When Opportunity Moves to You: Housing Deregulation and the Distribution of Child Outcomes*

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Abstract

A large literature documents the economic benefits of moving children to low-poverty neighborhoods. However, it is not clear if children will benefit when their current neighborhood improves around them, especially for households paying market rents. I study how neighborhood revitalization affects incumbent children, focusing on a 1998 Houston housing reform that incentivized building single-family homes downtown. The reform increased median household incomes by ten percent and rents by seven percent as affluent, college-educated households moved into these homes. Using administrative data from the U.S. Census Bureau, I examine impacts on children's long-run human capital attainment and economic self-sufficiency. Renter households moved out of revitalized areas, meaning their children grew up in similar neighborhoods to untreated individuals. In contrast, homeowner children lived in better neighborhoods and parent home equity increased. Young renter children had worse labor market outcomes, suggestive of disruption costs, while owner children attained more education and economic self-sufficiency. Owner children were also charged with crimes at higher rates, potentially due to increased policing in gentrified areas. The reform improved incumbent well-being on average, but large losses for renters mean that Pareto-improving transfers are practically challenging. These findings highlight the trade-offs faced by policymakers aiming to revitalize urban neighborhoods.

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1 Introduction

A large body of evidence shows that children benefit from growing up in low-poverty neighborhoods (Chyn and Katz, 2021). The strongest causal evidence comes from settings in which children move, allowing researchers to isolate "neighborhood effects" from family selection. Studies of public housing demolitions, voucher programs, natural disasters, and military personnel relocations consistently find positive effects for children who relocate to higher-opportunity areas on a range of long-run outcomes, including educational attainment, earnings, criminal justice involvement, and teen births (Chetty and Hendren, 2018; Chetty, Hendren and Katz, 2016; Chetty et al., 2026; Chyn, 2018; Kawano et al., 2024; Nakamura, Sigurdsson and Steinsson, 2022).

However, the policy implications from these studies are challenging: scaling mobility-based interventions is not straightforward, since moving large numbers of disadvantaged families to high-opportunity areas could diminish the benefits of moving in the first place (Fogli et al., 2025). In addition, because low-poverty areas are more desirable, they have higher rents in equilibrium. Capital markets typically do not allow parents to borrow from their children's future income streams, meaning high cost of living can limit access to high-quality neighborhoods (Katz, 2026).

An alternative approach to relocating families is to improve neighborhoods where children already live. Place-based policies such as empowerment zones (EZs), opportunity zones (OZs), and redevelopment programs have been shown to revitalize neighborhoods and improve local labor market conditions (Busso, Gregory and Kline, 2013; Rossi-Hansberg, Sarte and Owens III, 2010; Wheeler, 2022), though among adults most of the benefits accrue to individuals living outside these areas initially (Arefeva et al., 2025; Freedman, Kouchekinia and Neumark, 2025). Effects of these types of revitalization efforts on incumbent children are unclear: if housing costs increase with housing demand, this may dampen the benefits of neighborhood quality for children in renter households but could amplify benefits for children of homeowners if housing wealth increases. At the same time, disruptions to home communities and social networks could have their own harmful effects (Rojas-Ampuero and Carrera, 2026).

In this paper, I study the effects of large-scale neighborhood revitalization on the outcomes of the community's original children. To do so, I identify the effects of a 1998 housing initiative in downtown Houston. The city, concerned that its downtown was becoming unattractive to high-income residents who lacked access to desirable housing, reduced the minimum lot size (MLS) to build single-family homes by more than two-thirds, from 5,000 to 1,400 square feet. They targeted this reform towards less than 20 percent of neighborhoods nearest to the downtown core, a large area home to around 450,000 residents. The reform generated a burst of small single-family construction; these homes now represent 9 percent of all downtown Houston housing units (Mei, 2022; Wegmann, Baqai and Conrad, 2023).

Using individual-level address data from the U.S. Census Bureau linked to parcel-level property tax data, I first show that households who moved into these homes were White and previously lived in highly educated suburban areas, in contrast to the existing population, who were primarily Black or Hispanic and frequently poor. I then estimate synthetic difference-in-differences models showing that Houston's housing reform led to a ten percent increase in median household incomes, a 21 percent decrease in poverty rates, and although housing supply expanded, a seven percent increase in rents and a 15 percent increase in home values.[1] However, school quality did not improve, since new property tax revenue was redistributed throughout the district and most new entrants were childless. Because the affected area contained less than one-fifth of Houston's population and is separated from the rest of the city by a large highway, I do not detect price spillovers to untreated neighborhoods in the city's "outer ring."

I then test how this neighborhood change affected children's long-run outcomes using administrative address and survey data from the Census Bureau and the Texas Education Research Center. These datasets allow me to identify all households living in Houston at the time of the reform and compare their trajectories to observably similar households living in non-treated neighborhoods in other large Sun Belt cities. I estimate effects using doubly robust difference-in-differences models (Sant'Anna and Zhao, 2020), where I compare outcomes of downtown Houston children to those in other Sun Belt downtowns, using unexposed children in outer ring neighborhoods as the second difference. The chosen specification adjusts for differences in observable child and pre-reform neighborhood characteristics, while also accounting for unobservable citywide differences in labor markets or school policies that affect children's outcomes.

I find that Houston's housing reform had distributional impacts, with children of homeowners benefiting and children of renters being harmed. I observe a small increase in neighborhood displacement for renter children, in line with estimates from the gentrification literature (Brummet and Reed, 2019; Dragan, Ellen and Glied, 2020; French, Gandhi and Gilbert, 2023). Renter households are highly mobile, with 90 percent leaving their original unit within ten years in both Houston and other cities. However, when Houston renters moved, they relocated slightly farther and more frequently than they would have otherwise to compensate for higher rents.[2] These migration responses mean that on average, children of renters grew up in only marginally better neighborhoods. In contrast, children of Houston owners remained downtown at higher rates than the control group, growing up in higher-income and lower-poverty areas as a result.

Consequently, I find that younger children of Houston homeowners, who were primarily low- and middle-income, had higher human capital attainment and greater economic self-sufficiency into their young 30s. Following the literature (Bailey, Sun and Timpe, 2021; Kling, Liebman and Katz, 2007), I primarily measure these outcomes using standardized indices that pool together multiple correlated measures of educational or economic well-being reported in the American Community Survey. Children of homeowners

[1] Chaudhry (2026) previously documented these changes in home values resulting from the reform.

[2] Given the small size of the treated area, the absolute increase in the number of emigrants out of downtown is small, meaning equilibrium spillovers to untreated neighborhoods are highly unlikely.

who were ages 0-7 at the time of the reform scored approximately one tenth of a standard deviation higher on both these indices than control children. They obtained more years of schooling and were more likely to be employed in early adulthood. In addition to living in slightly lower-poverty neighborhoods and attending slightly better schools, parent housing wealth increased by 24 percent. I show that some parents liquidated this new wealth by selling their home or taking out second mortgages, potentially to finance human capital investments for their children. I find no effect on the outcomes of children who were ages 8-14 at the time of the reform, consistent with the age gradient in exposure (Chyn and Katz, 2021; Lovenheim, 2011).

In contrast to homeowners, younger children of renters had worse outcomes in early adulthood. Human capital attainment was slightly lower than control children, a difference that is not statistically different from zero, but early-career economic self-sufficiency was lower by more than 0.1 standard deviations. Because Houston renter children did not live in worse neighborhoods due to the reform, these effects could arise from disruptions occurring at critical developmental ages due to rapid changes in surrounding environments and labor markets. These negative effects are largest for children who left downtown but are present for those who stayed as well, indicating that they may capture broader channels beyond those associated with moving itself (Rojas-Ampuero and Carrera, 2026; Schwartz, Stiefel and Cordes, 2017). Once again, effects are only present for younger children.

I estimate that these changes in educational attainment and early-career labor market outcomes would translate to approximately a 21 percent increase in prime-age earnings for children of owners and an 18 percent decrease for children of renters. The magnitudes of these benefits and harms are similar to or larger than intent-to-treat estimates from similar studies of neighborhood effects (Brummet and Reed, 2019; Chetty, Hendren and Katz, 2016; Chyn, 2018). Consistent with prior work (Brummet and Reed, 2019), I do not find consistent evidence of positive effects on the labor market outcomes of adults living in Houston, including for parents of treated children.

In contrast to the positive effects on education and labor market outcomes, I find that children of homeowners, but not renters, were charged with and convicted of crimes at higher rates than control children. These increases are present for older children as well as younger children, and for both boys and girls. I argue that the pattern of effects is most likely explained by greater law enforcement in gentrifying areas (Verrilli, 2025), rather than increased opportunity for crime or social disruptions.[3]

Lastly, I consider the implications of these findings for public policy. Given the political emphasis on how revitalization initiatives affect original neighborhood residents (Been, Ellen and O'Regan, 2025), I focus on the effects of Houston's reform on the well-being of incumbent households with children. I estimate effects on the average household in annual dollar-equivalent terms, summing together effects on

[3] I also estimate a small increase in the likelihood of being a teen mother or single parent among girls in Houston, but the estimates are not robust to sample and specification checks.

the well-being of children and parents. For children, I estimate changes in lifetime earnings based on the reform's effects on human capital and early-career labor market outcomes. For parents, I consider how changes in housing costs, neighborhood amenities, and moving decisions affect the household's well-being.[4] Using a simple neighborhood choice model, I derive a set of sufficient statistics that allows me to estimate average effects on incumbents based on post-reform migration decisions, changes to home prices and rents, and the pre-reform household income distribution.

