disability and has lived in a shelter, safe haven, or place not meant for human habitation for 12 continuous months or four separate occasions in the last three years (must total 12 months).

characteristics obtained from the ACS data are aggregated to the Super CoC level using population weights.

2.4 Empirical Strategy

The objective is to identify the effect of LIHTC developments on homelessness. Due to data availability from merging various sources, the unit of analysis is the Super CoC-year level, and the analysis period is from 2009 to 2019.

Ideally, I want to estimate how homeless per capita is affected by the stock of LIHTC units per capita:

$$HomelessPerCapita_{ct} = \beta StockPerCapita_{ct} + \mathbf{X}_{ct}\Phi + \lambda_t + \mu_c + \varepsilon_{ct}, \tag{2.1}$$

where $HomelessPerCapita_{ct}$ is the homeless count in CoC c and year t divided by the population in CoC c, $StockPerCapita_{ct}$ is the stock of low-income units receiving LIHTC divided by the population in CoC c, $\mathbf{X}_{ct}$ is a vector of control variables that vary across CoC and year, λ_t is a year fixed effect common to all CoCs in year t, μ_c is a time-invariant fixed effect unique to CoC c, and ε_{ct} is the error term. An increase in LIHTC units in a given region could attract more households to migrate to the region, which leads to an increase in population counts. To prevent any biases caused by population changes due to LIHTC availability, I fix CoC population counts for all years to the CoC population counts in 2010.

Since only awarded LIHTC properties are recorded in the database, stock data of LIHTC properties and units are not available. Instead, I use the "flow" of LIHTC units and estimate the following first-differenced estimating equation:

$$\frac{\Delta Homeless_{ct}}{Population_{c,2010}} = \beta\left(\frac{Flow_{ct}}{Population_{c,2010}}\right) + \Delta\mathbf{X}'_{ct}\Phi + \Delta\lambda_t + \Delta\varepsilon_{ct}, \tag{2.2}$$

where $\Delta Homeless_{ct} = Homeless_{ct} - Homeless_{c,t-1}$, and $Flow_{ct}$ is the newly awarded LIHTC units in CoC c in year t. In the remainder of the paper, I denote the left-hand-side variable as $\Delta HomelessPerCapita_{ct}$ and the variable of interest as $FlowPerCapita_{ct}$. The parameter of interest is β, which is the effect of the flow of LIHTC units on the change in homeless per capita.

For the first-difference (FD) estimator to be unbiased, the strict exogeneity assumption $\mathbb{E}[\varepsilon_{ct}|x_{c1}, \ldots, x_{cT}] = 0$ is required (Wooldridge, 2010, p.316). In words, the error term is independent of the explanatory variables in Equation 2.2. A common reason that strict exogeneity is violated is omitted variable bias. The first-differencing transformation eliminates the unobserved time-invariant heterogeneity term μ_c, which minimizes the risk of confounding (Allison, 2009). However, the FD estimator does not prevent time-varying factors. I include relevant covariates to reduce the effect of omitted variable bias. Following Equation 2.2, I include variables in changes (instead of levels). Specifically, I include four sets of control variables: changes in economic characteristics, demographic characteristics, housing characteristics, and grant characteristics. Economic characteristics include poverty rate, unemployment rate, and the 60 percent area median gross income; demographic characteristics include race composition, percent female, and percent married; housing characteristics include homeowner rate and housing vacancy rate; grant characteristics include the monetary amount of CoC awarded.[6]

Another threat to strict exogeneity is reverse causality, which is when changes in the dependent variable cause changes in the independent variable (Wooldridge, 2010). LIHTC funding and placements are determined by QAPs, and areas of concentrated poverty (Qualified Census Tracts) receive thirty percent more tax credits (Baum-Snow and Marion, 2009; Ellen et al., 2018). LIHTC developments are often built in areas with a high need for affordable housing, which may include areas with high rates of homelessness. However, it is not clear whether homelessness is a determinant in siting decisions.

[6]The allocation of CoC grant funding is based on a formula that takes into account several factors, including the geographic area covered by the CoC, the number of homeless individuals and families in the area, and the performance of the CoC in providing services to homeless individuals and families. CoC grant funding is likely to be a confounding variable that directly affects the homeless counts in the area.

A common approach to determine reverse causality is the Granger causality test (Granger, 1969). The Granger causality test is a statistical method that helps to determine whether a time series is useful for forecasting another time series. The general idea is that if variable y causes variable x, the past values of y should provide information for predicting x even if past values of x were already controlled for. The Granger causality test requires the time series to be stationary, i.e. constant mean and variance, and no seasonal components. I test for reverse causality by estimating the following model:

$$\Delta x_{c,t} = \sum_{i=1}^{T} \alpha_i \Delta x_{c,t-i} + \sum_{i=1}^{T} \beta_i \Delta y_{c,t-i} + \varepsilon_{ct}. \tag{2.3}$$

The null hypothesis is that given the past values of x, the past values of y do not provide any additional information in predicting x. That is, I test the hypothesis that $\beta_1 = \cdots = \beta_T = 0$.

2.5 Results

Table 2.2 reports the main results from estimating Equation 2.2. The variable of interest is newly awarded LIHTC units in CoC per capita. The outcome variables are changes in homeless per capita for overall, sheltered, unsheltered, children (under 18), veterans, and chronically homeless. I control for year-fixed effects and time-variant CoC characteristics, including changes in economic, demographic, housing, and grant characteristics.[7] Column (1) reports the FD estimate on overall homeless counts, where I control for time-fixed effects and CoC characteristics. The coefficient for flow per capita is -1.107, statistically significant at the 0.01 level. In words, one additional LIHTC unit results in an expected decrease of 1.1 homeless individuals.

[7] Table 2.A.1 presents various specifications of changes in overall homeless per capita – no controls, with economic characteristics, with demographic characteristics, with housing characteristics, and with grant characteristics. The magnitudes and significance are similar across specifications. The adjusted within R-squared increases as more controls are included in the model, which is expected as the added control variables explain more of the variance in the dependent variable. In the remaining analysis, I include all four sets of characteristics in the regressions, which I label as *CoC characteristics*.

When the homeless population is split into sheltered and unsheltered homeless, the coefficient for sheltered homeless is -1.001 and statistically significant at the 0.01 level (column (2)), whereas the coefficient for unsheltered homeless is not statistically significant (column (3)).[8] This suggests that the increase in additional LIHTC units per capita is associated with a decrease in sheltered but not unsheltered homeless. One explanation could be that LIHTC provides affordable housing options that are more likely to target people who would otherwise be in shelters. It is also possible that other factors beyond the availability of affordable housing drive unsheltered homelessness.

Column (4) shows a coefficient of -1.207 for children under 18, which means an additional LIHTC unit results in an expected decrease of 1.2 homeless children. Note that the data for children experiencing homelessness is only available from 2013 to 2019. Compared to the change in overall homeless counts in Column (1), an increase in an additional LIHTC unit per capital has a more substantial effect on reducing children's homelessness than the overall homeless population. It is possible that LIHTC units are more targeted toward families and hence have a more significant impact on reducing homelessness for families with children.

Columns (5) and (6) show a coefficient of 0.143 for veterans experiencing homelessness and -0.137 for individuals experiencing chronic homelessness. The coefficients are small in magnitude due to the lower baseline counts for the two subgroups. The positive coefficient in column (5) indicates that the LIHTC policy is ineffective for veterans experiencing homelessness. Nevertheless, LIHTC units are relieving chronic homelessness in some capacity.

The main results show that children benefit more from the new LIHTC units than the overall homeless population. Priorities for LIHTC units depend on the QAP in each state, but I test whether the policy favors people in families in the homeless context. I test for heterogeneity for individuals versus people in a family, where *individuals* include households with a single

[8]Note the adjusted within R^2 for the unsheltered homeless model is only 0.034, which is relatively low compared to other models. This indicates that the covariates only explain a small portion of the variation in the changes in unsheltered homeless per capita.

member and *people in family* include households with more than one member. Table 2.3 displays results for estimating Equation 2.2.

The coefficient for flow per capita is -0.441 for *individuals* and -0.654 for *people in family*, both statistically significant at the 0.01 level. Comparing the magnitudes of the two coefficients suggests that the policy tends to benefit larger households. LIHTC properties give preference to families with children – 55 percent of units are targeted towards families (Table 2.1), which can make it harder for single individuals to access the units. Furthermore, LIHTC units typically have more than one bedroom, making them better suited for families with children. Single individuals may not need or want the extra space. Nonetheless, only the sheltered populations benefit from new LIHTC units for both individuals and people in a family.

I estimate whether relevant outcome variables in Table 2.2 Granger cause the variable of interest $FlowPerCapita$. The results from estimating Equation 2.3 with three lagged periods are presented in Table 2.A.2. I present three specifications for each subgroup: without year fixed effects and CoC characteristics, with year fixed effects, and with year fixed effects and CoC characteristics. The numbers in each column indicate the estimated coefficients, and the numbers in parentheses indicate the standard errors. After controlling for past values of $FlowPerCapita$, I test whether the coefficients for Δy_{t-1}, Δy_{t-2}, and Δy_{t-3} are jointly zero. P-values are reported for the joint tests.

Overall, the table reports that the lagged values of $FlowPerCapita$ are significant in explaining the current value of $FlowPerCapita$, while the lagged values of homeless variables are not significant. I do not find evidence that homelessness is an essential determinant for new LIHTC units, so I can rule out reverse causality in the model.

2.6 Conclusion

Understanding how place-based housing subsidy affects households at risk of homelessness is an important research question. However, it is challenging to answer due to the endogeneity of low-income housing placements and the limitation on data availability. In this paper, I study the effect of the Low-Income Housing Tax Credit on homelessness. Combining the LIHTC database and the HUD Point-in-Time (PIT) Reports, I find that an additional LIHTC unit is associated with a decrease of 1.1 individuals experiencing homelessness. The effects are particularly significant for families and children in shelters. However, the findings suggest that new LIHTC units do not benefit unsheltered homeless people.

The policy is designed to provide affordable housing for low-income individuals and families. However, it may not address the needs of unsheltered homeless people who lack a stable home. One reason LIHTC may not be effective in addressing unsheltered homelessness is that it primarily targets individuals or families with a source of income. Often, unsheltered homeless individuals may not have any income, making it difficult to qualify for LIHTC-funded housing. Additionally, even if they qualify, they may face other barriers to accessing affordable housing, such as poor credit history or lack of rental history. To effectively address unsheltered homelessness, a more comprehensive approach that combines affordable housing with wraparound services such as mental health and addiction treatment, employment assistance, and case management may be needed. Such an approach would require significant investment and coordination across multiple sectors, including housing, healthcare, and social services.

2.7 Acknowledgements

Chapter 2, "The Effect of Low-Income Housing Tax Credit on Homelessness," is currently being prepared for submission for publication of the material. The dissertation author was the sole author of the chapter.

2.8 Figures and Tables

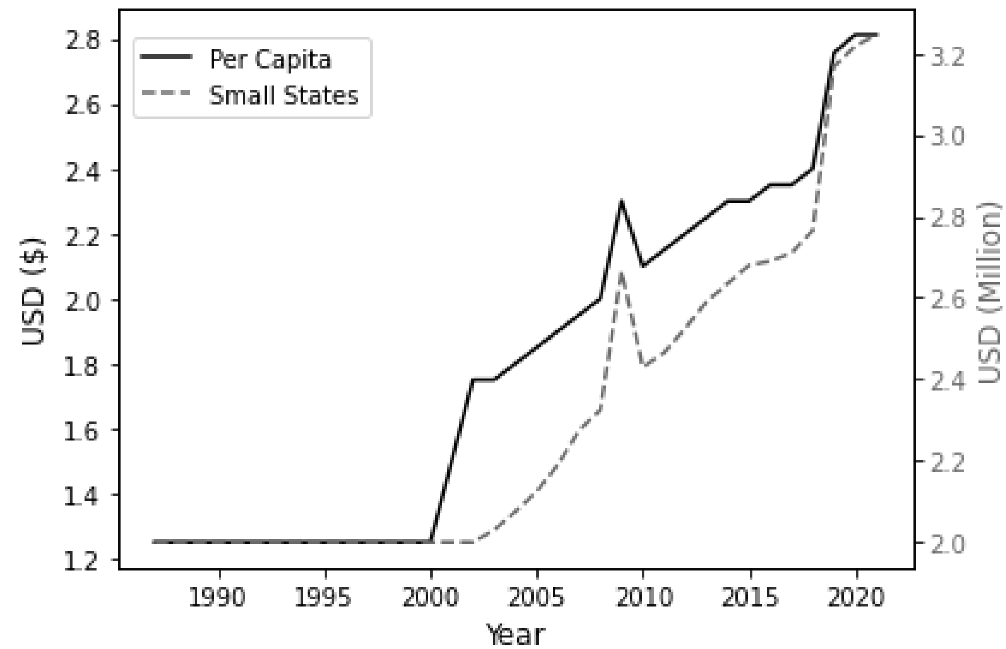


Figure 2.1. Historical LIHTC Credit Allocation.

Notes: Figure depicts historical LIHTC credits allocation rule. The black line depicts credit per capita for larger states from 1987 to the present, with the credits amount on the left axis. The dotted gray line depicts small state credits with the credits amount on the right axis.

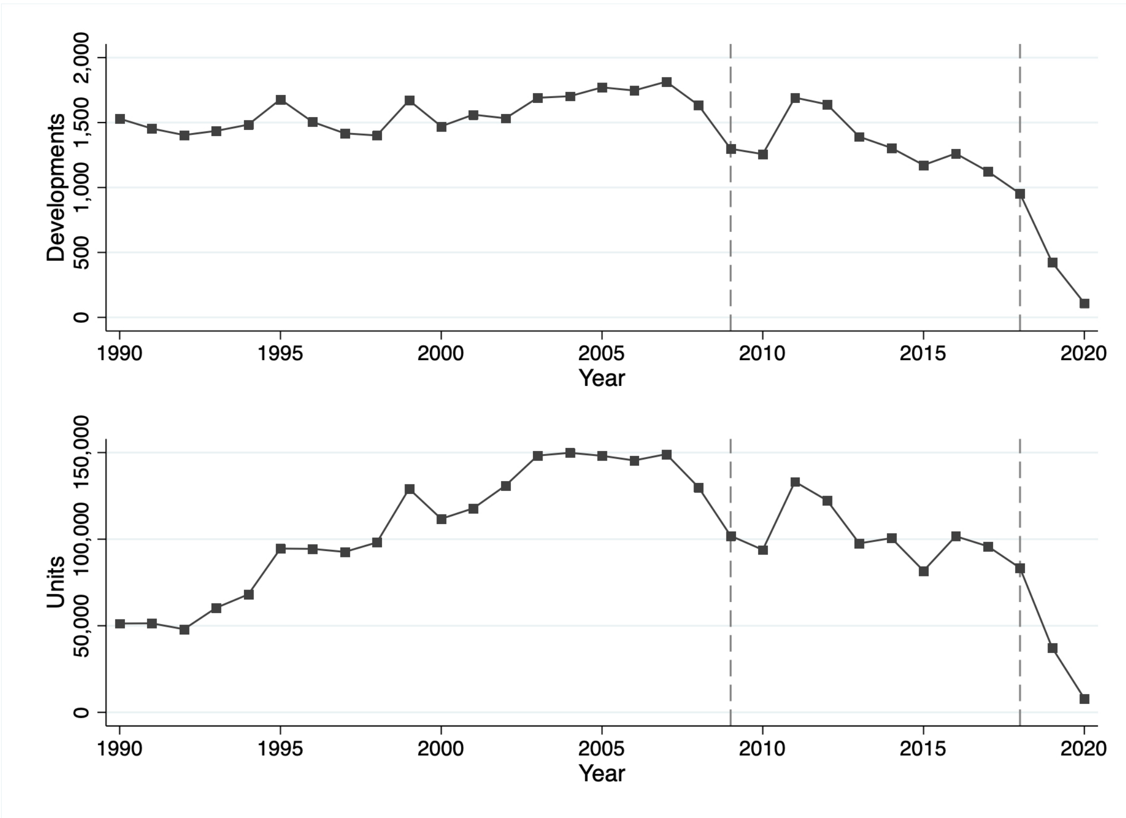


Figure 2.2. Annual LIHTC Developments and Units Placed In Service.

Notes: This figure depicts LIHTC trends since 1990, with dashed lines marking the study period for this study. The upper panel shows the total number of developments placed in service each year, and the lower panel shows the total number of units placed in service each year. On average, around 1,500 developments receiving LIHTC are placed-in-service each year, resulting in around 107,000 units.

Figure 2.3. Continuum of Care Map.

Notes: The figure shows the map of the Continuum of Cares (CoCs) in 2019. The black lines depict the jurisdictional boundaries of CoCs, and the gray lines depict the boundaries of counties. There were a total of 391 CoCs in 2019. The CoC boundaries could change yearly due to local strategic planning and funding decisions. A county can only be claimed by one CoC in a given year.

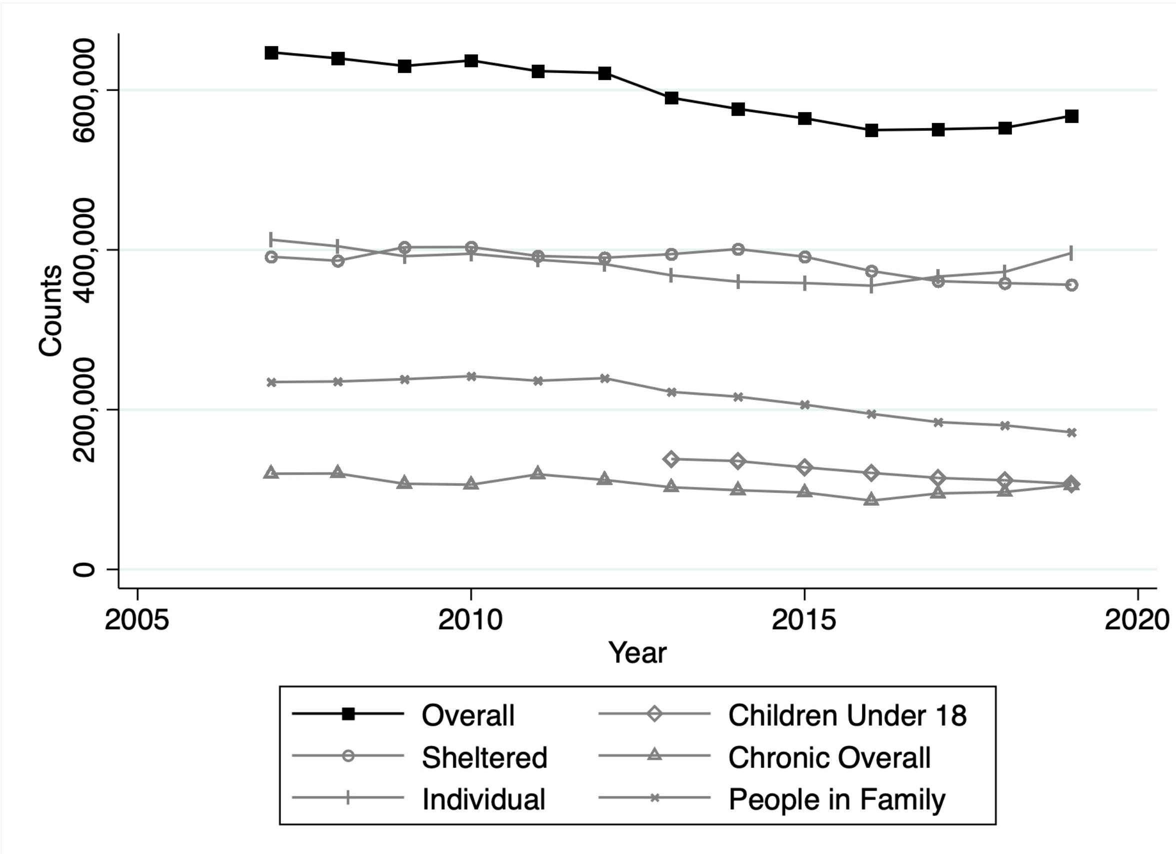


Figure 2.4. Homeless Trends in the US.

