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The effects of an Ellis Act eviction on neighborhood socioeconomic status

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ABSTRACT

Rent-control advocates argue that its strongest feature is offering tenants strong protections from economic displacement. Nonetheless, rent control may have negative effects on tenants, as previous research has shown that these tenants have longer commutes and higher unemployment rates because they are incentivized to stay in place even after their location is no longer optimal. I study what happens to tenants when they are displaced from their rent-controlled apartments by exploiting a California law called the Ellis Act that allows landlords in Los Angeles and San Francisco to evict tenants even if they are lease-compliant, under the condition that all the tenants in the building must be evicted at once and are compensated by the landlord with substantial relocation payments. In large apartment buildings (five units or more), these Ellis Act evictions act as an exogenous shock because these landlords are unlikely to be evicting all their tenants just to target an individual household. Using Infutor data, I identify over 900,000 people who lived in a five-plus unit rent-controlled apartment in either San Francisco or Los Angeles in 1999, 11,470 of whom were evicted between 2000 and 2007. I find that evicted tenants were less likely to stay in their original city and more likely to live in lower-income and lower-intergenerational-mobility neighborhoods than control tenants. The negative effects of these evictions appear to be highly persistent: neighborhood socioeconomic status is lower for the evicted group than the control group at least 12 years ex post. These findings support that the Ellis Act imposes steep costs on tenants and may be partially undermining California's recent attempts to improve housing affordability and stability.

JEL Classification Codes: J26, J22, J21, R23

Key Words: Rent control, evictions, neighborhood mobility, Mahalanobis matching

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Introduction

California’s ongoing challenge to provide mass affordable housing has fueled concerns that middle- and working-class families are getting priced out of high-opportunity areas. Affordable housing advocates claim that California has a law on the books that is only making a bad problem worse: the Ellis Act. This highly controversial law allows landlords, usually with rent-controlled buildings, to evict all of their tenants and withdraw the building from the housing supply, even if all the tenants are lease-compliant. The Ellis Act may thus be undermining California’s attempts to shield renters from escalating housing costs, such as its 2019 statewide rent-control ordinance, as previous research has shown that rent-controlled landlords are more likely to use the Ellis Act when their rents go up (Asquith, 2019a). However, there is very little empirical evidence of the impact of an Ellis Act eviction on tenants, leaving policymakers with an incomplete idea of how important it is to reform this law.

In this paper, I generate new evidence on the consequences of losing a rent-controlled apartment via an Ellis Act eviction by examining how it changed people’s mobility and location patterns. I generate causal evidence by exploiting the requirement in the Ellis Act that *all* tenants must be evicted. Tenants get at least 120 days to vacate, and in some cases receive relocation payments,¹ but a key provision is that the landlord cannot selectively remove tenants: all must go. I argue (and show evidence thereto) that landlords in sufficiently large apartment buildings—specifically, five-plus-unit buildings—could not optimize their decision to use the Ellis Act just to target one tenant household. Thus, Ellis Act evictions are an exogenous displacement shock to tenants in sufficiently large buildings, causing them to quasi-randomly lose their rent-controlled units.²

¹Berkeley, Los Angeles, San Francisco, Santa Monica, and West Hollywood mandate that landlords must give Ellis’d tenants relocation payments. These can be substantial, ranging from about ~\$13,000 to ~\$20,000, depending on the city.

²This is not to say that the landlord’s choice to do an Ellis Act eviction is random. As shown in Asquith (2019b), Ellis Act and other no-fault evictions are more likely to occur when rents rise. However, as long as Ellis Act evictions are independent of each household’s personal circumstances, it functions as an exogenous displacement shock.

One reason that the impact of an Ellis eviction on tenants is unclear is that the evidence on displacement's long-run impact on people is mixed: whether it is negative or positive (in economic terms) depends on context. For example, gentrification-pressured out-migrants do not seem measurably worse off (Brummet and Reed, 2021), but tenants evicted for being out of compliance with their leases do seem to suffer deleterious effects (Collinson et al., 2022). However, a much broader literature encompassing other displacement events, such as the U.S. government's Japanese interment program (Arellano-Bover, 2022), displacements of Germans and Poles after World War II (Becker et al., 2020; Bauer, Braun, and Kvasnicka, 2013), and Hurricane Katrina evacuees (Deryugina, Kawano, and Levitt, 2018), finds a mix of both positive and negative economic effects on the displaced.

One reason that displacement may yield some positive economic results is that displaced people are often forced to reoptimize their employment and location decisions for themselves and their children (Chyn, 2018; Sanbonmatsu et al., 2006; Kling, Ludwig, and Katz, 2005). Tenants with rent-controlled apartments may be particularly likely to reoptimize this way, as prior studies have shown that rent control encourages people to prioritize keeping their controlled apartments over shorter commutes (Krol and Svarny, 2005; Gross, 2020) or seeking the best job match (Svarer, Rosholm, and Munch, 2005). Further, California and municipal policies give Ellis'd tenants an adjustment cushion, both in terms of relocation payments and a long minimum notice period of 120 days. These factors may help displaced tenants do better on certain economic outcomes than comparable non-evictees. On the other side of the ledger, because rent control encourages landlords to reduce their housing supply (Asquith, 2019b), largely by converting to owner occupancy (Diamond et al., 2019), tenants can find it much harder to find a new, comparable apartment locally if they have no reason to seek a new job. Thus, it is hard to know from first principles how deleterious an Ellis Act eviction is on individual tenants.

To study how Ellis Act evictions impact displaced tenants, I leverage individual address histories offered by Infutor Data Solutions. The Infutor data are from a commercially avail-

able data set that longitudinally tracks individuals' migration histories within the United States using publicly available information. I match the Infutor data to property records provided by county assessors offices and CoreLogic Solutions, which allows me to construct a comprehensive sample of people who were living in a five-plus-unit building in either Los Angeles or San Francisco in 1999. From that starting point, I can follow their residential histories until 2019. I use Ellis registration data from the San Francisco Rent Board and the Los Angeles Housing Community and Investment Department to identify which buildings had Ellis Act eviction notices issued over the 2000–2007 time period, and am thus able to assign treatment and control status based on whether the building did or did not receive a notice. Overall, I build a large control sample of about 907,465 individuals and a smaller, but still substantial, treated sample of 11,470 individuals.

Unfortunately, the Infutor data do not have high-quality information on individual-level outcomes, such as income or occupation. Thus, I proxy for whether an Ellis Act eviction had a negative or positive impact on evictees by comparing how neighborhood-level measures differ between the treatment and control groups after an eviction. I get these neighborhood-level outcomes from the Neighborhood Change Database, which reports 2000 Census data in 2010 Census Tracts. Additionally, because my study design involves a large, never-treated control group, a treatment group that gets treated at different times (i.e., a staggered roll-out design), and a very long ex post observation period of 12+ years, I account for possible contamination bias using the event study regression method of Sun and Abraham (2021).³ Their method ensures unbiased weighted averages of treatment effects across all relative time periods, and I report results from their method as my preferred specification.

My regressions thus generate estimates of the effect of receiving an Ellis Act eviction notice on various measures of neighborhood quality, and have a causal interpretation under the assumption that Ellis Act evictions in large buildings were effectively independent of

³A recent literature has pointed out that naively using ordinary least squares to estimate event studies in the presence of staggered treatment times is very likely to produce coefficient estimates biased by treatment effects from other relative time periods. Sun and Abraham's method accounts for this potential bias and circumvents it. See their paper for greater discussion.

tenant characteristics. I present strong evidence that there is no selection into treatment on most observable building or individual characteristics for buildings with five or more units, and there is clear evidence that buildings in whiter, wealthier neighborhoods were more likely to be Ellis'd. This is almost surely because these buildings were most likely to be targeted for condo conversion, not because landlords are discriminating against better-off white tenants. I thus correct for the difference in initial conditions by using propensity score matching to rebalance my sample.

I estimate results for five outcomes. First, I look at outcomes related to job access: whether a person remains in their original local labor market, defined as either the San Francisco or Los Angeles commuting zones (Abbreviated as CZ; Tolbert and Sizer, 1996); and their probability of living in a census tract with long (> 25 minutes) commutes. Second, I look at changes in overall neighborhood socioeconomic status (SES) by estimating changes in the neighborhood median household income of the treated group. Further, while the Infutor data do not include moves for children, I quantify whether Ellis'd people move to areas with higher or lower intergenerational economic mobility using the Opportunity Atlas (OA) Census tract income data (Chetty et al., 2020). The OA income measure captures the average expected income attained of people who grew up in a given 2010 Census tract by the time they reach their midthirties.

I find in almost every analysis that an Ellis eviction does steeply increase the annual probability of moving—not just in the period the notice was received, but on a sustained basis. I also find across specifications that individuals became less likely than the control group to remain in their original labor market after being Ellis'd. I also find that the treated group sorts into lower SES neighborhoods ex post, as measured both by adult outcomes (median household income) and the children's future incomes (OA incomes). Treated adults find themselves living in neighborhoods with about 3 percent lower median household incomes and living in neighborhoods where their children can expect to make about 1 percent less than the control group 12 years after receiving an Ellis notice. Overall, being Ellis'd

makes people more likely to move out of the SF or LA metro areas altogether (and thus likely forcing them to change jobs) and relocate to somewhat poorer neighborhoods.

Primarily, this paper contributes to a recently reinvigorated literatures on evictions (Desmond, 2012; Collinson et al., 2022) and rent control’s impact on tenants (Diamond et al., 2019; Gross, 2020; Autor, Palmer, and Pathak, 2019; Asquith, 2019b). Secondly, this paper also hopes to generate additional evidence on adult outcomes after displacement in hopes of pointing the way forward on resolving several outstanding empirical debates on the value of job access and neighborhood effects for adults. The current evidence indicates that adults’ labor market outcomes change little when they move to new neighborhoods, but neighborhoods can influence physical and mental health, as well as subjective well-being (Chyn and Katz, 2021). However, most of these studies have focused on the lowest-income adults, because the key empirical difficulty is finding real-world examples of random (or quasi-random) shocks to people’s locations. Thus, the literature on the short- and long-run consequences of moving individuals across geographies has focused on policy experiments or one-off events. The Ellis Act, by contrast, has generated thousands of displaced individuals across urban California since the 1980s and continues to do so today. By introducing these evictions to the literature, I hope to spur further studies harnessing the Ellis Act’s power to act as an informative natural experiment on the value of location and the impact of displacement on individuals and their families.

The rest of the paper proceeds as follows. Section 1 gives an overview of the Ellis Act and other relevant legal points. Section 2 discusses the Infutor, property, and census data. Section 3 outlines how I generate causal, unbiased estimates of the impact of receiving an Ellis Act eviction notice. Section 4 reviews the results of the analysis. The paper concludes in Section 5.

1 Key Institutional Details

The Ellis Act was enacted into law by the California State Legislature on July 1, 1986, in response to the 1984 California Supreme Court Case *Nash v. City of Santa Monica*,⁴ where 17-year-old landlord Jerome Nash sued the city of Santa Monica for the right to evict all his tenants and demolish the rent-controlled apartment building his mother had helped him buy. Santa Monica had recently passed a rent-control law that also included restrictions on either demolishing or converting controlled units to condominiums. While Nash admitted he could achieve a “fair” return under Santa Monica’s laws, he claimed that Santa Monica’s laws amounted to an unconstitutional taking of his property without due compensation, saying:

“There is only one thing I want to do, and that is to evict the group of ingrates inhabiting my units, tear down the building, and hold on to the land until I can sell it at a price which will not mean a ruinous loss on my investment.” (*Nash v City of Santa Monica*, 37 Cal. 3d 97 (1984))

The California Supreme Court ruled against Nash, asserting that municipalities had a right to regulate their housing supply and that Santa Monica’s regulations did not amount to a violation of the U.S. Constitution’s Fourteenth Amendment. In response, the California Legislature effectively overruled the California Supreme Court by enshrining into law the right for a landlord to go out of business by evicting all of their tenants. Local municipalities could no longer prevent landlords from exercising their rights to leave the rental business, but were allowed to regulate Ellis Act evictions via notice requirements, relocation payments, and other restrictions. The legislature later amended the Ellis Act in the 1990s to require that landlords give at least 120 days notice to tenants of eviction and to allow cities with rent-control ordinances to enforce these on the Ellis’d buildings if the landlords put them back on the rental property market after two years of being withdrawn. Further, if a landlord seeks to return the vacant building to market within 10 years of pursuing an Ellis Act eviction,

⁴California Legislature (1986), p. 570-571.

the landlord must give the evicted tenants the right of first refusal to their original unit.

Initially, relatively few landlords used the Ellis Act. However, by the late 1990s, housing values in California had steadily risen, causing the opportunity cost of having a controlled property relative to having an uncontrolled property to rise.⁵ The Ellis Act became an increasingly popular means for landlords to vacate rent-controlled properties, which can command a higher sales price in rent-controlled jurisdictions.

1.1 Municipal Ellis Restrictions

In addition to the Ellis Act, San Francisco has additional regulations on Ellis Act evictions. San Francisco started imposing relocation payments for Ellis Act evictions in February 2000, but initially only for low-income tenants and for those where the household included someone who was aged 60+, disabled, or had a minor child. Los Angeles as well imposes some additional requirements on Ellis Act evictions. First, Los Angeles passed laws in February 1986 (partly to anticipate the Ellis Act) that mandated relocation payments for no-fault evictions. Relocation payments are paid out to each unit's house household but can vary in amount by each individual tenant's age, disability status, income, the presence of minor dependent children, and how long the tenant has been in their unit. More information, including a timeline of changes to the relocation payments and other relevant policy changes, can be found in Asquith and Reed (2021).

2 Data

Information on Ellis Act evictions comes from the Los Angeles Housing Community and Investment Department (HCIDLA) Custodian of Records and the San Francisco Rent Board. These eviction records were linked to individual buildings using public and private property records. In San Francisco, I obtained annual property records from the San Francisco County

⁵For example, in San Francisco, just 5 buildings were Ellis'd in 1995. By comparison, between July 1998 and June 1999, 205 buildings were Ellis'd (Castillo, 2001).

Office of the Assessor-Recorder, which has digitized property records for every parcel in the city from 2003 to the present. In Los Angeles, these data came from the Los Angeles County Office of the Assessor, with digitized annual records from 2006 to the present. These data include information such as property classification (residential or not), number of units, and year built. I use these variables to establish whether a building is multifamily, large (defined as five or more units), or rent controlled.⁶ Los Angeles eviction records were linked directly to the property records by their unique parcel number. For years prior to 2003 in San Francisco, I used a parcel crosswalk provided by the San Francisco Department of Planning to backfill in properties that might have been demolished or converted to condos between 1998 and 2003. For Los Angeles, I backfilled in the property panel by assuming that there were no major changes to the existing stock of buildings over this time period. This lowered my match rate between the property panel and the list of Ellis'd buildings, with the most important consequence of making my treatment group somewhat smaller than it otherwise could be. The San Francisco eviction data do not include the parcel number the Ellis Act eviction occurred in, so I performed an exercise to match the SF Rent Board's eviction address records to building parcel ID numbers.⁷

This linkage allowed me to identify my potential treatment buildings (rent-controlled buildings that filed an Ellis Act eviction notice with their respective cities) and control buildings (rent-controlled buildings that did not Ellis their tenants). I then linked longitudinal individual address histories from Infutor Data Solutions to determine which individuals were living in the treatment and control buildings. Infutor creates these histories using sev-

⁶In San Francisco, a building is rent controlled if it has two or more units and was built before June 13, 1979. In Los Angeles, the cutoff date is for buildings built before October 1, 1978.

⁷I describe this process more in Asquith (2019b). It is a difficult exercise, because buildings may be listed under several addresses, particularly large buildings that front multiple streets. Further, parcel numbers are often changed after an Ellis Act eviction when the landlord then changes the use type of the now-vacant building. For example, an apartment building has one parcel number, but in a condo association, each condo gets its own parcel number, reflecting that each is a unique, separable property. If a building were to be Ellis'd in January 2003, then by June 2003 (when the assessor generates its annual cross-section of the city's properties), the only matches in the assessor's records for the address of the Ellis'd building will be for all of the successor condos. You would thus need access to historical parcel records to find out what the original parcel number was.

eral public and private sources, such as USPS change of addresses, county assessor records, magazine subscriptions, or white page listings. Since these data are intended to be sold to direct marketers, they tend to be high quality and are reported down to the unit level. Addresses are assigned an estimated arrival date, and individuals have some limited demographic information, namely age (for only about one-third of the observations) and gender (for about 85 percent of the observations). Previous work has shown that the data closely match the census’s over-25 population at the tract level, without substantive differences in coverage by tract characteristics (Asquith, Mast, and Reed, 2021; Diamond, McQuade, and Qian, 2019; Phillips, 2020).

After matching people to buildings via addresses, I then mapped treatment and control group status from buildings to individuals. Since Ellis Act evictions occurred in the most substantial numbers in Los Angeles and San Francisco between 2000 and 2007, my sample is comprised of tenants living in either a treatment or control building in 1999.

Table 1 shows Ellis Act eviction counts of impacted buildings, units, and people by city and year, from 2000 to 2007. In total, 11,470 people were identified as living in a building in 1999 that would go on to receive an Ellis Act eviction notice between 2000 and 2007. Eviction notices reached their height during the housing boom, as many landlords moved to clear out buildings in hopes of ultimately converting the units to condos, particularly in Los Angeles.⁸ This treated group is reasonably large, and it is set against a vastly larger control sample of 907,465 individuals who were living in 36,258 non-Ellis’d buildings.⁹

Figures A1 and A2 are maps showing how the treatment and control buildings are distributed within San Francisco and Los Angeles, respectively. Figure A1 shows that the large

⁸See, e.g., Hyman (2006); Zahniser (2006). San Francisco likely saw less of a spike of Ellis Act evictions during this time, because the city only allowed landlords of two-to-six-unit buildings to condo convert via an annual lottery. Buildings with seven or more units are not allowed to condo convert at all. In 2013, the lottery was suspended until 2024, but two-unit buildings were allowed to condo convert under certain ownership restrictions (Asquith and Reed, 2021).