Summing together, I estimate that Houston's reform improved the well-being of the average incumbent household, with these increases bounded between $1,135 and $1,865 per year. However, this average change obscures strong distributional impacts, as renter households, who represent 57 percent of the sample, were worse off by more than $1,500 on average while owner households were better off by more than $5,000. Given that even homeowners in this sample are low- to middle-income, a local social planner would need to strongly prefer transfers to renter households for the reform to not have improved well-being on average. Nevertheless, given the large losses accrued by renters, policies attempting to create Pareto improvements, such as a property tax increase on owners, would require large transfers between households, reflecting the trade-offs faced by policymakers aiming to revitalize urban cores.

This paper finds that the benefits of neighborhood affluence for children depend on families' ability to afford access, building on an extensive literature estimating the effects of neighborhoods on child outcomes (Chetty, Hendren and Katz, 2016; Chetty et al., 2026; Chyn, 2018; Jacob, 2004; Nakamura, Sigurdsson and Steinsson, 2022). Rather than study the effects of moving to neighborhoods with more affluent households, I estimate the effects of high-income households moving in nearby, focusing on children in market-rate housing. Downtown Houston renters were less able to afford housing in newly revitalized areas, and resulting disruptions contributed to worse labor market outcomes for their children. Importantly, because Houston renter children moved to areas that were observably similar to those occupied by unexposed children, I am able to distinguish the effects of childhood disruption from the effects of neighborhood quality. This is similar to Rojas-Ampuero and Carrera (2026), who find that about half of the negative earnings effects of displacement from Chilean urban slums are due to children moving to worse neighborhoods with the remainder attributed to disruptions and social network dissolution.

In contrast to these negative impacts for renters, I show that revitalization can have large benefits for an understudied urban population: low- and middle-income homeowners with children. I find that children in these households obtained more schooling and had better early-career labor market outcomes than they would have otherwise, while parent housing wealth increased considerably. In this setting, these large home equity changes may be primarily responsible for the positive effects on children, since greater housing wealth during childhood can facilitate schooling investments that improve labor market outcomes (Daysal,

[4] Recall that I do not find evidence that the reform affected parent earnings.

Lovenheim and Wasser, 2023; Lovenheim, 2011). Among households with children living in U.S. cities, nearly 40 percent of those in the bottom quartile of the income distribution own their home;[5] the benefits to these individuals are therefore important when estimating overall effects of revitalization policies.

This work also complements research on the effects of gentrification on children. These studies have documented improvements in employment and credit outcomes for children able to remain in gentrifying areas (Baum-Snow, Hartley and Lee, 2023; Brummet and Reed, 2019), as well as small increases in anxiety, depression, and obesity, particularly among children in market-rate rental housing (Dragan, Ellen and Glied, 2019; Rick et al., 2023; Zhou, Schwartz and Elbel, 2024). Here, I consider gentrification at a larger scale, studying urban revitalization affecting a region containing approximately 450,000 residents. I also estimate effects of gentrification on a broader set of outcomes, including teen fertility and criminal justice involvement in early adulthood. As in Baum-Snow, Hartley and Lee (2023), I find no average effects on urban children. However, I show that this masks substantial heterogeneity by housing tenure, as younger children of renters, who were most exposed to disruptive price and migration effects, were harmed, while younger children of owners benefited. These benefits may come primarily from increased housing wealth rather than from school quality, which Baum-Snow, Hartley and Lee (2023) find drives the benefits of gentrification in suburban areas. These heterogeneous effects on children are similar to the effects on adults estimated in Qian and Tan (2021), who find that gentrification induced by entry of high-skilled firms primarily benefits college-educated households and homeowners while potentially harming renters.

I contribute to a large literature studying the effects of housing deregulation on neighborhoods and people. Loosening land-use regulations generates increases in housing supply that can help address the United States' housing shortage (Baum-Snow and Duranton, 2025; Blanco and Sportiche, 2026; Glaeser and Gyourko, 2018; Macek, 2024; Song, 2025), but can also change neighborhood amenities and cost of living depending on who occupies this housing (Anagol, Ferreira and Rexer, 2021; Diamond and Gaubert, 2022; Diamond and McQuade, 2019; Peng, 2023). I estimate the equilibrium effects of Houston's 1998 minimum lot size reduction, finding that while the reform increased the supply of single-family homes, as previously documented in Mei (2022) and Chaudhry (2026), these were primarily occupied by individuals who were not previously living in the city, and resulting changes in neighborhood amenities led to gentrification and increased rents. This places Houston's policy alongside other housing supply expansions that increased, rather than decreased, local cost of living (Blanco and Sportiche, 2026; González-Pampillón, 2022; Singh and Baldomero-Quintana, 2022). I provide a new direct measure of housing deregulation's welfare effects on incumbents, showing that when it spurs gentrification, resulting price appreciation can benefit children in homeowner households while harming children of renters.

[5] Author's calculations from the 2024 American Community Survey, downloaded from IPUMS (Ruggles et al., 2025). The ACS only identifies cities with populations of 65,000 or higher.

Lastly, this work provides new evidence on the effects of place-based policies. Evidence from the federal government's empowerment zone (EZ) and opportunity zone (OZ) initiatives shows that these policies are successful in generating new development, but that benefits often do not accrue to adults residing in these neighborhoods (Arefeva et al., 2025; Busso, Gregory and Kline, 2013; Freedman, Kouchekinia and Neumark, 2025; Wheeler, 2022). I show that the same is (partially) true of incumbent children: while I estimate small average effects on long-run child outcomes, this masks heterogeneity by housing tenure. The negative effects for incumbent renters imply that local policymakers face trade-offs when considering revitalization policies, which may improve places while harming some vulnerable households originally living there.

2 Policy Context

Historically, Houston has not used geography-based zoning common in U.S. cities, instead imposing some basic restrictions on building structure rather than use (Gray and Millsap, 2020). Perhaps the most binding of these is a minimum lot size (MLS). Until 1997, the city required all single-family homes to be built on a parcel at least 5,000 square feet in size. In 1998, Houston's Planning Commission overhauled its housing code by dividing the city into urban and suburban tracts. Urban tracts were defined as all neighborhoods inside the I-610 highway loop, an area containing 17 percent of the city's population. For the fourth largest city in the country, this "Inner Loop" corresponds to a region containing approximately 450,000 residents, similar to the total population of Minneapolis.

Inside the Inner Loop, the minimum lot size for single-family homes was reduced to 3,500 square feet for all homes and to as low as 1,400 square feet if the property met other rules (for example, providing at least 150 square feet of permeable surface; Ord. No. 99-262, § 2).[6] The reform was meaningful in practice, as evidenced in Figure 1, where I plot the lot size distribution of single-family homes built before and after 1998 using property tax records from the commercial data provider CoreLogic, described in Section 3.B. Homes built prior to the reform exhibit considerable bunching at the 5,000 square foot lot size.[7] Post-reform, the size distribution shifted leftwards, with newly built single-family homes bunching at the lower MLS of 1,400 square feet.

Houston instituted the reform to encourage downtown redevelopment. While the city had begun to recover from a loss of affluent households since the 1980s oil market crash (Bryant, 2004; Rodriguez, 2001; Wray, 2020), many downtown areas were still relatively disadvantaged. In 35 percent of neighborhoods, poverty rates exceeded 25 percent; in nearly a quarter of neighborhoods, the share of residents with a college degree was less than five percent. City officials believed that relaxing lot size constraints, by

[6] This reform almost exclusively affected single-family homes, as the median multifamily building lot size in Houston is 12,000 square feet. Only 10 percent of multi-family buildings built in Houston are on lots smaller than 5,000 square feet.

[7] Note that the pre-reform minimum lot size was not perfectly binding: a small share of homes built pre-reform existed on lots smaller than 5,000 square feet. Developers in Houston are allowed to petition for individual exemptions to housing regulations, which in practice are frequently granted.

increasing density and walkability, would encourage new residents to move downtown; in the words of former Planning Department director Robert Litke, the original lot size "really impeded redevelopment and revitalization in the inner city because it was a very old, suburban ordinance" (Schwartz, 1998).

These redevelopment goals were controversial at the time and remain so. Households in poor neighborhoods protested against what they perceived as rapid gentrification and rising rents, particularly in the city's historic Black and Hispanic cultural districts, the Third Ward and the East End, respectively (Buggs, 2004; Houston Chronicle Editorial Board, 2004). Meanwhile, high-income neighborhood associations, concerned that increased density would hurt property values, successfully lobbied the city council to amend the original resolution and allow city blocks to choose their own "special MLS" (Kapur, 2004), which partially delayed full implementation until 2003.[8]

In spite of opposition, downtown supply of these small homes grew rapidly during the 2000s (Figure 2a). Fifteen years after the reform, small homes comprised more than 15 percent of all single-family homes. In one out of ten neighborhoods, the number of new small homes exceeded the *total* number of pre-existing single-family homes. Neighborhoods with more new construction also saw increases in homeownership rates, as one would expect given that the large majority of single-family homes are owned (Figure A.1).[9]

Neighborhoods with strong housing growth also became more affluent (Figure 2b); between 2000 and 2013, median household incomes increased by more than $46,000 (in 2024 dollars) in neighborhoods in the top quartile of new housing growth compared to approximately $6,000 elsewhere. At the same time, these neighborhoods became Whiter and gained college-educated residents (Figure A.2), while neighborhood rents rose and poverty rates fell (Figure A.3) relative to neighborhoods with slower housing growth.