Notes: The figure shows trends in the number of people experiencing homelessness in the United States from 2007 to 2019. *Overall* includes both sheltered and unsheltered households. *Children Under 18* includes individuals under 18 years old, accompanied by an adult or unaccompanied. *Sheltered* includes individuals residing in homeless emergency shelters, transitional housing, or safe haven. *Chronic Overall* includes individuals with a disability who have lived in a shelter, safe haven, or place not meant for human habitation for 12 continuous months or four separate occasions in the last three years (must total 12 months). *Individual* includes counts of single-member households. *People in family* include all individuals in households with more than one member. Data is obtained from the HUD Point-in-Time (PIT) Reports and Continuum of Care (COC) Homeless Populations and Subpopulations Reports.

(a) Washington State, 2017.

(b) Washington State, 2018.

Figure 2.5. Creating Super CoC.

Notes: Figures demonstrate the creation of Super CoCs in Washington state. Panel (a) shows the jurisdictional boundaries of CoCs in Washington State in 2017, and Panel (b) shows the boundaries of CoCs in 2018. As WA-507 (Yakima City and County CoC) was merged into WA-501 (Washington Balance of State CoC), I combined WA-507 and WA-501 into one *Super CoC* across all periods in the analysis. The boundaries of WA-500, WA-502, WA-503, WA-504, and WA-508 did not change during the study period, so they each consist of a CoC.

Table 2.1. Summary Statistics of LIHTC Properties.

	All	Active
A. Status		
Active	0.9189	1
Inconclusive	0.0811	0
B. Construction Type		
New Construction	0.6170	0.6163
Acquisition and Rehabilitation	0.3613	0.3618
Both	0.0217	0.0219
C. Credit Type		
4% Tax Credit	0.3255	0.3275
9% Tax Credit	0.5430	0.5404
4% and 9% Tax Credit	0.1315	0.1321
D. Developer Type		
For-Profit	0.5430	0.5367
Non-Profit	0.2346	0.2369
Multiple	0.2102	0.2141
Public Entity	0.0122	0.0123
E. Target Tenant Type		
Family	0.5530	0.5494
Elderly	0.0960	0.0973
Disabled	0.0028	0.0028
Elderly or disabled	0.2476	0.2493
Mixed	0.0991	0.0996
Special Needs	0.0015	0.0015
Number of Properties	35,132	33,395
Number of Units	2,696,901	2,588,182
Number of Low-Income Units	2,452,717	2,349,947

Notes: This table provides summary statistics of properties that have received LIHTC since 1987. The left column records all properties, and the right column records active properties currently rented to tenants at subsidized prices. Data is obtained from HUD's LIHTC database and available files from FOIA requests.

Table 2.2. Effects of LIHTC on Homelessness.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Overall	Sheltered	Unsheltered	Children	Veterans	Chronic
$FlowPerCapita$	-1.107***	-1.001***	-0.095	-1.207***	0.143***	-0.137***
	(0.280)	(0.272)	(0.060)	(0.385)	(0.034)	(0.050)
Adjusted R^2	0.307	0.518	0.204	0.593	0.419	0.144
Observations	3,102	3,102	3,102	3,102	3,102	3,102
Number of CoCs	347	347	347	347	347	347
Controls						
Year Fixed Effects	X	X	X	X	X	X
CoC Characteristics	X	X	X	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the regression estimates of Equation (2.2). Each column represents a different dependent variable: overall, sheltered, unsheltered, children under 18, veterans, and chronically homeless. The dependent variables are the changes in each variable per capita. The variable of interest, $FlowPerCapita$, is defined as the flow of LIHTC units per capita. CoC characteristics include time-varying economic, demographic, housing, and grant characteristics. Standard errors are clustered by CoC, and the adjusted R^2's are reported.

Table 2.3. Individual versus Family Effects of LIHTC on Homelessness.

	Individuals			People In Family		
	(1) Overall	(2) Sheltered	(3) Unsheltered	(4) Overall	(5) Sheltered	(6) Unsheltered
FlowPerCapita	-0.441***	-0.363***	-0.078	-0.654***	-0.637***	-0.017
	(0.101)	(0.112)	(0.061)	(0.181)	(0.161)	(0.028)
Adjusted R^2	0.277	0.325	0.248	0.387	0.547	0.103
Observation	3,102	3,102	3,102	3,102	3,102	3,102
Number of CoCs	347	347	347	347	347	347
Controls						
Year Fixed Effects	X	X	X	X	X	X
CoC Characteristics	X	X	X	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows regression estimates of Equation (2.2). *Individuals* include households with a single member, and *people in family* include households with more than one member. Please see the notes to Table 2.2.

2.9 Appendix Figures and Tables

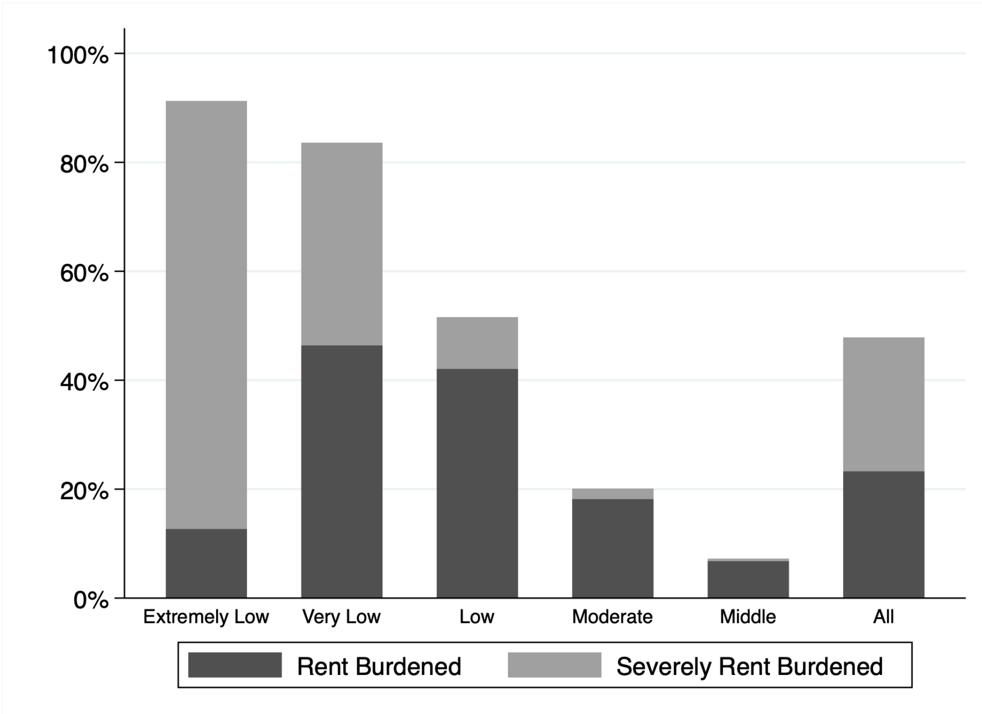


Figure 2.A.1. Percent Rent-Burdened Households by Income Status in 2018

Notes: The figure shows the percentage of rent-burdened and severely rent-burdened households by income status in 2018 in the US. A household is considered *rent-burdened* if it spends more than 30 percent of household income for housing and *severely rent-burdened* if it spends more than 50 percent of household income on housing. *Extremely low-income* household is defined as a household earning 30 percent or less of Area Median Income (AMI); *very low-income* household earning 30 to 50 percent AMI; *low-income* household earning 50 to 80 percent AMI; *moderate-income* household earning 80 to 120 percent AMI; *middle-income* household earning 120 to 165 percent AMI. AMI is defined by HUD annually and differs by household size; the 2018 NYC AMI for a household of four is $104,300. Data is obtained from 2018 ACS one-year estimates.

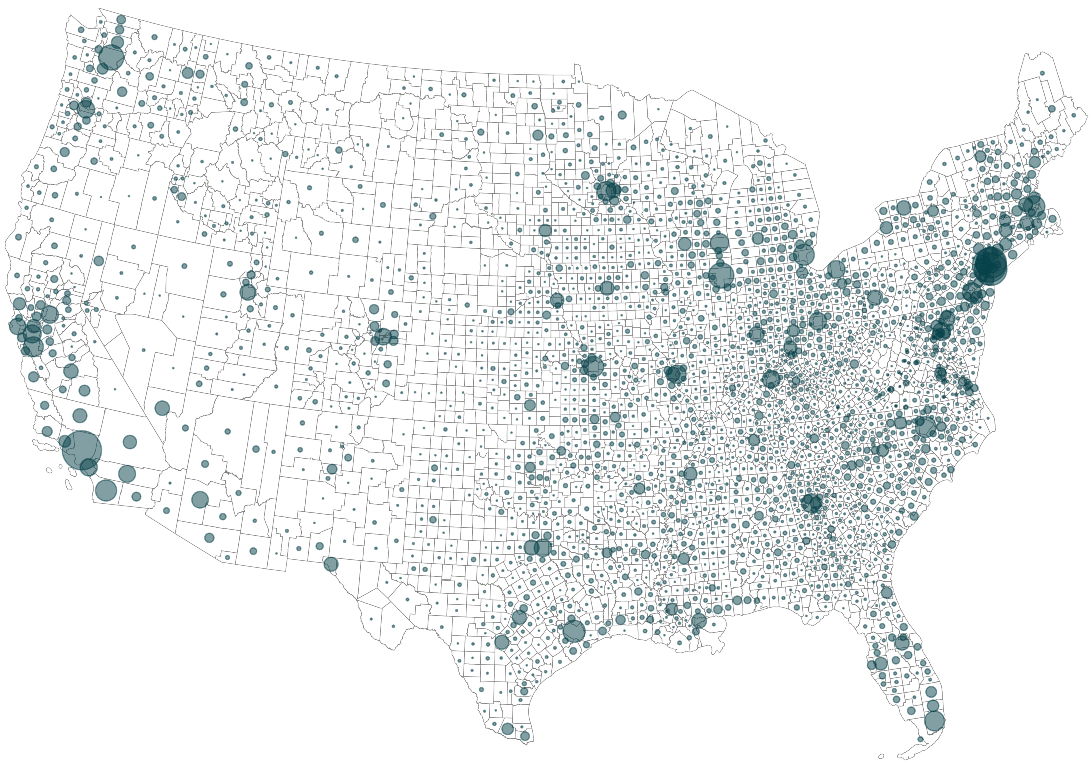


Figure 2.A.2. Map of LIHTC properties.

Notes: The figure depicts the spacial distribution of LIHTC properties by county. The size of the circles on the map represents the density of LIHTC properties in each county. Larger circles indicate a higher density of LIHTC properties, while smaller circles represent a lower density.

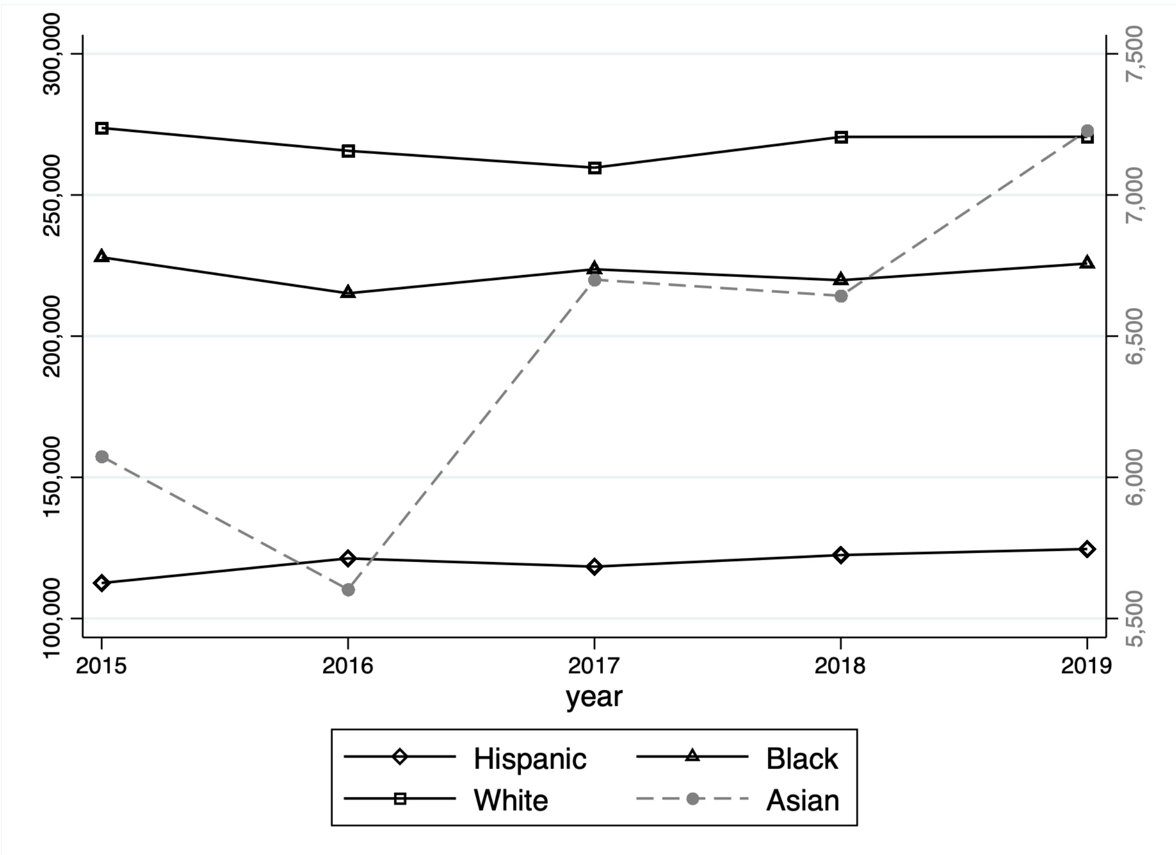


Figure 2.A.3. Homeless Trends in the US by Race.

Notes: The figure depicts homeless trends by race from 2015 to 2019. Hispanic, Black, and White homeless counts follow the left axis labels; Asian homeless counts follow the right axis labels. Data is obtained from Continuum of Care (COC) Homeless Populations and Subpopulations Reports.

Table 2.A.1. Effects of LIHTC on Overall Homelessness.

	(1)	(2)	(3)	(4)	(5)	(6)
FlowPerCapita	-0.9652***	-1.0114***	-1.1243***	-1.1096***	-1.1055***	-1.1068***
	(0.3480)	(0.3331)	(0.2733)	(0.2630)	(0.2596)	(0.2805)
Adjusted R^2	0.138	0.166	0.243	0.259	0.266	0.307
Observation	3,786	3,786	3,102	3,102	3,102	3,102
Number of CoCs	347	347	347	347	347	347
Controls						
Year Fixed Effects		X	X	X	X	X
Economic Characteristics			X	X	X	X
Demographic Characteristics				X	X	X
Housing Characteristics					X	X
Grant Characteristics						X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table contains regression results with six different specifications. Economic characteristics include poverty rate, unemployment rate, and the 60 percent area median gross income; demographic characteristics include race composition, percent female, and percent married; housing characteristics include homeowner rate and housing vacancy rate; grant characteristics include the monetary amount of CoC awarded. Please see the notes to Table 2.2.

Table 2.A.2. Granger Causality Tests.

	Dependent variable: $FlowPerCapita_t$											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Δy:		Overall			Sheltered			Children			Chronic	
$FlowPerCapita_{t-1}$	0.418***	0.406***	0.349***	0.457***	0.451***	0.396***	0.450***	0.445***	0.395***	0.451***	0.446***	0.395***
	(0.103)	(0.103)	(0.098)	(0.130)	(0.132)	(0.138)	(0.134)	(0.135)	(0.139)	(0.133)	(0.135)	(0.139)
$FlowPerCapita_{t-2}$	-0.077	-0.078	-0.093	-0.062	-0.059	-0.072	-0.058	-0.055	-0.070	-0.059	-0.056	-0.070
	(0.121)	(0.122)	(0.112)	(0.117)	(0.118)	(0.111)	(0.117)	(0.117)	(0.112)	(0.116)	(0.117)	(0.111)
$FlowPerCapita_{t-3}$	0.034	0.026	-0.005	0.039	0.036	0.012	0.038	0.035	0.013	0.038	0.036	0.013
	(0.066)	(0.066)	(0.052)	(0.078)	(0.078)	(0.068)	(0.077)	(0.077)	(0.068)	(0.077)	(0.077)	(0.068)
Δy_{t-1}	-0.062	-0.064	-0.050	-0.005	-0.004	0.018	0.101	0.090	-0.044	0.046	0.042	-0.004
	(0.046)	(0.047)	(0.046)	(0.025)	(0.025)	(0.020)	(0.128)	(0.133)	(0.079)	(0.059)	(0.060)	(0.031)
Δy_{t-2}	0.012	0.009	0.006	-0.062	-0.060	-0.060	-0.099	-0.111	-0.116	-0.025	-0.027	-0.033
	(0.064)	(0.063)	(0.053)	(0.048)	(0.048)	(0.045)	(0.159)	(0.158)	(0.165)	(0.055)	(0.054)	(0.057)
Δy_{t-3}	0.010	0.006	-0.011	0.046	0.041	0.037	0.225	0.213	0.133	0.061	0.056	0.038
	(0.062)	(0.062)	(0.042)	(0.045)	(0.045)	(0.036)	(0.282)	(0.287)	(0.188)	(0.090)	(0.091)	(0.064)
Adjusted R^2	0.339	0.349	0.398	0.333	0.340	0.388	0.329	0.336	0.385	0.328	0.335	0.385
Observations	2,722	2,722	2,722	2,722	2,722	2,722	2,722	2,722	2,722	2,722	2,722	2,722
Number of CoCs	347	347	347	347	347	347	347	347	347	347	347	347
Joint Test	0.173	0.181	0.168	0.545	0.596	0.547	0.618	0.906	0.435	0.631	0.787	0.456
Controls												
Year Fixed Effects		X	X		X	X		X	X		X	X
CoC Characteristics			X			X			X			X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the regression estimates of Equation (2.3). The dependent variable is the $FlowPerCapita$. Controlling for the lagged values of $FlowPerCapita$, I check whether changes in overall, sheltered, children, and chronically homeless and the corresponding lagged values Granger-causes $FlowPerCapita$. Adjusted within R^2 is shown in the table. The joint test shows the p-value of whether the coefficients for Δy_{t-1}, Δy_{t-2}, and Δy_{t-3} are jointly zero for each column.

Chapter 3

The Impact of Right to Counsel to the Poor: Evidence from New York City Housing Courts

3.1 Introduction

Access to the formal justice system and counsel has been a right for civilians in the United States. The Sixth Amendment and the landmark Supreme Court case *Gideon v. Wainwright (1963)* guarantee the right to counsel in criminal cases to defendants who cannot afford their attorneys. The right to counsel has also been extended to civil cases, including family law matters, medical treatment, and other types of cases over the past few decades (Abel and Rettig, 2006). However, the right to counsel in housing courts was lacking until recently.

In August 2017, New York City (NYC) Mayor Bill de Blasio signed the right to counsel bill (Intro. 214-B) into law. After years of battle, NYC became the first city in the US to provide the right to counsel in housing courts. The bill requires the Office of Civil Justice (OCJ) to establish programs to provide all tenants facing eviction with access to legal services within five years. Tenants with household incomes below 200 percent of the federal poverty line (FPL) receive full legal representation, while other tenants receive brief legal assistance.[1] This policy is an advancement for low-income households to more fairly access the formal justice system.

In this paper, I study the causal effect of the right-to-counsel (RTC) policy on evictions in NYC. Specifically, I assess changes in landlords' eviction filing rates, tenants' court appearance and legal representation rates, and overall eviction rates. The policy started being phased in on the zip code level in 2018 and was fully implemented citywide by May 2021. Utilizing the NYC Housing Court data and taking advantage of the staggered rollout schedule, I estimate a difference-in-differences model that compares outcomes in zip codes where tenants are provided free legal counsel to zip codes that have not yet implemented the policy. For the identification strategy to be valid, the timing of policy implementation has to be uncorrelated with trends at the zip code level, and I verify this assumption in several ways.