⁹After 2007, the number of Ellis Act evictions falls significantly. While they pick up again before the COVID-19 Recession, they do not reach the heights observed during the housing boom. To avoid possible contamination from buildings that were treated later, I drop all buildings that were Ellis’d between 2008 and 2019.

apartment buildings are concentrated largely in northern and eastern parts of San Francisco, with a particularly large concentration of both treatment and control buildings in downtown San Francisco (the northeast corner) and the Mission District (central east section) parts of the city. Figure A2 shows that these apartment buildings were reasonably evenly distributed across Los Angeles. In Section 3.1, I quantify how the treated buildings are distinct from the control buildings.

2.1 Race Assignment

Following Diamond, McQuade, and Qian (2019), I employ a two-step procedure to assign an imputed race/ethnicity category to individuals from the Infutor data set. The first step is to use the NamePrism algorithm, an ethnicity/race classification program primarily intended for academic research (Ye et al. 2017). NamePrism uses first and last names to compute probabilities an individual is non-Hispanic black, non-Hispanic white, Hispanic, non-Hispanic Asian or Pacific Islander (API hereafter), non-Hispanic American Indian or Alaska Native, or non-Hispanic of two or more races.¹⁰ The second step uses Bayes’s rule to update the name-based probabilities exploiting the local race/ethnicity distribution in the census block of the building the tenant lived in when the individual first enters the sample.

Specifically, the second step involves updating each tenant’s baseline racial/ethnic probabilities with the 2000 racial/ethnic distribution of the census tract of their initial in-sample building. This information comes from the Neighborhood Change Database (NCDB), which reports census data from 1970 to 2010 in standardized 2010 census tract geography. As in Diamond et al. (2019), I then use Bayes’s rule to obtain posterior probabilities for the six classifications.¹¹ I assign a person to a given racial category if their posterior probability for

¹⁰NamePrism uses a training data set of 74 million labeled names from 118 different countries, representing 90 percent of the world’s population. They then map these to a taxonomy of 39 leaf nationalities, and condense this down further to the six racial/ethnic categories used by the U.S. Census Bureau.

¹¹For both San Francisco and Los Angeles, I use the subset of census tracts that actually contain one of my in-sample buildings. Census tracts in San Francisco are perfectly coterminous with its municipal boundaries, because it is a consolidated city-county, but not in Los Angeles. For these, I do a spatial merge to determine what fraction of a given tract’s land area is within Los Angeles’s municipal boundaries, and use this fraction

that category is at least 80 percent.

2.2 Neighborhood Data

I also analyze outcomes based on various neighborhood characteristics drawn from the NCDB, where neighborhood is defined as being synonymous with their census tract. I focus on the neighborhood’s 1999 median household income as my chief neighborhood outcome of interest. I also use median household income as a way to examine heterogeneity in response to displacement—i.e., are outcomes different for people displaced from high-median household income versus low-median household income tracts?

While I cannot directly observe the locations of children, I measure how displacement might have likely impacted their long-run outcomes by using data from the OA, a project that used tax data from the U.S. Internal Revenue Service (IRS) to quantify intergenerational income mobility by census tract (Chetty et al., 2020). The OA uses census and IRS data to first identify where children born between 1978 and 1983 grew up, and then use their incomes at the ages of 31–37 (corresponding to their mean incomes in 2014 and 2015) to calculate what these children made on average as adults. The authors also perform these calculations by race and gender, by parents income rank, and for other outcomes, such as incarceration and teenage pregnancy rates. From the OA, I chiefly use the children’s average adult incomes, both overall and by race, as my outcome of interest. Thus, estimating an Ellis notice’s impact on the adult’s neighborhood’s OA income helps indicate whether people are moving to high (or low) opportunity areas for their children, separate from whether they are moving to higher or lower SES areas for themselves.

2.3 Description of the Pooled Data Set

Table A3 reports the mean and standard deviation of my outcomes of interest: whether the individual moved (“Moved”); whether the individual still lives in their initial sample

as a weight in the calculation of posterior probabilities.

CZ (“In Sample CZ”), which proxies for whether they are able to stay in their same local labor market and thus keep their job after receiving an Ellis notice; whether the person has a long commute, defined as being 25 minutes or longer (“Long Commute”);¹² the median 1999 household income (in 2020 dollars) of the initial census tract the individual lived in (“Median HH Income”); and the income (in 2020 dollars) at age 35 of the people who grew up in that neighborhood (“OA Income”) drawn from the Chetty et al. (2020) OA.

I report these mean outcomes overall (specifically, across all the five-plus-unit rent-controlled buildings) and by selected subsamples. About 4.6 percent of the sample moves annually, and about 94 percent of the sample stays in their original CZ over the entire study period (1999–2019). The average neighborhood has about 51 percent of the residents facing a long commute; the average neighborhood median household income is about \$59,400; and those who grew up in there can expect to make about \$44,300 by the age of 35.

The other subsamples I report include by people’s initial (1999) tenure in their buildings; by race; for those living in the bottom half of tracts sorted by 1999 median household income; and those living in the top half of tracts sorted by the difference between 1999 median household income and OA income—capturing which tracts had the highest upside potential for children relative to adult incomes. The means here vary substantially by subgroup. Whites have the highest OA and median household incomes, highest moving rates, and lowest attachment to the SF and LA community zones (CZs). People with the shortest tenures move the most, and those with the longest tenures live in the neighborhoods with the highest median household incomes, but otherwise people look fairly similar across tenure length. It is key to note here that the average median household income in the low-income neighborhoods is \$45,616—while this is around \$14,000 below the sample average, this figure would have been close to the median income in many other neighborhoods throughout the United States in 1999. This reflects that incomes, and housing costs, are particularly high

¹²In the 2000 Census, self-reported commute times are top coded at 25 minutes. While this is the best metric available for this analysis, San Francisco and Los Angeles are infamous for having particularly long commutes for residents, so some important variation may be lost.

in these two cities, even 20+ years ago.

3 Empirical Strategy

I study the impact of an Ellis Act evictions as an exogenous shock to residence in a rent-controlled apartment and then compare the outcomes for the evictees against a large sample of tenants who were not evicted. I generate causal estimates of the treatment effect under the assumption that these evictions are quasi-randomly assigned to tenants in sufficiently large buildings, so that it was impracticable for landlords to target an Ellis Act eviction at individual tenants based on their personal circumstances or characteristics. Under this assumption, ex post differences in outcomes can be attributed to the effects caused by receiving an Ellis Act eviction notice.

To study the impact on outcomes Y_{it} , I first specify a base event study regression model:

$$Y_{it} = \alpha + \sum_{e=-8}^{19} \beta^{e=g-t} E_{it}^{b(i),g} + X_{it}\theta + \delta_t \times Tract_{b(i)} + \delta_i + \epsilon_{it}, \quad (1)$$

where $E_{it}^{b(i),g}$ is a dummy variable for whether individual i in year t received an Ellis Act eviction notice in building $b(i)$, where $b(i)$ indexes the building each individual lived in their initial period, and g indexes the building’s eviction (treatment) year. Functionally, g indexes the year treatment was first received, called hereafter the treatment cohort. The regression includes nonparametric controls for time-invariant characteristics via fixed effects for individuals (δ_i), which also subsumes fixed effects for the initial in-sample buildings ($\delta_{b(i)}$). I also include initial tract of residence-by-year fixed effects ($\delta_t \times Tract_{b(i)}$), which control for annual shocks to neighborhoods. This also subsumes controls for city- or state-level policy changes that could have impacted the treatment effect, such as city-level adjustments to Ellis Act relocation payments. X_{it} has vectors for the building’s average residential tenure and for the number of years since person i first moved into $b(i)$ out to a fourth-order polynomial, which controls for how the length in residence may influence outcomes. Some specifications

augment X_{it} with vectors of age out to fourth-order polynomial and dummies for whether an individual qualifies as an “elderly” tenant in their initial city.¹³ These age-based controls are not included in every regression because, as mentioned above, age is only observed for about a third of the sample.

Thus, β_e measures the net impact of receiving an Ellis Act eviction notice on each person’s outcomes e time periods either before or after the eviction occurred. Specifically, the net effect accounts for not just the subsequent displacement event (if it occurred), but also any relocation payments or assistance the evictee might have received. The key identification assumption for a causal interpretation of β^e is that the assignment of $E_{it}^{b(i),g}$ is independent of pretreatment individual-level outcomes.

For three reasons, the results may be biased nonetheless, even if this assumption holds. One reason is that eviction at the level of the building itself is nonrandom, because these landlords are likely responding to local neighborhood price signals. This nonrandomness from location effects can translate into the composition of the treatment and control groups looking quite different on observables, inasmuch as tenant SES characteristics correlate with the neighborhood’s SES characteristics. This could bias the results because moves in the treatment group are then going to mechanically look quite different than the control group. To make this problem more concrete, let’s assume that the treatment group lives in higher-income neighborhoods than the control group on average. Were two otherwise completely identical individuals from each group then move to a neighborhood at the exact citywide average, the difference in income levels between the origin and destination neighborhoods is going to mechanically be wider for the treated individual.

The second reason is that the control group may not actually serve as a proper counterfactual for how the treatment group would have fared if they had not been evicted, either because tenants in the treated buildings are actually being targeted even at higher unit counts or because the control group and treatment groups were evolving along fundamen-

¹³In San Francisco, tenants aged 60 and over are entitled to additional relocation payments. In Los Angeles, it is those aged 62 and over.

tally different paths even prior to eviction. The last reason is that a recent literature has shown that the event study regression given in Equation (1) can yield biased estimates of β_e when effects from other time periods contaminate the time period of interest when there is treatment effect heterogeneity across treatment cohorts. I address all three possible sources of bias.

3.1 Addressing Imbalance between Treatment and Control Groups

Panel A in Table 2 illustrates the problem of imbalance between treatment and control groups by showing mean values of key characteristics for individuals at base year by treatment and control status, the difference between them, and the p-value for that difference. I report these separately for San Francisco and Los Angeles in Appendix Table A1.¹⁴

In general, the two samples differ meaningfully on several observable individual and neighborhood characteristics. In part, this is driven by the large size of the sample, ensuring that most differences are statistically significant at the 1 percent level. Nonetheless, it is qualitatively clear that some of these differences are indicative of differential landlord behavior between the treatment and control sample.

The first difference is that the treated units are much smaller—about 13 fewer units than in the control sample (33 versus 46). Appendix Figures A3 and A4 confirm that people in the largest (50+ units) and smallest (2 unit) multifamily buildings are relatively less likely to get Ellis'd, but people in the 4- to 8-unit range were particularly likely. The treatment buildings as well are about five years older than the control buildings. These differences track with what we know about incentives under the Ellis Act. Older buildings in cities with sufficiently high housing prices are going to be more attractive to redevelop (Glaeser and Gyourko, 2005; Brueckner and Rosenthal, 2009; Rosenthal, 2014), making them more attractive for landlords to Ellis. Further, bigger buildings are going to be, on balance, less attractive to Ellis not only

¹⁴From Table 1, the treated share of the population is very small—1.2 percent of the pooled sample; 0.8 percent of the San Francisco sample; and 1.3 percent of the Los Angeles sample. Thus, for all intents and purposes, summary statistics for the control group can be taken as the baseline summary statistics for the sample as a whole.

because total relocation payments increase with each unit, but also because the probability that at least one tenant will litigate the eviction in court increases with each unit as well.

Other differences support the idea that neighborhood characteristics matter in addition to building characteristics. Buildings slightly farther away from their downtowns (“Ln(Distance)”) were more likely to be Ellis’d.¹⁵ Also noteworthy is that the treated sample is 21.2 percent whiter than the control sample (57.2 percent versus 47.2 percent), located in neighborhoods with fewer people having long commutes (49.4 percent versus 51.0 percent) and higher median household incomes (\$64,635 versus \$58,316). While it is theoretically possible that landlords were using the Ellis Act to discriminate against white, better-off tenants, the most likely explanation is that tenant characteristics correlate with neighborhood characteristics, and older, smaller buildings in whiter, wealthier neighborhoods were the most attractive targets for redevelopment that an Ellis Act eviction would hasten.

These differences in means are not uniform across the two cities. Table A1 reveals that San Francisco and Los Angeles are idiosyncratically imbalanced. For example, the San Francisco sample is fairly well racially balanced, with no statistically significant differences in the shares of whites, Hispanics, and APIs, but the unit count gap between treated and control groups is a yawning 47.5 units (7.2 versus 54.7). In Los Angeles, by contrast, the treated sample is markedly whiter than the control sample (55.6 percent versus 41.2 percent), but the difference in unit counts is only 9.6 (33.2 versus 44.8).

These differences in treatment versus control neighborhoods, buildings, and (ultimately) people mean that the results could be biased, because an Ellis Act eviction may mechanically force people to move to poorer neighborhoods in a kind of mean reversion. Second, because the treated group comes disproportionately from the smallest buildings where the identification assumption is weakest. I address this issue first by using propensity score matching (PSM), which will generate inverse probability weights (IPWs) to simulate random assignment to treatment based on the covariates (including unit count), and then directly

¹⁵Distance from downtown is measured in meters as the distance from the individual building and each city’s respective City Hall.

examining if there is any evidence that there is differential selection into treatment by unit count.

3.2 Propensity Score Matching

I address the imbalance in neighborhood and individual socioeconomic characteristics by using PSM to create probability weights that simulate randomizing treatment status based on observable characteristics. While there is not a canonical approach to creating weights using PSM in panel fixed effects settings, Arkhangelsky and Imbens (2022) point out that they can be created in cross-section and then used across all time periods for each unit.¹⁶

To calculate the PSM weights, I use each individual’s initial observation as the basis for my cross-section. This ensures that the sample is balanced at least in the initial period, making it more likely that the treatment and control groups are balanced throughout the pretreatment period. With slight abuse of notation, I define $E_i^{b(i)} = \mathbb{1} \left\{ \sum_t E_{it}^{b(i)} > 0 \right\}$, i.e., a dummy variable for whether a person is ever subject to an Ellis Act eviction. The weights are generated using the following logit regression:

$$\begin{aligned} \text{logit}(E_i^{b(i)}) = & \pi_0 + \pi_1 \text{Tenure}_{it(i)} + \pi_2 \text{AvgTenure}_{it}^{b(i)} + \pi_3 \text{White}_i + \pi_4 \text{API}_i + \pi_5 \text{Hispanic}_i \\ & + \pi_6 \text{Black}_i + \pi_7 \text{UnitCount}_i^{b(i)} + \pi_8 \text{YearBuilt}_i^{b(i)} \\ & \pi_9 \text{LAT}_i^{b(i)} + \pi_{10} \text{LONG}_i^{b(i)} + \text{InSFInteractions}_i \chi + \gamma_{\text{Tract}(i)} + \eta_{it(i)} \end{aligned} \quad (2)$$

where Tenure_{it} is the person’s tenure in building $b(i)$ in 1999; $\text{AvgTenure}_{it}^{b(i)}$ is the average tenure of all the tenants in building $b(i)$ in 1999; and White_i , API_i , Hispanic_i , and Black_i represent the four major racial groupings of interest in this paper: non-Hispanic whites, Asian and Pacific Islanders (API), Hispanics of any race, and blacks. I also include four other building-level variables. $\text{UnitCount}_i^{b(i)}$ and $\text{YearBuilt}_i^{b(i)}$ are building $b(i)$ ’s unit count and year built, respectively. I also control for building $b(i)$ ’s latitude ($\text{LAT}_i^{b(i)}$) and longi-

¹⁶I also note that Callaway and Sant’Anna (2021) adopt this approach in their groundbreaking paper on a doubly robust approach to staggered treatment roll-out designs.

tude ($LONG_i^{b(i)}$) to (imperfectly) make the building better balanced on location.¹⁷ Lastly, *InSFInteraction* collects the interaction terms between a dummy indicating whether the building is in San Francisco and the other variables. These interaction terms recognize that the substantive differences between the SF and LA samples discussed above that may predict selection into treatment.¹⁸ I then use the predicted probabilities, $\hat{E}_i^{b(i)}$, generated by (2) to create IPWs via the following stepwise function:

$$W_i = \begin{cases} \frac{1}{\hat{E}_i^{b(i)}} & \text{if } E_i^{b(i)} = 1 \\ \frac{1}{1 - \hat{E}_i^{b(i)}} & \text{if } E_i^{b(i)} = 0 \end{cases} \quad (3)$$

Table 2 Panel B shows the balance of building and individual characteristics after applying the IPWs. It's immediately clear that the matched sample is better balanced between the treatment and control groups. For example, the average unit count across buildings for the treated group is now 35.8 versus 33.6 in the control group, with a p-value of 0.88, indicating that there's no statistical difference between these averages. A similar pattern emerges for year built (p-value for the difference between treatment and control now 0.113) and percent API (p-value now 0.138). The log of the average distance to downtown is now almost identical between the treatment and control groups, meaning that the two groups are now more similarly distributed spatially. While there's little improvement on median household and OA incomes overall, this is an improvement over Panel A, as it is particularly important that the samples look similar in terms of building characteristics, given that selection into treatment occurs at the building level.

¹⁷Nearest-neighbor matching using Mahalanobis distance would probably yield a more accurate match between the treatment and control groups, but the econometrics literature has not yet determined how to generate heterogeneity-robust event study estimates after matching by this method.