These changes in resident demographics are evidenced by the characteristics of households who moved into new homes built post-reform. In the first column in Table 1, I match CoreLogic property tax records to U.S. Census address records, described in Section 3.C, to identify the households who moved into single-family homes built in downtown Houston after 1998. I restrict to homes on lots smaller than 5,000 square feet, meaning these households moved into new houses directly encouraged by the reform. Individuals who moved into new small homes differed starkly from incumbent Houston households. They were much less likely to have children (less than 9 percent, compared to more than 30 percent among incumbents). New home occupants were mostly White (63 percent), unlike incumbent residents (32 percent), and they previously lived in neighborhoods where more than half of residents had a college degree and fewer than eight percent were poor.

Notably, only 21 percent of new single-family occupants lived in Houston's downtown core before

[8] These opt-outs, while popular in some high-income neighborhoods (Dobbels and Tavakalov, 2024), were not large in size: by 2010 they comprised less than 1 percent of total land area in the inner core. If they had any effect, these opt-outs likely exacerbated gentrification effects from the reform by pushing new development into lower-income areas.

[9] More than 88 percent of single-family homes in urban areas are owned rather than rented (author's calculations from the 1998-2013 American Housing Survey Metropolitan sample).

moving into the new home, and only 33 percent lived in the city of Houston, though 71 percent lived somewhere in Harris County prior to the move. This supports the idea that the reform encouraged new households to move to the Inner Loop, rather than expanding access to new single-family housing for incumbents. Indeed, only three percent of children living in downtown Houston in 2000 *ever* lived in one of these small single-family homes during childhood. Lastly, these new small homes may have enabled a pathway to homeownership for these households; the share who owned their home increased from 45 to 69 percent between 2000 and 2010.[10]

3 Data

I combine a number of public, commercial, and administrative data sources in this project. I first describe the characteristics I use to document the housing reform's effects on neighborhood characteristics, including demographics and rents. I then describe the individual- and household-level data I use to study the reform's effects on incumbent children.

3.A Comparison Cities and Neighborhoods

Throughout the analyses, I compare outcomes in Houston to those in a sample of other similar large cities. This consists of other cities in Sun Belt states (Arizona, California, Colorado, Florida, Georgia, Nevada, New Mexico, North Carolina, Oklahoma, Texas) among the 100 largest U.S. cities in 2000; see Table A.1 for the full list. I exclude a handful of cities that operate as suburbs for larger cities in the sample (e.g., Glendale, Arizona and Plano, Texas).

This results in a set of 30 relatively comparable cities that were "untreated" by a minimum lot size reform during this time period, including other cities that experienced rapid population and demographic change during this period like Atlanta, Austin, Phoenix, and Raleigh.[11] I split each untreated city into comparable "downtown" and "outer ring" neighborhoods by drawing a radius around their central business district such that the proportion of the city's population contained within downtown neighborhoods matches the proportion in Houston's treated area in 2000 (17.5 percent). Figure A.4 maps these cores for Dallas and San Antonio, respectively.

In my main analyses, I restrict to the set of neighborhoods that were not already affluent prior to 2000. Specifically, I exclude all neighborhoods in Houston and control cities with 2000 median household incomes above \$150,000 (in 2024 dollars). I make this restriction, which excludes approximately eight percent of neighborhoods, to isolate the effects of the reform that operate through revitalization. Because

[10] This 69 percent share may be an underestimate, since I cannot perfectly measure homeownership at the exact time households moved into their home as the Census only collects this information from all households every ten years.

[11] All but four of these cities had a minimum lot size of at least 4,000 square feet throughout the sample period. They are Albuquerque (3,500 ft^2), Jacksonville (1,100 ft^2), San Jose (1,500 ft^2), and San Francisco (2,500 ft^2). While some cities initiated small tweaks to their MLS during the post-period, none instituted a large-scale reform on par with Houston's.

downtown Houston is home to a small number of unusually high-income neighborhoods, such as the area surrounding Rice University, this also reduces the likelihood that unobservable predictors of child outcomes differ between Houston and control cities. However, I show that estimates are robust to including these high-income areas.

3.B Neighborhood-Level Data

I compile a large set of neighborhood-level characteristics to measure changes in local amenities, housing stock, and rents over time. I measure population demographics and housing characteristics using census tract-level data from the 1990, 2000, and 2010 U.S. Decennial Census and tract-level population characteristics from the 2005-2009 through 2020-2024 American Community Survey (ACS) five-year estimates.[12] To measure construction of small single-family housing post-reform, I use 1990-2017 parcel-level records from the private data firm CoreLogic. CoreLogic compiles data on the near-universe of properties in the U.S. from county and city tax and deeds records, including geographic coordinates, the year of construction, and square footage.[13]

I also measure neighborhood amenities that affect both neighborhood demand and child outcomes. The first is pollution, which I measure as the concentration of fine-grained PM2.5 particles using estimates from Meng et al. (2019); exposure to high levels of PM2.5 can affect children's cognitive development and increase the risk of asthma (Sram, Veleminsky and Stejskalová, 2017; Zhang et al., 2023). The second is restaurant density, an important driver of urban housing demand (Couture and Handbury, 2020). I use establishment-level data from Data Axle, a digital "white pages" with information on the majority of U.S. business establishments beginning in 1997. I identify restaurants from 6-digit NAICS codes, following the classification in Cook (2023). I measure the number of restaurants per census tract and also estimate a smoothed "restaurant access" measure that accounts for each tract's proximity to other neighborhoods that may themselves have high restaurant concentration, following Couture et al. (2024).[14] Lastly, to measure changes in public school inputs, I compile geographic, enrollment, demographic, and expenditures information from the National Center for Education Statistics' Common Core of Data and the Texas Education Agency (TEA).

[12] I use 2010 census tracts across all years. I assign each set of 5-year ACS estimates to the middle year, e.g., 2012 characteristics are measured using the 2010-2014 estimates.

[13] In Figure A.5, I show that neighborhood-level housing counts from CoreLogic are strongly correlated with both levels and changes in housing counts and owner-occupied unit counts from the Decennial Census and ACS, though CoreLogic tends to slightly understate total housing counts.

[14] Specifically, I estimate a CES utility function with each neighborhood's restaurant count as an input, where access to each neighborhood's restaurants is decreasing in distance and the elasticity of substitution is calibrated to $\sigma = 6.5$, estimated in Couture et al. (2024). Figure A.6 shows how this method smooths restaurant exposure across space, and Figure A.7 shows that Houston neighborhoods with the most new single-family construction also saw the largest increases in restaurant access post-reform.

3.C Census Microdata

I use individual-level administrative and survey data from the U.S. Census Bureau to study how Houston's reform affected child outcomes. I first use responses to the Decennial Census to identify households living in Houston or a relevant control city at the time of the lot size reform. The "short-form" 2000 Census contains key information for every household in the United States, in particular a roster of all individuals in each household and whether they owned or rented their home. In some analyses, I also use responses to the "long-form" Census, a longer survey completed by approximately one in six households who report other characteristics including income, education, and rents and home values. Because the reform was instituted in 1998, using 2000 responses means I am unable to identify migration responses occurring in the first two years post-reform. However, as I show in Section 4, small single-family construction did not begin en masse until 2003, in part due to policy uncertainty from resistance to the reform among some incumbent homeowners. Given this, using the 2000 Census to measure baseline exposure is unlikely to introduce much bias.[15]

I restrict attention to individuals with Personal Identification Keys (PIKs), an anonymized unique identifier present for all individuals with social security numbers (SSNs) or individual taxpayer identification numbers (ITINs).[16] Using PIKs, I link 2000 Census respondents to various internal administrative datasets to identify key covariates. These include age and sex, which come from the Numident (maintained by the Social Security Administration), and race and ethnicity, which come from the Best Race and Ethnicity Composite File (Ennis et al., 2018). Households also self-report these characteristics in the Decennial Census; I impute survey responses when the administrative record is missing.

I also link individuals to annual address data maintained by the Census Bureau, known as the Master Address File Auxiliary Reference File (MAF-ARF). I use the MAF-ARF, which has coverage beginning in 2000, to track migration responses to the housing reform. I link addresses to census tracts to study changes in exposure to neighborhood characteristics for treated children, and to CoreLogic data to study the characteristics of households who moved into new small single-family homes (Table 1).

Lastly, I study how the housing reform affected children's long-run outcomes using American Community Survey (ACS) responses and administrative criminal justice and birth data. In the ACS, fielded annually starting in 2005, respondents report their education level, employment status, individual and household income, and other characteristics. I measure criminal justice involvement using the Criminal Justice Administrative Records System (CJARS), which contains adjudication and incarceration records from states covering 83 percent of Americans (Finlay and Mueller-Smith, 2023). While data coverage varies

[15] I also use a commercial address panel from Infutor to show that moving rates did not differ during this period between Houston and control cities; see Appendix D.

[16] For more details, see Wagner and Lane (2014).

over time, CJARS has near-universal coverage of counties in this study from 2000 onwards. Lastly, I use the Numident and Census Household Composition Key (CHCK) to identify birth outcomes for treated individuals, including teen births and single parenthood. See Section 5.C for details on how I construct and measure outcomes.