Eviction, the process of removing a tenant household from a rental property, is a concerning issue for many low-income households in the US. In 2016, more than six percent of renter households were threatened with an eviction notice, and 2.34 percent were indeed evicted from their residence (Desmond et al., 2018). Over the past three decades, real household incomes remained relatively flat while median asking rents skyrocketed, leaving many renter households with little resources to combat day-to-day life. Figure 3.1 shows that real median asking rent has grown over forty percent, whereas median household income has grown only ten percent since 1989. Around 48 percent of households in the US were rent-burdened or severely rent-burdened in 2018.[2] Tenants' financial hardships paired with landlords' profit-seeking behavior put tenants facing eviction in a vulnerable position in court.

Availability of attorneys should lead to decreases in evictions for tenants, assuming there

[1]The federal poverty levels depend on family size and are updated annually. For reference, the 2018 FPL is $12,140 for a family of one and $25,100 for a family of four. A complete schedule of the FPL can be found at https://aspe.hhs.gov/prior-hhs-poverty-guidelines-and-federal-register-references.

[2]See Figure 3.2 Panel (a). Defined by the US Department of Housing and Urban Development (HUD), a household is considered *rent-burdened* if it spends more than 30 percent of household income on housing and *severely rent-burdened* if it spends more than 50 percent of household income for housing.

do exist wrongful evictions in status quo (Greenberg et al., 2016). The RTC policy requires tenants to appear in court to receive legal counseling services. As more than fifty percent of tenants facing eviction did not show up in court in 2017 in NYC, they might be unaware of the program. Even if tenants are informed of the program, i.e. from their social network or online information, they might still not take up free legal counsel as tenants believe they are in the wrong and have no chance of winning with or without professional involvement. Furthermore, if the provided attorneys are not as equipped as landlords' attorneys, chances for tenants to win their cases remain low. Whether the RTC policy has any short-term effects on eviction rates is unclear.

In my analysis, I find that eviction filings remain relatively constant after policy implementation. Tenants' court appearances remain constant, while their representation increases by 54.3 percent. Furthermore, I observe a 16.9 percent decrease in evictions in the study period. These findings suggest that the RTC policy does not change landlords' and tenants' behaviors in the short run, but the decrease in evictions indicates the effectiveness of tenants' court representation. However, an event-study style analysis implies the effect of RTC on evictions is delayed. One potential explanation is that the RTC policy is only effective when other pro-tenant policies are implemented simultaneously. More specifically, the Housing Stability and Tenant Protection Acts of 2019 (HSTPA), passed in New York State in June, provides more protection for tenants in the rental and eviction processes. New rights for tenants in HSTPA would include more time during the court process, disposal of non-payment proceedings if all back rent was paid before the eviction date, and limitation of landlords requesting non-rent charges in non-payment proceedings. The RTC policy, coupled with HSTPA, provides compelling evidence to decrease eviction in court.

A major caveat of the analysis is that I am unable to account for informal evictions, that is when landlords execute evictions without going through the legal process.[3] It is possible, upon policy implementation, that landlords turn to informal evictions since legally evicting tenants

[3]Some examples of informal evictions include changing the locks, removing tenants' belongings from the property, paying off tenants.

becomes more costly and time-consuming. Unfortunately, such data are not currently available.

The RTC program cost $77 million US dollars in the Fiscal Year 2018 (FY2018) and is projected to cost $155 million in FY2023 according to the NYC Mayor's Office of Management and Budget. Similar to other welfare programs, there are supporters and skeptics of the welfare implications of RTC in housing. Supporters believe that RTC advocates tenant rights and provides neighborhood stability. If tenants are not forced to move out of their residences, they form a sense of belonging to the neighborhood and consequently raise overall neighborhood quality (Poortinga et al., 2017). Decreases in evictions also decrease social costs, including public housing, shelter, and medical costs (Collinson and Reed, 2018). Contrarily, skeptics argue that the policy only shifts the burden from tenants to landlords. With RTC, landlords are forced to have non-paying tenants in place longer and cannot collect rent from succeeding tenants. Moreover, the policy could have negative long-term impacts on tenants. Landlords could raise tenant screening processes or reduce long-term rental supply to avoid non-paying tenants.[4] Whether the RTC is indeed welfare-improving for society requires a detailed welfare analysis and is beyond the scope of this paper.

This paper contributes to three strands of literature. The first is the literature on access to the formal justice system. There is a growing body of literature on the effectiveness of publicly provided legal services in criminal prosecutions. However, a consensus has yet to be reached. Hoffman et al. (2005) find that public defenders achieve poorer outcomes than their privately retained counterparts, whereas Anderson and Heaton (2012) find that public defenders reduce clients' murder conviction rate by 19 % and reduce overall expected time served in prison by 24 % compared to private pro bono attorneys. Ogletree Jr. and Sapir (2004) argue that the right to counsel is only guaranteed to certain classes of defendants, and the quality of defense is both related to legal factors (i.e. representation standard) and structural factors (i.e. funding availability). Abel and Rettig (2006) provides an overview of effective RTC as policies expand

[4]For example, landlords could convert long-term rental properties to short-term home-sharings such as Airbnb and Vrbo.

from criminal to civil cases, including family law matters, medical treatment, and other types of cases. Kleinman (2003) and Petersen (2020) provide legal arguments for the necessity of the right to counsel in eviction proceedings. However, it was not until 2018 that the policy was implemented in the US. This is one of the first papers to explore the right-to-counsel policy in housing courts.

The second strand is the literature on eviction. Collinson and Reed (2018) construct courtroom leniency measure and use the random assignment of courtroom as an instrumental variable to measure the causal effect of evictions on poverty. The authors find that evictions cause an increase in the risk of homelessness and elevate long-term residential instability, but the results do not suggest that evictions worsen employment outcomes. Humphries et al. (2019) explore the near-universe of Cook County court records and find that eviction negatively impacts credit access and durable consumption for several years. On the other hand, Desmond and Gershenson (2017) explore demographics of evicted tenants and conclude that family size, job loss, neighborhood characteristics, and network disadvantage are significant predictors of eviction, net of missed rental payments and other relevant factors. This paper builds upon previous findings in the literature to explore heterogeneous effects of the RTC policy on zip code eviction rates. I find that zip codes with higher poverty rates respond to the policy more.

Finally, this paper relates to the broader literature on tenants' rights and housing policies. Whether pro-tenant reforms benefit tenants in the short and long term has been an ongoing debate. Dating back to Friedman and Stigler (1946) and Olsen (1972), enactment of rent control is a well-explored topic in the economics literature. More recently, Glaeser and Luttmer (2003) and Diamond et al. (2019) draw to negative impacts of rent control on tenants, including misallocation of housing and limitation of renters' mobility. Similarly, impacts of inclusionary zoning on rental markets have also been studied (Schuetz et al., 2011; Freeman and Schuetz, 2017). This paper adds to the literature on short-term impacts of pro-tenant policy by looking at a novel program introduced in NYC.

The paper is organized as follows. Section 3.2 contextualizes the institutional background of eviction in New York City. Section 3.3 describes the data. Section 3.4 describes the empirical strategy. Section 3.5 presents the results. Section 3.6 concludes.

3.2 Institutional Background

New York City, the most populous and diverse city in the United States, is a pertinent context to study the access to counsel in housing courts for the poor. The city consists of five boroughs: the Bronx, Brooklyn, Manhattan, Queens, and Staten Island (SI), with a total population nearing 8.4 million and a population density of 27,000 people per square mile in 2018. With a growing population and limited housing stock, the city is notorious for its severe shortage of affordable housing. Many households in the area are facing financial hardships due to the high cost of living (Gyourko et al., 2013; Sieg and Yoon, 2020).

Compared to a 64.4 percent homeownership rate in the United States, less than one-third of New Yorkers own their homes in NYC. Furthermore, the rental vacancy rate in NYC is as low as 3.5 percent, compared to a national average of 6.4 percent. In 2018, the median asking rent ranged from $1,625 in parts of the Bronx to over $5,250 in Soho, Manhattan. The citywide median asking rent in 2018 is $2,650, compared to the national median asking rent of around $1,000. Many New Yorkers are financially constrained as a result of high rents. Approximately 85.0 percent of extremely low-income renter households, 79.1 percent of very low-income renter households, 53.9 percent of low-income renter households, and 25.6 percent of moderate-income renter households were moderately or severely rent-burdened in 2018.[5]

It is helpful to point out that NYC is a tenant-friendly city. Besides federal policies, including the Fair Housing Act, fundamental habitation rights, tax credits, and the housing

[5]See Figure 3.2 Panel (b). *Extremely low-income* household is defined as a household earning 30 percent or less of Area Median Income (AMI); *very low-income* household earning 30 to 50 percent AMI; *low-income* household earning 50 to 80 percent AMI; *moderate-income* household earning 80 to 120 percent AMI. Rent-burdened definition in footnote 2.

choice vouchers program (Section 8), NYC also provides rent regulations that protect over one million rent control and rent-stabilized units (Favilukis et al., 2023). Other pro-tenant state-wide legislation, such as the Housing Stability and Tenant Protections Act of 2019 (HSTPA), strives to end high-rent vacancy deregulation and strengthens anti-harassment and tenant protection programs.

Despite various housing policies in place, eviction is still a common occurrence in NYC, especially in low-income neighborhoods. In the remainder of this section, I provide details on the eviction process in NYC and the right-to-counsel policy.

3.2.1 Eviction Process

Eviction is the process where a landlord removes a tenant household from a rented apartment, house, or mobile home. *Formal eviction* occurs when a landlord files an eviction notice through the court system and legally evicts the tenants, whereas *informal eviction* (or *self-help eviction*) happens without legal paper trails. Some examples of informal eviction include paying off tenants, taking the door off, changing locks, removing tenants' belongings to the sidewalk, or turning off essential services.[6] In the Milwaukee Area Renters Study (MARS) conducted in 2011, one of the few sizeable surveys studying renter households, it was observed that informal evictions account for 48 percent of all forced moves and are twice as common as formal evictions (Desmond, 2016; Desmond and Gershenson, 2017). However, informal evictions are not observable since they are illegal. An alternative channel to legally remove a tenant household from a rental unit is through action in ejectment, which is initiated in either Civil or Supreme Court.[7] An ejectment is a process for the owner to obtain possession of an apartment and is generally exercised when there is no preexisting landlord-tenant relationship. In

[6]Changing locks to force a tenant to move is called an *illegal lockout*, and turning off essential services such as water and heat is called a *constructive eviction*. Both of these actions could result in criminal proceedings.

[7]If the assessed value of the rental property is less than $25,000, then the ejectment action is filed in Civil Court. If the assessed value is greater than $25,000, then the action is filed in Supreme Court.

my study, I focus on formal evictions that are carried out in the Housing Court.

There are two types of eviction cases that a landlord can file: non-payment filings and holdover disputes. Non-payment cases occur when tenants fail to pay rent or fees. On the other hand, holdover disputes are filed for reasons other than failure to pay rent or fees. For example, when a tenant sublets illegally, becomes a nuisance to other tenants, participates in criminal activity, or violates the lease. Figure 3.3 depicts the proportion of types of evictions cases from 2016 to 2019. Of all filed landlord-tenant cases in 2016, approximately 71 percent were non-payment cases, and 22 percent were holdover disputes. The remaining seven percent of housing court cases were filed by tenants, which are not discussed in this paper since these cases are not qualified for the RTC policy.[8]

The procedure to file a holdover case is complicated. Depending on the type of tenancy and the nature of the case, different paperwork needs to be served before initiating the case in court.[9] However, there is also more leeway for landlords to dispute in court as such cases are not as straightforward to quantify as non-payment cases. I include both non-payment and holdover cases in the analysis below, but I only describe the process of non-payment cases in the remainder of this section. Figure 3.4 presents a simplified timeline.

A non-payment case can be initiated by the landlord at least five days past the rent due date. The landlord must deliver a written demand to request overdue rents from the tenant and warn the tenant of potential eviction if demand is not met. The tenant is given fourteen days to resolve said issues before the landlord can file a non-payment petition against the tenant in the Housing Court. After filing and paying a $45.00 dollar court fee, the landlord serves the tenant with a Notice of Petition and a Petition.[10] The tenant must appear in court to answer the Petition

[8]Cases filed by tenants fall under three categories: housing part proceeding (HP Action), illegal eviction proceedings, and 7A proceedings. Most tenant-filed cases request the landlord to rectify delinquency in management or repairs.

[9]Some examples of holdover paperwork include Notices to Quit, Notices to Cure a Substantial Violation of the Lease, Notices of Termination, or Notices of Intent Not To Renew a Lease.

[10]Notice of Petition provides the time and location of an obligatory court visit. Petition enumerates names of landlord and tenant, reason for starting the case, and the amount of rent due.

within ten days after the papers are served. The court clerk then set a date for court hearing between three and eight days after the answer.

There are seven courthouses in New York City hearing landlord and tenant cases: one for each borough (the Bronx, Brooklyn, Manhattan, Queens, and Staten Island), and two specialized community courts in Harlem and Redhook. Courthouse assignment for each case is based on the property address being filed for eviction. The Housing Court is separated into Parts, and a Housing Judge is assigned to each Part. Cases filed by landlords will first be assigned to a Resolution Part (more commonly referred to as a courtroom). When a case is called, the landlord and tenant, or their attorneys, will meet with the judge or a court attorney to discuss the case. If the case is related to repairment issues, an inspection request can be granted by the judge and the court date will be rescheduled to a later date. The landlord or tenant can request an adjournment if they do not feel ready for any reason. Both parties are entitled to one adjournment for at least fourteen days. If after discussing the case, the landlord and tenant agree to a settlement of the case, a Stipulation of Settlement (or a "stip") will be written up.[11] If conditions were not met by the promised date, the landlord can file a motion to the court to execute the warrant for eviction. If a settlement cannot be reached between the landlord and tenant at court, the judge approves a trial and reassigns the case to the Trial Part.

A landlord can file for a default judgment under two scenarios. The first is when the tenant fails to answer to the Notice of Petition within ten days of receiving the court order and the rent is still not paid. The landlord may ask the court to enter a judgment and request an eviction warrant. After the judge signs the judgment, the clerk issues the warrant to the marshal, and the marshal proceeds to evict the tenant. The second scenario is when the tenant fails to show up at the appointed time for the court hearing ("no-show"). For no-shows, the landlord may ask the judge to enter a judgment against the tenant, which will normally grant a five-day stay of the issuance of the warrant. If the tenant still fails to respond, the marshal can execute the warrant

[11] *Stipulation* is a binding agreement where both parties agree to do certain things by certain dates.

and carry out the eviction.

Two remarks can be made about the Housing Courts. First, there is a low turn-out rate for tenants in court. In 2016, roughly 67.3 percent of tenants summoned to NYC's Housing Court did not show up – compared with a 0.3 percent no-show rate for landlords. Analogously, Larson (2006) documents 35 to 90 percent no-show rates in other cities and states. There are several reasons why a tenant fails to show up in court. First, the tenant believes she has no chance of winning the case, either because she is in the wrong or because she cannot win without legal help. Second, the tenant is unable to miss work or unable to find childcare. Third, the tenant is unaware or confused by the court process. Lastly, the tenant is afraid that she is responsible for landlord's legal fees accrued by the court process or afraid of landlord retaliation.[12] It is favorable to landlords when tenants do not appear in court as tenants cannot challenge and defend against landlords' claims. Next, even if the tenants do appear in court, most are self-represented. Of the tenants that do show up in court, only a quarter are represented by counsel. Compared to 96 percent of landlords represented by counsel, the representation status gap between landlords and tenants are stark. The main reason behind this is the hefty price tag to obtain lawyers. Legal fees in NYC typically range from $200 to $500 dollars an hour depending on the complexity of a case. Since over fifty percent of tenants in NYC are rent-burdened or severely rent-burdened, it is unlikely that tenants are able to afford professional legal services. These two observations indicate profound resource disparities between landlords and tenants in housing courts.

3.2.2 The Right to Counsel Program

As tenants' rights became salient in recent years, NYC passed the Intro. 214-B bill in August 2017 to provide right to counsel to tenants facing eviction in order to mitigate resource disparities between landlords and tenants. Under the new policy, income-eligible tenant has

[12]Most residential leases have a clause noting tenants' responsibility of legal fees if landlords win legal cases in housing courts.

a right to legal counsel in housing courts and, if the tenant cannot afford an attorney, the law requires the government to appoint one or pay the tenant's legal expenses.

The objective of the policy is to reduce and eventually eliminate wrongful evictions. As it is challenging and costly to find space, train staff, and develop procedures that honor tenants' integrity, the RTC is being phased in by zip codes. In 2018, twenty zip codes were selected to implement RTC. An additional five zip codes were selected in 2019. Figure 3.5 displays a visual representation of the RTC implementation. The phased-in schedule is based on factors including number of evictions, shelter entries, and rent stabilized units in each zip code, but the precise algorithm of the selection process has yet to be released. The initial plan was to fully implement the policy throughout the five boroughs by 2022. However, due to housing hardships during the Coronavirus Pandemic (COVID–19), Mayor Bill de Blasio expedited the RTC implementation and signed a bill in May 2021 that expands the program to every zip code in the city.

The RTC program works as follows. The landlord starts an eviction case with the same procedure as described above (Figure 3.4). The landlord serves the written demand and, if conditions are not met by the tenant in fourteen days, the landlord files the case at the Housing Court. The tenant answers to the Notice of Petition and Petition, and a court hearing is set. On the assigned court date, the tenant appears at court and is given the opportunity to meet with an attorney before the court hearing. Upon verifying eligibility, the tenant can accept or reject the provided counsel. If the tenant agrees to counsel, the court delays the hearing to a future date. The attorney will then assist and represent the tenant throughout the eviction case. Figure 3.6 presents a flowchart of the court process.

Tenants accepting free legal help are expected to have a higher chance of "winning the case", i.e. not being evicted and having a more feasible stipulation agreement. If the attorney wins the case, the tenant is allowed to stay in their residence. Nonetheless, if there is an undersupply or undertraining of attorneys and staff, the policy could have no effect on the overall eviction rate. As the RTC program is ramping up quickly, the OCJ needs to ensure the quantity and quality of the attorneys supporting the RTC program.

During the phase-in period, tenants did not receive information regarding the free counsel prior to answering to the Notice of Petition and appearing in court on the assigned hearing date. As 67.3 percent of tenants did not appear in court in 2016, the awareness of the policy could be low which in turn leads to a low take-up rate. According to a survey conducted by Community Action for Safe Apartments (CASA) in the Bronx, around 52 percent of tenants living in implementing zip codes were not aware of the policy until they first arrived at court.[13] Furthermore, there could be a delayed response due to psychological frictions, which includes program confusion, informational complexity, and stigma (Bhargava and Manoli, 2015). However, Larson (2006) argues in theory that "participation is more likely if there are social situational resources that support participation." Hence, the policy is likely to increase the turn-out rate for tenants in court but a delayed response is likely to be observed. In the following analysis, I estimate whether tenants' appearance rates change after policy implementation.

3.3 Data

In this section, I describe various data sources used to estimate the effects of the RTC policy on evictions in New York City.

3.3.1 Eviction Data

I obtained anonymized court case-level data from the Office of Court Administration (OCA) located within the New York State Unified Court System. The data comprise all filed cases in the seven New York City Housing Courts since January 1, 2016. A moratorium suspending all evictions was implemented in New York State from March 16, 2020 to January 15, 2022 due

[13] Although CASA is a small-scaled survey with only 115 respondents, it is the only survey data available on the subject. The report can be found at: https://www.northwestbronx.org/publications.

to the COVID–19 pandemic, so there are no recorded evictions during this period.[14] I limit my analysis to eviction cases filed between January 2016 to December 2019. The RTC policy only applies to residential evictions initiated by landlords so I drop all commercial cases and cases filed by tenants (see footnote 8).

A unique index number is assigned to each court record. For each record, I observe the zip code of the rental property, filed and disposed dates, case status, judgment status, disposed reason, primary claim amount, total judgment amount, petitioner's type and representation type, respondent's type and representation type, events, appearances, motions, warrant execution type, and warrant vacated date. A detailed description of the data can be found in Appendix 3.10.