¹⁸Another important reason to do this is that the two cities have somewhat different policy environments surrounding the Ellis Act that may nonlinearly influence selection into treatment. These interaction terms account for these differences better than could a simple dummy variable for whether the building is in San Francisco.

3.3 Pretrends and Robustness

Next I address whether my control group serves as a proper counterfactual for the treatment group. I begin by showing the qualified existence of parallel trends between the treatment and control groups in the data with the IPWs applied.

Appendix Figure A5 shows mixed evidence of parallel pretrends. Mean outcomes for the treatment group are very unstable, relative to the control group, in the first six pretreatment years. This is driven by the nature of the staggered roll-out design. For example, only the group treated in 2007 contributes to measuring the mean outcomes at event time $e = -8$. Similarly, trends in the treatment group again become very unstable starting at event time $e = t + 13$, because event time $e = 13$ is when some treatment groups (specifically, the 2007 treatment cohort) stop contributing to the average. I address this issue by narrowing the estimation scope to cover just $e = -2$ through $e = 12$.¹⁹

That said, accounting for the jumps caused by the staggered rollout design, there is some clear evidence in favor of both parallel pretrends and that receiving an Ellis notice causes people to change residences. Figure A5 Panel A shows a clear spike in the probability of moving at $e = 0$ that persists through at least $e = 3$. Similarly, focusing on just the period covering pre-periods $e = \{-1, -2\}$ shows some stabilizing once enough of the treatment groups start contributing to the averages. Neighborhood median household income rises for both the treatment and control groups between $e = -2$ and $e = -1$ by similar amounts. If you exclude the 2000 treatment group, which enters the sample at $e = -1$, then the year-on-year changes between $e = -2$ and $e = -1$ for average neighborhood median household income are 0.0037 log points for the control group and 0.0027 log points for the treatment group. Similarly, average neighborhood OA incomes rise by 0.0002 and 0.0004 over that same time period for the two groups respectively. For being the original CZ, these figures are -0.0096 and -0.0058 . While not exactly the same, the changes have similar magnitudes

¹⁹I retain $e = 2$ even though not all cohorts participate, because it is important to show a lack of pretrends in at least one pretreatment period. Typically, the coefficient on $e = -1$ is set to zero, leaving just $e = -2$ as my sole eligible pretreatment period.

as well as the same sign. Only for moving are these figures divergent: -0.0035 and 0.0024 for the treatment and control groups, respectively. However, given the very large spike in moving observed at $e = 0$, it is fairly clear that the Ellis notices are generating a moving effect that is independent of whatever “false positive” nonparallel pretrends might generate.

I next empirically test my assumption that tenants in five-plus-unit apartment buildings are not likely to be targeted for eviction based on their individual characteristics. For rent-controlled landlords, the most important factor determining how much rent they can collect is based on the tenant’s length of tenure. All else being equal, the longer the tenure, the steeper the discount between the tenant’s contract rent and the rent the landlord could collect on the vacant unit (Basu and Emerson, 2000). Thus, the lowest unit count where landlords seem to stop discriminating based on length of stay offers the best opportunity to generate a sufficiently large sample of treated people whose treatment status is assigned independently of their personal characteristics.

To identify the right cutoff, I split the sample by building unit count into 11 groups for unit counts 2 through 12. I then run a logit regression that tests various individual- and building-level characteristics on their probability of predicting the individual being Ellis’d:

$$\begin{aligned} \text{logit}(E_i^{b(i),U=u}) &= \psi_0 + \psi_1 \text{Tenure}_{it(i)} + \psi_2 \text{AvgTenure}_{it}^{b(i)} + \psi_3 \text{White}_i + \psi_4 \text{API}_i \\ &+ \psi_5 \text{Hispanic}_i + \psi_6 \text{Black}_i + \psi_7 \text{YearBuilt}_i^{b(i)} + \psi_8 \text{LAT}_i^{b(i)} \\ &+ \psi_9 \text{LONG}_i^{b(i)} + \gamma_{\text{Tract}(b(i))} + \eta_{it} \end{aligned} \quad (4)$$

where U is the unit count of building b , $u = \{2, 3, \dots, 12\}$.

Appendix Figure A6 shows the results of regressing Equation (4) and reports the values of ψ_1 in Panel A and the values of ψ_2 in Panel B for each value of u . Overall, Appendix Figure A6 shows strong empirical support for the five-unit cutoff assumption. Panel A shows tenure does not predict being treatment at any unit count, reinforcing that landlords do not use the Ellis Act to target individual tenants. Panel B shows that the building’s average

tenure *negatively* predicts being Ellis'd in four-unit buildings, but the effect is slight. By $u = 5$, the effect is no longer statistically significant at the 5 percent level.²⁰ Overall, there is little evidence that longer-staying tenants are targeted for eviction. While there is some evidence that landlords in four-unit buildings become less likely to Ellis their tenants as average length of tenure rises, this effect loses significance in five-unit buildings, and more or less disappears in six-plus-unit buildings. Thus, I proceed with using five-unit buildings as my cutoff, but in Section 4.2, I present results from both two- and six-unit buildings and show that changing building size does not meaningfully change the results.

3.4 Estimating Unbiased Average Treatment Effects on the Treated

While the relative rarity of Ellis Act evictions means that it is advantageous to exploit multiple years' worth of evictions to boost the study's power, recent econometric studies have determined that a staggered treatment rollout design (wherein different units receive treatment at different times) can generate biased estimates of the average treatment effect on the treated (ATT) when there are heterogeneous treatment effects across treatment times and cohorts.

In an event study context, I am interested in calculating an unbiased estimate of the cohort-specific average treatment effect on the treated (CATT) e time periods from the initial treatment for each cohort g using the Sun and Abraham (2021) method, where each $CATT_{g,e}$ represents the cohort-specific average treatment effect on the treated e time periods from the initial treatment of units in cohort g . I then supplant the biased β_e 's from Equation (1) with the weighted average of the CATTs across treatment cohorts as:

$$v_e = \frac{1}{|\mathcal{L}|} \sum_e \sum_g CATT_{g,e} \mathbb{P} \{G_i = g | G_i \in [-e, T - e]\} \quad (5)$$

where e is (as above) a set of relative periods, $e = \{-2, -1, \dots, 12\}$, and \mathcal{L} collects the

²⁰While not presented in this paper, the coefficients on the other characteristics also tend to be statistically indistinguishable from zero. These results are available on request.

disjoint sets of e , or $\mathcal{L} = \cup_e e$. G_g is a dummy variable that equals 1 when a unit’s first treatment year is equal to g . I use the STATA program developed by Li (2021) to calculate the empirical analog of v_e , \hat{v}_e , for each of my outcomes of interest.

4 Results

I first confirm that receiving an Ellis notice causes people to relocate. It is critical to establish that I can empirically observe a spike in moving contemporaneous with being issued an Ellis Act notice because my data are not administrative. It is also important qualitatively to check the magnitude of this effect, because a large spike in moving at event time $e = 0$ is strong evidence that most treated tenants actually relocate.²¹

Fortunately, Figure A7 shows that the annual moving rate increases by 4.8 percentage points immediately (*i.e.*, at event time $e = 0$). The baseline moving rate in the control sample is 4.6 percent, according to Appendix Table A3, so being Ellis’d more than doubles a tenant’s annual probability of moving. This elevated treatment effect exists even well after the notice is issued: treated tenants are 4.1 percentage points more likely to move at $e = 1$, and the treatment effect persists at an average of 4.1 percentage points per annum thereafter. Without question, receiving an Ellis notice did induce people to move and very likely caused treated tenants to become persistently more mobile even well after the notice was received.

With that in mind, Figure 1 shows the results for whether the tenant remains in their original labor market; the share of people in their tract of residence with a “long” (> 25 min) commute; the logged 1999 median HH income of their neighborhood; and the logged OA income for their neighborhood. In lieu of more precise individual-specific measures, the general idea is that the first two act as proxies for access to jobs, either in the sense that people were able to stay in the same labor market after being Ellis’d ($Pr(InSampleCZ)$) or that they did not have to accept substantially longer commutes to keep the same job

²¹*Ex ante*, one should not expect \hat{v}_0 here to be very close to 1, because some Ellis notices were fought in court and some tenants as well get extra time to vacate the building based on their characteristics.

($Pr(Commute > 25mins)$). The two neighborhood income measures proxy for the economic mobility of adults after an Ellis Act eviction, and the change in the OA income proxies more directly for how an eviction altered the expected adult incomes of evicted kids. While adults' incomes are generally less responsive to these sorts of neighborhood effects than kids' (see *e.g.*, Nakamura et al., 2021; Kling, Liebman, and Katz, 2007; Chyn, 2018), the ability to find a job can be influenced by neighborhood socioeconomic characteristics (Hellerstein, Kutzbach, and Neumark, 2013; Asquith et al., 2021).

Panel A shows unambiguously that the treated group became steadily less and less likely to be in their original local labor market as time goes on relative to the control group. Interestingly, it appears that the treated group initially tries to stay in their original local labor market for the first few time periods *ex post*: \hat{v}_0 is only 0.1 percentage points and is not statistically significant, while \hat{v}_1 and \hat{v}_2 are equal to just -0.8 and -0.9 percentage points and only significant at the 10 percent level. Thereafter, however, the treatment effect steadily declines until it reaches -2.6 percentage points by $e = 12$. Since 90.2 percent of the control sample is still in their original CZ 12 years or more *ex post*, then the treated group is only about 2.9 percent less likely. While this is not a major difference, it is notable because workers in the San Francisco and Los Angeles labor markets command some of the highest average wages (Diamond and Moretti, 2021), making it quite likely that a small but significant share of tenants had to take lower-paying jobs after being Ellis'd. More research is needed to understand employment changes within the same CZ after an eviction.

This does not necessarily mean that the treated sample as a whole had to see a new job or that those who did were worse off on every dimension. Figure 1 also shows the results for the average commute time in the neighborhood of residence. It shows that while treated tenants initially move to neighborhoods with a higher share of the residence taking long commutes, this effect drifts down to a precise zero between $e = 1$ and $e = 4$, before rebounding to a very slightly positive number between 0.1 to 0.3 percentage points for the rest of the study period. Essentially, the treated group's *ex post* location decisions look no different than the

control groups on this dimension, even accounting for the fact that many members of the treated group had to relocate at least once.

By contrast, Panel B shows strong evidence that treated tenants moved to poorer neighborhoods and that this effect is very persistent. While the treated group starts off in slightly better-off neighborhoods by both measures *ex ante*, they start to sort into worse neighborhoods even by $e = 0$, with \hat{v}_0 being equal to -0.006 and -0.004 log points for median household income and OA income, respectively. This effect grows to -0.013 log points by $e = 5$ for the OA income neighborhood measure before retreating slightly and stabilizing around -0.011 log points, while for median household income, the effect continues to grow until $e = 8$ when it reaches -0.031 log points and stabilizes thereafter around that level. These results imply that treated tenants were sorting into neighborhoods where the median adult could expect to make \$1,841 less than the neighborhoods of the control group. Similarly, using the average neighborhood OA income level of \$44,320 from Appendix Table A3, this implies that the children of treated tenants wound up in neighborhoods where they could expect to make \$576 less per year by the time they were 35 than the children of control tenants.

Overall, the evidence indicates that evicted tenants initially react by finding apartments in neighborhoods that have broadly similar commute times as their originating neighborhoods, but are nonetheless in poorer neighborhoods. Some of this effect may be a mechanical one due to people moving out of the SF and LA commuting zones, where most neighborhoods are going to have lower median household incomes. It is very likely that the draw of these poorer neighborhoods for evictees is that prevailing rents are more likely to be similar to the rent-controlled discounted rents they were paying in their originating apartments than the prevailing rent for a vacant apartment in the same originating neighborhood. While the decline in neighborhood SES for the evictees is stark, the silver lining is that the adults appear to have been targeting neighborhoods whose upper mobility penalty for their children was substantially smaller than the implied penalty for the adults themselves. While this

study can only observe neighborhood-of-residence changes to the treated group, Figure 1 presents strong evidence that Ellis Act evictions left the average evictee living in a neighborhood with lower SES than if they had not been evicted, and more likely to have left the high-compensation, high upward mobility SF and LA metro areas altogether.

4.1 Heterogeneity by Individual Characteristics

4.1.1 Results by Tenure in Unit

I next explore Ellis treatment effects for various subgroups. I focus on effects by tenure and by race. Ex ante, one would generally expect that those with longer tenures would be the most likely to experience downward neighborhood mobility, because they would be the most likely to fan out into poorer neighborhoods to find rents closest to the ones they were paying before the eviction. However, one should be cautious in interpreting the tenure effect, because it is likely confounded with the tenants' age: longer tenures are mechanically associated with being older, all else being equal. Higher age and tenure also predict having greater wealth, both because people tend to make more money with age and because longer-tenured tenants with rent control are going to be able to build wealth faster. Greater wealth could cushion the effect of the eviction, meaning that it is possible that effects on the longest-tenured tenants may be attenuated.

Figure 2 shows the results by three tenure groups: those initially living in their apartments for less than three years; for between three and six years; and for more than six years. Figure 2 shows indeed that tenants of the shortest tenures did not experience downward neighborhood SES mobility, nor did they generally have to move to neighborhoods with higher shares of residents taking long commutes. While there is evidence that they did wind up leaving the SF and LA local labor markets, they only become statistically less likely to be living in these CZs at $e = 4$, probably coinciding when the effect of being in a rent-controlled apartment prompts significantly higher attachment in the control group.

For the middle-tenure group, there is no evidence that they are any more or less likely

to leave the SF or LA CZs than the control group, even 12 years ex post, but this group nonetheless appears to move to lower SES neighborhoods upon being Ellis'd, and the difference between the treated and control groups magnifies over time. The results for this group are strong evidence that the decline in neighborhood SES observed in Figure 1 is not being driven solely by people who move to different CZs. For at least this group of tenants, they compensate for being evicted by moving to a poorer neighborhood where rents are presumably lower.

The longest tenured group, by contrast, is both more likely to leave their home CZ as well as initially settle into lower SES neighborhoods. The effect of relocating out of their original CZ is quite persistent: at $e = 12$, treated tenants are 5.3 percentage points less likely to be in their original CZ than the control group. However, unlike for the middle-tenured group, the longer-tenured group shows evidence of reverting back to neighborhoods with similar median household and OA incomes as the control group. By $e = 7$, the difference in OA incomes is no longer statistically significant, and by $e = 12$, the coefficient itself is quite close to zero. For median household income, the difference is no longer statistically significant at the 5 percent level at $e = 12$, and \hat{v}_{12} is just -0.030 versus -0.037 for \hat{v}_8 .

The most plausible explanation for the differences between the middle-tenured versus longer-tenured groups is that middle-tenured people are those most exposed to negative displacement effects, because they fall into a gray zone where they have not lived in a rent-controlled apartment long enough to enjoy a heavily discounted rent but also have not risked staying in an apartment past the income-maximizing optimal tenancy. This group thus appears the most tied to their CZ for a variety of reasons, and finds the easiest adjustment margin to be moving to a new neighborhood with lower incomes and thus (likely) lower rents.

4.1.2 Results by Race

Figure 3 shows results for four major racial groupings: non-Hispanic whites, APIs, non-Hispanic blacks, and Hispanics. The results across racial groupings show some pretty pro-

found differences in outcomes. Non-Hispanic whites have the strongest evidence of downward neighborhood SES mobility: at period $e = 0$, treated whites have sorted into neighborhoods with 0.005 lower median HH income log points as well as 0.009 lower OA income log points, although only the latter is statistically significant. These differences with the control group continue to grow all the way through the end of the summary period until they are 0.034 log points and 0.025 log points lower than the control group for median HH income and OA income, respectively, by $e = 12$. Some of this effect may be driven by people leaving the SF or LA metro areas for other CZs, where median incomes are almost always lower, as Panel A shows that treated whites also sort out of their original CZ at a growing rate so that treated whites are almost 5 percentage points less likely to be living in their original CZ by $e = 12$.

In contrast, all non-white groupings show much stronger attachment to their original local labor market and weaker evidence of downward neighborhood SES mobility. APIs show the least amount of evidence for downward neighborhood SES mobility, or really, much of a change to their overall neighborhood status at all upon being prompted to move. Treated APIs appear to stay in the SF or LA areas at similar rates to their control group but sort into neighborhoods with somewhat longer average commutes. Most likely, this represents a migration from neighborhoods closer to downtown (where gentrification pressures may be stronger) to more outlying areas. While the point estimates on the treated group's neighborhood median household income and OA income are negative, the confidence intervals are wide enough that a null effect cannot be ruled out for most periods ex post.

Hispanics, if anything, show much stronger evidence of suburbanization within their existing local labor market. Treated Hispanics are *more* likely than their control group to stay in the SF or LA areas, with the effect growing to 1.8 percentage points (or about 1.9 percent more likely) by the end of the study period. As well, they sort into neighborhoods with longer commutes, and this difference with the control group grows to 0.5 percentage point more likely by $e = 12$. In terms of neighborhood SES, Hispanics sort into neighborhoods with somewhat lower median HH income and OA income (-0.012 and -0.009 log points,

respectively, at $e = 12$) than those their control group. However, the median HH income estimate is only significant at the 10 percent level at $e = 12$, and the estimates at several time points are not statistically significant at all. This suggests that treated Hispanics are more likely to suburbanize than their control group, but select neighborhoods are not too different from the control group on household income. Nonetheless, the fact that treated Hispanics sort into neighborhoods where OA incomes are lower is a point of concern, because it indicates that they may have trouble finding schools in suburban areas that provide the same kind of opportunities for their children as the neighborhoods from which they were displaced.