3.D Student-Level Data

I supplement the Census data with administrative education and earnings data provided by the University of Texas-Dallas Education Research Center (ERC). I use these data for two purposes. First, I validate the estimated effects on long-run outcomes using an alternative data source. Second, I use granular student and staff data to measure the reform's effects on peer characteristics and measures of school quality, including test score value-added and teacher experience.

In Texas administrative data, I observe a range of short-run and long-run student outcomes, including standardized test scores, high school graduation and dropout, and disciplinary events, for all students enrolled in a Texas public school from 1994 to present. I also observe enrollment and graduation from post-secondary institutions and, for individuals living in Texas as adults, I observe quarterly employment and earnings in state Unemployment Insurance records.[17] Lastly, I observe school staff characteristics including teacher experience and salary.

The ERC data permit me to study effects on a large set of child outcomes, and to do so for a much larger sample compared to the ACS. However, I am unable to observe exact student addresses. Rather, I observe the school where students attend and define neighborhoods based on the catchment zone of their public school.[18] Houston has an intra-district school choice system, meaning I will incorrectly assign some untreated students to the treatment, and vice versa. Estimates from 2022 suggest more than 80 percent of elementary school students and 70 percent of middle and high school students attend their locally zoned school (Molina, Baumgartner and Bao, 2023).

For the same reason, I also do not observe household homeownership status, a key factor determining exposure to the reform. I use Census data to train a simple model that predicts homeownership based on child and neighborhood characteristics that I observe in both the Census and ERC data.[19] I then

[17] Approximately 80 percent of all children born in Texas still live in-state in their twenties, estimated from the 2023 American Community Survey. I find no evidence of differential sample attrition among treated students compared to control students.

[18] I use information from the School Attendance Boundary Information System (SABINS) to estimate annual neighborhood characteristics for each Texas school zone. SABINS contains geospatial boundaries for individual school zones for nearly all schools in large U.S. cities, for three school years starting in 2009-10. I overlay these boundaries with census tracts and aggregate neighborhood information to the school zone level, weighting tract characteristics based on the share of that tract contained in each catchment. The mean (median) elementary school zone in Texas contains 1.6 (1.3) tracts; for middle schools, the mean (median) is 3.7 (2.4) tracts, while for high schools it is 8.8 (7.3). For schools absent from SABINS, I estimate catchment zones by using the closest N tracts to each school (starting with the tract containing the school), where N is drawn from the distribution of tract counts for schools serving that age. Estimated characteristics closely replicate actual catchment zone characteristics.

[19] The set of characteristics available in both Census and ERC data is relatively limited. I use the following: child race and ethnicity; city region of residence (downtown or outer ring); age in 2000; and the neighborhood's proportion of White residents or

assign predicted homeownership status to children in the Texas administrative data based on the model. A validation test on a hold-out sample suggests the model correctly classifies housing tenure for approximately 70 percent of children.

4 Effects on Houston Neighborhoods

I first briefly sketch the theoretical channels by which housing deregulation affects neighborhoods. This general sketch applies to a broad set of policies that expand housing supply, for example by reducing minimum lot sizes, increasing allowable building height, or permitting apartment buildings in single-family neighborhoods. Policies encouraging this "infill development" have played a prominent role in recent housing reform efforts, with seven states passing new laws just since 2024 (Kahn and Furth, 2025).

First and most obviously, housing deregulation leads to new construction. A robust literature shows that land-use restrictions result in less housing (Baum-Snow and Duranton, 2025; Glaeser and Gyourko, 2018; Glaeser, Gyourko and Saks, 2005), and that loosening these restrictions increases supply (Anagol, Ferreira and Rexer, 2021; Blanco and Sportiche, 2026; Büchler and Lutz, 2024; Greenaway-McGrevy and Phillips, 2023; Peng, 2023; Rollet, 2025). Minimum lot size restrictions in particular generate large distortions in housing supply and home prices and generate income segregation among homeowners across neighborhoods (Gyourko and McCulloch, 2023; Macek, 2024; Mei, 2022; Song, 2025).

Second, deregulation likely affects neighborhood demographics. This is because newer housing is typically occupied by higher-income households, with older stock "filtering" through to lower-income households (French and Gilbert, 2023; Hembre, Collins and Wylde, 2024; Rosenthal, 2008). The same is true for single-family homes, since homeownership demonstrates a strong income gradient (Hembre, 2024). New market-rate single-family construction may therefore increase a neighborhood's concentration of higher-income households.

Third, initial changes in neighborhood composition can affect *future* demand for housing. When higher-income, more educated adults move in, local amenities endogenously improve (Baum-Snow and Hartley, 2020; Couture et al., 2024; Diamond, 2016; Guerrieri, Hartley and Hurst, 2013)—for example, high-income households support more restaurants (Couture and Handbury, 2020) and generate more requests for city services (Feigenbaum and Hall, 2015). This endogeneity can generate self-reinforcing cycles where rising neighborhood quality attracts further demand from high-income households, driving rents higher in a process often called "gentrification" (Lees, Slater and Wyly, 2013).

Taken together, these channels mean that overall effects on rents are unclear: demand-side responses from amenity improvements may mitigate or reverse potential rent decreases from greater supply. Indeed, the literature is mixed, with some studies finding that new urban housing increases nearby rents (Blanco and

residents with a college degree, unemployment rate, median household income and rent, and marriage and owner occupancy rates. I use a simple logistic regression for predictions, as more complex models did not improve performance.

Sportiche, 2026; González-Pampillón, 2022; Singh and Baldomero-Quintana, 2022) and others finding that it decreases them (Asquith, Mast and Reed, 2023; Li, 2022; Pennington, 2021; Rollet, 2025). This ambiguity makes housing deregulation controversial among policymakers concerned that high-income residents will displace lower-income households (Been, Ellen and O'Regan, 2025). It also distinguishes housing deregulation from other revitalization policies—such as public transit expansions, pollution reduction, or public housing demolitions—that do not directly expand housing supply or actively reduce it (Almagro, Chyn and Stuart, 2023; Hong and Macek, 2025; Qiang, Timmins and Wang, 2021; Wang, 2025).

4.A Post-Reform Neighborhood Change in Downtown Houston

I begin by estimating the effects of Houston's 1998 minimum lot size reduction on the downtown's housing market and neighborhood characteristics. I compare changes in single-family housing supply, housing prices, and neighborhood demographics and amenities in downtown Houston neighborhoods to neighborhoods in the downtowns of other large Sun Belt cities. While these cities, listed in Table A.1, are relatively similar to Houston in size, demographics, and internal geography, Figure B.1 shows that the demographics of downtown Houston neighborhoods were not trending in parallel to these other Sun Belt cities in the decade preceding the housing reform, a challenge for estimating causal effects using standard difference-in-differences models.

I instead estimate the reform's effects using synthetic difference-in-differences (SDID, Arkhangelsky et al. 2021). SDID combines the identification strategies from difference-in-differences (DID) and synthetic control (SC) estimators by re-weighting control neighborhoods across both units and time periods, and is more robust than either DID or SC alone to violations of their respective identifying assumptions. Formally, the method constructs unit weights, $\widehat{\omega}_j$, to match pre-reform trends in the dependent variable between downtown Houston and control neighborhoods. It also constructs time weights, $\widehat{\lambda}_t$, to re-weight pre-reform periods to match average levels in the post-reform period. Using these weights, I then estimate the following weighted least squares regression:

$$(\widehat{\tau}, \widehat{\mu}, \widehat{\alpha}, \widehat{\beta}) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{j=1}^{N} \sum_{t=1}^{T} (Y_{j,t} - \mu - \alpha_j - \beta_t - \mathbb{1}\{\text{Houston}\} \cdot \mathbb{1}\{\text{Post}\} \cdot \tau)^2 \widehat{\omega}_j \widehat{\lambda}_t \right\} \tag{1}$$

where $\widehat{\tau}$ is the estimated treatment effect, $\widehat{\alpha}_j$ are neighborhood fixed effects, $\widehat{\beta}_t$ are time fixed effects, and $\widehat{\mu}$ is a constant.

Figure 3 shows how downtown Houston's supply of small single-family homes changed post-reform.[20] I include the average treatment effect on the treated (ATT) in the figure note and present ATTs for all outcomes in Table 2. I follow the procedure in Arkhangelsky et al. (2021) and Clarke et al. (2023) for

[20] For all estimates, I omit neighborhoods in the top or bottom 1 percent of the distribution to avoid outliers driving the effects, though results are similar without this restriction.

inference, drawing random samples of neighborhoods in untreated downtowns and estimating a series of placebo regressions to estimate the underlying variance of the ATT parameter and derive standard errors.[21] I measure post-reform outcomes through 2013, fifteen years after the MLS reduction. This provides enough years post-Great Recession to measure changes in the underlying housing equilibrium, but represents the last year before Houston expanded the lower minimum lot size beyond just the downtown, applying it to outer ring neighborhoods as well.

In the figure's left panel, the red line shows housing in downtown Houston, while the grey line shows the optimally weighted controls. Prior to the reform Houston neighborhoods had only small numbers of these homes (which were not permitted without special permission from the city Planning Commission). Houston and the controls moved closely in parallel pre-reform, but single-family construction in Houston accelerated in the early 2000s. The right panel plots the annual difference (relative to the base year) to show the reform's effects more clearly. By 2013, the number of small homes in downtown neighborhoods increased by 75 percent compared to the number of homes in 1997, more than 50 percentage points greater than the increase in the synthetic control. The average post-reform difference is statistically significant with a p-value of less than 0.001.