I define three measures to capture the extent of evictions in a given zip code and quarter. First, the eviction filing rate is defined as filed cases divided by total renter households. Next, the *de facto* eviction rate is defined as evicted cases divided by total renter households. Lastly, the *de jure* eviction rate is defined as evicted cases divided by total filed cases. The de facto eviction rate captures the overall eviction occurrence in a given zip code whereas the de jure eviction rate provides an overall status of court outcomes. Note, importantly, the eviction status of a case is only determined when the case is no longer active in court (identified by *case status* in the court records).

After dropping irrelevant cases, there are a total of 679,927 filed cases and 56,364 evicted cases in the seven Housing Courts in NYC over the study period. I aggregate court cases to the zip code–quarter level, which is the unit of analysis in this paper. Appendix Figure 3.A.3 shows the 2017 annual eviction filing rate and de facto eviction rate in Panels (a) and (b) respectively. As shown in the figures, there are variabilities in eviction filing rates and de facto eviction rates across zip codes, but positive correlation between the two variables can be observed.

The eviction filing rate sheds light on landlords' filing patterns. If the eviction filing rate

[14]Additionally, the Centers for Disease Control (CDC) issued an order to temporarily halt residential evictions to prevent further spread of COVID-19. The nationwide order was effective from September 4, 2020 to October 3, 2021.

decreases whereas the de jure eviction rate remains constant, it could be inferred that the policy effect is coming from a decrease in filing.[15] On the other hand, if the eviction filing rate remains relatively constant but the de facto eviction rate decreases, it could be inferred that the policy effect is strictly stemming from courtroom representation for tenants. Certainly, there could be a decrease in both the eviction filing rate and eviction rate, which indicates that the policy effect comes from changes in landlords' filing patterns and tenants' representation type. Differentiating between these measures allows me to disentangle the mechanisms driving the changes in executed evictions.

Appendices 3.A.4 and 3.A.5 show the time series plots of quarterly eviction filing rate and de facto eviction rate respectively.[16] In Appendix Figure 3.A.4, it can be observed that the zip codes implemented in 2018 have a similar trend as the control zip codes both before and after RTC implementation. In Appendix Figure 3.A.5, it is evident the gaps between the treated zip codes and the control zip codes are closing over time.

As de facto and de jure eviction rates are dependent upon cases being closed (not active), I plot the quarterly active rate in Appendix Figure 3.A.7 to ensure there are no systematic differences between control and treatment zip codes. I define active rate as active cases divided by total filed cases in a given zip code. The control zip codes on average have a higher active rate than the implemented zip codes. Although there is a large jump from the last quarter in 2017 to the first quarter in 2018, control and treatment groups follow similar patterns.[17] Thus, the comparisons of eviction measures are valid.

For each case, I observe landlord and tenant characteristics. Landlord and tenant types can be categorized as follows: individual, business, and agency. As I am only looking at residential eviction cases, more than 99 percent of respondents are individual tenants. On the other hand,

[15]Note that it is also reasonable to have a decreasing eviction filing rate and an increasing de jure eviction rate. This could occur if the cases that are still being filed after the policy implementation are more likely to lead to an eviction outcome.

[16]Appendix Figure 3.A.6 shows the quarterly de jure eviction rate.

[17]Most housing court cases are closed within 90 days. The high active rate after 2018 is likely due to the lack of updating in the court case management system, but this has yet to be verified by the OCA.

around sixteen percent are individual landlords, seventy percent are property management firms, and the remaining fourteen percent are state agencies such as the New York City Housing Authority (Figure 3.7). There are more individual landlords in control zip codes, but overall the types of landlords are similar across time and across treatment and control zip codes.

Figure 3.8 shows the proportions of landlords' and tenants' representation types over year by implementation status. Panel (a) shows that around 96 percent of landlords consistently have legal representation and less than one percent do not show up in court. Panel (b) shows that proportions of tenants' no-shows are high across zip codes. Nonetheless, an increase in tenants' representation can be observed in zip codes with RTC implementation since 2018.

I take a closer look at tenants' appearance rate and representation rate to understand how the policy affects tenants' behaviors. I define the appearance rate as the total number of cases where the tenant responded to the Notice of Petition and appeared at assigned court date over total filed cases; I define the representation rate as the total number of cases with legal representation over total filed cases.[18] From Appendix Figure 3.A.8, it can be inferred that the appearance rate remains relatively constant over time. Appendix Figure 3.A.9, however, shows an evident increase in legal representation in treated zip codes. I use both measures as outcome variables in Section 3.5.

I also examine whether legal representation has any effect on monetary outcomes for non-payment cases. Two monetary variables given in the data are primary claim total and total judgment amount. Primary claim total is the dollar amount that the landlord claimed the tenant owed; total judgment amount, which is only available for cases that are disposed, is the final dollar amount settled between the two parties and signed by the judge. I first remove negative values from the data, and I winsorize both variables at the 95th percentile to remove extreme values. Appendix Figure 3.A.10 depicts the dollar amount for primary claim total.[19] Lastly, I define claim

[18]Appearance rate, representation rate, and self-represented rate should add to 100 percent for each zip code–quarter.

[19]See Appendix Figure 3.A.11 for figure of total judgment amount.

difference by subtracting total judgment amount from primary claim total. This measure provides a general notion on whether legal representation has any impact on the monetary outcome.[20]

Additionally, I examine whether the policy affects court efficiency. I take the number of appearances, events, motions, and judgments as outcome variables to examine whether court participation is higher when legal counsel is present. Appearance is the participation in proceedings by a party summoned by court, either in person or through an attorney; motion is a request to court, usually in writing, to make a decision about the case; judgment is the final decision of the judge regarding the case, and a determination of rights and obligations of the parties; event registers any activities in court, including but not limited to eviction filing, first appearance, subpoena, pre-trial conference, and trial. Each variable captures the frequency of activities involved with the case.

I also look at different measures of court duration to determine court efficiency. Specifically, I look at the following four durations: days from filed date to first appearance, days from first appearance to last appearance, days from filed date to evicted date, and days from filed date to disposed date. Note these measures are only available depending on the status of the case. For instance, the first two measures (days from filed date to first appearance and days from first appearance to last appearance) are conditioned on at least one appearance occurred for the case. The third measure (days from filed date to evicted date) is conditioned on the tenants being evicted for the case, and the last measure (days from filed date to disposed date) is conditioned on the case being disposed. Improving disposition rates and times is a goal in court to raise efficiency. Hence, it is crucial to understand whether providing legal counsel offsets court efficiency.

[20]See Appendix Figure 3.A.12 for the claim difference over time by implementation status.

3.3.2 Zip Code Characteristics Data

I acquire zip code characteristics data from the annual estimates of Easy Analytic Software, Inc. (EASI).[21] The main data sources of EASI's estimates are the 2010 United States Census, American Community Survey (ACS) 1-Year and 5-Year Estimates, and the ACS PUMS (Public Use Microdata Sample). EASI summarizes from the United States Postal Service (USPS) mailable households at the county, zip code, census tract, and block group level. EASI also tabulates data that are not part of the standard release of the Census or the ACS using the most recent ACS and PUMs. Furthermore, EASI constructs various indices such as for crime (assault, burglary, larceny, murder, rape, robbery, and total crime), weather, and quality of life. I utilize the indices in my analysis to capture the overall living condition of a given geographical unit.

Variables of interest include basic demographic measures such as population counts, population density, race composition, and gender composition for each zip code. Households and housing measures such as number of households, number of occupied housing units, median rent, and percentage of housing available for rent in each zip code are also of interest.

I also utilize rent payment measures in each zip code. Typically, there are three common measures for rent: median contract rent, median gross rent, and median asking rent.[22] I use median gross rent as the main measure to capture the affordability of the rental market. Gross rent is the monthly housing expenses, which includes contract rent, cost of utilities (electricity, gas, and water and sewer) and fuels (oil, coal, kerosene, wood, etc.) if paid by renters. Median gross rent includes all housing types, which best represent the overall rental market landscape in a given geographical unit.

Table 3.1 columns (1) to (5) present selected variables for each borough in 2017, and column (6) shows statistics of the US as a comparison for the housing landscape and demographic

[21] Details can be found on https://www.easidemographics.com/index.asp.

[22] Median asking rent is the median rent for units being advertised for lease in the private rental market, which does not include properties that are rent-stabilized or subsidized (i.e. public housing). However, as the RTC policy mainly targets lower-income households, median asking rent is not a suitable measurement to capture rental affordability for the targeted population.

composition in NYC. Panel A records annual counts and rates of evictions and eviction filings. There exists heterogeneity across boroughs, but de facto eviction rates are extremely lower than eviction filing rates in all five boroughs. Compared to the US with a 6.12 percent eviction filing rate and a 2.34 percent eviction rate, NYC has a high eviction filing rate and a low eviction rate. That is, most cases being filed in the Housing Courts do not end up in evictions. This could be due to New York being a tenant-friendly state, and landlord-tenant laws being more lenient towards tenants. From Panel B in Table 3.1, it is evident that boroughs with higher poverty rates and lower incomes per capita have higher eviction filing and eviction rates. Panels C and D record borough-level median rent and race composition.

Pre-intervention summary statistics of selected variables from 2017 are presented in Table 3.2. Column (1) presents the means and standard deviations of the 20 zip codes with RTC implementation in 2018; column (2) presents the five zip codes with implementation in 2019; column (3) presents the remaining zip codes in NYC, which I refer to as the control group. All values are weighted by total renter households in each zip code. Panel A records eviction and eviction filing statistics. It is clear that zip codes with earlier implementation have higher de facto eviction and eviction filing rates. From Panel B, zip codes selected for earlier implementation have a higher percentage of renter households and single mothers. Panel C shows that treated zip codes have more African American comparing to the control zip codes. Panels D and E show that treated zip codes have, on average, lower educational attainment, lower household income per capita, higher poverty, and a higher proportion of households participating in the Supplemental Nutrition Assistance Program (SNAP).[23] Panel F summarizes crime indices provided by EASI. Overall, the zip codes selected for RTC implementation in 2018 and 2019 have fewer resources and are more likely to face housing instability.

[23]SNAP, formerly known as the Food Stamp Program, provides income-eligible households monthly supplement to purchase nutritious food. See Hoynes and Schanzenbach (2015).

3.4 Empirical Strategy

The objective is to identify the average treatment effect of the right to counsel program on eviction in the implementing zip codes, i.e. estimating the average intent-to-treat (ITT). More specifically, I want to compare the eviction rate with RTC implementation to the counterfactual, that is, the eviction rate without RTC implementation at the same point in time. However, since the counterfactual is never observed, it needs to be estimated. Ideally, I would like to randomly assign RTC implementation across zip codes or across eligible households, and compare the average outcomes of the two groups. In the absence of a randomized controlled trial, I turn to quasi-experimental methods.

A major methodological concern is that the zip codes that were chosen first are distinct from the remaining zip codes and these differences may be correlated with the outcome variables. For example, zip codes with higher poverty rates and higher eviction rates, are selected to implement RTC first (see Table 3.2). The correlation between RTC implementation and the eviction rate would be confounded. However, as many of the unobservable (and observable) characteristics that might confound identification are those that vary across zip codes but stay fixed over time, I can identify the policy effect by estimating a difference-in-differences (DID) model.

The DID estimator compares the treated zip codes (or the *treatment group*) to the untreated zip codes (or the *control group*), before and after the implementation of the policy. The control group captures common changes across the implementation period. The main estimating equation takes the following form:

$$y_{zt} = \alpha + \beta \cdot RTC_{zt} + \mathbf{X}_{zt}\phi + \lambda_t + \mu_z + \varepsilon_{zt}, \tag{3.1}$$

where y_{zt} is the outcome variable in zip code z in time t, RTC_{zt} is an indicator that takes on a value of one if zip code z has implemented the policy in time t and zero otherwise, $\mathbf{X}_{zt}$ is a vector of control variables that vary across zip code and time, λ_t is a time fixed effect common to all zip codes in time t, μ_z is a time-invariant fixed effect unique to zip code z, and ε_{zt} is a zip code time-varying error term. The parameter of interest is β, the effect of the policy on the outcome variable y_{zt}. Time t represents year-quarter in the following analysis.

A particular challenge in estimating the regression concerns statistical inference. The standard errors need to account for serial correlation within zip codes over time and a common approach is to use cluster-robust standard errors (Bertrand et al., 2004). However, Conley and Taber (2011) show that the cluster-robust estimates of standard errors might be biased when there are few treated units and many control units. The context of this paper falls into the described setting. I follow the method proposed in Conley and Taber (2011), and construct reliable confidence intervals for DID estimates in the presence of a small treatment group.[24] Under this framework, homoskedasticity of residuals across treatment and control groups is assumed, and confidence intervals are formed using information from control group residuals.

3.5 Results

In this section, I present the results of the RTC policy on various outcome variables using the difference-in-differences model. The study period for the following analysis is from January 2016 to December 2019 on the zip code-quarter level. I disregard the five zip codes implementing in 2019 as the parallel trend assumption for these zip codes does not hold. There are 177 zip codes (20 treated zip codes and 157 control zip codes) across 16 quarters, which corresponds to a total of 2,832 observations.

I begin by analyzing the effects of RTC on eviction filings and evictions. As the dis-

[24]Conley and Taber (2011) propose two procedures: $\hat{\Gamma}$ and $\hat{\Gamma}^*$. The $\hat{\Gamma}$ estimator resamples only from the residuals from the control group as the underlying reference distribution; the $\hat{\Gamma}^*$ estimator forms residuals under the null hypothesis for the treatment groups, and draws N_1 residuals without replacement from the $N_0 + N_1$ residuals, where N_0 and N_1 denote the number of control and treatment groups respectively. I use the $\hat{\Gamma}^*$ procedure as it works better with small samples.

tributions of eviction filing and eviction rates are heavily skewed to the right and contain zero values, I apply the inverse hyperbolic sine (*asinh*, or ihs) transformation to the filing and eviction counts. The transformation is as follows: $ihs(y) = asinh(y) = \log\left(y + \sqrt{y^2+1}\right)$, and preserves observations with zero values. With the main variable of interest being a dichotomous variable (the RTC_{zt} indicator in Equation 3.1), the coefficients can be interpreted as the percentage change of the outcome variables when the value of the outcome variables are large enough(Bellemare and Wichman, 2020).[25]

I then estimate the effects of RTC on tenants' court appearances and representation. Likewise, I apply the inverse hyperbolic sine transformation to the two variables. Furthermore, I provide estimated effects of RTC on monetary outcomes and court efficiency measures.

3.5.1 Main Results

Table 3.3 reports the DID results for eviction filings by estimating Equation 3.1. I show four specifications for $ihs(\text{filings})$ as the outcome variable with different sets of controls: no controls, demographics, rent distribution, and crime and market segment indices.[26] From the magnitude of the DID coefficients in Columns (1) and (2), it can be inferred that the policy first targeted zip codes with specific characteristics. As zip code fixed effects are included in the estimating equation, the variation is coming from the correlation between changes in demographics and the outcome variable. Specifically, the policy is more likely to be implemented in zip codes with higher poverty rate, and higher proportion of African American and single mothers. From Columns (3) and (4), however, it is clear that controlling for rent distribution

[25] See the arcsinh–linear specification with dummy independent variables in Bellemare and Wichman (2020). In general, the authors suggest using values no less than ten to reduce approximation error.

[26] Demographics include population density; poverty rate; household income per capita; and the percent of population African American, male, and single mother. Rent distribution include percent of rental properties in the following rent brackets: \$250 or less, \$250-\$499, \$500-\$749, \$750-\$999, \$1,000-\$1,249, \$1,250-\$1,499, \$1,500-\$1,999, and \$2,000 and up. Crime indices include measurements of assault, burglary, larceny, murder, rape, robbery, and vehicle theft; market segment indices include measurements of available apartments, long time residents, and recent movers.

and relevant indices provides similar results to only controlling for demographics. Nonetheless, all four specifications give negative although not statistically significant effects. I include the Conley-Taber 90 % and 95 % confidence intervals to correct for inference.

The OLS results with *ihs*(evictions) as the outcome variable is presented in Table 3.4. The regression controlling for demographics indicates that quarterly eviction decreases by 16.9 percent with RTC implementation (column (2)). This effect is statistically significant at the 0.01 level, and the results are still significant after adjusting for standard errors using the Conley-Taber method. To be ascertained that court case active rate is not different in implemented zip codes, I provide the OLS results with active rate as the outcome variable in Table 3.5. There are not statistically significant effects in all four specifications.

To determine whether more tenants show up in court due to the policy, I estimate Equation 3.1 with *ihs*(appearances) as the outcome variables. Columns (1) and (2) in Table 3.6 show the regression results without and with demographic variables as controls. There is no statistically significant effect on tenants' appearances in court. As tenants do not receive any information regarding the RTC policy prior to appearing in court, the awareness of RTC remains low. Nonetheless, for tenants that did answer the Notice of Petitions and appear in court, the legal representation indeed increased. Columns (3) and (4) in Table 3.6 shows the regression results with *ihs*(represented) as the outcome variable. Controlling for demographics, the representation increases by 54.3 percent with RTC implementation (column (4)). The estimate is statistically significant at the 0.01 level.

The DID results for monetary outcomes are shown in Appendix Table 3.A.1 There is no significant effect in the amount of primary claim total filed by landlords. Total judgment amount, the final dollar amount settled between the two parties and signed by the judge, decreases with policy implementation. The claim difference, which is the difference between the primary claim total and total judgment amount, increases with policy implementation. However, the parallel trend assumption is likely violated so the estimates should be interpreted with caution (Appendix

Figure 3.A.11 and 3.A.12).

Table 3.7 shows the results of court efficiency measures. Columns (1)-(4) show court frequency measures: number of appearances, events, motions, and judgments; Columns (5)-(8) show duration measures: days from filed date to first appearance, first appearance to last appearance, filed date to evicted date, and filed date to disposed date. All eight estimations control for demographics. Number of motions decreases by 0.167 occurrences and number of judgments decreases by 0.056 occurrences in zip codes with RTC implementation (Columns (3) and (4)). This indicates the court finalized a decision more promptly in treated zip codes. The outcomes in Columns (5) and (6) are conditioned on cases having at least one appearance in court. Although the results are not statistically significant, the sign of the estimates indicate a potential increase in court efficiency. Assuming court appearance remains constant pre- and post-implementation (as demonstrated in Table 3.6 column (2)), having legal representation could speed up the initial court process. Nonetheless, column (7), though also not statistically significant, indicates an increase in the whole court process from landlord filing to the court disposing the case. Finally, column (8) shows the days between filed date and evicted date have decreased by 11.45 days for implemented zip codes. The measure is only available for evicted cases. Hence the decrease in duration between filed and evicted date could simply be mechanical.

3.5.2 Threat to Internal Validity

The identification strategy hinges on the assumption that timing of implementation is exogenous. I utilize event study style analysis to detect the presence of pre-trends in the outcome variable y_{zt}. I estimate the following specification:

$$y_{zt} = \sum_{j} \alpha_j \cdot Qtr_{j=t} + \beta_z \cdot RTC_z + \sum_{j \neq -1} \gamma_j \cdot Qtr_{j=t} \cdot RTC_z + \mathbf{X}_{zt}\phi + \mu_z + \varepsilon_{zt}, \tag{3.2}$$

where y_{zt} is the outcome variable in zip code z in quarter t. The right-hand side includes dummies for each quarter, dummies for each implementation group (implemented in 2018, implemented in 2019, and not yet implemented), the interaction between quarter and implementation group dummies, and a vector of control variables. The interaction term omits the quarter just before implementation, which is denoted by $j \neq -1$. The DID coefficient γ_t can be interpreted as the extensive margin effect in quarter t relative to the pre-implementation quarter.

I plot the DID event studies of the main outcome variables in Figure 3.9. The 95 percent confidence intervals are constructed from robust standard errors clustered by zip code and are presented by dashed lines. Panel (a) shows that there are no significant changes in landlords' eviction filing patterns. Panel (b) shows there are significant changes in evictions five quarters after the implementation. The figure highlights the delayed response of the policy on evictions.

Panels (c) and (d) in Figure 3.9 depict tenants' court appearances and representation. There is no significant change in tenants' court appearances, but tenants' legal representation has increased significantly. Note, however, the upward trend in pre-implementation periods for tenants' court representation. I discuss this issue in detail in the following section.