Blacks, by contrast, seem to show the most resilience to displacement among the four groups, although it is worth noting again that they are the smallest group in my sample, and their results are correspondingly quite noisy. Nonetheless, I find that treated blacks are more likely to sort out their original CZ, and this effect strengthens over time. In contrast, they initially move into neighborhoods with slightly lower shares of long commuters, although this effect attenuates over time. Similarly, treated blacks initially sort into neighborhoods with *higher* median HH incomes: at $e = 0$, treated blacks are living in neighborhoods with 0.036 log points higher than those in the control group, and this effect is statistically significant. However, the treatment effect peaks around $e = 4$ and then declines and loses statistical significance so that by $e = 12$, the treatment effect is only 0.021 log points. Interestingly, treated blacks do not sort into neighborhoods with higher or lower OA incomes than the control group. It is disappointing to note that treated blacks sorting into neighborhoods with higher median incomes and shorter commutes does not automatically mean they sorted into neighborhoods associated with higher expected adult incomes for their children. This is clearly an area where more research is needed.

4.2 Robustness Checks

I next consider some ways the results in Figures 1–3 may be biased in spite of the steps taken in Section 3, mostly by the fact that Ellis Act evictions are more likely to occur in whiter, more affluent neighborhoods, even after weighting the sample. I address the mean reversion issue discussed above in Section 3.1 by trying to show more directly that the declining neighborhood SES effects found among whites, Hispanics, and middle-tenured tenants are not an artifact of how the sample was constructed, but are in fact attributable to the causal effect of being Ellis’d. I thus test the robustness of my main results in three ways. First, I look at results from buildings in the bottom half of the median HH income distribution. This will now change the direction of mean reversion from downward to upward. Next, I check the robustness of my exclusion assumption by adjusting the unit cut-offs. Although age is only observed for about half the sample, I add age as a fourth-order polynomial as a control, as well as a dummy variable for getting additional time to quit a unit due to being “elderly” according to SF and LA eviction rules.²²

4.2.1 Neighborhood Affluence

Since Ellis Act evictions are slightly skewed toward better-off neighborhoods, I look at the subset of census tracts that are at the bottom half of their city’s median household income distribution. This in part changes the direction of any mean reversion from downward to upward. The results are reported for my outcomes of interest in Figure A8a. Panel B shows little evidence of mean reversion in terms of neighborhood SES: while the estimates are no longer statistically significant at the 5 percent level, the results still show a clear pattern of people moving to neighborhoods with lower median household incomes, although the estimates for neighborhood’s OA incomes is closer to zero. The most plausible explanation remains that displaced tenants move to poorer neighborhoods as they seek housing costs after the eviction that are close to the rents they were paying for the controlled apartment

²²As mentioned above, this is 60 years and over in San Francisco and 62 and over in Los Angeles.

from which they were displaced.

Interestingly, this group shows a rising pattern of attachment to their original CZ so that by $e = 12$, treated tenants are 0.7 percentage points more likely to be in their original CZ than control tenants. While there is not enough evidence to draw a firm conclusion on why this may be, one possibility is that rent control allows people in poorer areas (likely poorer themselves) more breathing space to do the transition out of expensive San Francisco and Los Angeles on their own time and with greater resources. Further research is needed here to better understand this phenomenon.

4.2.2 Unit Counts

The unit cutoff was chosen based on finding the lowest unit count where the coefficient on tenure and the building's average tenure was not statistically significant *and* there were no higher unit counts whose coefficients on these measures are statistically significant. However, this rule itself is based on the assumption that these are the most important observable variables for a landlord weighing whether to Ellis his or her tenants, and may obscure that there are unobservable factors that could make the "true" cutoff higher or lower. As discussed above, a higher cutoff is likely less biased but will introduce more variance because the treated sample of individuals shrinks to 9,235. A lower cutoff has the converse problem, but the fears of bias may be overstated: Figure A6 shows no obvious reason to think that people are being targeted based on their tenure at even the two-unit building level. Thus, I report results for the sample of people in two-plus-unit buildings and six-plus-unit buildings in Figure A9.

There are few differences between the results in the two-plus-unit sample, the six-plus-unit sample, and the five-plus-unit main sample reported above. The ex post probabilities of moving are quite similar across samples. Comparing Subfigure A9b with Figure 1, a higher cutoff produces a somewhat lower probability of being in the original CZ about six years or more ex post, but the overall trend is quite similar. Further, the two-plus-unit cutoff in Subfigure A9c shows stronger persistence of treated tenants moving to lower SES neighbor-

hoods than those in the six-plus-unit sample or in the main sample. Overall, though, it does not appear that including even the smallest buildings would have meaningfully changed the results, which reinforces the findings that a landlord's decision to Ellis his or her tenants is likely driven by neighborhood price trends and idiosyncratic factors, rather than tenant characteristics.

With that in mind, I next present results by racial grouping for two-plus-unit buildings. Since blacks and APIs in particular suffer from potential small sample bias at the five-plus-unit cutoff, using all two-plus-unit buildings may recover a better idea of what the treatment effects on these two groups are without introducing undue bias. Under this change, the treated number of blacks rises from 235 to 618, and the treated number of APIs rises from 486 to 767. Results in Figure A10 largely confirm those in Figure 3, with some small but important differences. Results for whites are almost exactly the same, but APIs now show a slightly weaker tendency to be out of their original CZ in the latter time periods ($e \geq 7$), mostly due to wider confidence intervals and a somewhat stronger tendency to be in neighborhoods with lower OA incomes. The most important difference between the two-plus-unit and five-plus-unit samples for APIs is that there is no longer any statistically significant evidence that this group moves to neighborhoods with lower median household incomes. If anything, they initially sort into neighborhoods with higher median incomes at times $t = 0$ and $t = 1$, before settling down to a statistically zero difference with the control group thereafter.

Blacks show the greatest differences between the full two-plus-unit sample and the main five-plus-unit sample. Their previous tendency to move out of their original CZs and to neighborhoods with a lower probability of having a long commute vanish: Figure A10d shows that all of the ex post coefficients on the probability of being in the original CZ are now statistically indistinguishable from zero, and the coefficients on having a long commute are now statistically insignificant from $e = 5$ onward. The evidence on neighborhood SES quality is now more mixed. In the main sample, there are no statistically significant differences

between treated and control groups in terms of neighborhood OA incomes ex post. In the full sample, treated blacks move into neighborhoods with about 0.01 lower logged OA incomes than the control group between $e = 1$ and $e = 7$, but this effect then goes effectively to zero from $e = 8$ onward. Similarly, treated blacks now gradually sort into higher income neighborhoods than the control group, until by $e = 8$, they are living in neighborhoods with 0.033 log points higher median household incomes and this effect persists until the end of the study period. In contrast, the main sample shows a reversed treatment gradient: blacks initially move into about 0.035 log points higher median household income neighborhoods between $e = 0$ and $e = 2$, before the effect declines and becomes statistically insignificant thereafter.

Overall, Figures A9 and A10 confirm that changing the unit cutoffs does not meaningfully change the direction of the results, except in the case of blacks, in part because there may be significant estimation improvements by accessing a larger treated sample.²³

4.2.3 Age Controls

I now introduce age controls by including age as a fourth-order polynomial as well as a dummy variable for whether someone is considered an elderly tenant by their respective city. This effectively cuts the sample by 62.5 percent but still leaves about 3,274 treated individuals.²⁴ Figure A11 shows that adding age controls nearly doubles the size of the treatment effect on moving. The annual probability of moving spikes 8.3 percentage points, or a 184 percent increase over the baseline, as compared to 4.8 percentage points in the baseline model presented in Figure A7. While the treatment effect on moving is higher across all study periods when age controls are added, from $e = 10$ onward, the treatment

²³However, one should be careful before assuming the estimates for blacks are more accurate in the two-plus-unit sample than the main sample. Lowering the unit cutoff may make it much more likely that tenants are being targeted for their race, so the exogeneity assumption may be violated, introducing bias. I found no evidence from Equation (4) that being black had a statistically significant effect on being selected into treatment, but the coefficients were positively signed for unit counts two and three.

²⁴Fortunately, the remaining treated individuals are relatively evenly divided among the three tenure groups: 992 were living in their units for less than three years in 1999; 1,214 between three and six years; and 1,069 for more than six years.

effects converge so that the estimates with and without age controls are quite close. This strengthens the finding that treated individuals remain more mobile than the control group even a decade or more ex post.

Overall, Figure A12 shows that adding age controls does not substantively change the main findings by tenure group, but it does alter the trajectories of the treatment path for some outcomes that is worth commenting on. First, the confidence intervals on almost all results widen substantially, which means that one should have somewhat less confidence in these results due to the smaller treated group problem discussed above. While the probability of living in neighborhoods with longer commutes does not change much, the probability of staying in one's original CZ declines much more quickly and sharply. By $e = 12$, the average treated person goes from being 2.6 percentage points less likely to be in their original CZ in Figure 1 to being 5.2 percentage points less likely once age controls are added—a tidy doubling of the treatment effect.

In contrast, the effects on neighborhood SES substantially weaken. For OA income, the coefficients become generally somewhat smaller, the confidence intervals somewhat wider, and those from $e \geq 10$ lose statistical significance, but the overall pattern remains the same as what was reported in Figure 1. For median household income, none of the ex post coefficients are now statistically significant, but nonetheless, a new, distinct pattern emerges compared to the main findings omitting age controls. As in Figure 1, the treated group's neighborhood median household income steadily declines through $e = 8$, but then unlike the baseline version, neighborhood median household income then starts to rise so that by $e = 12$, it is only about 0.01 log points lower than the average for the control group, compared to 0.03 log points that is reported from the main sample. Overall, the main story—that people were more likely to move to neighborhoods with somewhat lower SES status as well as move out of their original CZ, but not necessarily to further out suburbs—holds up even when age is explicitly controlled for.

My results by unit tenure are likely the most sensitive to omitting age controls, so I lastly

present in Figure A13 the results by the same tenure groups as before with the age controls now included in the regressions. With notable difference that the middle-tenured group (3–6 years) now shows much stronger evidence that they leave their original CZ, the treatment trajectories tell the same story as the one seen in Figure 2, as the shortest tenured tenants (< 3 years) and the longest tenured tenants (> 6 years) have pretty similar results as in the main sample, suggesting that age is mostly not confounding the results for what are likely the youngest and oldest groups. The main difference is that the evidence that the shortest tenured group leaves their original CZ grows substantially: this group is now 8.9 percentage points less likely to be in their original CZ by the end of the study period compared to 2.9 percentage points less likely in Figure 2.

The evidence that the longest tenured group moves to neighborhoods with lower SES status is weaker once age controls are added: all coefficients on neighborhood median household income are now no longer statistically significant, although the point estimates tend to be a bit larger. Similarly, the negative OA income effect looks more persistent, but not strongly so, as compared to the main sample.

5 Conclusion

In this paper, I study the consequences on neighborhood mobility of a unique California law, the Ellis Act, that forces landlords to evict all of their tenants at once if they wish to leave the rent-controlled business. Los Angeles and San Francisco experienced a large wave of these evictions between 2000 and 2007, which generates a relatively large treated sample of over 11,000 tenants and an even larger control sample of over 900,000 individuals that I am able to identify in a proprietary individual migration history data set. I argue that for sufficiently large buildings, which I define as having five or more units, landlords cannot optimally choose the eviction time just to target any one tenant, so the tenants experience the Ellis Act eviction as an exogenous shock to their individual circumstances. I am able to

follow these adults from 1999 until 2019, giving me a long window to observe their ex post behavior.

My regressions confirm that an Ellis Act eviction does force people to relocate to a new location, particularly within the first 2 years of receiving the eviction notice. Further, I find that this effect persists even 12 years later, although at a slightly more attenuated rate. I find that Ellis'd individuals are more likely to leave the San Francisco or Los Angeles labor markets than the control group, and move to neighborhoods with lower median household incomes and lower expected adult incomes for the children who grow up in those neighborhoods. Together, these results confirm that receiving an Ellis Act eviction notice is a disruptive event in spite of recent measures undertaken by both cities to mitigate the effects, such as mandating relocation payments from landlords to tenants. The most parsimonious explanation is that in spite of these relocation payments, tenants are forced to find apartments in cheaper areas where prevailing rents are similar to the ones they paid at the time of eviction. Investigations into various subgroups, such as by race or length of tenure in the unit, reveal that the most negative effects are concentrated among whites, Hispanics, and those with tenures between three and six years in their units at the start of the sample. Various robustness checks confirm the main results. Policymakers looking to ameliorate the situation of Ellis evictees should thus use these results to consider more aggressive actions, perhaps by mandating larger relocation payments or extending the time tenants have to leave.

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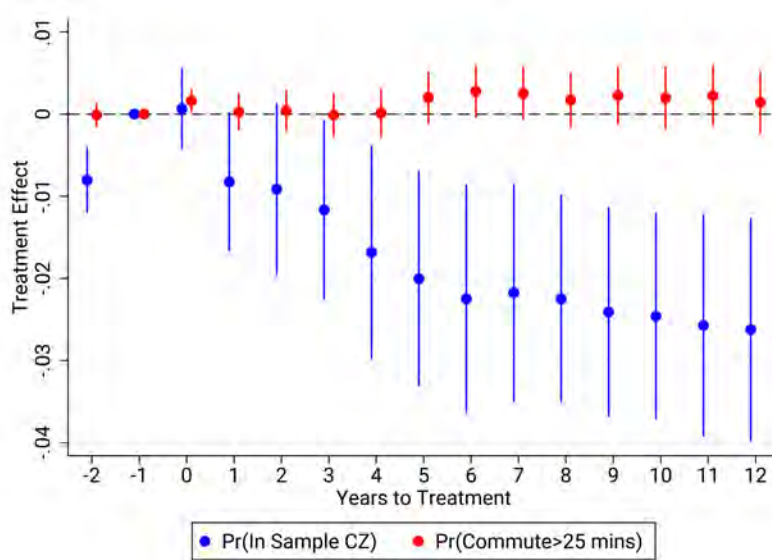
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Figure 1: Initial Results for Building and Neighborhood-Level Outcomes

Panel A: Job Proximity Measures



Panel B: Neighborhood Income Measures

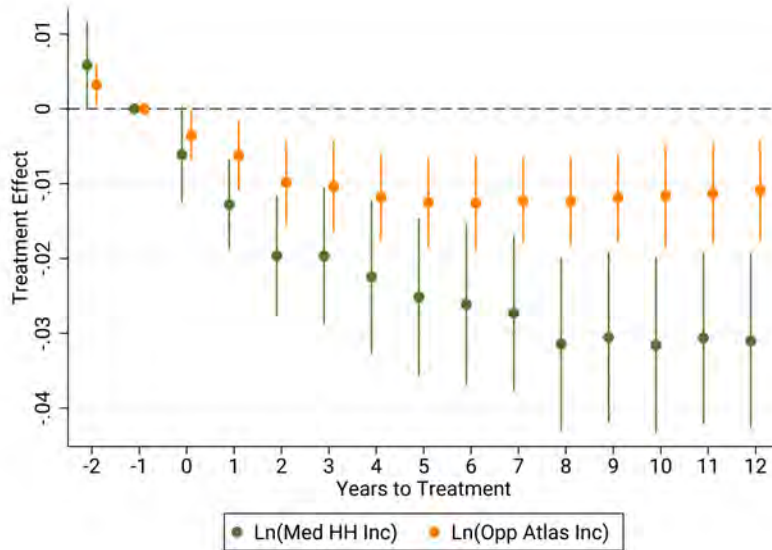
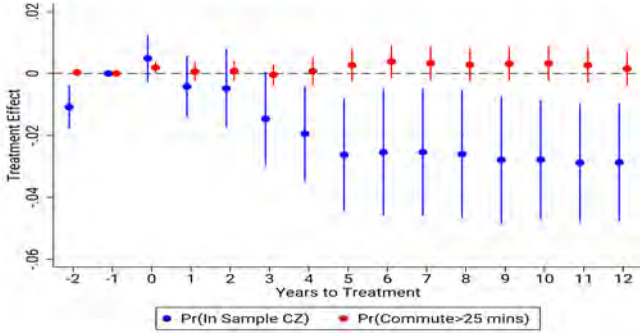


Figure 1 shows the results from estimating Equation (5) on four outcomes of interest. Panel A shows the results for the job proximity measures: the probability of being in the tenant’s original commuting zone (CZ) and their census tract’s share of people having long commutes (defined as greater than 25 minutes long). Panel B shows the neighborhood income measures: the log of their census tract’s median household and Opportunity Atlas (OA) incomes. Standard errors are clustered at the building level, with the regressions are weighted using inverse propensity weights generated by the process described in Section 3.2.