This construction translated to an overall increase in single-family housing in downtown Houston (Figure B.2). Notice that the increase in total homes exceeds the increase in small homes. This aligns with evidence from Wegmann, Baqai and Conrad (2023) that most new housing did not replace demolished older homes. It also may imply that the reform, to the extent that it increased neighborhood desirability, generated a broader increase in local development.

Consistent with the theoretical sketch above, the increase in single-family housing supply corresponded to changes in the characteristics of downtown Houston residents. The leftmost panel in Figure 4 shows that after the reform, median household incomes in downtown Houston neighborhoods increased by 10.5 percent more than control units. Similarly, post-reform downtown Houston neighborhoods saw increases in the share of residents with a college degree and decreases in poverty rates relative to untreated downtowns (middle and right panels). The college degree share grew by 2.7 percentage points relative to the synthetic control, a 10 percent increase from the pre-reform mean, while the average poverty rate fell by more than four percentage points (21 percent). However, while the share of White residents increased in Houston post-reform, it did not change relative to the synthetic control.

Given that Houston's reform increased new housing construction but also caused in-migration of higher-income residents, effects on rents and home prices are unclear; while the supply expansion should lead to lower downtown housing costs, if downtown gentrification generated increased demand to live downtown, then this demand shift could increase housing costs. Figure B.3 shows that neighborhood median rents

[21] I estimate 999 placebo regressions per model.

increased in Houston's downtown relative to other Sun Belt cities, with an average post-reform difference of 7.4 log points.[22] Similarly, self-reported home values increased by approximately 15 percent.[23]

I also study effects of the reform on neighborhood amenities, since these can endogenously improve in response to gentrification (Couture et al., 2024; Diamond, 2016). I first use CoreLogic parcel-level deeds data to estimate repeat-sales regressions for homes built prior to 1998. Unlike the SDID estimates for changes in equilibrium home prices and home values, these regressions isolate demand changes from increasing housing supply. I find that resale prices for pre-existing downtown Houston homes increased by 17 to 29 log points post-reform relative to homes in control cities. I also show that downtown restaurant density increased by approximately 0.1 standard deviations, consistent with previously documented high demand for consumption amenities among gentrifier households (Couture and Handbury, 2020). I estimate a very small increase in downtown air pollution, within the EPA's acceptable margin of measurement error (U.S. Environmental Protection Agency, 2006).

I do not find effects on school quality inputs, with small and statistically insignificant changes in school funding per pupil, teacher experience, and teacher salaries. The lack of effects is perhaps surprising, since 90 percent of Houston school funding comes from local property tax revenues (Houston Independent School District, 2021),[24] and home values increased post-reform. However, because Houston allocates school funding based on student enrollments, any increased revenue from downtown homes would be redistributed across all schools. Given the downtown's small size relative to the city, the overall effect was muted. I also do not observe an increase in the White enrollment share, perhaps indicative of the fact that less than ten percent of the households who moved into newly constructed homes had children (Table 1).

4.B Robustness and Extensions

I explore multiple potential threats to identification, which I describe in detail in Appendix B. I show that estimates are not driven by my choice to exclude pre-gentrified neighborhoods from the sample. I show that estimates are similar if I use standard difference-in-differences (DID) regressions rather than SDID,

[22] Note that I cannot distinguish housing quality from quantity in the ACS; that is, this increase could be in part driven by changes in the quality of new housing rather than increased prices for the same pre-existing units. While housing quality can matter for household well-being (Hembre, Collins and Wylde, 2024), this still represents a meaningful change in cost of living for the typical household. In addition, the migration responses of renter households I show in Section 5.D.1 are consistent with rents increasing for the housing market overall.

[23] I also measure changes in home prices using the Federal Housing Finance Agency's Home Price Index (HPI). The HPI uses repeated sales and refinances for the same properties to estimate neighborhood price changes. Because it holds other house characteristics constant (such as square footage or number of bedrooms), it provides a clearer measure of quality-adjusted demand than simply studying changes in home values. This is especially true in the case of Houston's housing reform, which explicitly encouraged construction of smaller (and therefore cheaper) homes. Home prices increased more than home values, by approximately 28 percent.

[24] Public schools in Texas are mostly funded by a flat property tax set at the municipal level; in Houston, for example, this district tax was 1.38 percent of the property's value in 1998 (City of Houston Controller's Office, 1999). This tax rate changed only slightly during the post-reform study period (Texas Education Agency, 2024), though it has now fallen to approximately 0.86 percent (Harris County Tax Office, 2026).

where I select the control group for the DID regressions to match Houston pre-trends along a number of characteristics following Sun, Ben-Michael and Feller (2025). I also restrict my sample of control cities to just the largest ten cities or just other cities in Texas and confirm that the effects I attribute to Houston's lot size reform were not generated by other contemporaneous, smaller-scale reforms instituted by the city. Lastly, I estimate triple-differences regressions that incorporate trends in outer ring neighborhoods, in order to account for broader city-wide demographic changes, and arrive at very similar estimates.

I also study whether the downtown housing reform generated changes to Houston's housing market *outside* of the downtown. Because housing costs are set in equilibrium, it is possible that the price effects observed in the downtown spilled over to other nearby areas. While this is a common concern when studying geographically targeted policies (Jardim et al., 2022), I argue that spillovers are *a priori* less likely in this setting. First, downtown Houston is small relative to the city as a whole. The estimated changes in household migration attributable to the reform only represent about one percent of the relevant populations in outer ring areas, and broader demographic changes in outer ring neighborhoods track those in control cities quite closely (Figure B.7). Second, the treated area is separated from the rest of Houston by Interstate 610, a multi-lane highway that presents a meaningful physical and noise disamenity (see Figure B.8), which likely impedes spatial spillovers (Caro, 1974).[25] I estimate boundary discontinuity regressions (Jardim et al., 2022; Lee and Lemieux, 2010) that compare changes in neighborhood characteristics on either side of the I-610 loop and do not detect evidence of spillovers.

4.C Discussion

Overall, I find that Houston's downtown minimum lot size reduction resulted in new small single-family construction that increased the downtown's desirability to prospective residents. As a result, home prices and rents increased, and the region's demographics changed as more affluent and college educated households moved in. This places Houston in a class of housing expansions that increase local cost of living rather than decrease it (Blanco and Sportiche, 2026; González-Pampillón, 2022; Singh and Baldomero-Quintana, 2022). These changes may have affected the outcomes of incumbent children, whose neighborhoods improved but also became more expensive. I study these impacts in the next section.

5 Effects on Incumbent Children

I now turn to estimating the effects of Houston's housing reform and subsequent neighborhood redevelopment on child outcomes, doing so separately for incumbent households that owned and rented their homes prior to the reform. Studies of neighborhood effects often focus on renters, particularly those receiving housing assistance (Chetty, Hendren and Katz, 2016; Chetty et al., 2026; Chyn, 2018; Jacob, 2004). However,

[25] This view is supported by conversations with Houston residents and policymakers, who discuss the "Inner Loop" as a distinct entity from the rest of Houston.

housing assistance programs enroll only a small fraction of renters.[26] Therefore, most Houston renters must choose between staying in an improved neighborhood and paying more to do so, or moving to a cheaper neighborhood elsewhere. Both of these have potential costs for children: paying higher rents could reduce household resources for investment, while moving could disrupt social ties and harm socioemotional well-being (Sandstrom and Huerta, 2013). However, both choices also have potential benefits: staying in a revitalized area could benefit children through standard channels, while moving to suburban areas could reduce exposure to crime. Therefore, the overall effects on incumbent renter children are ambiguous.

In contrast, children of homeowners likely benefit from revitalization. For incumbent homeowners, cost of living remains relatively fixed even as local amenities improve, but home equity increases (Table 2). This is because mortgage payments are anchored to the home's previous sale price, and property taxes typically increase more slowly than the value of the home.[27] If families leverage their increased wealth to invest in their children, then this can benefit children's future outcomes in addition to any benefits from improved neighborhood quality. Note that because land prices in Texas are quite low, homeownership is relatively common even among low-income households in this setting. In Houston's downtown area, nearly one-third of households earning less than $35,000 per year owned their home in 2000.[28]

5.A Empirical Strategy

An ideal experiment would assign a neighborhood revitalization initiative to a random subset of U.S. cities with identical households living in identical neighborhoods. In practice, households living in different cities differ along a number of dimensions. While some of these are observable to the researcher, many are not; for example, Houston's labor market, police enforcement, and school quality may differ from other Sun Belt cities along dimensions that are not directly measurable.

I instead use a similar but distinct estimation approach to the synthetic difference-in-differences (SDID) framework used in estimating effects on Houston neighborhood characteristics. SDID requires that there exists a series of untreated cohorts of incumbent Houston children observed pre-reform that can be used to re-weight control cities to estimate a counterfactual outcome for treated Houston children. However, because Houston's neighborhood change plausibly affected adults as well as children, even cohorts of older children who were living in Houston pre-reform are still potentially exposed to the reform's effects. In addition, the Census data infrastructure only allows researchers to link longitudinal address data to demographic information and surveys beginning in 2000, meaning I cannot observe the outcomes of children

[26] In Houston, for example, only three percent of all renters receive a housing voucher (Sherman, 2025).

[27] A Harris County law states that appraised property values increase by the lower of the property's market value increase or ten percent (Harris Central Appraisal District, 2025). In practice, appraised property values in Harris County are 31 percent lower than estimated sale prices (Johnson, 2012). There is no evidence that gentrification-induced property tax increases lead to greater homeowner displacement (Martin and Beck, 2018).