Figure 3.A.13 shows the event study graph for case active rate. There are no significant differences between control and treatment zip codes. Furthermore, I plot the DID event studies of the court efficiency measures in Appendix Figure 3.A.14. Note that only disposed cases are included in the analysis. In Panel (a), there are no observable trends for appearances in the event study graph. There is no evidence RTC changes the frequency of court appearances. Panel (b) shows the event study graph of events. No statistically significant change is observed, but standard errors are large after policy implementation. Panels (c) and (d) show the event study graph for motions and judgments. Downward trends can be observed in both panels. However, this could be due to the fact that active cases, which are mechanically longer, are not included in later quarters.

3.5.3 Discussion

I estimate both landlords' and tenants' responses to the RTC policy from housing court filings. To sum up, landlords do not appear to change their filing patterns, and tenants have the same propensity answering to the Notice of Petitions and appearing in court upon policy implementation (Tables 3.3 and 3.6). Thus, the composition of households facing evictions and appearing in court should be comparable pre- and post-implementation, at least in the short run.

Nonetheless, a decrease of 16.9 percent in quarterly evictions is observed (Table 3.4). One would naturally attribute the change in eviction to an increase in tenants' legal representation (Table 3.6). However, as shown in Figure 3.9 Panels (b) and (d), there is a pre-trend in tenants' representation yet a delay in eviction decline. From the event study graph, the eviction started decreasing four quarters after implementation, or the first quarter in 2019.

In addition to the RTC policy enacted in 2018, New York State passed the Housing Stability And Tenant Protections Act (HSTPA) in 2019. The law extends tenant protections in several areas including eviction proceedings and security deposits. For instance, the HSTPA strengthens protections against retaliatory evictions, i.e. prohibiting the use of tenant blacklists. The HSTPA also provides more time for tenants in the eviction process. Thus, it could be the case that the RTC policy is only effective in decreasing eviction when coupled with other tenant protection policies. Unfortunately, I cannot directly observe whether each case is represented by RTC counsel with anonymized court records, so I am unable to estimate the effectiveness of provided counsel.

The incentives for landlords to file in court have changed as it is more difficult to remove tenants from properties. It is counterintuitive that landlords are not changing their filing patterns after implementation. I propose three possible explanations for this finding. First, landlords are unaware of the new policy and hence not alter their filing patterns. Although this is unlikely to be true assuming NYC business or agency landlords have legal counsel on retainer. Second, landlords do not believe the policy is effective. As landlords do not observe any negative impacts in the short run, they do not adjust their filing patterns. The third explanation is that even with RTC, landlords still receive desired outcomes. Instead of a forthright eviction either due to tenant's no-show or inability to self-represent, counsels could devise alternative outcomes that are preferable for both parties. To thoroughly understand landlords' filing patterns and incentives requires identifiable court records and is left for future work.

Unfortunately, landlords could also turn to other avenues to remove tenants. First, landlords could file an ejectment action in New York State Civil or Supreme Court to remove a tenant from the residence.[27] Since RTC only applies to Housing Courts, tenants are not protected under ejectment actions. Second, landlords could take the chance and turn to informal evictions. Prior to the RTC, the eviction process takes at least two months without any delay. With the policy in place, a tenant is allowed to reschedule court hearing if she accepts assistance from a legal counselor. This would further lengthen the eviction process, which is more costly to landlords as they are foregoing future rent. An increase in ejectment actions can be accounted for by obtaining court records from the Supreme Court. However, informal evictions cannot be reconciled. As there is currently no available survey on informal evictions in New York City, this is not an issue that can be easily resolved.

3.6 Conclusion

In principle, we live in a society of laws in the United States. However, the cost of accessing the formal legal system is immense and essentially inaccessible to the poor. The novel right to counsel policy in housing courts implemented in New York City provides an opportunity to low-income households to contest wrongful evictions. As an extension of the Sixth Amendment and *Gideon v. Wainwright (1963)*, the policy provides free legal counsel to

[27] An ejectment action is a procedure for an owner to obtain possession of the property. It can be filed in the Civil or Supreme Court in lieu of a non-payment or holdover eviction in Housing Court. If the assessed value of the property is less than $25,000, the action is filed in the Civil Court. Otherwise, it is filed in the Supreme Court.

households with income below the 200 percent federal poverty level facing evictions.

In this paper, I present first evidence on the effect of the RTC policy on evictions by using the staggered roll-out schedule of the policy. By estimating a difference-in-difference model, I find that tenants' representation increases by 54.3 percent and eviction decreases by 16.9 percent with RTC implementation. However, from the event study analysis, I conclude that the RTC policy is only effective when coupled with other tenant protection policies. Assuming the rate of informal evictions remains constant, the result suggests that tenants benefit from the policy in the short run. A decrease in evictions not only provides housing stability to tenants, but it also decreases social costs such as homeless shelter and emergency room costs (Collinson and Reed, 2018).

As the analysis suggests, more emphasis should be put on raising awareness of the policy. If more households are informed of the right to counsel when facing eviction, tenants' court appearance is likely to increase, which could lead to a further decrease in evictions. The annual investment of the RTC policy in NYC is budgeted at $155M in FY2022. Whether the benefits from the program offset the cost of implementation is an important policy question yet to be answered. A detailed welfare analysis is required. The Marginal Value of Public Funds (MVPF) introduced in Hendren and Sprung-Keyser (2020) can be used to compare RTC with other active housing policies in NYC.

The impact of the policy on tenants' long-term well-being remains unclear. It is possible that landlords enforce stricter screening processes (i.e. raising rent) as it is more burdensome to evict tenants with the policy in place. Landlords might also limit the supply of long-term rental housing by selling or converting their properties to short-term home-sharing. Both mechanisms could lead to a permanent increase in rent, which would in turn hurt the tenants on the margins. The effect of the RTC policy on long-term rental market is not explored in this paper as the policy is too novel. This is left for future work.

The consequences of eviction can be severe. Not only does eviction lead to temporary loss of housing, it could also lead to adversity in finding new residence with a permanent eviction record. This paper provides a positive outlook on the RTC policy in the short-run. New York City is contemplating an RTC 2.0 policy by relaxing the income eligibility to 400 percent federal poverty level. Other major cities in the United States including San Francisco, Washington D.C., Newark, Philadelphia, and Minneapolis are also introducing similar policies. It is crucial for policymakers to examine both the short-term and long-term effectiveness of the policy and whether it justifies social, monetary, and moral costs.

3.7 Acknowledgements

Chapter 3, "The Impact of Right to Counsel to the Poor: Evidence from New York City Housing Courts," is currently being prepared for submission for publication of the material. The dissertation author was the sole author of the chapter. The researcher's own analyses were calculated based in part on data obtained from the Office of Court Administration in the New York State Unified Court System. The conclusions drawn from the Office of Court Administration data are those of the researcher and do not reflect the views of the New York State Unified Court System. The New York State Unified Court System is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

3.8 Figures and Tables

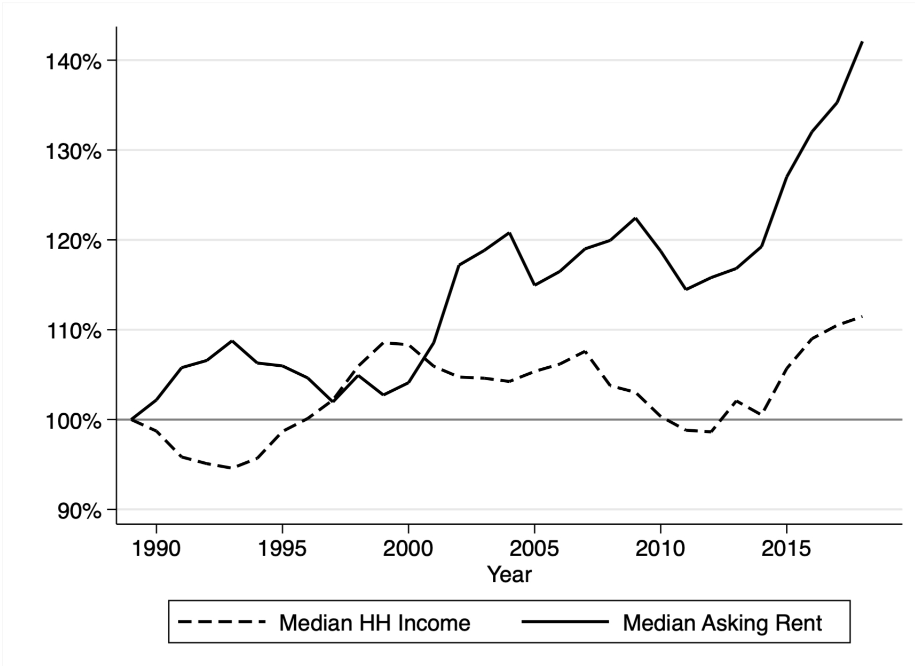
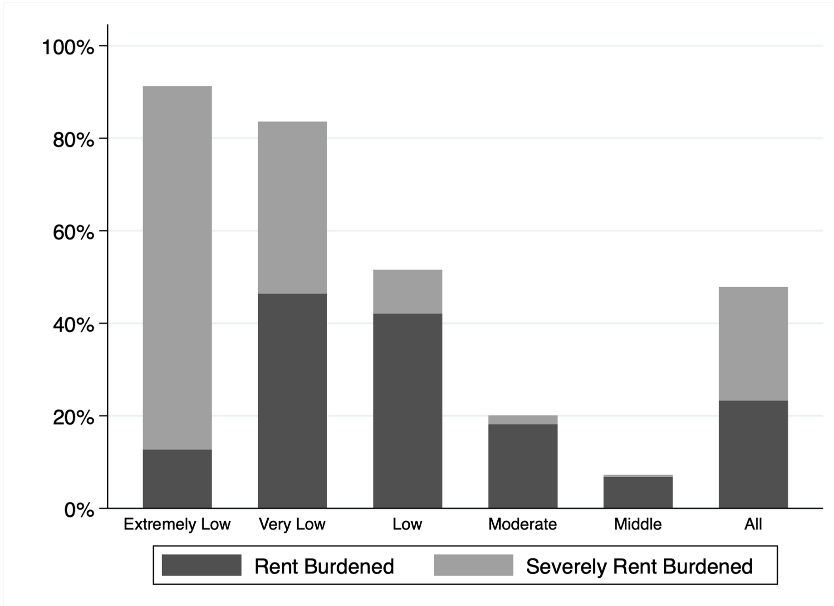


Figure 3.1. Adjusted Annual Median Household Income vs Median Asking Rent.

Notes: Figure shows relative growths of median household income and median asking rent from 1989 to 2018 in the US, using 1989 as the base year. All values are inflation-adjusted to 2018 dollars using the Consumer Price Index for All Urban Consumers (CPI-U). The median asking rent is obtained from CPS/HVS and median household income is obtained from CPS.

(a) United States.



(b) New York City.

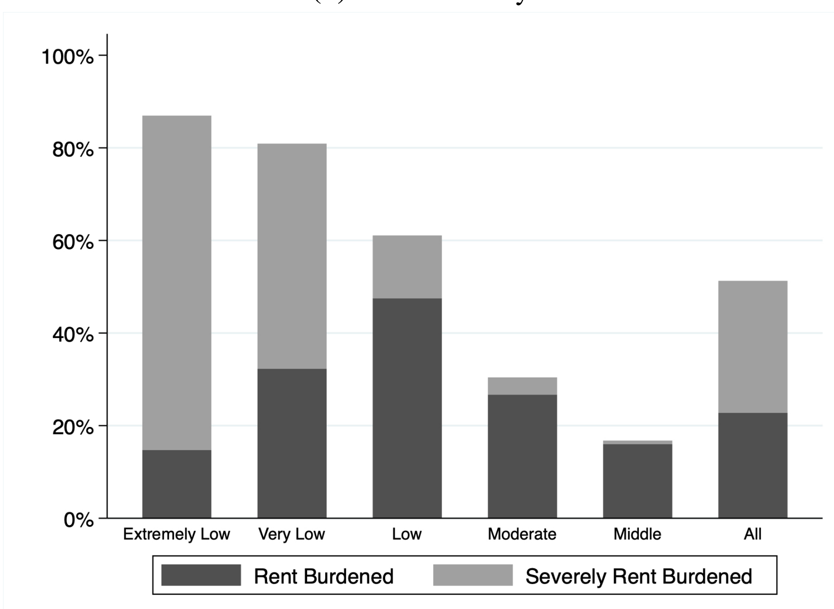


Figure 3.2. Percent Rent-Burdened Households by Income Status in 2018.

Notes: Figure shows the percentage of rent-burdened and severely rent-burdened households by income status in 2018 in the US and in NYC respectively. A household is considered *rent-burdened* if it spends more than 30 percent of household income on housing and *severely rent-burdened* if it spends more than 50 percent of household income on housing. *Extremely low-income* household is defined as a household earning 30 percent or less of Area Median Income (AMI); *very low-income* household earning 30 to 50 percent AMI; *low-income* household earning 50 to 80 percent AMI; *moderate-income* household earning 80 to 120 percent AMI; *middle-income* household earning 120 to 165 percent AMI. AMI is defined by HUD annually and differs by household size; the 2018 NYC AMI for a household of four is $104,300. Data is obtained from 2018 ACS one-year estimates.

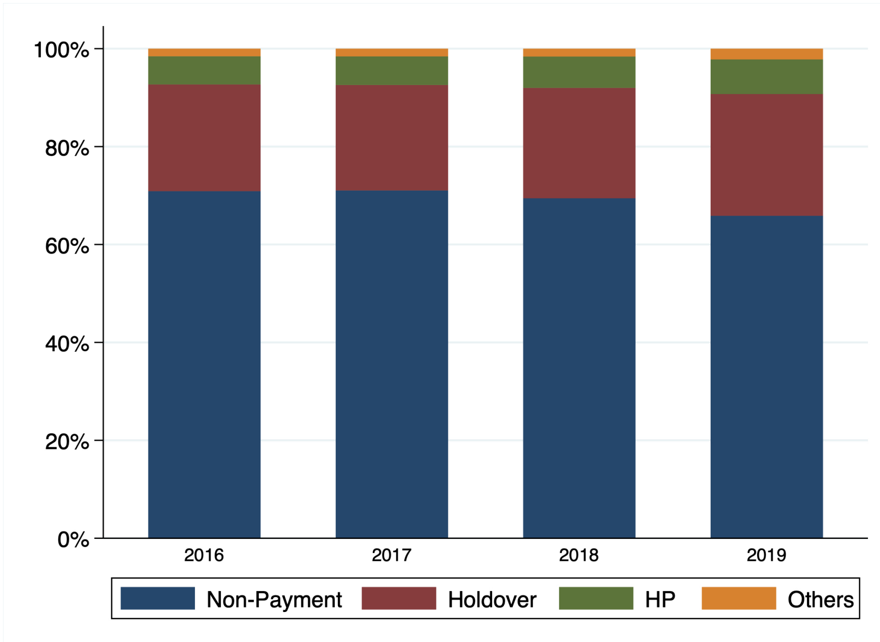


Figure 3.3. Eviction Types.

Notes: Figure depicts the proportion of types of filed residential cases in NYC from 2016 to 2019. In 2016, approximately 71 percent of cases were non-payment filings, 22 percent were holdover disputes, and 5.8 percent were HP cases. The remaining 1.2 percent cases under *Others* consist of Article 7A, harassment, illegal activity, and breach of warrant of habitability proceedings. All cases under *HP* and *Others* were filed by tenants, which are not covered in this paper.

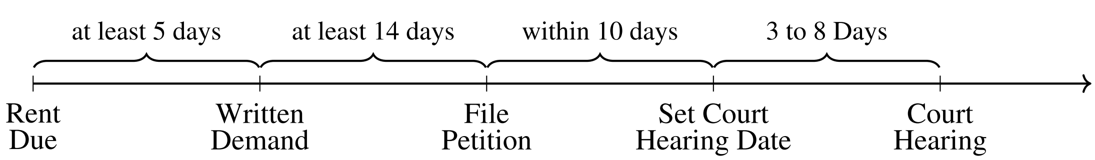


Figure 3.4. Court Process Timeline.

Notes: Figure depicts the timeline for a non-payment case. The landlord can initiate a case at least five days after the rent due date. A written demand needs to be properly delivered to the tenant, in which the tenant has fourteen days to make amends. If the demands were not met, the landlord can file a petition in court. The tenant needs to answer the petition within ten days, and a court hearing date is set. The court hearing is usually set between three to eight days after the tenant answers to the petition. For all cases filed in 2016, it took an average of 275 days from petition filing to case disposition (with 2.6 percent of cases remaining active).

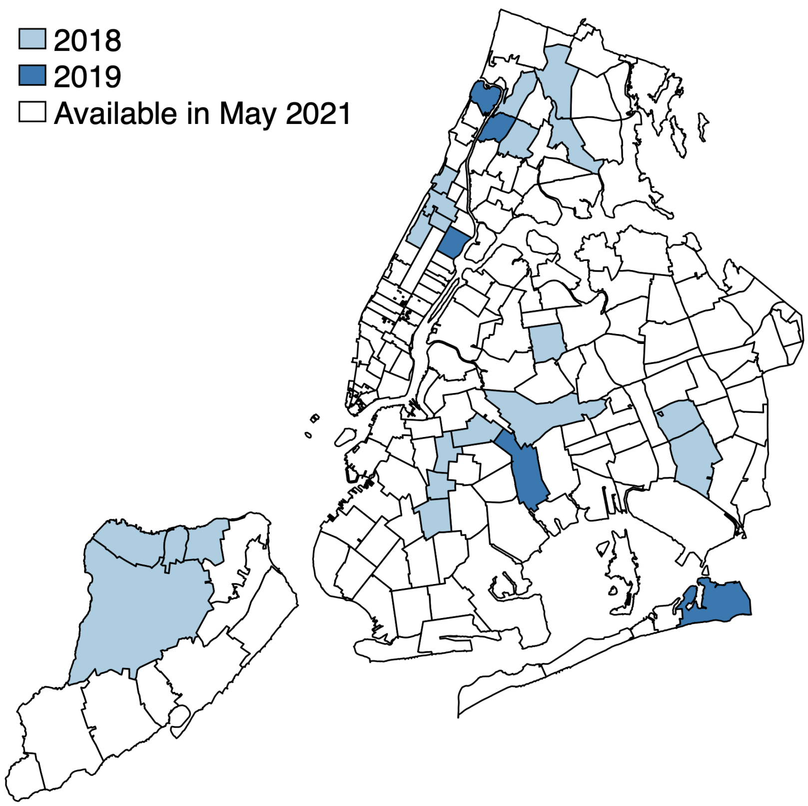


Figure 3.5. RTC Roll-Out Schedule.

Notes: Figure depicts the roll-out schedule of the right-to-counsel policy in New York City. A total of 20 zip codes implemented the policy in 2018, and an additional 5 zip codes were added in 2019. The remaining 152 zip codes received RTC in May 2021.