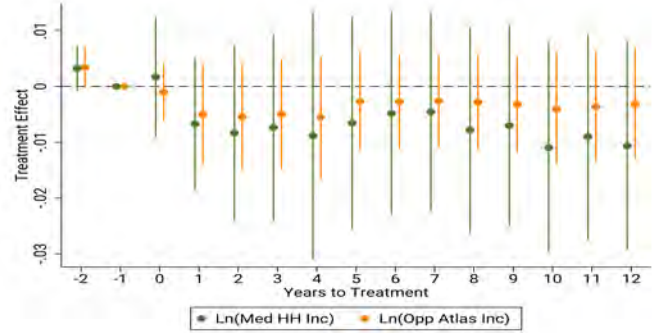
Figure 2: Results by Initial Tenure Length

(a) Tenure < 3 Years

Panel A: Job Proximity Measures

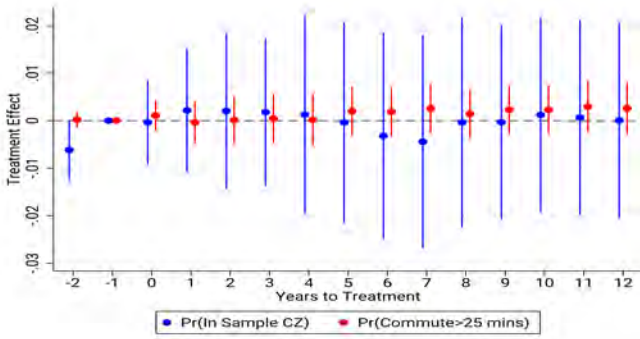


Panel B: Neighborhood Income Measures

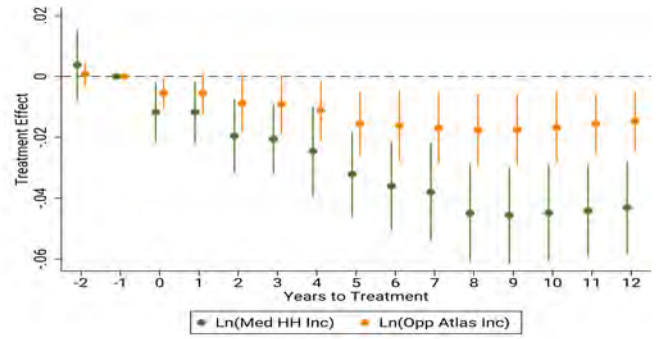


(b) Tenure 3 – 6 Years

Panel C: Job Proximity Measures

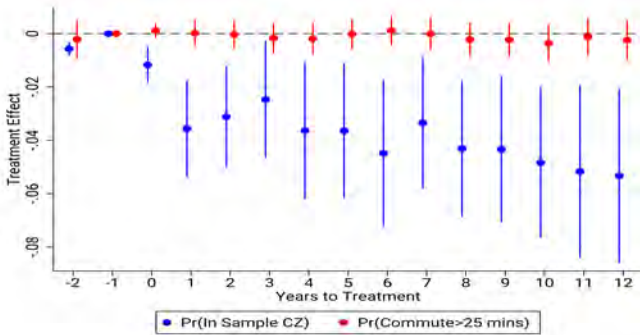


Panel D: Neighborhood Income Measures



(c) Tenure > 6 Years

Panel E: Job Proximity Measures



Panel F: Neighborhood Income Measures

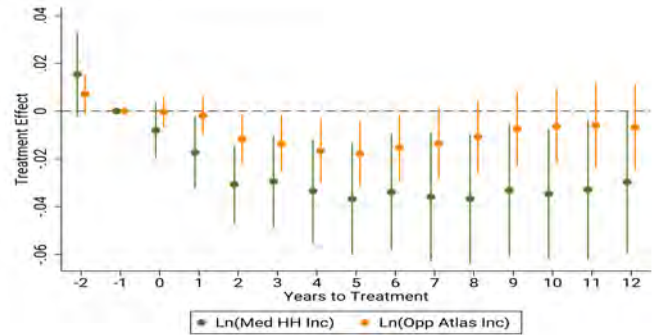
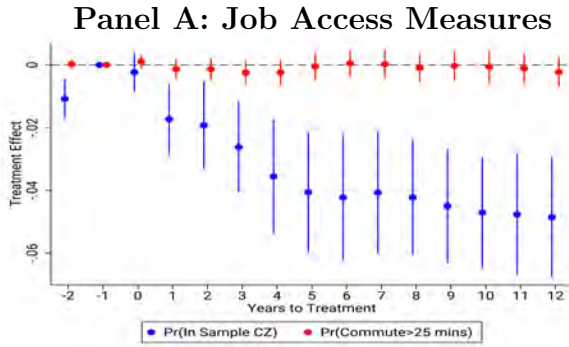


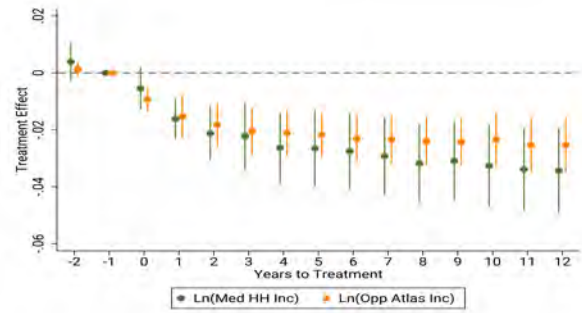
Figure 2 shows the results from estimating Equation (5) on four outcomes of interest grouped by how long people were living in their apartments when they entered the sample in 1999. Panels A, C, and E show the results for the job proximity measures: the probability of being in the tenant’s original commuting zone (CZ) and their census tract’s share of people having long commutes (defined as greater than 25 minutes long). Panels B, D, and F show the neighborhood income measures: the log of their census tract’s median household and Opportunity Atlas (OA) incomes. Standard errors are clustered at the building level. The regressions are weighted using inverse propensity weights (IPWs) generated by the process described in Section 3.2, with the IPWs computed for each tenure group separately.

Figure 3: Results by Race

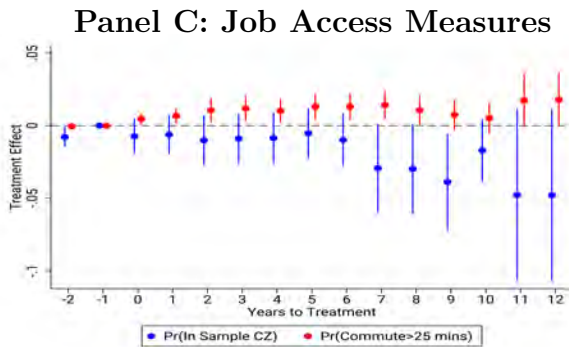
(a) Non-Hispanic Whites



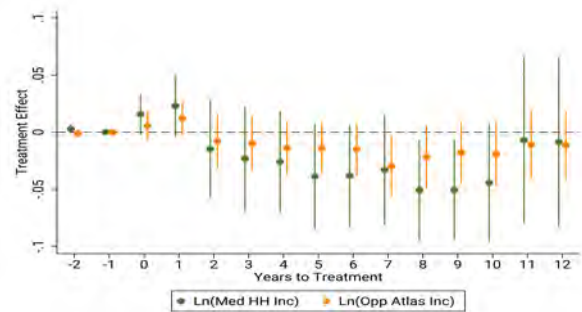
Panel B: Neighborhood Income Measures



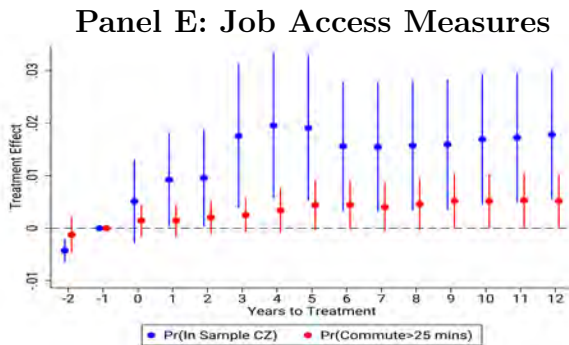
(b) Asian and Pacific Islanders



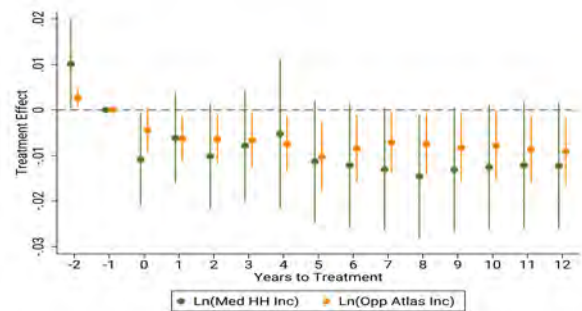
Panel D: Neighborhood Income Measures



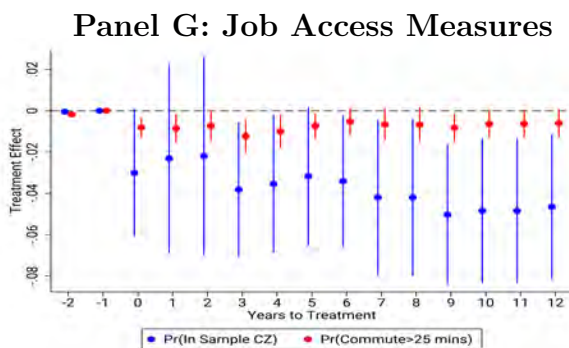
(c) Hispanics



Panel F: Neighborhood Income Measures



(d) Blacks



Panel H: Neighborhood Income Measures

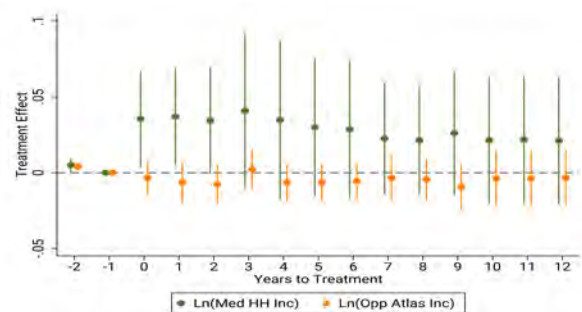


Figure 3 shows the results from estimating Equation (5) on four outcomes of interest by four racial groupings: non-Hispanic whites, Asian and Pacific Islanders, Hispanics of any race, and non-Hispanic blacks. Panels A, C, E, and G show the results for the job proximity measures: the probability of being in the tenant’s original commuting zone (CZ) and their census tract’s share of people having long commutes (defined as greater than 25 minutes long). Panels B, D, F, and H show the neighborhood income measures: the log of their census tract’s median household and Opportunity Atlas (OA) incomes. Standard errors are clustered at the building level. The regressions are weighted using inverse propensity weights (IPWs) generated by the process described in Section 3.2, with the IPWs computed for each racial grouping separately.

TABLE 1
Total In-Sample Ellis Act Evictions from Buildings with 5+ Units by City, 2000–2007

	San Francisco			Los Angeles			Total		
	Buildings	Units	People	Buildings	Units	People	Buildings	Units	People
2000	12	68	111	22	414	627	34	482	738
2001	9	58	102	20	189	365	29	247	467
2002	4	25	28	18	237	383	22	262	411
2003	4	27	49	18	196	293	22	223	342
2004	16	99	131	32	394	533	48	493	664
2005	23	156	277	150	2,130	2,687	173	2,286	2,964
2006	21	162	241	181	2,856	3,108	202	3,018	3,349
2007	26	184	340	95	1,315	2,195	121	1,499	2,535
Total Ellis	115	779	1,279	536	7,731	10,191	651	8,510	11,470
Total Non-Ellis	8,309	108,951	160,698	27,949	430,726	746,767	36,258	539,677	907,465

NOTE: Table 1 presents the count of all units and buildings withdrawn under the Ellis Act from buildings with 5 or more units from San Francisco and Los Angeles, as well as the count of people affected by the Ellis Act.

TABLE 2
Comparison of Ellis'd and Non-Ellis'd Adults at Baseline

	Treatment	Control	Diff	p-value
<i>Panel A: Unmatched Sample</i>				
Unit Counts	33.06	46.31	-13.25	0.200
Year Built	1944.61	1949.33	-4.72	0.000
Tenure	4.61	4.35	0.27	0.085
Avg Tenure	4.60	4.33	0.26	0.076
Moved	0.060	0.076	-0.016	0.000
White	0.572	0.472	0.100	0.001
Black	0.025	0.049	-0.023	0.000
Hispanic	0.346	0.402	-0.056	0.056
Asian	0.056	0.077	-0.021	0.001
Ln(Distance)	8.764	8.755	0.008	0.000
Long Commute	0.494	0.510	-0.015	0.001
Med HH Inc	\$64,635	\$58,316	\$6,318	0.000
Ln(Med HH Inc)	10.97	10.87	0.10	0.002
OA Income	\$46,623	\$44,422	\$2,201	0.002
Ln(OA Income)	10.72	10.67	0.05	0.002
<i>Panel B: Matched Sample</i>				
Unit Counts	35.75	33.62	2.14	0.877
Year Built	1942.91	1944.79	-1.87	0.114
Tenure	4.92	4.59	0.332	0.070
Avg Tenure	4.92	4.58	0.346	0.055
Moved	0.063	0.080	-0.017	0.000
White	0.663	0.568	0.095	0.001
Black	0.0137	0.0282	-0.015	0.000
Hispanic	0.278	0.349	-0.072	0.010
Asian	0.046	0.054	-0.008	0.138
Ln(Distance)	8.765	8.764	0.001	0.581
Long Commute	0.475	0.491	-0.017	0.001
Med HH Inc	\$70,404	\$62,525	\$7,879	0.000
Ln(Med HH Inc)	11.063	10.943	0.120	0.001
OA Income	\$48,817	\$46,167	\$2,650	0.000
Ln(OA Income)	10.772	10.713	0.059	0.000

Table 2 reports the mean values for the treatment and control groups, the difference between them, and the p-value of difference between the mean values against a null hypothesis of zero. “Long Commute” represents the share of people who had a commute of 25 minutes or longer; “Med HH Inc” represents the 1999 median household income; and “OA Income” represents the income at age 35 for those people born between 1978 and 1983 who grew up in that census tract (Chetty et al. 2020).

Additional Results Appendix

Figure A1: Control and Treatment Buildings by Eviction Year in San Francisco

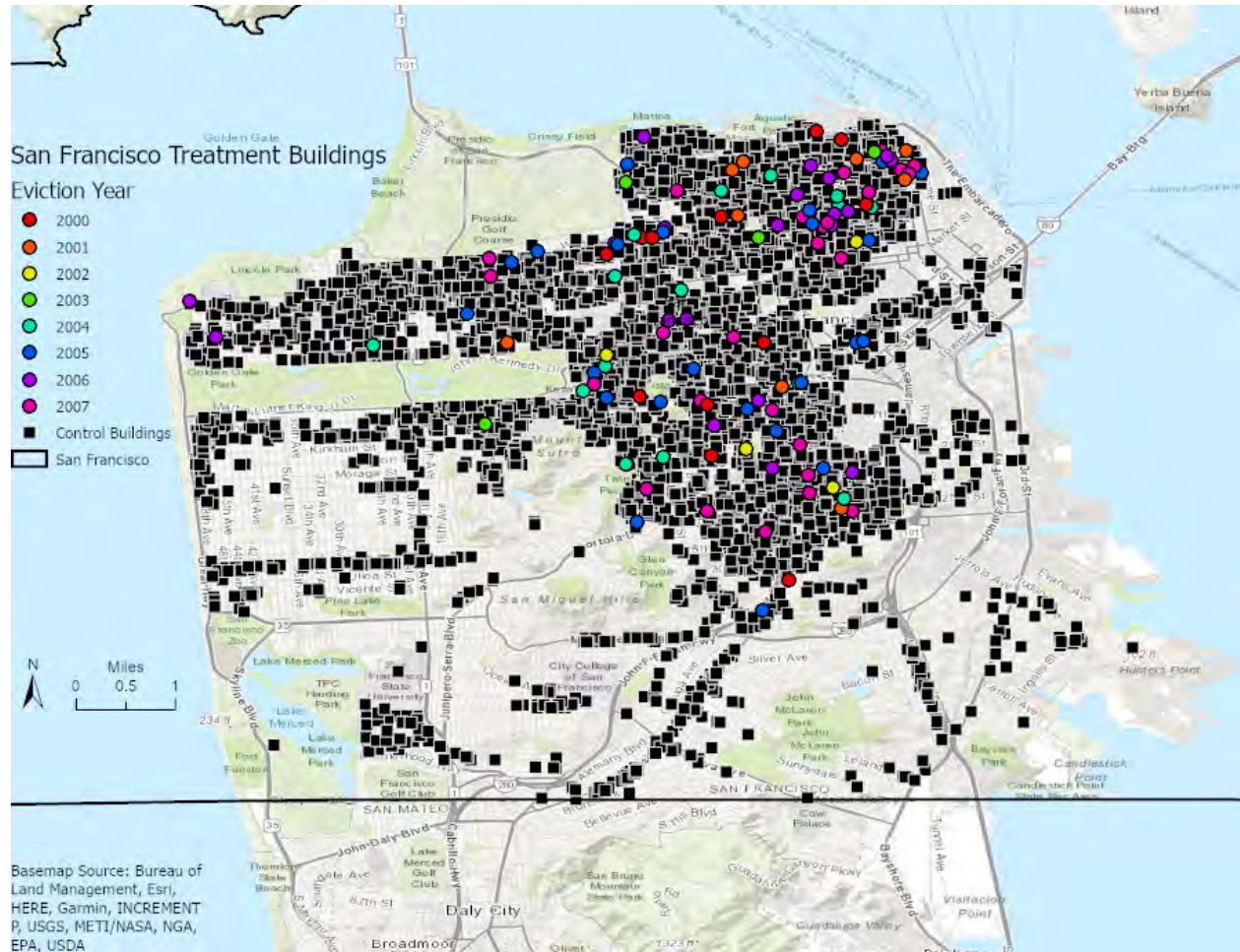


Figure A1 shows the location of each treatment and control building within San Francisco's municipal boundaries. Control buildings are in black squares, and treatment buildings are in circles and color coded with their eviction year.