[28] 2000 dollars, from 2000 Census tract-level estimates.

who were living in Houston in pre-reform years if they moved prior to 2000.

Rather than re-weight children in control cities to match pre-reform outcomes in Houston (as in SDID), I instead re-weight controls to match contemporary outcomes of Houston children who were not exposed to the housing reform. In doing so, I take advantage of the fact that the reform only affected a small subset of neighborhoods in Houston, meaning more than 80 percent of the city's children were unexposed. By including these children in a weighted difference-in-differences framework, I am able to better account for unobservable differences in outcomes between Houston and other places.

To formalize the approach, consider a simple potential outcomes model where children, living in a Sun Belt city's downtown region ($r = 1$) or outer ring region ($r = 0$), are exposed to neighborhood revitalization when $H_i = 1$ (i.e., when they live in Houston):

$$Y_{i,r} = H_i Y_{i,r}(1) + (1 - H_i) Y_{i,r}(0) \tag{2}$$

We are interested in identifying τ, the average treatment effect on the treated, of Houston's housing reform and the neighborhood revitalization that resulted. Conditional on observable characteristics (X) that affect $Y_{i,r}$, we can write τ as:

$$\begin{aligned} \tau &= E[Y_{i,1}(1) - Y_{i,1}(0) | H_i = 1, X] \\ &= E[Y_{i,1} | X] - E[Y_{i,1}(0) | H_i = 1, X] \end{aligned} \tag{3}$$

where the second line comes from the linearity of the expectation operator.

Houston's housing reform affected only its downtown region; that is, treated children have $H_i = 1$ and $r = 1$, and their expected outcome is $E[Y_{i,1}|X]$. In order to identify τ, we need to estimate $E[Y_{i,1}(0)|H_i = 1, X]$, the expected outcome of these children in the absence of treatment.

One natural idea would be to include children in the downtowns of untreated Sun Belt cities as this control group, i.e., children with $H_i = 0$ and $r = 1$. Because they live in comparable areas of relatively similar cities, these households are likely exposed to similar environments. Conditional on observable child characteristics such as gender and age at the time of the reform, and pre-reform neighborhood characteristics such as the poverty rate, their potential outcomes may therefore be relatively similar. However, this fails to account for unobservable differences between Houston and other cities that are predictive of child outcomes. Therefore, I instead invoke a weaker assumption: that absent treatment, the difference in outcomes between children in downtown Houston ($r = 1$) and outer ring Houston ($r = 0$) would be similar to the difference between children living in these regions of other cities. Formally, I assume the following:

Assumption 1: $E[Y_{i,1}(0) - Y_{i,0}(0) | H_i = 1, X] = E[Y_{i,1}(0) - Y_{i,0}(0) | H_i = 0, X]$

Under Assumption 1, and denoting the plug-in estimator for $E[Y_{i,r}|H_i = c, X]$ as $\overline{Y}_{r,c}$, we can replace

$E[Y_{i,1}(0)|H_i = 1, X]$ in Equation 3 with:

$$E[Y_{i,1}(0)|H_i = 1, X] = \overline{Y}_{0,1}(0) + (\overline{Y}_{1,0}(0) - \overline{Y}_{0,0}(0)) \quad (4)$$

Then the estimator for $\widehat{\tau}$ is:

$$\widehat{\tau} = [\overline{Y}_{1,1} - \overline{Y}_{1,0}] - [\overline{Y}_{0,1} - \overline{Y}_{0,0}] \quad (5)$$

In summary, I estimate the difference in outcomes between children in downtown Houston and the downtowns of other large Sun Belt cities, conditional on observable characteristics. I account for underlying differences between Houston and control children by including untreated "outer ring" regions of Houston and control cities as a second difference, netting out baseline city-level unobservables predictive of outcomes.

I estimate $\widehat{\tau}$ using doubly robust difference-in-differences (DR-DID; Sant'Anna and Zhao 2020). This estimator combines inverse probability weighting (IPW) with regression adjustment (RA) in a single framework that correctly identifies the treatment effect if *either* the propensity score model or the outcome regression model is correctly specified. The method reweights controls using propensity scores and incorporates outcome regression predictions as a bias-correction term. Importantly, the approach allows for covariate-specific trends in potential outcomes, meaning it does not require parallel trends to be identical across different demographic groups. For example, the difference in potential outcomes of White children who live in downtown and outer ring areas of Sun Belt cities may not match the difference for Black or Hispanic children, due to historic patterns of residential segregation.

I include the following covariates in the estimation. Child covariates, measured in the 2000 Decennial Census, are the child's age in 2000, race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic, other race), and gender. Household covariates are tenure status (renter or owner), the number of children and total number of household members, whether the household head is female, and whether the household is single-parent. Lastly, I control for the following baseline census tract characteristics measured in 2000: median rent and household income, poverty rate, share of White residents and residents with a college degree, unemployment rate, marriage rate, and a "criminal justice index" capturing the share of residents with prior exposure to the criminal justice system (Finlay, Mueller-Smith and Street, 2023). To estimate standard errors, I use a block bootstrap at the census tract level. In each of 999 iterations, I draw children in a balanced number of census tracts in each cell of the two-by-two design (downtown Houston, downtown control, outer ring Houston, outer ring control). This accounts for arbitrary correlations in unobservables within neighborhoods while retaining treatment-control balance across iterations.

5.A.1 Identification

There are three major threats to identification in this setting. First, there cannot be pre-existing differences in the behavior or outcomes of households who live in different parts of Houston relative to other cities. If children in *downtown* Houston systematically have better or worse outcomes than children in *outer ring* parts of Houston, relative to the downtown vs. outer ring gradient in control cities, then the DR-DID strategy would attribute those differences to the housing reform, biasing estimates of treatment effects. I test for any evidence of this differential geographic selection with a series of "balance tests" using outcomes measured contemporaneously to the reform. Because I do not observe pre-reform outcomes such as high school graduation or teen pregnancy for children in the main estimation sample, I instead implement these tests using a sample of slightly older children, who were ages 15-19 in 2000 and therefore reached adulthood before the reform's effects arose.

I estimate the same DR-DID regressions as my main estimates, where the outcomes (high school graduation, teen pregnancy, criminal justice involvement) are measured no later than age 19 (i.e., in 2004 or earlier). I present these estimates in Table C.1. The DR-DID estimates are close to zero and statistically insignificant for high school graduation, teen motherhood, or charges for violent or property crimes.[29] I only observe a difference between Houston and control children for one outcome, drug crime charges. While the difference is relatively large (1.7 to 1.9 percentage points), the lack of difference for the other 14 balance tests, including those for other crime types, suggests that pre-reform differences are unlikely to bias estimated effects.

Second, there cannot be other time-varying shocks that occurred during the sample period and affected the outcomes of children in just one sub-region of Houston relative to control cities. I addressed two of these in Section 4.B, showing that other reforms instituted by Houston do not appear to drive neighborhood change. I study additional potential differences in robustness checks in Section 5.G and Appendix E. Lastly, there cannot be direct policy spillovers from downtown to outer ring Houston neighborhoods due to the reform. As discussed in Section 4.B, these do not appear to be significant in practice.[30]

5.B Estimation Sample

My analysis sample consists of children who were ages 0-14 in 2000. I present sample characteristics in Table 3. Recall that this sample restricts to neighborhoods with 2000 median household incomes below $150,000, though I relax this restriction in robustness checks. Overall, 57 percent of downtown Houston children lived in renter households in 2000. Renter children were relatively disadvantaged: they were mostly Black or Hispanic (27 and 64 percent, respectively), 43 percent were poor, and only six percent

[29] Houston children in these age cohorts also do not have different human capital attainment or economic self-sufficiency through age 33, measured with the standardized index I use for my main results (Table C.11). While these two measures are potentially contaminated by the reform's broader effects on Houston's labor and housing markets, the similarity between treated and control children is additionally reassuring.

[30] These analyses also require that Houston's reform did not directly affect migration between Houston and control cities. Using Census address data, I also calculate the change in net migration flows between Houston and other sample cities. Between 2000 and 2002, the annual net migration rate into Houston from control cities equaled -0.1 percent of the city's 2000 population. From 2003-2010, it equaled +0.1 percent. These estimates suggest that the reform does not violate the Stable Unit Treatment Value Assumption.

lived with an adult who had a college degree. Downtown Houston children in owner households grew up in slightly but not dramatically more advantaged circumstances. Only 18 percent were White, while 22 percent were poor and 13 percent lived in a household with a college-educated adult. Neighborhood circumstances were even more similar between these two groups, as owner children lived in neighborhoods with poverty rates only five percentage points lower than renters. Children in control cities are relatively similar observably, but are slightly less likely to be Black and lived in neighborhoods with slightly higher rents and lower median household incomes.

I estimate effects separately for children whose parents owned versus rented their homes at baseline, since as described above, the effects of the reform likely differ meaningfully for these two groups. Based on prior evidence showing that neighborhood effects differ meaningfully across child ages (Chetty and Hendren, 2018; Chetty et al., 2026), I also split the sample into younger and older children. Downtown Houston's physical infrastructure and demographic composition changed dramatically between 2000 and 2010, after which neighborhoods reached an approximate steady state with a higher share of high-SES households and increased cost of living. For younger children, defined as those ages zero to seven in 2000, this neighborhood change occurred early in life, since they were no older than 17 when this demographic transition was mostly complete. In contrast, for older children (ages eight to fourteen in 2000), these changes occurred during young adulthood, often after moving out of their childhood homes.