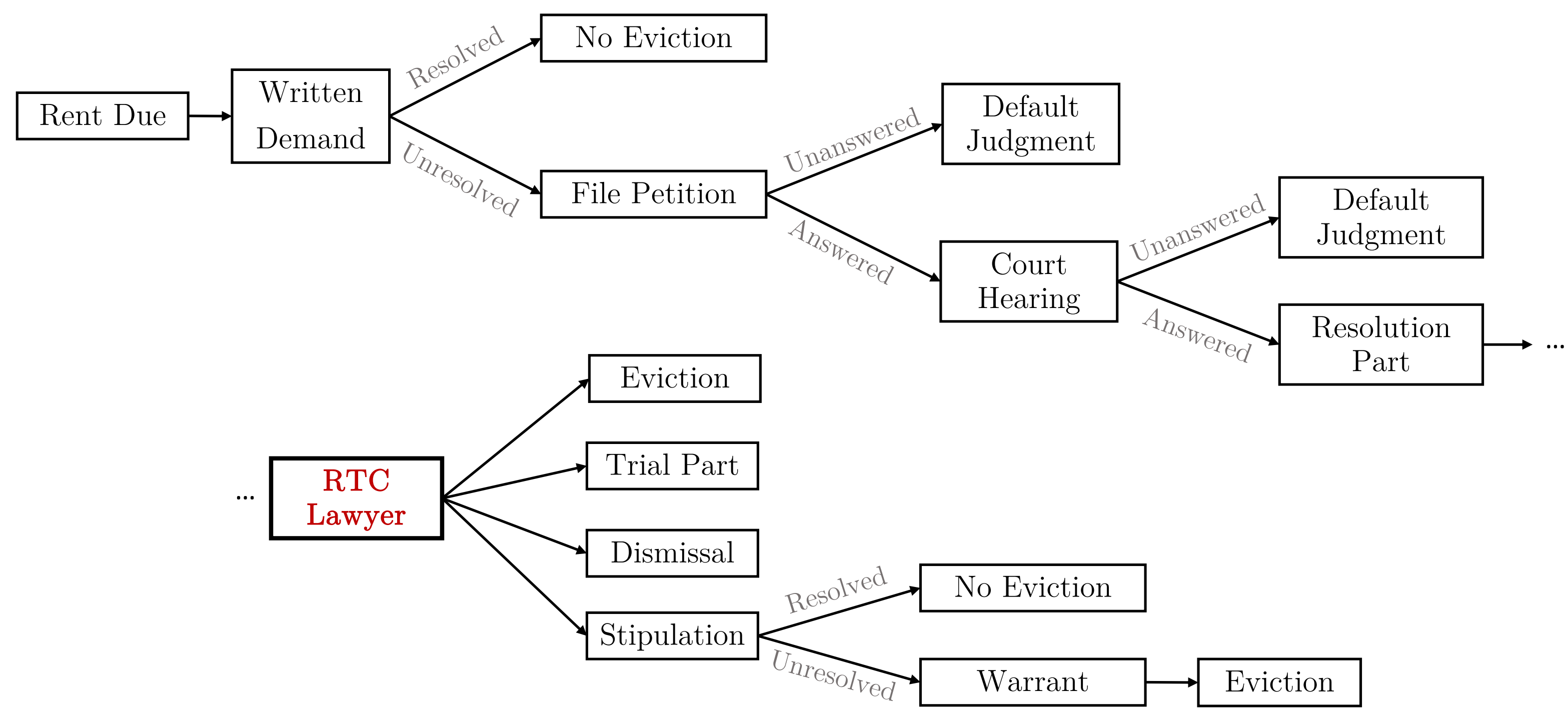


Figure 3.6. Non-Payment Cases Court Process.

Notes: Figure depicts the court process for a non-payment case filed by the landlord. Please see Figure 3.4 for the time frame for each step. The bolded text box only applies to zip codes with RTC policy implementation. On the assigned court date, qualified tenants can accept free legal counselors provided by the RTC policy. The counselor would assist the tenant throughout the case, even if the case goes to trial.

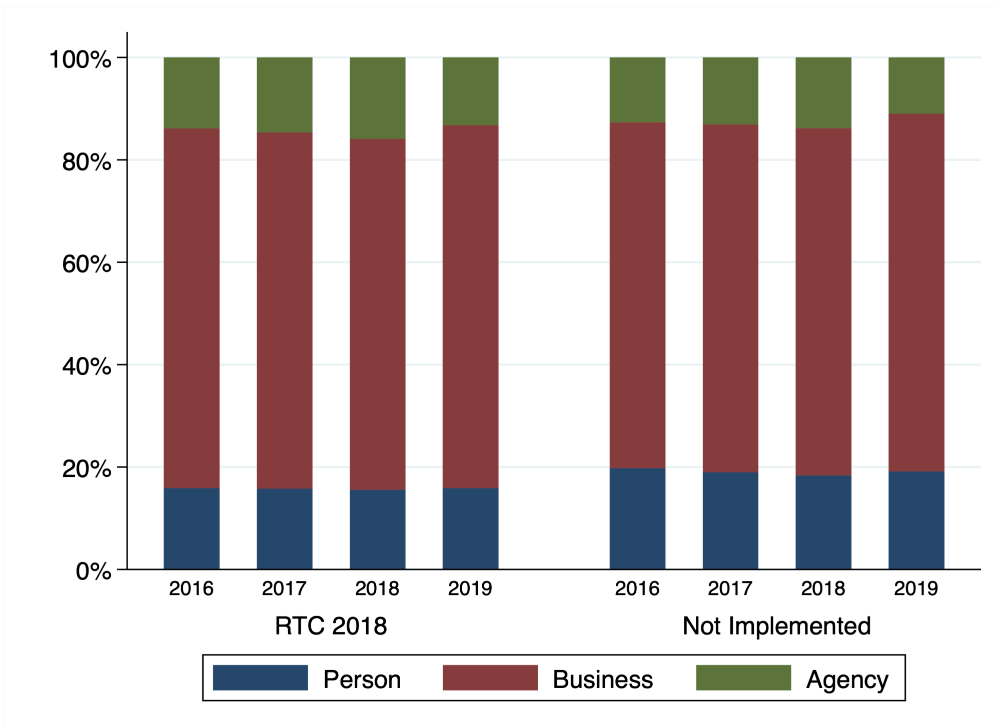
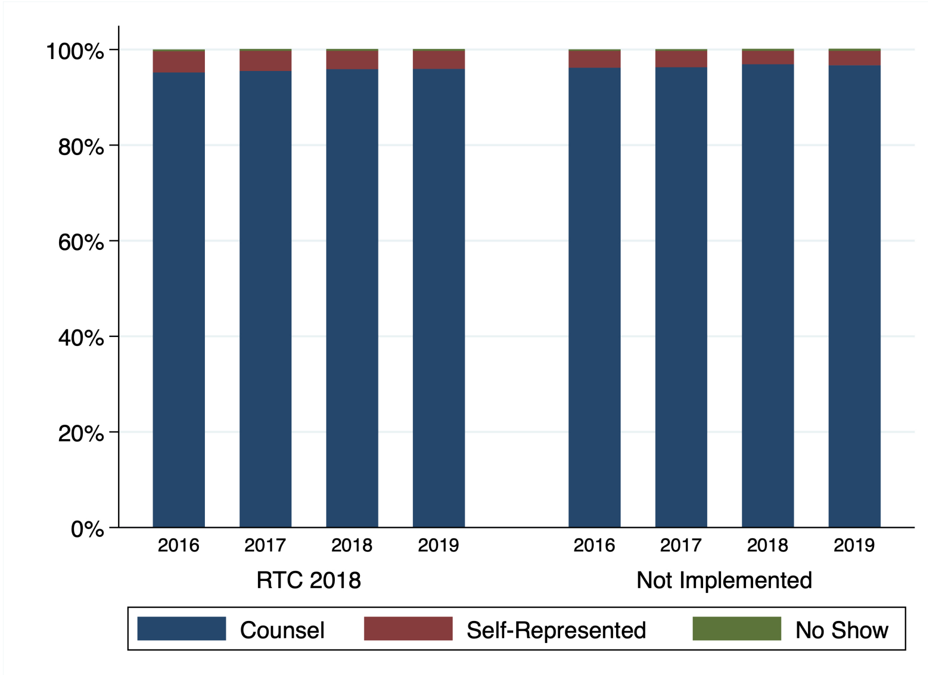


Figure 3.7. Landlords' Types.

Notes: Figure shows landlords' types by policy implementation over time.

(a) Landlords' representation types.



(b) Tenants' representation types.

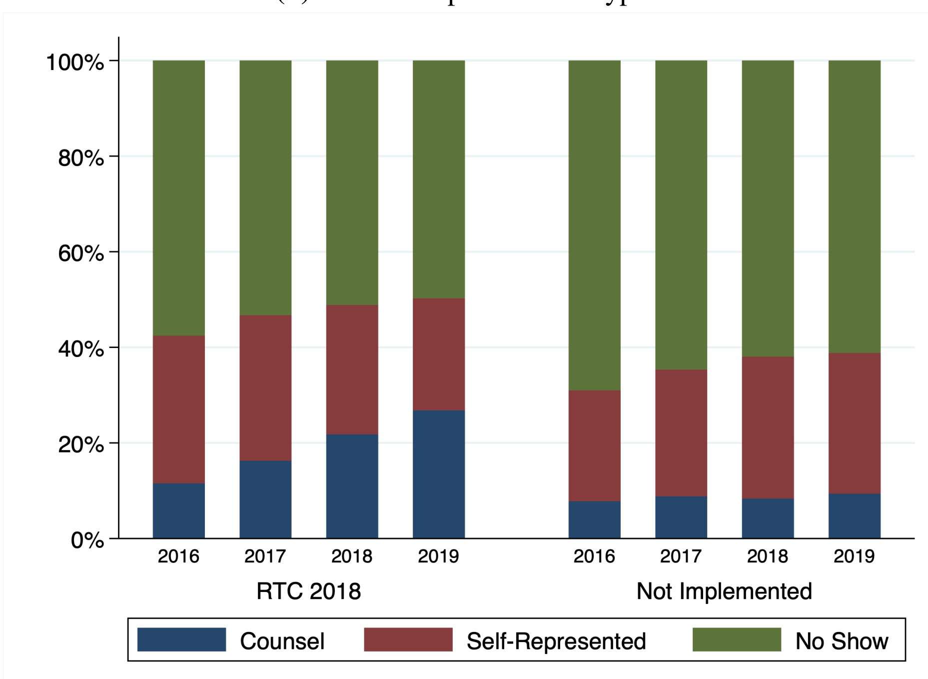
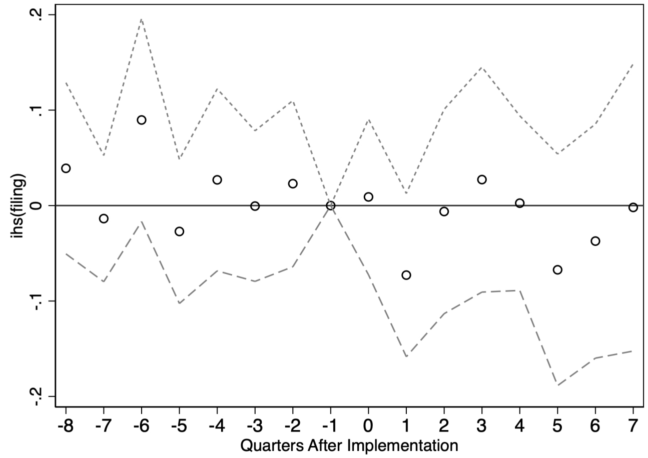
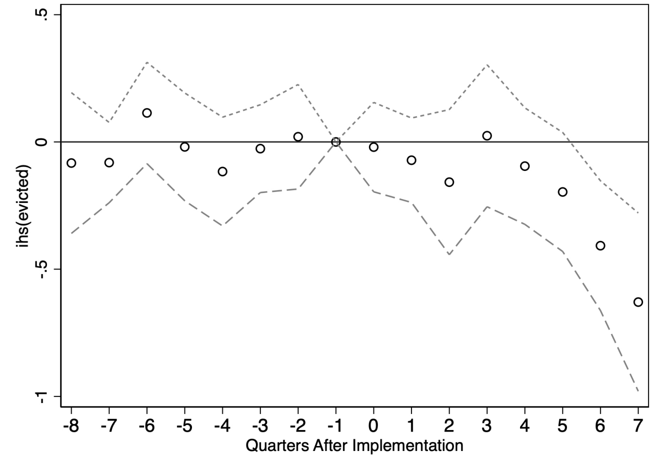


Figure 3.8. Landlords' and Tenants' Representation Types.

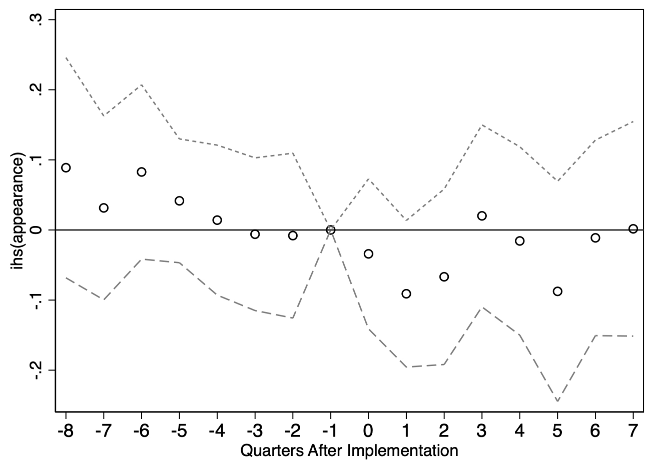
Notes: Figure shows landlords' and tenants' representation types by policy implementation over time. Panel (a) depicts landlords' types; panel (b) depicts tenants' types.



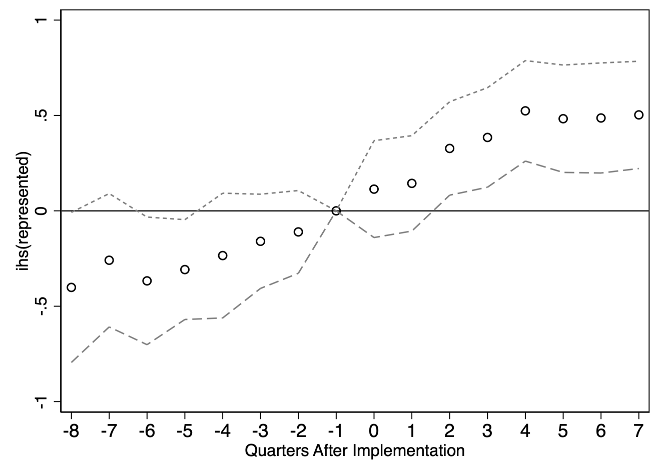
(a) Eviction Filing



(b) Evicted



(c) Tenants' Appearance



(d) Tenants' Representation

Figure 3.9. Difference-in-Differences Event Studies of Main Outcome Variables.

Notes: Figure shows the DID event studies for the main outcome variables. The hollow circles are point estimates of γ_t based on Equation 3.2 with demographics variables as controls. The short dash and dash lines represent the upper and lower bounds of the 95 % confidence intervals based on robust standard errors clustered at the zip code level.

Table 3.1. Borough Characteristics (2017).

	(1) Bronx	(2) Brooklyn	(3) Manhattan	(4) Queens	(5) SI	(6) USA
A. Eviction Statistics						
Evictions	7,881	4,128	2,998	1,269	513	898,479
Eviction Rate (%)	1.874	0.593	0.494	0.265	0.812	2.34
Eviction Filings	87,593	65,649	47,833	35,219	5,145	2,350,042
Eviction Filing Rate (%)	20.83	9.425	7.878	7.364	8.144	6.12
B. Population Characteristics						
Population	1,464,233	2,596,806	1,630,581	2,387,618	477,000	324,209,120
Total Households	513,171	955,792	790,669	834,540	169,240	122,847,896
Zip Codes	25	37	46	64	12	–
Poverty (%)	29.79	22.90	15.75	15.98	12.26	15.76
Income Per Capita ($)	22,534	32,912	79,264	35,205	41,544	35,542
C. Rental Market						
Renter Homes (%)	81.95	72.87	76.79	57.31	37.33	36.90
Median Gross Rent ($)	1,074	1,215	1,519	1,367	1,169	750
Median Asking Rent ($)	1,700	2,500	2,800	2,250	1,980	864
D. Race						
White (%)	25.26	40.97	58.64	38.95	70.74	70.14
African American (%)	35.30	34.42	13.66	17.04	11.06	13.21
Asian (%)	4.218	11.33	12.90	25.35	8.568	5.643
Hispanic (%)	54.47	18.91	22.23	26.76	18.39	17.42

Notes: This table includes borough characteristics of selected variables in 2017. Eviction statistics are obtained from NYC Housing Court data, median asking rent is obtained from the Street Easy database, and the remaining variables are obtained from the ACS 5-year estimates. I only include zip codes with positive household counts, i.e. commercial zones and public spaces with no households are excluded.
Eviction rate is defined as eviction counts divided by total renters' households, and eviction filing rate is defined as eviction filing counts divided by total renters' households.
Median gross rent is the median monthly housing cost expenses for public housing and private rental market. The median asking rent is the median cost for units being advertised for lease in the private rental market.

Table 3.2. Pre-Intervention Descriptive Statistics (2017).

	(1)		(2)		(3)	
	RTC 2018		**RTC 2019**		**Control**	
	Mean	SD	Mean	SD	Mean	SD
A. Eviction Statistics						
Evictions	243	214	362	236	92	142
Eviction Rate (%)	1.052	0.795	1.525	0.899	0.525	0.640
Eviction Filings	2,770	1,859	4,363	1,415	1,326	1,426
Eviction Filing Rate (%)	12.18	6.557	18.74	4.583	8.074	6.527
B. Population Characteristics						
Population	79,219	23,504	77,247	18,188	57,343	24,477
Households	29,431	9,438	26,966	6,222	22,376	8,687
Renter Home (%)	76.40	16.28	86.71	8.380	67.57	17.85
Male (%)	46.98	1.742	46.25	0.574	47.50	2.422
Population, 65+ (%)	12.79	2.713	12.05	2.499	15.04	4.594
Single Mother (%)	21.81	7.979	30.79	4.661	14.35	9.657
C. Race						
White (%)	29.99	20.93	21.79	9.315	47.41	25.49
African American (%)	37.95	26.84	42.15	17.34	20.45	25.17
Asian (%)	9.184	11.29	3.268	2.479	15.36	14.37
Hispanic/Latinx (%)	34.79	19.02	49.59	16.85	24.87	20.00
D. Education Attainment						
Less Than High School (%)	20.99	7.795	29.57	2.971	18.04	11.22
High School (%)	25.95	6.602	27.13	4.113	22.18	9.146
Some College (%)	17.19	3.864	15.83	2.368	14.00	4.263
College and Above (%)	27.53	13.14	19.64	7.267	35.92	18.33
E. Economic Characteristics						
Income Per Capita ($)	31,197	13,439	22,373	5,266	46,849	32,143
Poverty (%)	23.61	7.057	32.13	5.588	18.74	10.19
Unemployment (%)	5.125	0.902	4.992	0.885	4.900	0.769
Received SNAP (%)	28.44	13.45	45.11	8.668	22.01	15.72
Median Gross Rent ($)	1063.9	136.3	892.6	91.13	1255.5	367.2
F. EASI Crime Index						
Total Crime Index	113.2	24.58	130.7	8.885	108.0	27.94
Assault Index	112.1	21.78	120.5	16.29	120.9	25.61
Burglary Index	66.67	14.52	66.62	9.445	62.62	17.40
Larceny Index	83.57	17.87	89.82	5.847	76.81	22.48
Murder Index	122.9	22.45	136.9	7.836	114.5	30.58
Rape Index	69.46	27.79	104.0	26.56	68.18	27.06
Robbery Index	91.38	18.11	95.64	21.27	92.04	24.11
Vehicle Theft Index	129.3	22.08	147.7	8.481	121.3	26.58
Zip Codes	20		5		157	

Notes: This table includes summary statistics prior to the intervention. Column (1) shows annual statistics of the zip codes implemented in 2018, column (2) shows statistics of zip codes implemented in 2019, and column (3) shows statistics of the remaining zip codes. Means are weighted by total renter households in each zip code. Data is obtained from NYS Office of Court Administration, ACS one-year estimates and the crime index is obtained from EASI.

Table 3.3. Difference-in-Differences Regression for Eviction Filings.

Dependent variable:	*ihs*(filing)			
	(1)	(2)	(3)	(4)
DID	-0.007	-0.035	-0.029	-0.024
	(0.028)	(0.026)	(0.027)	(0.028)
Control Mean	5.433	5.433	5.433	5.433
95 % CI	[-0.063 , 0.049]	[-0.087 , 0.016]	[-0.082 , 0.024]	[-0.080 , 0.031]
Conley-Taber 90 % CI	(-0.070 , 0.051)	(-0.092 , 0.029)	(-0.082 , 0.034)	(-0.075 , 0.035)
Conley-Taber 95 % CI	(-0.083 , 0.065)	(-0.103 , 0.041)	(-0.093 , 0.046)	(-0.085 , 0.046)
Observations	2,832	2,832	2,832	2,832
Number of Zip Codes	177	177	177	177
Controls				
Demographics		X	X	X
Rent			X	X
Indices				X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the difference-in-differences result by estimating Equation 3.1. The analysis is on the zip code–quarter level from 2016Q1 to 2019Q4, and the outcome variable is the inverse hyperbolic sine transformation of quarterly eviction filing counts. Standard errors are clustered at the zip code level. 95 % confidence intervals (CIs) are based on clustered standard errors, and the Conley-Taber 90 % and 95 % CIs are simulated following Conley and Taber (2011).