Figure A2: Control and Treatment Buildings by Eviction Year in Los Angeles

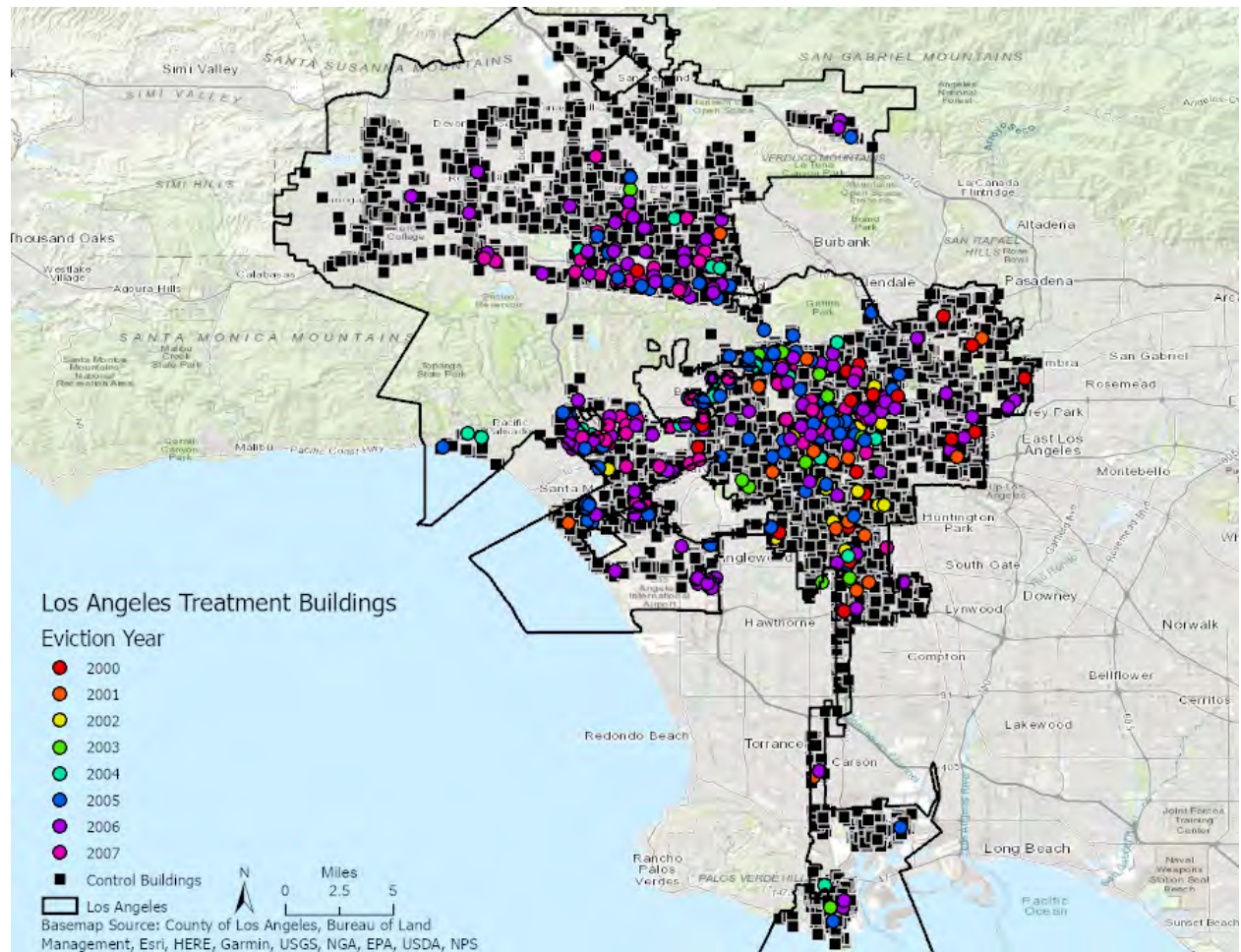


Figure A2 shows the location of each treatment and control building within Los Angeles's municipal boundaries. Control buildings are in black squares, and treatment buildings are in circles and color coded with their eviction year.

Figure A3: Distribution of Ellis'd People by Initial Building's Unit Count

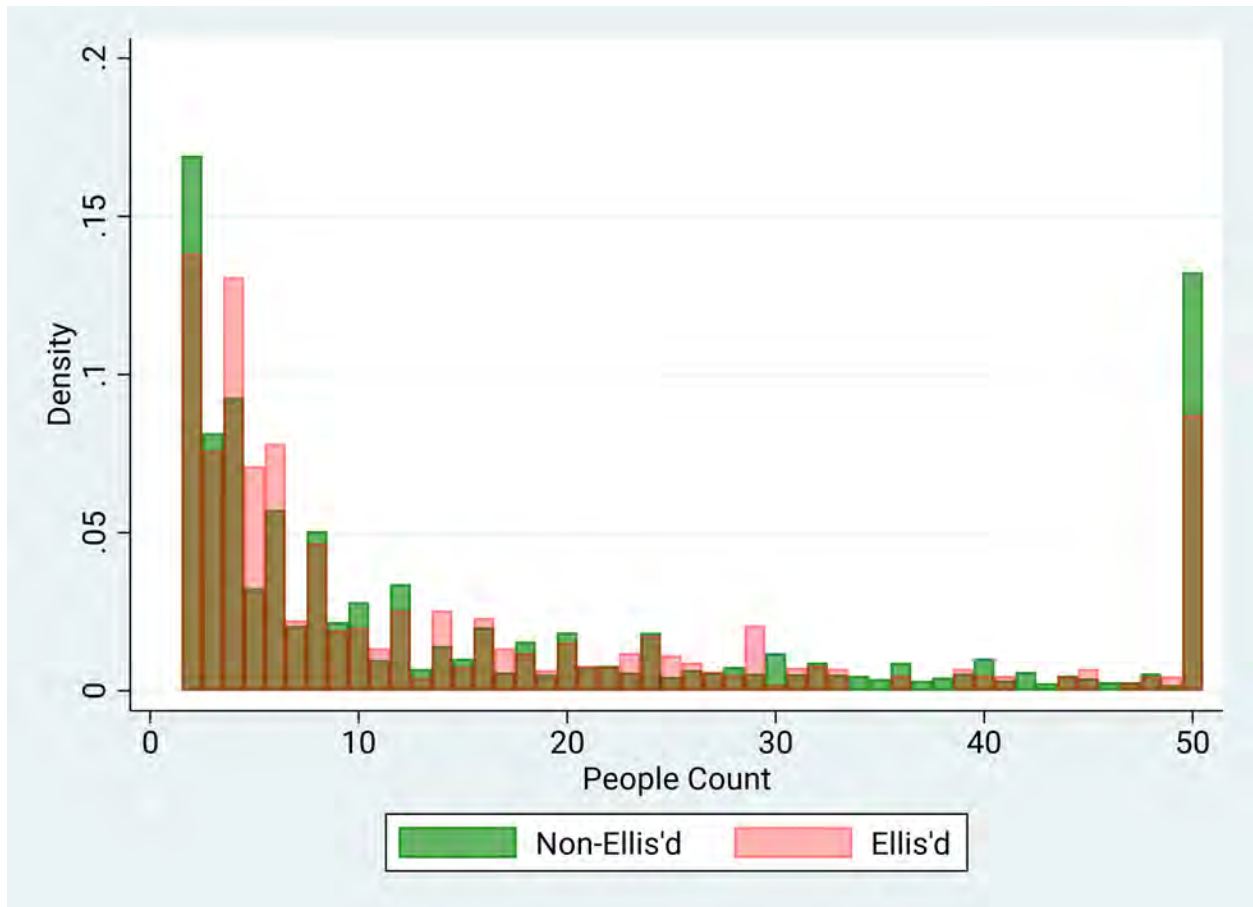


Figure A3 shows the kernel density of people by their initial building's unit count. Those whose initial buildings were Ellis'd between 2000 and 2007 are in transparent red and those whose initial buildings were not Ellis'd over that period are in solid green.

Figure A4: Distribution of Ellis'd Buildings by Unit Count

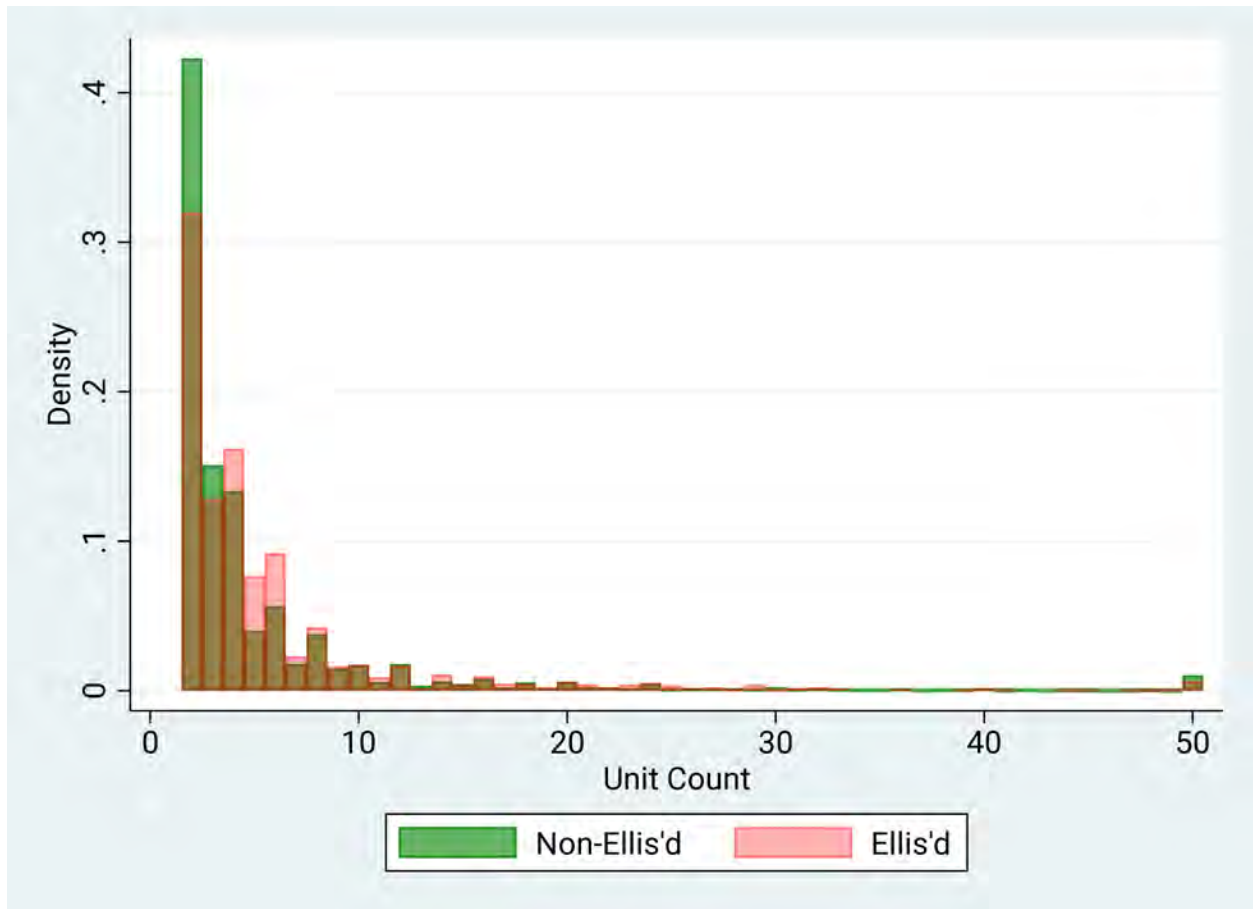
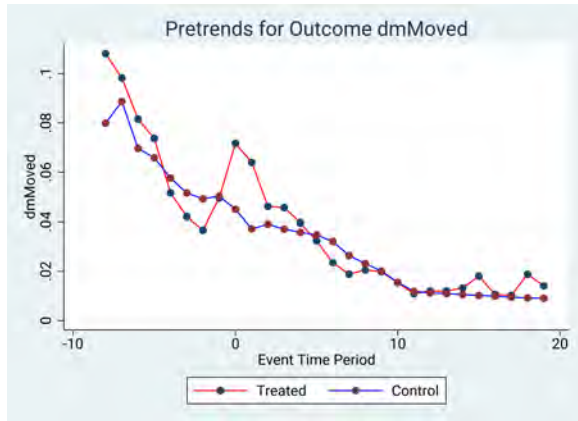


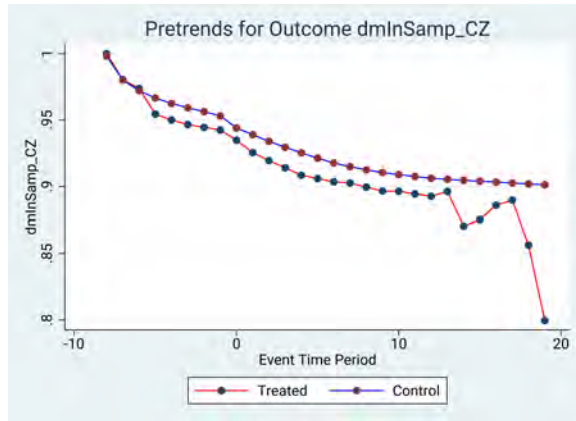
Figure A4 shows the kernel density of the sample buildings' unit counts. The buildings Ellis'd between 2000 and 2007 are in transparent red and the non-Ellis'd buildings over that period are in solid green.

Figure A5: Pretrends of Selected Outcomes of Interest

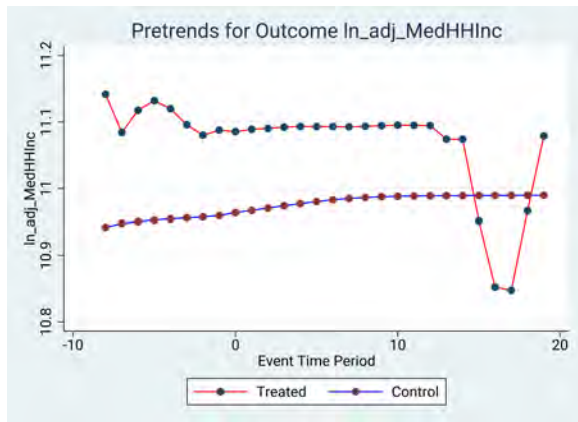
Panel A: Pr(Moved)



Panel B: Pr(In Sample CZ)



Panel C: ln(Median HH Inc)



Panel D: ln(Opportunity Atlas Income)

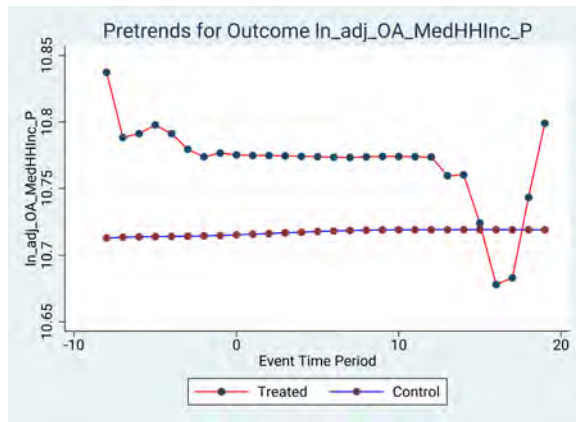


Figure A5 shows the IPW-weighted average differences between the treatment and control groups by event time. The graphs account for the staggered rollout design in the treatment group by first averaging by treatment cohort, and then averaging across cohorts by cohort size. The control group's average is weighted only by each observation's IPW. Panel A shows the differences for the annual probability of moving; Panel B, the probability of being in the individual's original commuting zone (CZ); Panel C, the log of the median household income of the neighborhood of residence; and Panel D, the log of the Opportunity Atlas income (Chetty et al., 2020) of the neighborhood of residence.

Figure A6: Selected Covariates' Effect on Assignment to Treatment by Building Unit Count

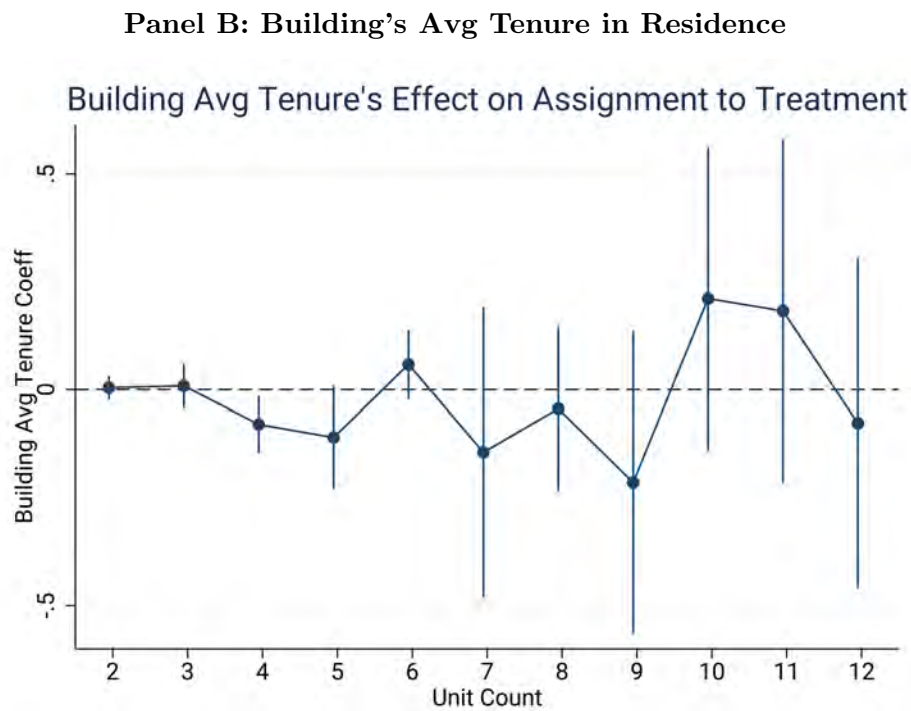
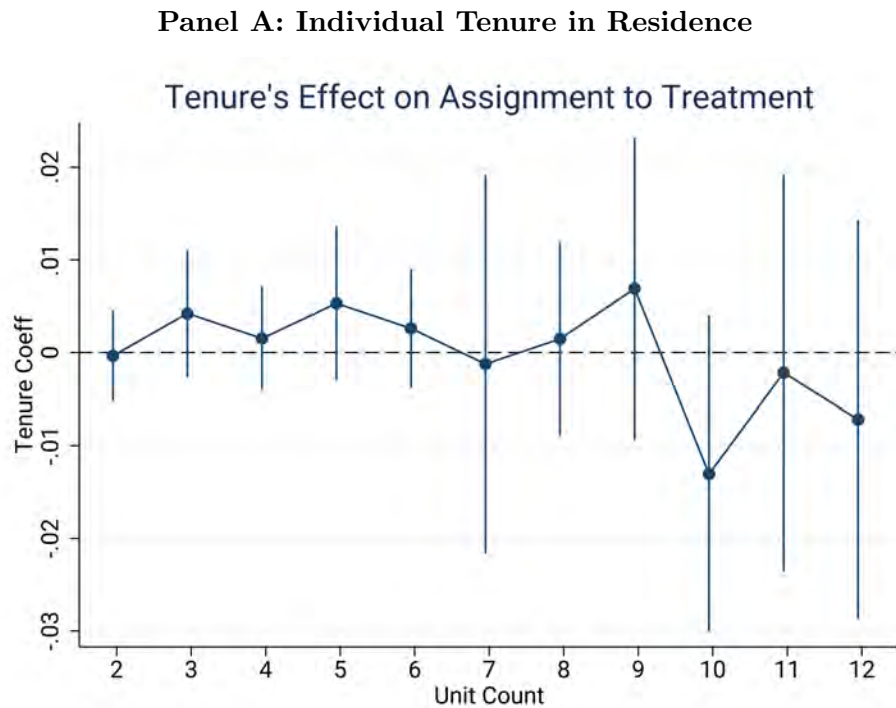


Figure A6 shows the coefficient estimates of the effect of the individual's tenure in their residence (Panel A) and the building's average tenure across tenants (Panel B) on their probability of being selected into treatment, generated by the regression in Equation (4) by building unit count (on the x-axis). Standard errors are clustered at the building level.