5.C Measuring Outcomes

5.C.1 Human Capital and Economic Self-Sufficiency

I measure human capital and labor market outcomes using individual-level responses to the American Community Survey. Following the literature, I construct summary indices that combine multiple measures (Bailey, Sun and Timpe, 2021; Williamson, 2024).[31] By pooling outcomes that co-move in the data, I improve statistical power and reduce the scope for identifying spurious effects (Kling, Liebman and Katz, 2007). To support interpretation, I also present estimates for each sub-component.

The human capital (HC) index contains six measures. These are binary indicators for a high school diploma or GED, any college attendance, a bachelor's degree, a professional or doctorate degree, or working in a management occupation; and a continuous measure of years of schooling. The "economic self-sufficiency" (ESS) index contains five continuous measures of labor market activity: usual hours worked per week, weeks worked last year, the log of wage income, the log of the family's income-to-poverty ratio, and the log of income from public assistance, which is reverse-coded so that positive values reflect less reliance on public programs. I standardize each outcome within each ACS sample year and average the

[31] I deviate from these indices by not including income from other non-governmental sources in the ESS index. I do this because these income sources, which often reflect investment, rental, or pension income, are relatively rare in the sample given their young age.

sub-components.

I estimate effects using individuals who responded to the ACS between ages 24 and 33. I impose the age minimum to ensure that respondents have mostly completed their schooling at the time of response. I impose the age maximum to improve the comparability of my estimates for children in older and younger cohorts, given strong life cycle patterns in the outcomes I study; for similar reasons, I also include age-at-response fixed effects in regressions. Note that while there is not perfect overlap in the ages included for younger and older samples, because all outcomes are standardized, estimates are still relatively comparable; I also report within-sample means and standard deviations.

Approximately six percent of the sample responds to the ACS between these ages (for downtown Houston, this corresponds to about 4,000 respondents). I apply ACS person weights in my main estimates, which account for the survey's sampling scheme.[32] I estimate a very small degree of differential response bias: using in the DID specification described above, Houston children are 0.3 percentage points more likely to have responded to the ACS between ages 24 and 33 (significant at the 5 percent level), with similar differences for renters and owners. I test the robustness of results to alternate weighting schemes that account for differential response rates, finding similar results (Table E.1).

5.C.2 Criminal Justice Involvement

I use administrative adjudication and incarceration records from CJARS to measure criminal justice involvement. My primary outcomes are whether children were ever charged with or convicted of a crime, whether this crime was a felony versus a misdemeanor, and whether they were ever incarcerated. I also use CJARS offense codes to distinguish charges for violent crimes, property crimes, drug crimes, and DUIs (Finlay and Mueller-Smith, 2023). I measure these outcomes through age 24. This is approximately the age when criminal activity peaks (Farrington, 1986), and also comprises the longest follow-up period I can measure for every sample individual.

5.C.3 Motherhood

I use the Numident and Census Household Composition Key (CHCK) files to measure fertility outcomes for girls in the sample. The CHCK is a linkage between all U.S. children and their parents. I focus on the following outcomes: teen births, births by age 24, and single parenthood. I identify teen births using the mother's and child's date of birth from the Numident. I identify single-parent households as those where the father either was not listed on the birth certificate, or was listed but did not live with the mother during any of the first five years of the child's life. I impose the coresidency restriction given the acute challenges associated with leading a household as a single mother (Edin and Lein, 1997).

[32] The ACS one-year weights are designed to produce population statistics for the smallest geographic unit (city or county) with at least 65,000 individuals, which in this sample corresponds to the city (U.S. Census Bureau, 2025).

5.D Estimated Effects of Reform

5.D.1 Household Migration

I first study how the reform affected migration of incumbent downtown Houston households. Figure 5 contains "hazard plots" showing the probability that children moved from their original address in each year from 2000-2010, where I exclude observations when the child is 18 or older. I plot lines for both downtown Houston and control children, where the control lines are propensity-score weighted. The left panel shows the likelihood that the family moved at all, while the right panel shows the likelihood that the family moved at least five miles. This distance represents moves that would present relatively large disruptions to household circumstances. Parent commute times could change by meaningful amounts, and the child would almost certainly live in a different school catchment zone. This five-mile threshold is also the approximate radius of Houston's treated area.[33]

About half of children in homeowner households moved between 2000 and 2010.[34] The share of Houston children who moved is lower than that in control cities; as the left panel shows, this difference was approximately six percentage points in 2010. Migration rates for Houston renter households moved differently. Renter children are highly mobile; 40 percent moved within just one year, and by 2010, 90 percent lived somewhere other than their 2000 address. However, children of renters moved farther away when they did move. By 2010, the likelihood that families lived at least five miles from their original home was more than five percentage points higher in Houston compared to control cities. This is in line with the existing literature on gentrification, which suggests that direct displacement is relatively low because landlords tend not to increase rents much for existing tenants (Brummet and Reed, 2019). Rather, gentrification may instead only affect where families move when they choose to do so (Dragan, Ellen and Glied, 2020): that is, families may not move at increased rates but may move farther than they would have otherwise if market rents have increased everywhere nearby.

I test for both of these forms of instability using DR-DID regressions. First, I find that renter children made slightly more moves during childhood: as Table C.2 shows, the average downtown Houston renter child made an additional 0.08 moves before turning 18, a two percent increase relative to children in untreated downtowns; owner children did not move at higher rates.[35] Second, in Figure 6, I plot DR-DID regressions showing the reform's treatment effect on household migration. The dependent variable,

[33] More than half of all hypothetical 5+ mile moves originating in downtown Houston end outside of the treated area (Figure C.1).

[34] Census data do not allow me to measure household migration prior to 2000, meaning these estimates may partially reflect underlying differences in migration patterns of downtown Houston households that are unrelated to the reform. In Appendix D, I use commercial address data from Infutor, which measures residential locations prior to 2000, to investigate this possibility. I find that household in-migration between the time of the reform and 2000 was very similar in Houston and other cities, in both the treated downtown areas and untreated outer ring.

[35] Note that this estimate, and the control mean, likely understates the degree of instability among these households, since the MAF-ARF only records address changes annually and low-income renters often maintain multiple addresses within a year.

measured in the year the child was 17, is an indicator for whether the child lived within various distance radii of their original home address post-reform, including a distance of zero (i.e., living at exactly the same address). As the figure's left panel shows, downtown Houston homeowner children were 3.1 percentage points more likely to live at their original home address (a distance of zero) compared to control children. The difference between Houston and control cities fades to zero at approximately the five mile radius. In contrast, renter children were similarly likely to remain at their original address (right panel), but renter children overall moved farther than control children, experiencing a two percentage point reduction in the likelihood of living within five miles of their original home (significant at the ten percent level). The difference persists for at least a ten mile radius, remaining close to two percentage points and becoming more statistically significant. Taken together, the evidence suggests Houston renters experienced small but meaningful increases in housing instability due to revitalization.

5.D.2 Neighborhood and School Quality During Childhood

Next I test how migration responses affected the neighborhood characteristics children experienced during childhood. I use the MAF-ARF to identify each child's census tract of residence in each year from 2000 through age 17 and calculate their average exposure to various neighborhood characteristics over these years. I then estimate differences in average exposure using DR-DID regressions. I control for the child's age in 2000, which accounts for differences in length of exposure post-reform, and control for levels of each neighborhood characteristic in 2000, such that the estimates represent effects on *changes* in experienced neighborhood characteristics.

Table 4 shows these estimates. The left set of columns shows the neighborhood characteristics children *would have* experienced had they remained in their 2000 neighborhood. For example, the table's top panel shows that homeowner children's original neighborhoods saw college degree shares increase by 4.0 percentage points as a result of the reform. In the right set of columns, we see that homeowner children grew up in neighborhoods with college degree shares higher by 2.9 percentage points; this difference is attenuated because some households move out of their original neighborhoods. Nevertheless, homeowner children lived in neighborhoods with college degree shares higher by 18 percent, poverty rates lower by four percent, and median household incomes higher by ten percent.

These patterns stand in sharp contrast to those for children in Houston renter households. As the table shows, had these renter households remained in their original neighborhood, they would have needed to pay rents that were higher by 3.8 percent. At the same time, children would have lived in neighborhoods during childhood with poverty rates lower by 17 percent, median household incomes higher by 18 percent, and the share of neighbors with a college degree higher by 36 percent. Household migration attenuates more than three-quarters of these quality improvements across outcomes, and for poverty rates, the differences attenuate nearly entirely. Renter children lived in neighborhoods that were in fact slightly cheaper on average with median rents lower by 1.6 percent, perhaps due to longer-distance moves to suburban areas.

Because the reform took time to affect neighborhood characteristics, original home neighborhoods changed much more dramatically for younger children. This is shown clearly by comparing the bottom two panels, where I split children into those who were ages 0-7 versus 8-14 at the time of the reform. For example, younger renter children's original neighborhoods experienced increases in median household income of 19.7 log points, versus only 12.0 log points for older renter children. However, actual neighborhoods of residence were very similar regardless of age. These age patterns are similar for children of owners, except younger owner children lived in slightly better neighborhoods than older owner children, especially as measured by the share of college-educated adults. Moreover, younger children of owners spent more than fourteen years exposed to these neighborhoods, compared to only six years in the older sample.