Table 3.4. Difference-in-Differences Regression for Evictions.

Dependent variable:	*ihs*(eviction)			
	(1)	(2)	(3)	(4)
DID	-0.198***	-0.169***	-0.160***	-0.170***
	(0.0549)	(0.0534)	(0.0543)	(0.0553)
Control Mean	2.324	2.324	2.324	2.324
95 % CI	[-0.307 , -0.090]	[-0.274 , -0.064]	[-0.267 , -0.053]	[-0.279 , -0.061]
Conley-Taber 90 % CI	(-0.305, -0.098)	(-0.257, -0.061)	(-0.248, -0.056)	(-0.259, -0.064)
Conley-Taber 95 % CI	(-0.322, -0.076)	(-0.276, -0.041)	(-0.256, -0.038)	(-0.274, -0.046)
Observations	2,832	2,832	2,832	2,832
Number of Zip Codes	177	177	177	177
Controls				
Demographics		X	X	X
Rent			X	X
Indices				X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Please see the notes to Table 3.3. The outcome variable is the inverse hyperbolic sine transformation of quarterly eviction counts.

Table 3.5. Difference-in-Differences Regression for Case Active Rate.

Dependent variable:	active rate			
	(1)	(2)	(3)	(4)
DID	-0.005	0.005	0.003	0.002
	(0.017)	(0.016)	(0.016)	(0.016)
Control Mean	0.2096	0.2096	0.2096	0.2096
95% CI	[-0.039 ,0.028]	[-0.027, 0.038]	[-0.029, 0.035]	[-0.029 , 0.034]
Conley-Taber 90 % CI	(-0.033, 0.023)	(-0.021, 0.029)	(-0.023, 0.026)	(-0.023, 0.026)
Conley-Taber 95 % CI	(-0.036, 0.025)	(-0.023, 0.034)	(-0.026, 0.029)	(-0.028, 0.032)
Observations	2,832	2,832	2,832	2,832
Number of Zip Codes	177	177	177	177
Controls				
Demographics		X	X	X
Rent			X	X
Indices				X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Please see the notes to Table 3.3. The outcome variable is the quarterly case active rate.

Table 3.6. Difference-in-Differences Regression for Tenants' Appearance and Representation.

Dependent variable:	*ihs*(appearance)		*ihs*(represented)	
	(1)	(2)	(3)	(4)
DID	-0.062	-0.065	0.533***	0.543***
	(0.050)	(0.036)	(0.071)	(0.071)
Control Mean	4.292	4.292	2.825	2.825
95 % CI	[-0.160 , 0.037]	[-0.137 , 0.006]	[0.393, 0.674]	[0.402 , 0.684]
Conley-Taber 90 % CI	(-0.134, 0.026)	(-0.128, 0.036)	(0.394, 0.589)	(0.421, 0.600)
Conley-Taber 95 % CI	(-0.142, 0.033)	(-0.136, 0.045)	(0.375, 0.607)	(0.402,0.616)
Observations	2,832	2,832	2,832	2,832
Number of Zip Codes	177	177	177	177
Controls				
Demographics		X		X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Please see the notes to Table 3.3. The outcome variables are the inverse hyperbolic sine transformation of quarterly tenants' appearance and representation counts.

Table 3.7. Difference-in-Differences Regression for Court Case Frequency and Duration.

	Frequency				Duration			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Appearance	Event	Motion	Judgment	First Appear – Filed	Last Appear –First Appear	Disposed – Filed	Evicted – Filed
DID	0.044	-0.045	-0.167***	-0.056***	-9.275	-1.176	2.155	-11.450*
	(0.068)	(0.039)	(0.046)	(0.021)	(9.823)	(2.611)	(1.663)	(6.608)
Control Mean	2.206	2.250	0.794	0.592	49.528	63.005	12.550	95.090
Observations	2,832	2,832	2,832	2,832	2,832	2,832	2,832	2,832
Number of Zip Codes	177	177	177	177	177	177	177	177
Controls								
Demographics	X	X	X	X	X	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Please see the notes to Table 3.3. The outcome variables are measured in levels.

3.9 Appendix Figures and Tables

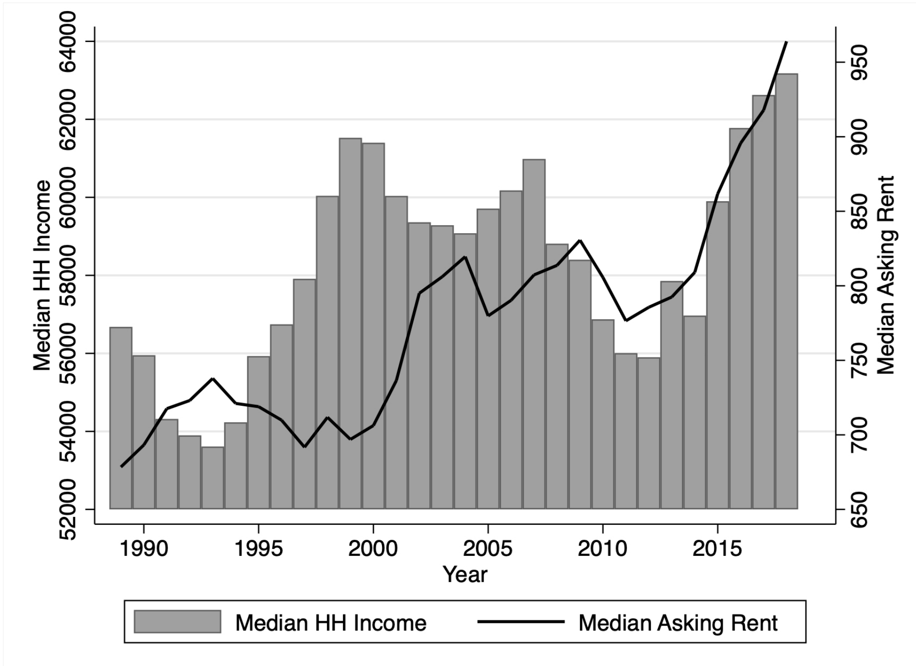


Figure 3.A.1. Adjusted Annual Median Household Income vs Median Asking Rent.

Notes: Figure shows the median household income and median asking rent from 1989 to 2018 in the US. All values are inflation-adjusted to 2018 dollars using the Consumer Price Index for All Urban Consumers (CPI-U). Median asking rent is obtained from CPS/HVS, and median household income is obtained from CPS.

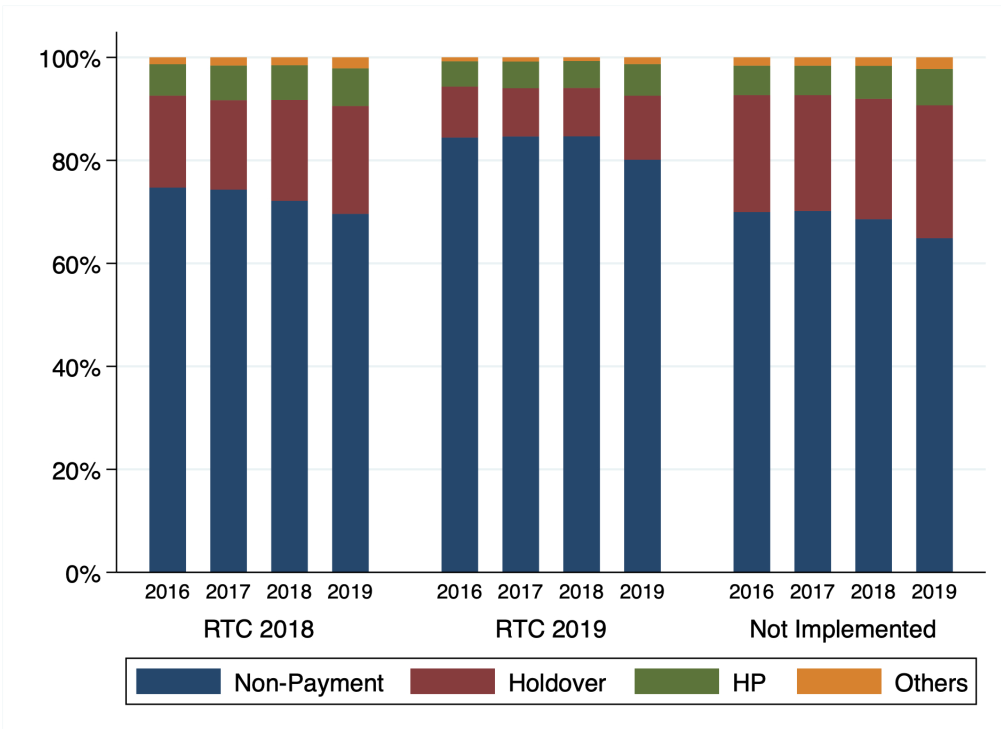
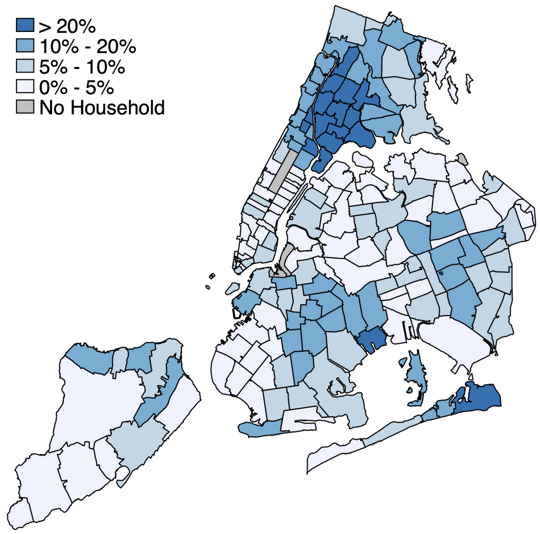


Figure 3.A.2. Eviction Types.

Notes: Figure depicts the proportion of types of filed residential cases in NYC from 2016 to 2019 by RTC implementation status. All cases under *HP* and *Others* were filed by tenants, which are not covered in this paper.

(a) Eviction filing rate.



(b) De facto eviction rate.

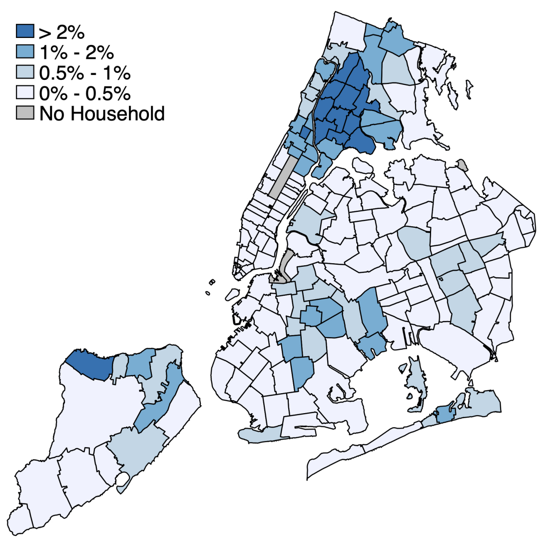


Figure 3.A.3. Pre-intervention Eviction Filing and De Facto Eviction Rates by Zip Code.

Notes: Figures show pre-intervention annual rates by zip code from 2017. Panel (a) depicts eviction filing rate; panel (b) depicts de facto eviction rate. Data is obtained from NYC Housing Court database.

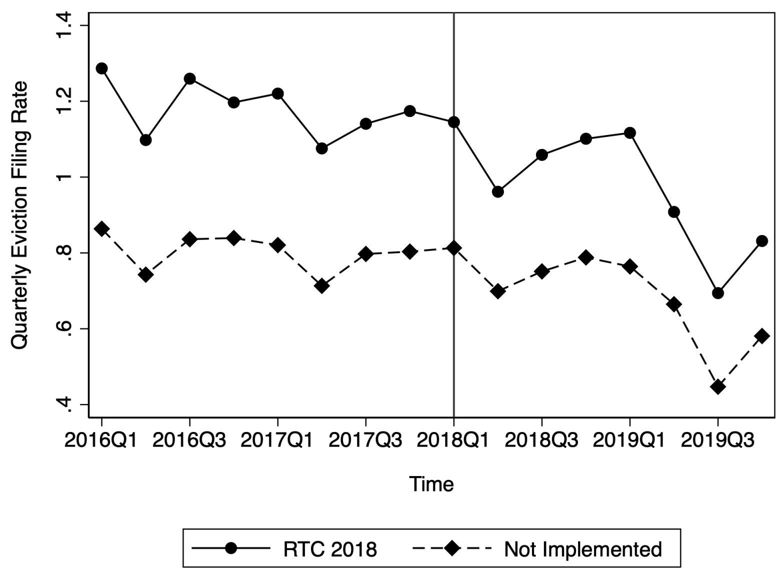


Figure 3.A.4. Quarterly Eviction Filing Rate.

Notes: Figure shows quarterly eviction filing rate, which is defined as the number of filed cases in a quarter over total renters' households in a given zip code.

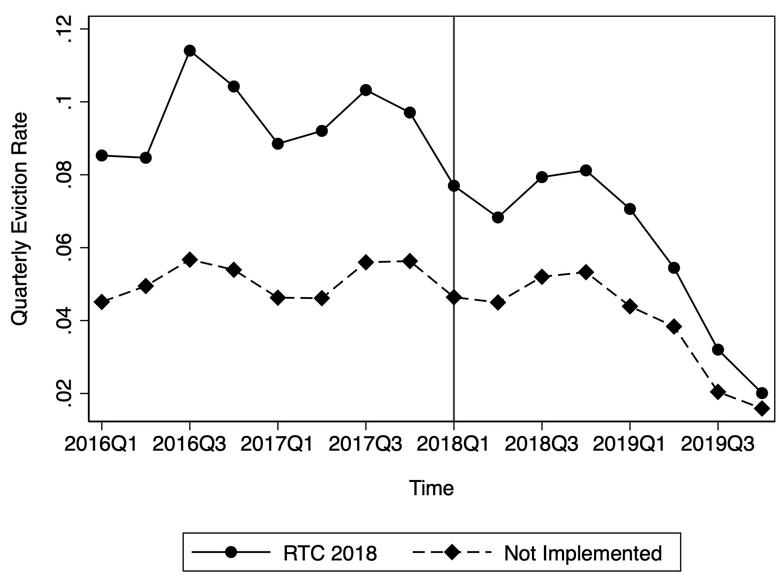


Figure 3.A.5. Quarterly De Facto Eviction Rate.

Notes: Figure shows quarterly de facto eviction rate, which is defined as the number of evicted cases in a quarter over total renters' households in a given zip code.

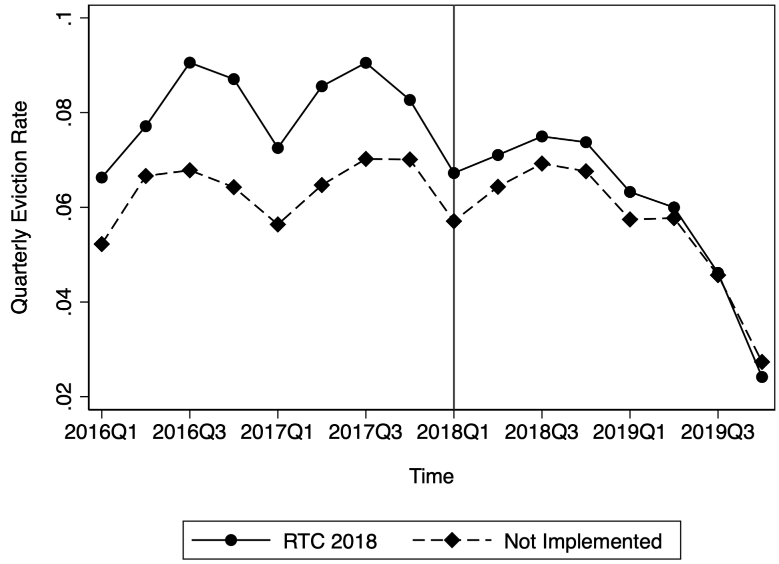


Figure 3.A.6. Quarterly De Jure Eviction Rate.

Notes: Figure shows quarterly de jure eviction rate. De Jure eviction rate is defined as evicted cases over total filed cases in a given zip code.

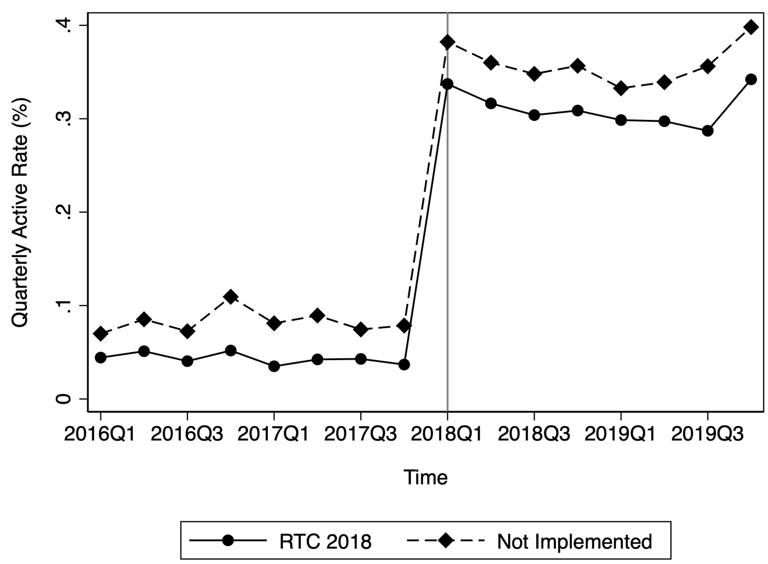


Figure 3.A.7. Quarterly Active Rate.

Notes: Figure shows quarterly active rate. Active rate is defined as active cases over total filed cases in a given zip code.

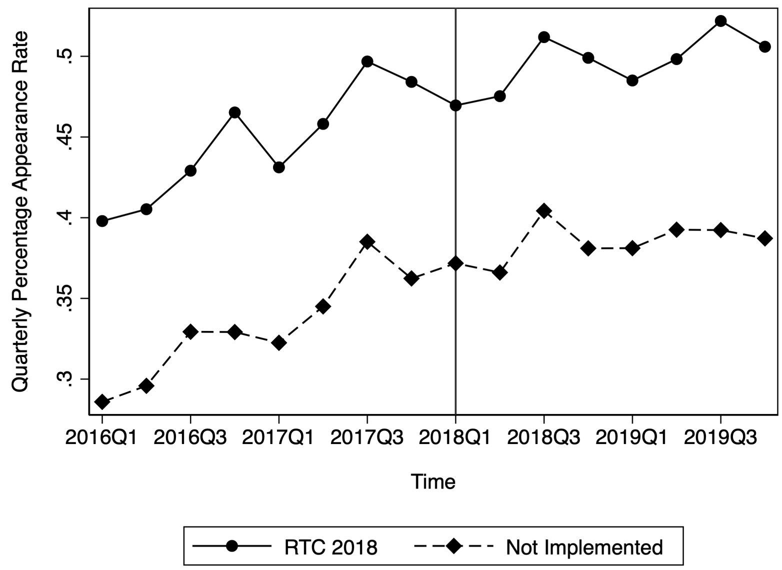


Figure 3.A.8. Quarterly Tenants' Appearance Rate.

Notes: Figure shows tenants' appearance rate by policy implementation over time. Appearance rate is defined as the total number of cases where the tenant responded to the Notice of Petition and appeared at assigned court date over total filed cases.