Figure A7: Baseline Event Study on Pr(Moved)

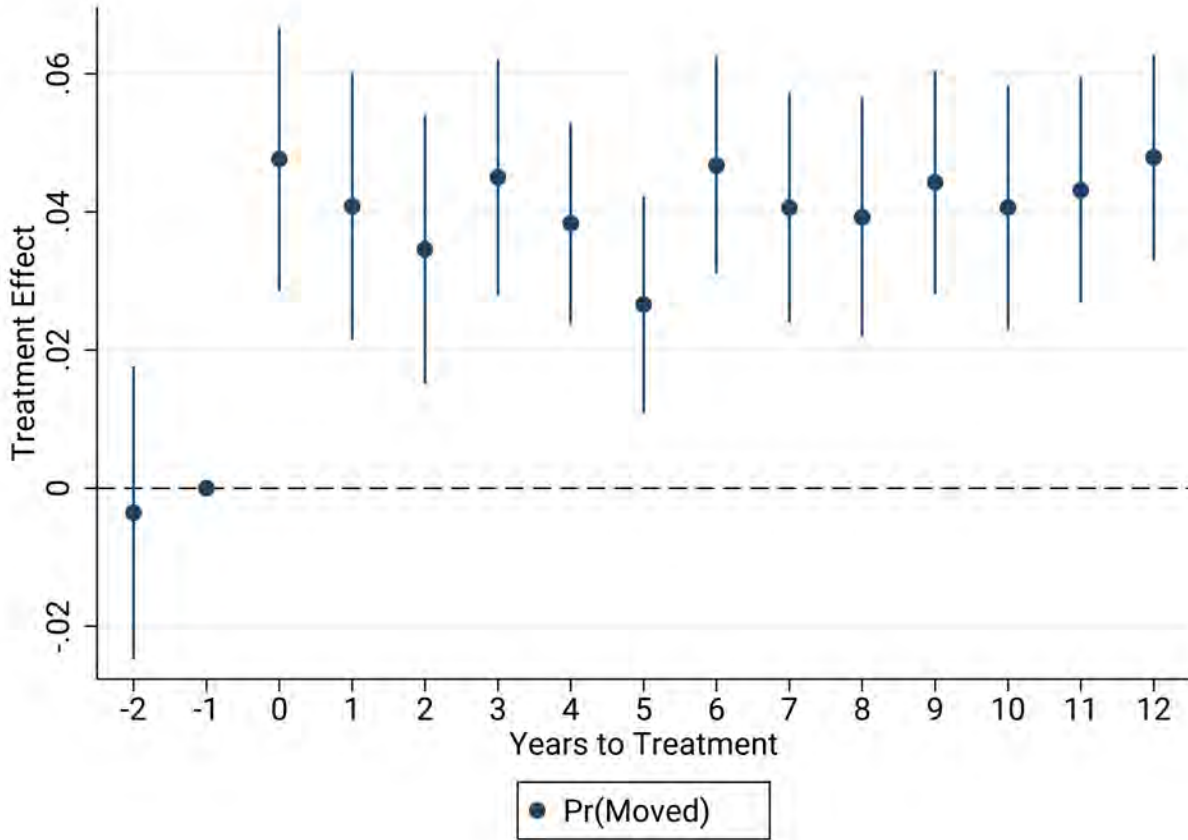
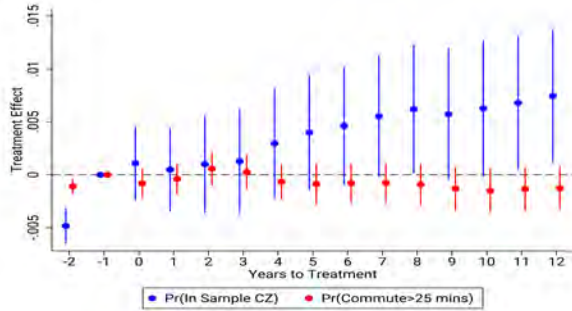


Figure A7 shows the event study estimates the treatment effect of receiving an Ellis eviction notice on the probability of having moved over the course of the prior year. Standard errors clustered at the building level. The regression is weighted using inverse propensity weights (IPWs) generated by the process described in Section 3.2.

Figure A8: Results by Neighborhood Income Status

(a) Low Median HH Income Neighborhoods

Panel A: Job Proximity Measures



Panel B: Neighborhood Income Measures

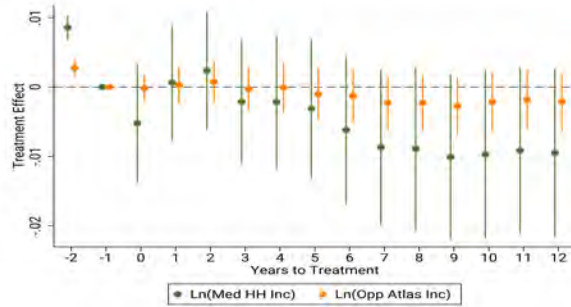
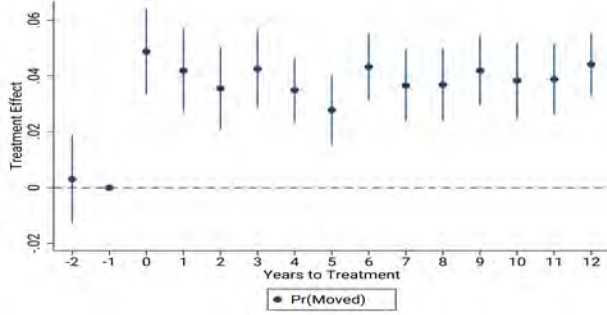


Figure A8 shows the results from estimating Equation (5) on four outcomes of interest first for people living in census tracts in the bottom half of the neighborhood median household income distribution. Panel A shows the results for the job proximity measures: the probability of being in the tenant’s original commuting zone (CZ) and their census tract’s share of people having long commutes (defined as greater than 25 minutes long). Panel B shows the neighborhood income measures: the log of their census tract’s median household and Opportunity Atlas (OA) incomes. Standard errors are clustered at the building level. The regressions are weighted using inverse propensity weights (IPWs) generated by the process described in Section 3.2, with the IPWs computed for each subsample separately.

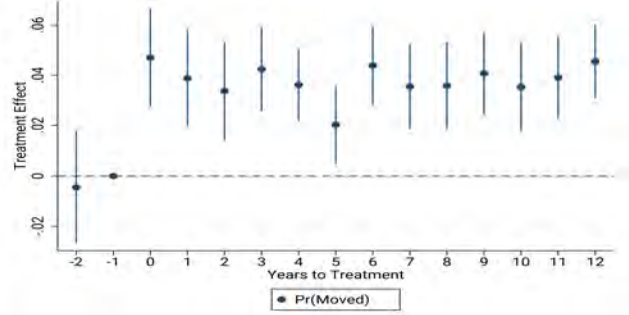
Figure A9: Results for Different Unit Cutoffs

(a) Pr(Moved)

Panel A: 2+ Units

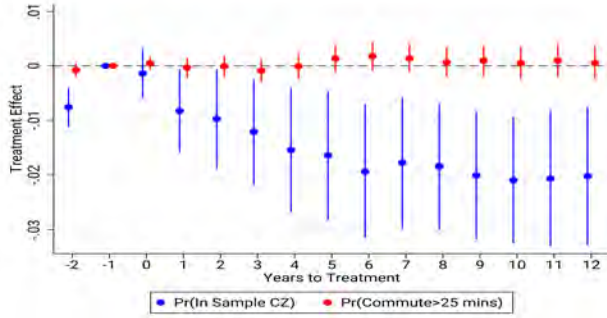


Panel B: 6+ Units

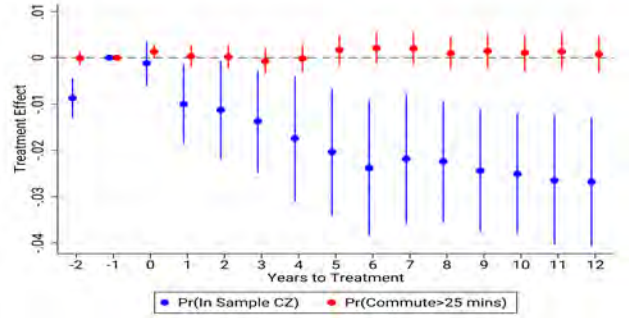


(b) Pr(Commute >25 mins) and Pr(In Original CZ)

Panel C: 2+ Units

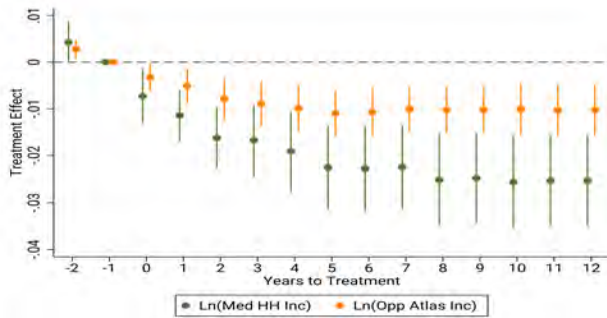


Panel D: 6+ Units



(c) Ln(Median HH Inc) and Ln(OA Income)

Panel F: 2+ Units



Panel G: 6+ Units

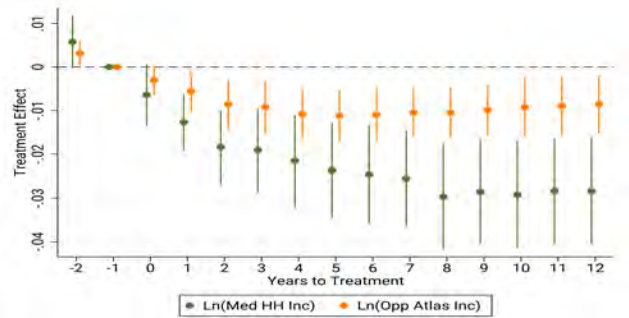
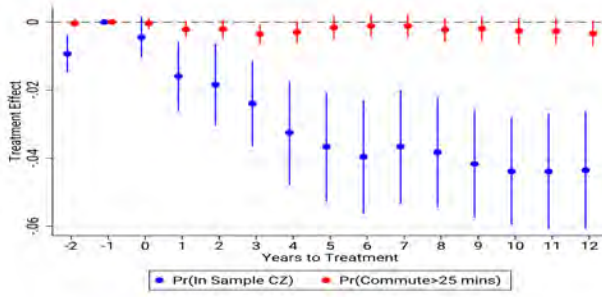


Figure A9 shows the results from estimating Equation (5) on five outcomes using first all rent-controlled buildings with two or more units and then all buildings with six or more units. Panels A and B show the results on the annual probability of moving for the two samples. Panels C and D show the results for the job proximity measures: the probability of being in the tenant’s original commuting zone (CZ) and their census tract’s share of people having long commutes (defined as greater than 25 minutes long). Panels F and G show the neighborhood income measures: the log of their census tract’s median household and Opportunity Atlas (OA) incomes. Standard errors are clustered at the building level. The regressions are weighted using inverse propensity weights (IPWs) generated by the process described in Section 3.2, with the IPWs computed for each subsample separately.

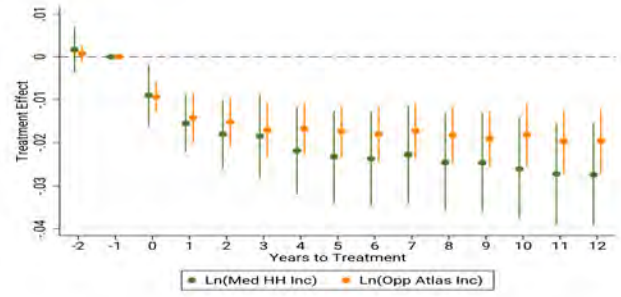
Figure A10: Results by Race across All 2+ Unit Buildings

(a) Non-Hispanic Whites

Panel A: Job Access Measures

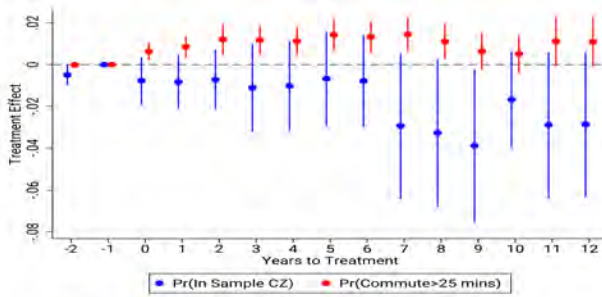


Panel B: Neighborhood Income Measures

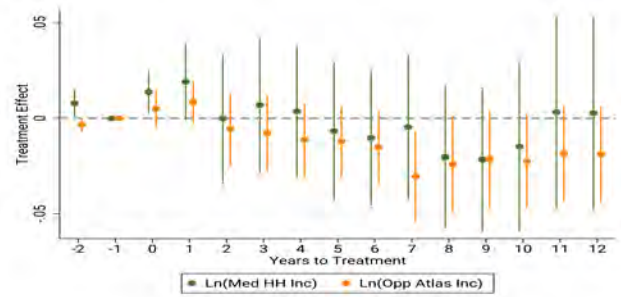


(b) Asian and Pacific Islanders

Panel C: Job Access Measures

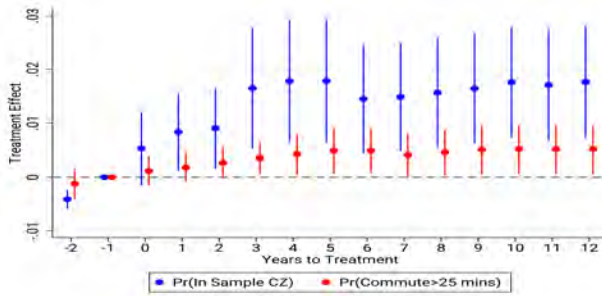


Panel D: Neighborhood Income Measures

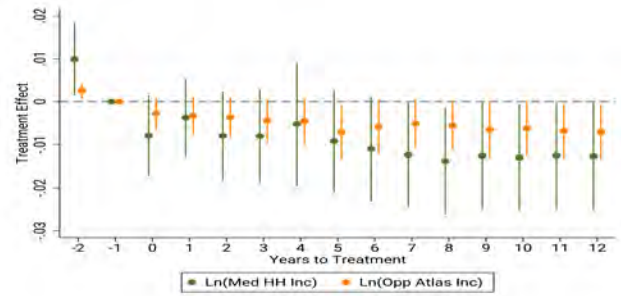


(c) Hispanics

Panel E: Job Access Measures

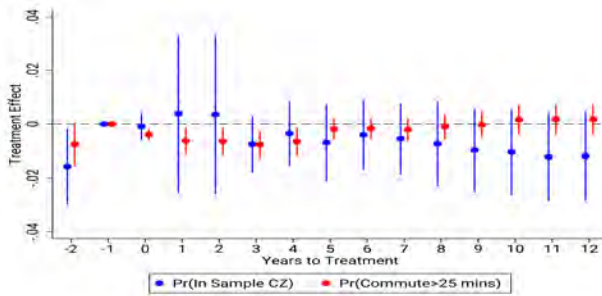


Panel F: Neighborhood Income Measures



(d) Blacks

Panel G: Job Access Measures



Panel H: Neighborhood Income Measures

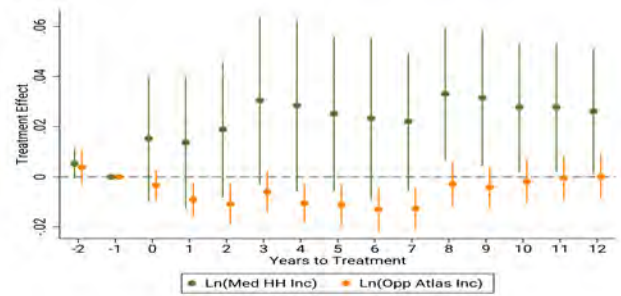


Figure A10 shows the results from estimating Equation (5) on four outcomes of interest in the two-plus unit buildings by four racial groupings: non-Hispanic whites, Asian and Pacific Islanders, Hispanics of any race, and non-Hispanic blacks. Panels A, C, E, and G show the results for the job proximity measures: the probability of being in the tenant's original commuting zone (CZ) and their census tract's share of people having long commutes (defined as greater than 25 minutes long). Panels B, D, F, and H show the neighborhood income measures: the log of their census tract's median household and Opportunity Atlas (OA) incomes. Standard errors are clustered at the building level. The regressions are weighted using inverse propensity weights (IPWs) generated by the process described in Section 3.2, with the IPWs computed for each racial grouping separately.

Figure A11: Baseline Event Study on Pr(Moved) with Age Controls Added

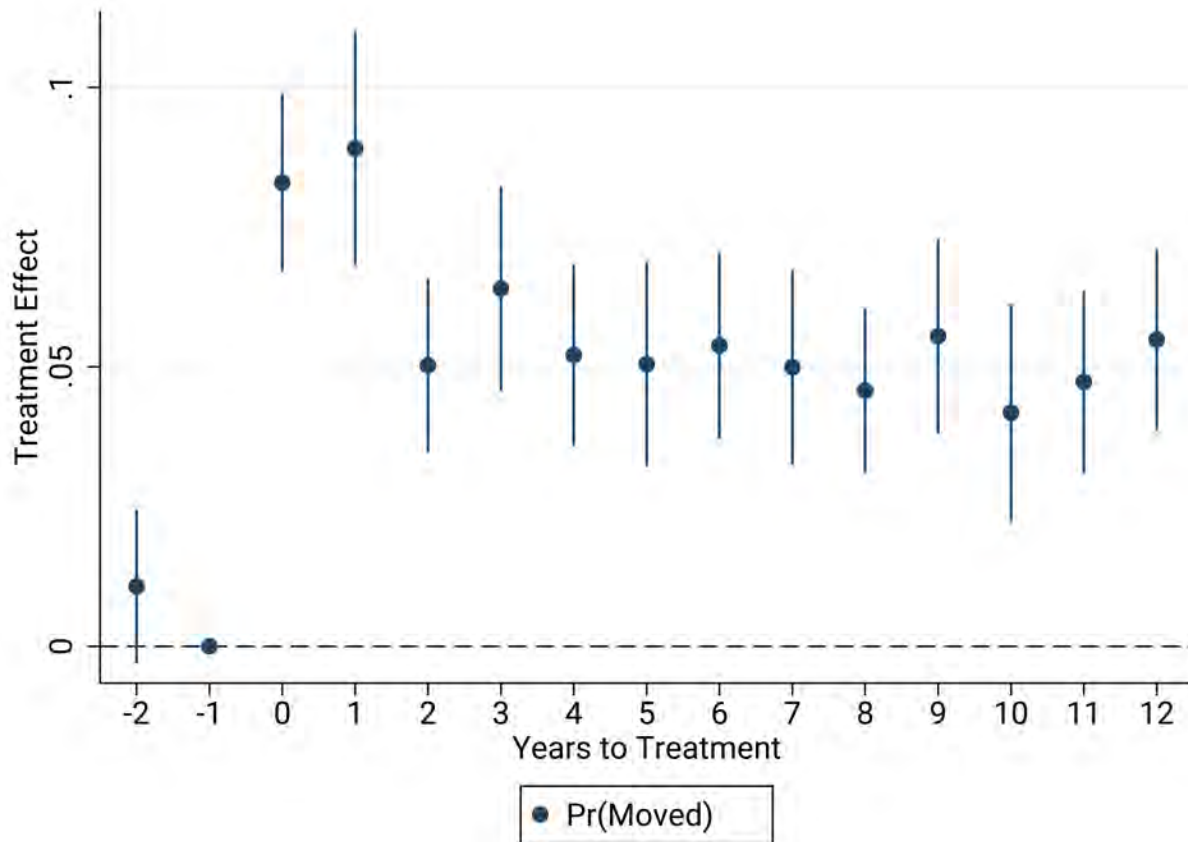


Figure A11 shows the results from estimating Equation (5) augmented with controls for tenants' age on the treatment effect of receiving an Ellis eviction notice on the probability of having moved over the course of the prior year. Standard errors clustered at the building level. The regression is weighted using inverse propensity weights (IPWs) generated by the process described in Section 3.2.

Figure A12: Initial Results for Main Outcomes with Age Controls Added

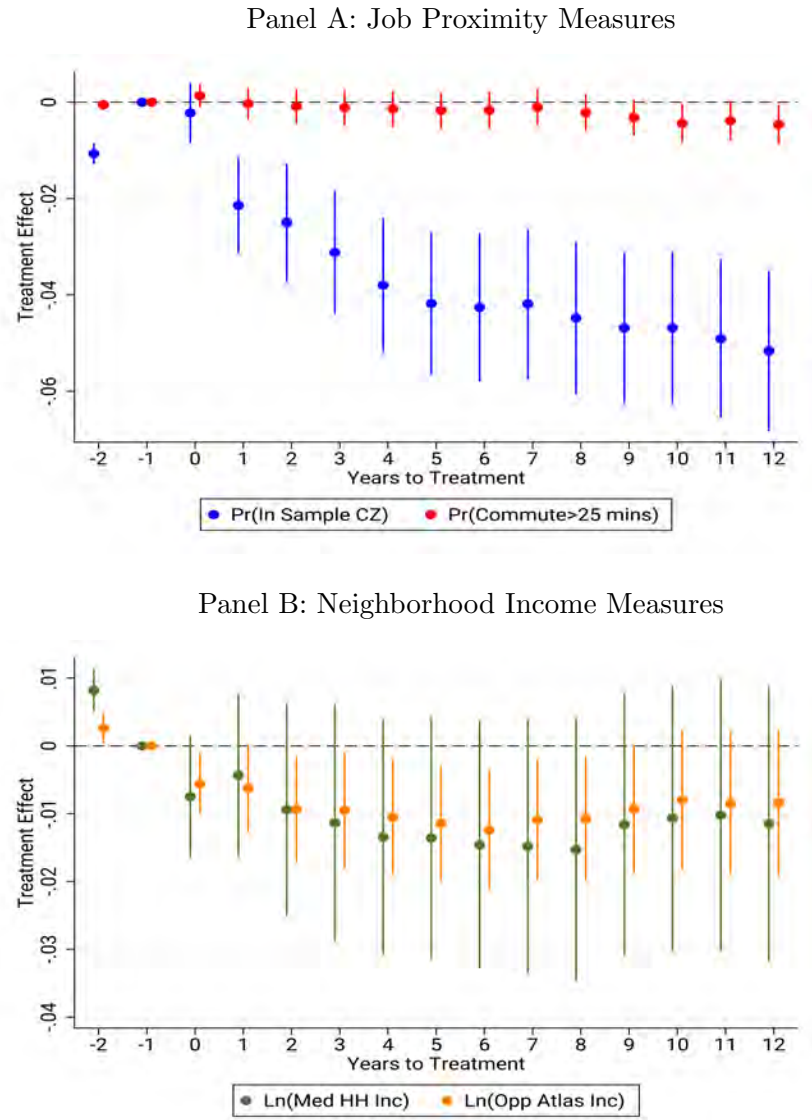
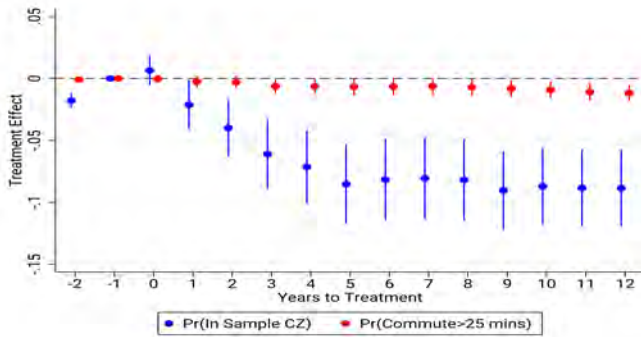


Figure A12 shows the results from estimating Equation (5) augmented with age controls on four outcomes of interest. Panel A shows the results for the job proximity measures: the probability of being in the tenant’s original commuting zone (CZ) and their census tract’s share of people having long commutes (defined as greater than 25 minutes long). Panel B shows the neighborhood income measures: the log of their census tract’s median household and Opportunity Atlas (OA) incomes. Standard errors are clustered at the building level, with the regressions are weighted using inverse propensity weights generated by the process described in Section 3.2.

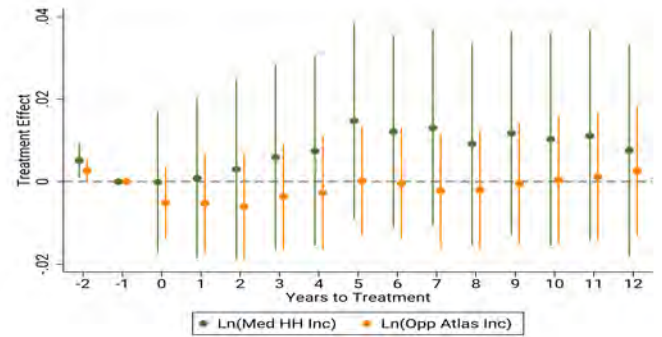
Figure A13: Results by Initial Tenure Length with Age Controls Added

(a) Tenure < 3 Years

Panel A: Job Proximity Measures

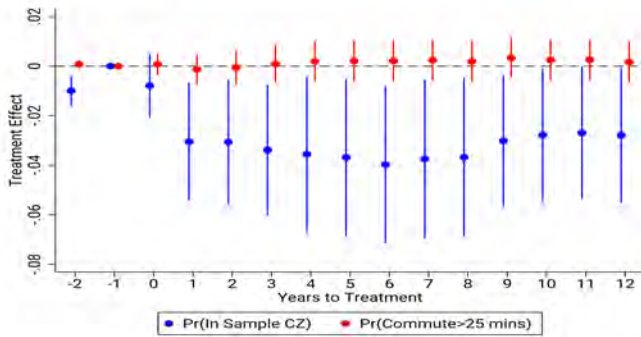


Panel B: Neighborhood Income Measures

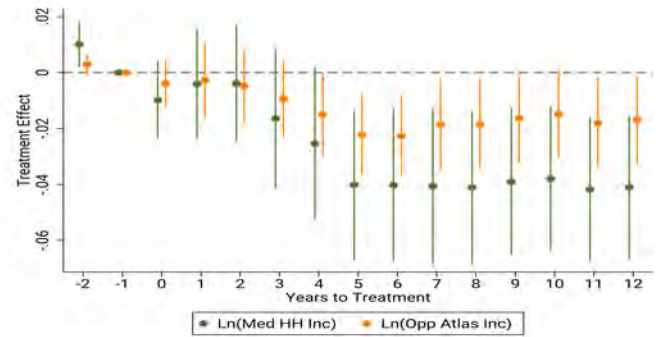


(b) Tenure 3 – 6 Years

Panel A: Job Proximity Measures

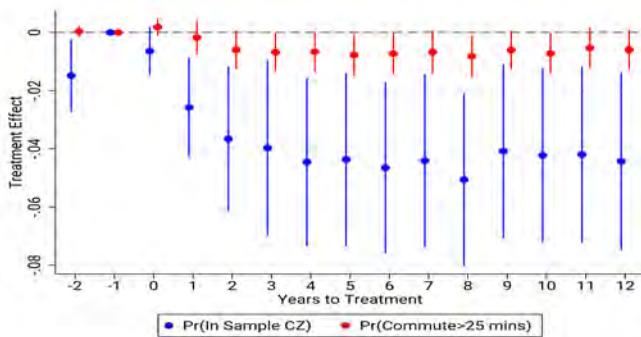


Panel B: Neighborhood Income Measures



(c) Tenure > 6 Years

Panel A: Job Proximity Measures



Panel B: Neighborhood Income Measures

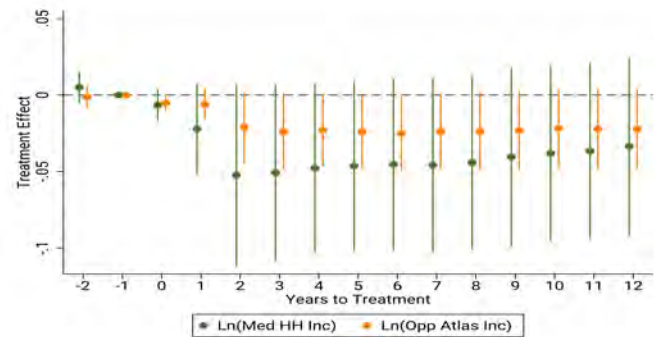


Figure A13 shows the results from estimating Equation (5) augmented with controls on tenants' age on four outcomes of interest grouped by how long people were living in their apartments when they entered the sample in 1999. Panels A, C, and E show the results for the job proximity measures: the probability of being in the tenant's original commuting zone (CZ) and their Census tract's share of people having long commutes (defined as greater than 25 minutes long). Panels B, D, and F show the neighborhood income measures: the log of their Census tract's median household and Opportunity Atlas (OA) incomes. Standard errors are clustered at the building level. The regressions are weighted using inverse propensity weights (IPWs) generated by the process described in Section 3.2, with the IPWs computed for each tenure group separately.

TABLE A1
Comparison of Ellis'd and Non-Ellis'd Adults at Baseline by City

	Treatment	Control	Diff	p-value
<i>Panel A: SF Unmatched Sample</i>				
Unit Counts	7.19	54.68	-47.49	0.000
Year Built	1917.25	1930.30	-13.06	0.000
Tenure	5.447	4.879	0.568	0.011
Avg Tenure	5.202	4.880	0.322	0.113
Moved	0.055	0.097	-0.041	0.000
White	0.711	0.754	-0.043	0.271
Black	0.003	0.012	-0.009	0.013
Hispanic	0.146	0.096	0.049	0.146
Asian	0.140	0.137	0.003	0.921
Ln(Distance)	8.663	8.663	0.000	0.485
Long Commute	0.500	0.504	-0.003	0.699
Med HH Inc	\$94,183	\$86,313	\$7,870	0.038
Ln(Med HH Inc)	11.370	11.275	0.095	0.050
OA Income	\$55,313	\$55,072	\$241	0.857
Ln(OA Income)	10.895	10.893	0.002	0.927
<i>Panel B: LA Unmatched Sample</i>				
Unit Counts	35.23	44.79	-9.56	0.350
Year Built	1947.80	1953.58	-5.78	0.000
Tenure	4.42	4.22	0.205	0.200
Avg Tenure	4.474	4.218	0.257	0.099
Moved	0.060	0.071	-0.011	0.001
White	0.556	0.412	0.144	0.000
Black	0.028	0.056	-0.028	0.000
Hispanic	0.370	0.468	-0.097	0.003
Asian	0.046	0.064	-0.019	0.001
Ln(Distance)	8.775	8.775	0.000	0.530
Long Commute	0.496	0.511	-0.016	0.002
Med HH Inc	\$59,846	\$51,480	\$8,366	0.000
Ln(Med HH Inc)	10.905	10.772	0.133	0.000
OA Income	\$45,120	\$41,760	\$3,360	0.000
Ln(OA Income)	10.693	10.617	0.076	0.000

Table A1 reports the mean values for the treatment and control groups, the difference between them, and the p-value of difference between the mean values against a null hypothesis of zero. “Long Commute” represents the share of people who had a commute of 25 minutes or longer; “Med HH Inc” represents the 1999 median household income; and “OA Income” represents the income at age 35 for those people born between 1978 and 1983 who grew up in that census tract (Chetty et al. 2020).

TABLE A3
Mean Outcomes by Group

	All		White		Black		Hispanic		Asian & PI	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Pr(Moved)	0.0456	0.209	0.0632	0.243	0.0341	0.182	0.0262	0.160	0.0577	0.233
Pr(In Sample CZ)	0.940	0.238	0.908	0.288	0.975	0.155	0.967	0.179	0.943	0.232
Long Commute	0.506	0.0957	0.474	0.0917	0.572	0.0817	0.537	0.0871	0.489	0.102
OA Income	44,320	11,080	50,676	11,219	30,680	5,622	38,613	6,202	48,787	10,674
Med HH Inc	59,387	29,474	75,367	32,310	41,368	16,401	44,946	16,819	59,501	30,059
	Tenure 1-3 Yrs		Tenure 4-6 Yrs		Tenure 7+ Yrs		Low Med HH Inc			
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Pr(Moved)	0.0570	0.232	0.0284	0.166	0.0503	0.219	0.0357	0.185		
Pr(In Sample CZ)	0.928	0.259	0.955	0.208	0.940	0.237	0.955	0.208		
Long Commute	0.505	0.0978	0.511	0.0931	0.499	0.0947	0.534	0.0922		
OA Income	44,075	11,030	43,650	10,754	46,248	11,611	39,976	8,845		
Med HH Inc	58,695	29,356	57,541	28,176	64,752	31,567	45,616	19,916		

Table A3 reports the mean and standard deviation (SD) of the outcomes of interest, by selected subgroups. “Low Med HH Inc” are those living in census tracts which are in the bottom half of the median household income distribution in their respective city. “High Opp” are those living in census tracts which are in the top half of the distribution of tracts’ difference between its OA income and its median household income.