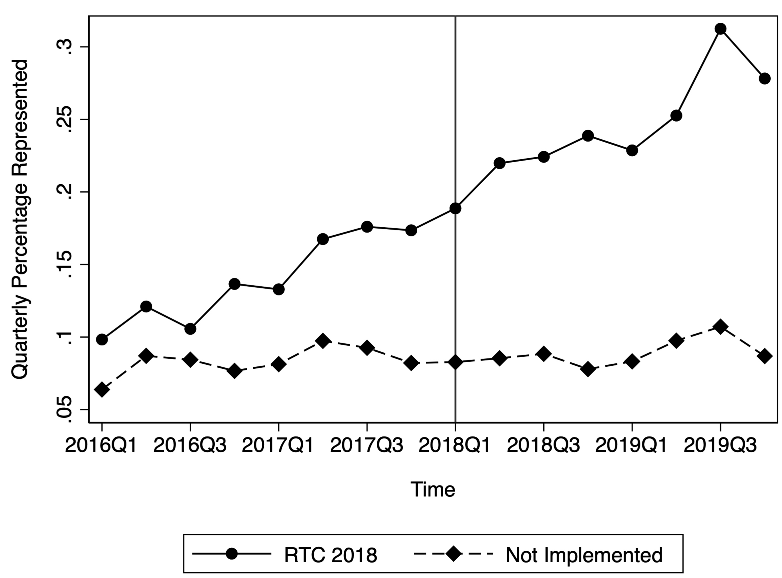


Figure 3.A.9. Quarterly Tenants' Representation Rate.

Notes: Figures show tenants' representation rate over time by policy implementation. Representation rate is defined as the total number of cases with legal representation over total filed cases.

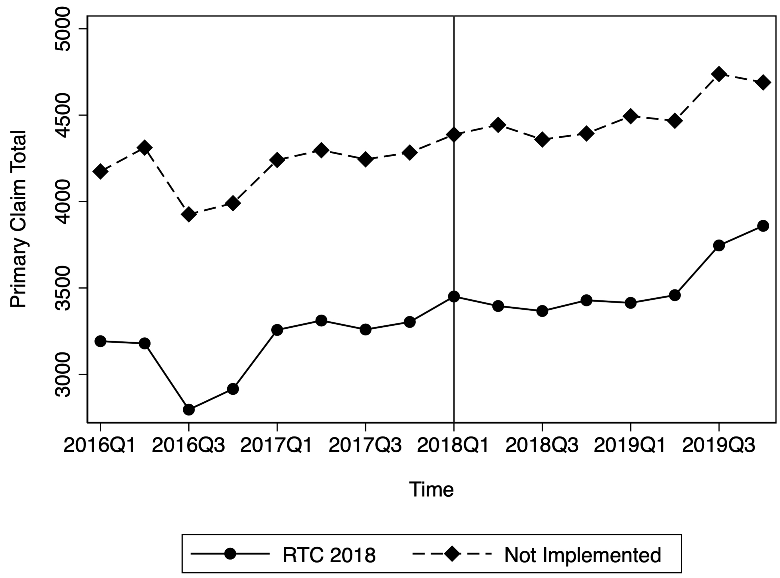


Figure 3.A.10. Primary Claim Total.

Notes: Figure shows the mean primary claim total over time by policy implementation. Primary claim total is the dollar amount that the landlord claimed the tenant owed.

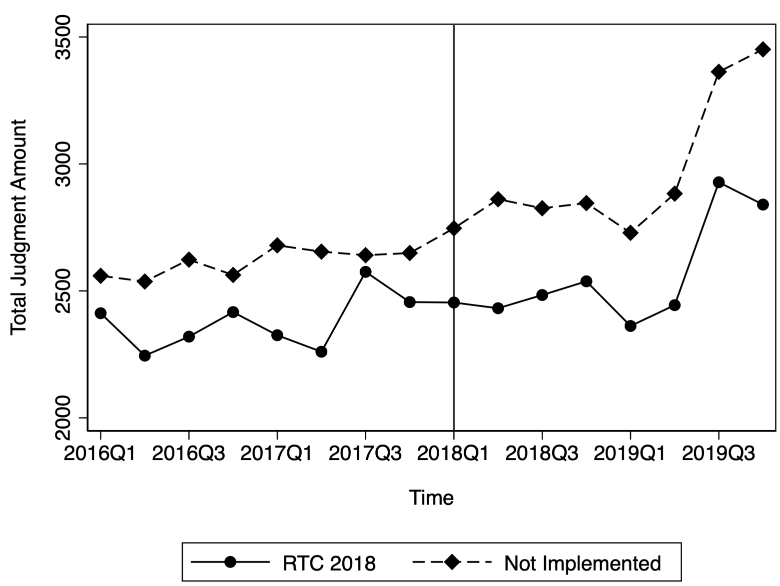


Figure 3.A.11. Total Judgment Amount.

Notes: Figure shows total judgment amount over time by policy implementation. Total judgment amount is the final dollar amount settled between the two parties and signed by the judge.

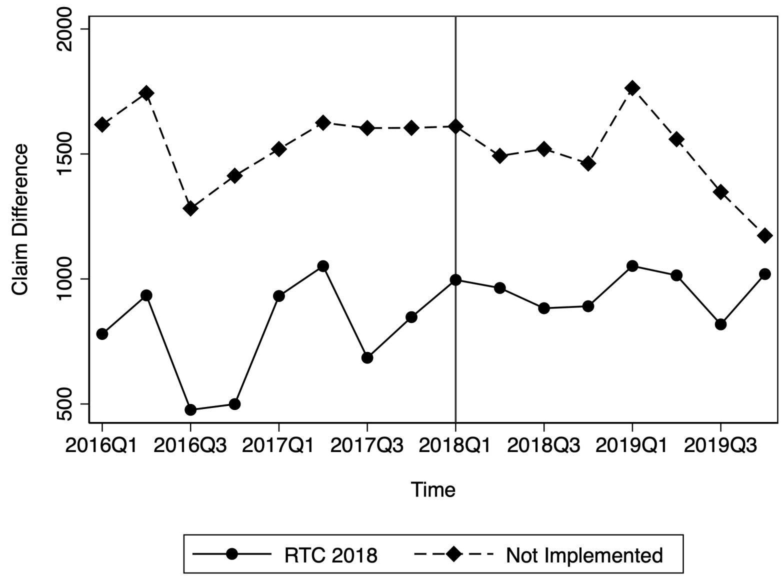


Figure 3.A.12. Claim Difference.

Notes: Figure shows claim difference over time by policy implementation. Claim difference is the difference between the primary claim total and the total judgment amount.

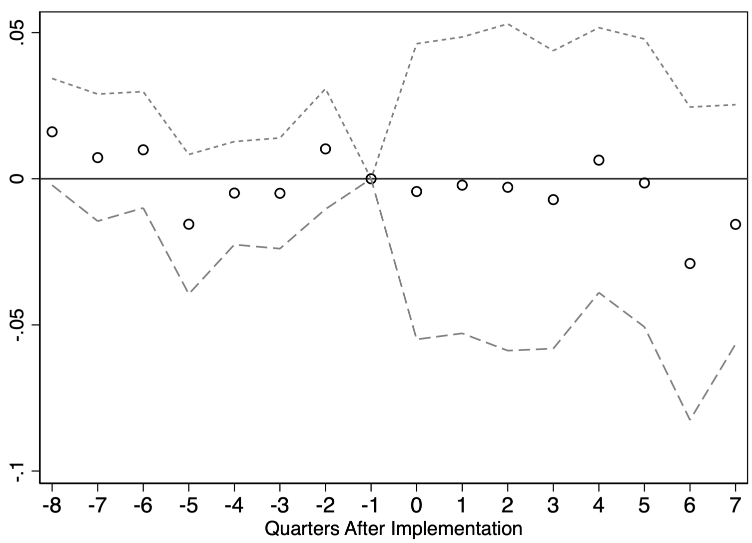
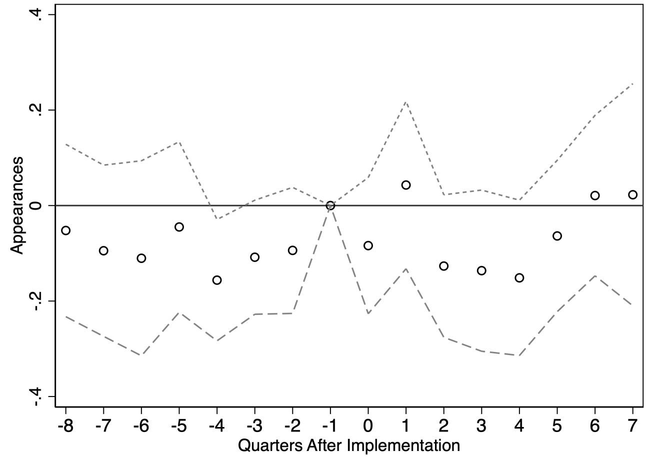
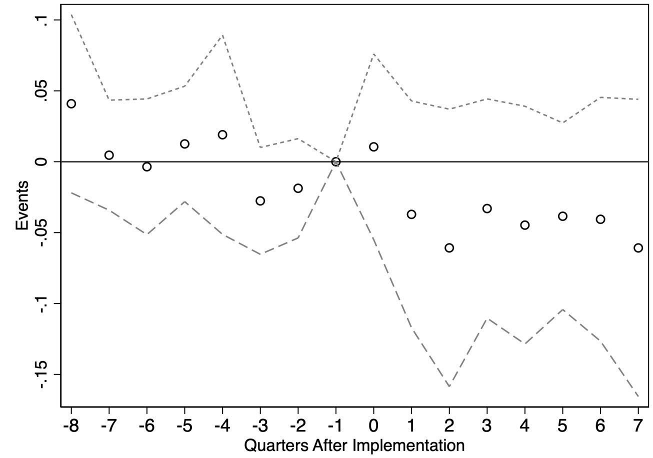


Figure 3.A.13. Difference-in-Differences Event Study of Active Rate.

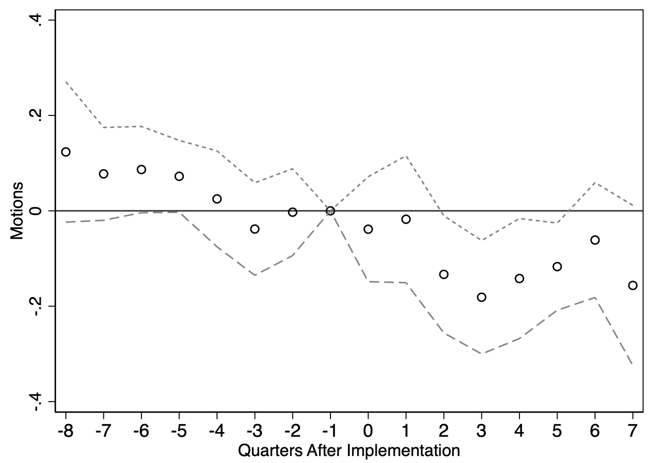
Notes: Figure shows the DID event study for court case active rate. The hollow circles are point estimates of γ_t based on Equation 3.2 with demographics variables as controls. The short dash and dash lines represent the upper and lower bounds of the 95 % confidence intervals based on robust standard errors clustered at the zip code level.



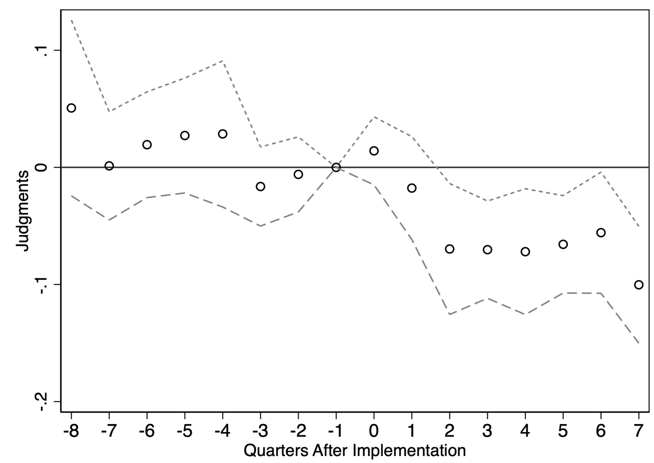
(a) Appearances



(b) Events



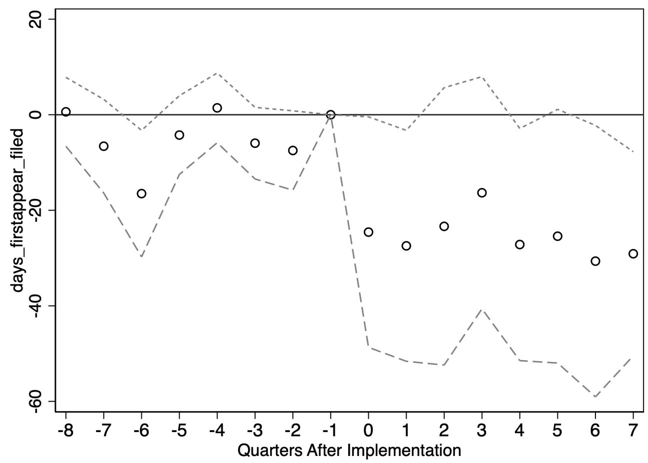
(c) Motions



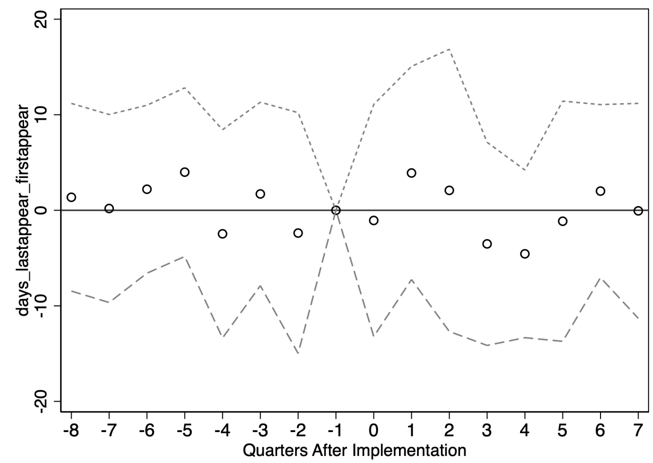
(d) Judgments

Figure 3.A.14. Difference-in-Differences Event Studies of Court Efficiency Measures.

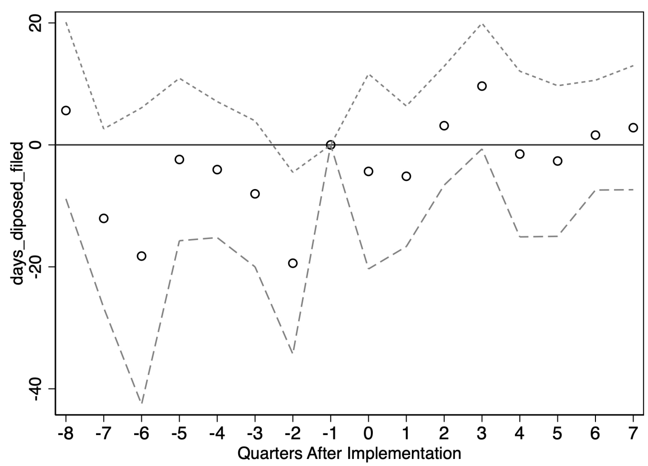
Notes: Figure shows the DID event studies for the court efficiency measures. The hollow circles are point estimates of γ_t based on Equation 3.2 with demographics variables as controls. The short dash and dash lines represent the upper and lower bounds of the 95 % confidence intervals based on robust standard errors clustered at the zip code level.



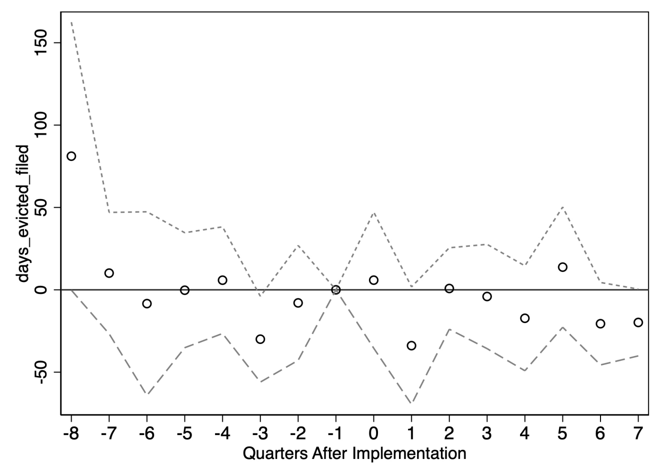
(a) First Appearance Date − Filed Date



(b) Last Appearance Date − First Appearance Date



(c) Disposed Date − Filed Date



(d) Evicted Date − Filed Date

Figure 3.A.15. Difference-in-Differences Event Studies of Court Duration Measures.

Notes: Figure shows the DID event studies for the court duration measures. The hollow circles are point estimates of γ_t based on Equation 3.2 with demographics variables as controls. The short dash and dash lines represent the upper and lower bounds of the 95 % confidence intervals based on robust standard errors clustered at the zip code level.

Table 3.A.1. Difference-in-Differences Regression for Monetary Outcomes.

	Primary Claim Total		Total Judgment Amount		Claim Difference	
	(1)	(2)	(3)	(4)	(5)	(6)
DID	53.376	70.310	-170.664**	-140.652*	223.406**	217.966***
	(105.564)	(97.603)	(71.714)	(72.105)	(93.781)	(77.553)
Control_mean	4266.841	4266.841	2759.612	2759.612	1476.048	1476.048
95 % CI	[-155.0 , 261.7]	[-122.3 , 262.9]	[-312.2 , -29.12]	[-283.0 , 1.667]	[38.30 , 408.5]	[64.90 , 371.0]
Conley-Taber 95 % CI						
Observations	2,764	2,764	2,725	2,725	2,725	2,725
Number of Zip Codes	175	175	174	174	174	174
R-squared	0.099	0.106	0.053	0.057	0.014	0.018
Controls						
Demographics		X		X		X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Please see the notes to Table 3.3. The outcome variables are the levels of primary claim total, total judgment amount, and claim difference.

3.10 Appendix NYC Housing Court Records

I requested New York City housing court records from the Office of Court Administration (OCA) under the New York State Unified Court System on April 22, 2020. I received the anonymized court records from January 1, 2016 to December 31, 2019 provided by Annette Parisi from OCA on July 15, 2020. Data extract is stored in the XML (Extensible Markup Language) file format.

I list the variables and definitions in the XML file.

- Index number ID: Unique number to identify the index.
- Court: The full name of the court.
- Filed date: Date the index was filed with the court.
- Property type: Indicates whether the property is “commercial” or “residential”.
- Classification: Indicates whether the filed reason is “non-payment”, “harassment”, “HP”, or “others”.
- Specialty designations: A court-defined way to further classify an index.
- Status: Current status of the index, i.e. “Active”, “Disposed”.
- Disposed date: Date the index was disposed.
- Disposed reason: The action that disposed of the case.
- Primary claim total: The total amount of the primary claim (sum of all primary claim cause of actions).
- Primary claim cause of actions: The reasons why the index is before the court. Cause of action type (i.e. breach of lease, breach of rental agreement) and monetary amount are included for each entry.

- Property address: The address of the property that the case concerns. As the data is anonymized, I only receive the zip code of the property.
- Parties: The parties on the index. For each party, the role (petitioner, respondent, or interested party), the type (person, business, or agency), representation type (counsel, self-represented litigant, or no appearance), and status of undertenant are listed.
- Events: The events of the index. Each event entry includes the event name, filed date, fee type, filing parties, and answer type (oral or written).
- Appearances: Appearances for the index. Each appearance entry includes date and time, purpose of appearance (hearing, conference, or motion), reason, Court Part, and appearance outcomes.
- Motions: Applications to request certain decisions to be made by the court for the index. Each entry includes the type (order to show cause, general, or ex-parte), primary relief (i.e. restore to possession, restore to calendar, vacate judgment), filed date, filing party, motion decision (granted or denied – index disposed), and motion decision date.
- Judgments: Decisions made by the court for the index. Each judgment entry includes the type (i.e. failure to answer, failure to appear, hearing), filed date, entered date, latest judgment status, latest judgment status date, total judgment amount, creditors and debtors.
- Warrant: A warrant ordered for the index. The entry includes created reason, ordered date, issuance type, issuance stayed date, issued date, execution type, execution stayed date, marshal request date, vacated date, returned date, returned reason, and execution date.

I identify the court case outcome as "evicted" if there exists a warrant vacated date or if the court record can be matched to the NYC marshal eviction log.

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