Lastly, in Table C.3, I use administrative student and school staff data from the state of Texas to study how school peer composition, spending per pupil, teacher experience and salaries, and school value-added differed for renter and owner children exposed to the reform.[36] Recall that I only compare Houston children to those in the other largest school districts in Texas, and that I predict household homeownership status since I do not observe it directly. I observe small improvements in school environments for Houston owner children, who attended schools with more experienced teachers and slightly (albeit not significantly) greater spending per pupil. In contrast, compared to renter children in other Texas school districts, Houston renters attended schools with lower spending per pupil by approximately 10 percent; other school characteristics were relatively similar.

5.D.3 Outcomes Through Early Adulthood

On average, I do not observe large effects of the housing reform on either human capital (HC) or economic self-sufficiency (ESS) indices. These estimates, presented in Table 5, show that the average human capital or economic self-sufficiency of downtown Houston children did not differ from children in other Sun Belt downtowns. Children in owner households had slightly higher human capital attainment by 0.039 standard deviations, which is statistically different from zero at the 10 percent level, while Houston renter households had very similar HC and ESS outcomes to control households.

However, I observe large effects on younger children. Among those who were 0-7 years old in 2000, I again find a null overall treatment effect on both outcomes, but this masks heterogeneity by housing tenure. Younger children of homeowners experienced a 0.097 standard deviation increase in human capital attainment and a 0.142 standard deviation increase in economic self-sufficiency. Relative to the control

[36] I estimate school value-added following the empirical Bayes approach in Kane and Staiger (2008). I residualize student test scores in year t on a number of observable student characteristics, most importantly lagged test scores from year $t-1$. I then construct a precision-weighted average of school-year residuals and shrink these average residuals towards zero based on the "reliability" of the estimate, a function of the year-to-year correlation in school average residuals and the school's enrollment. While these estimates rely on a strong selection-on-observables assumption, school test score value-added estimates have been shown to be approximately forecast-unbiased in similar settings (Deming, 2014).

means in untreated downtowns of -0.122 and -0.121, this reflects improvements to approximately the national average among same-age peers. In contrast, the human capital attainment of younger children of renters was lower by 0.057 standard deviations (not significant) and economic self-sufficiency was lower by 0.112 standard deviations relative to renter children who were unaffected by the reform. The reform did not have an effect on either outcome for children who were older than seven.

These differences reflect large increases in schooling for children of homeowners and reductions in employment for children of renters (Tables C.4 and C.5). Younger owner children have 0.54 more years of schooling and are four to six percentage points more likely to graduate from high school, attend college, and get a bachelor's degree. While only the continuous years of schooling measure is statistically significant at traditional levels, I can rule out declines larger than 1.1 percentage points on any of these binary schooling outcomes with 95 percent confidence. For renters, the largest human capital declines appear to come from post-graduate degrees and management occupations rather than changes in high school or college-going. Meanwhile, owner children have improvements on every input into the ESS index (including employment) except for earnings; because outcomes are only measured into their early 30s, this could reflect delayed earnings profiles from getting more education. Renter children experience declines on nearly every input, working fewer hours per week, fewer weeks per year, and earning 22 percent less than control children.

5.E Other Socioeconomic Outcomes

I also study the effects of the reform on other socioeconomic outcomes of interest. I find that children of owners were more likely to experience contact with the criminal justice system (Table 6). Overall, they were nine percent more likely to be charged with a felony, 15 percent more likely to be charged with a misdemeanor, and 13 percent more likely to be incarcerated. The misdemeanor increases are similar in percent terms for older and younger children, though I only observe increases in felony charges for older children (Table C.6). They are present for both violent and property crimes (Table C.7). Male children of owners were charged with both felonies and misdemeanors at higher rates, while effects are only present for misdemeanors for females (Table C.8). Younger renter children were less likely to have criminal justice contact, while older renter children were more likely to be charged with violent crimes (Table C.7).

I find small increases in both teen births and single parenthood among owner children in Houston, but no changes among renter children (Table C.9). Owner children are one percentage point more likely to be a teen mother, a five percent increase, and two percentage points more likely to be a single mother, an eight percent increase. This could imply that living in a rapidly changing neighborhood has negative social spillovers on households (Anderson, 2013; Sampson, Raudenbush and Earls, 1997). However, the estimates are not robust to alternative samples and specifications; see Section E.

5.F Effects on Parents and Other Adults

Lastly, I study whether the reform affected parents of Houston children or other adults living in the downtown. I estimate DR-DID regressions using parents of sample children who responded to the ACS while the child was still living at home. These regressions are identical to those used to estimate effects on children, except I control for human capital attainment since this is a pre-determined characteristic for adults.

Parents' economic self-sufficiency was unaffected by the reform (Table C.10). Earnings were slightly lower for both owners and renters but these differences are not statistically different from zero. In line with the estimated changes in neighborhood characteristics, I do not find that renter parents paid more in rent. However, I observe large increases in self-reported home values among homeowner parents of more than $80,000, approximately 24 percent of the control mean. Owner parents were slightly less likely to still own a home, and were 85 percent more likely to have taken out a second mortgage. This is potentially indicative of households leveraging their additional home equity, either by selling or refinancing, which could support child investments; I return to this discussion in Section 5.H.

Similar to other studies of place-based interventions (Arefeva et al., 2025; Freedman, Kouchekinia and Neumark, 2025), I do not observe large changes in economic self-sufficiency for older incumbent individuals in Houston. Table C.11 shows no change for individuals who were 20-29 or 45-54 in 2000. Among Houston adults who were ages 30-44, I estimate a positive effect on ESS of 0.06 to 0.07 standard deviations. While the estimate is statistically significant at the 1 percent level, it is less than half the increase for young homeowner children. However, it is present for both owner and renter adults.

5.G Robustness

I test the robustness of these results in multiple ways, described in detail in Appendix E. Estimated effects on economic self-sufficiency and human capital are not a product of my decision to use ACS sampling weights. They are also similar when I include children in neighborhoods that were already high-income at baseline. They are not sensitive to the exact set of control cities used, nor are the economic self-sufficiency estimates driven by differential impacts of the COVID-19 pandemic on downtown Houston. The results also do not reflect negative spillover effects from the 2005 influx of Hurricane Katrina evacuees to Houston. The same is generally true for the criminal justice estimates, which show increases in felony and misdemeanor charges for owner children and no change or small decreases in charges for renter children. However, the increase in teen or single parenthood for female owner children is not present in alternative specifications.

Lastly, I show that the divergence in effects for younger owner and renter children is present in Texas administrative data as well. While these estimates are subject to the previously mentioned caveat about mismeasured homeowner and renter status, I estimate that children of predicted Houston homeowners had better educational outcomes and early career economic outcomes, and even estimate a positive effect on earnings here, which I did not observe in the ACS. Children of predicted renters, in contrast, are less likely to enroll in college and spend fewer quarters employed. Note that I also observe an increase in suspensions for children of homeowners but not renters, consistent with the patterns of criminal justice involvement for these groups.

5.H Discussion

As a result of Houston's minimum lot size reform, incumbent children of homeowners in the city's downtown lived in higher-income and lower-poverty neighborhoods during childhood. In contrast, renter households moved to cheaper neighborhoods farther away, meaning their children did not live in better neighborhoods than the control group. Younger renter children got less education and were worse off economically in early adulthood, while same-age owner children experienced improved educational outcomes and early-career economic self-sufficiency. I do not observe any effects on these outcomes for older children, but do find increases in charges and arrests, particularly in homeowner households.

Because I am only able to observe individuals through their early 30s, I follow Bailey, Sun and Timpe (2021) and predict the change in permanent earnings we would expect based on the observed changes in human capital and early-career employment for younger children. The authors, who study the long-run effects of Head Start using a similar approach to this paper, predict the relationship between inputs of the HC and ESS indices and log earnings in individuals' prime working years for the NLSY79 cohort, measured between 2002 and 2014 at ages 35 to 57. I use their regression coefficients and the estimated change in each input for younger Houston children to estimate the change in log earnings attributable to the reform. I predict that the reform led to a 21 percent (18.8 log point) increase in prime-age earnings for children of owners and an 18 percent (19.4 log point) decrease for children of renters (Table C.12). These magnitudes are about 40 percent larger than the positive effects of Head Start. Overall, the estimated benefits for homeowner children, and losses for renter children, are similar in percent terms to the effects of the Moving to Opportunity experiment on younger children and to the effects of moving to better neighborhoods as a result of public housing demolitions (Chetty, Hendren and Katz, 2016; Chyn, 2018).

There are many reasons why younger children of homeowners may have benefited economically while children of renters did not. First, homeowner children, particularly those who were seven or younger in 2000, lived in neighborhoods with more college-educated adults and lower poverty rates. Reduced exposure to poverty may have benefited homeowner children's human capital development. However, these changes may not be large enough to fully explain the reform's positive effects. For example, downtown Houston owner children lived in neighborhoods with poverty rates lower than controls by one percentage point for fourteen years. In the Moving to Opportunity (MTO) experiment, the average treated child lived for ten years in a neighborhood with a poverty lower by 10.3 percentage points (Chetty, Hendren and Katz, 2016). If the effects of poverty exposure on outcomes are additive and linear (both strong assumptions), then a back of the envelope calculation implies we should find effects only 14 percent as large as those in MTO. In contrast, I estimate larger effects on schooling and predict that prime-age earnings effects would be similar to MTO in percent terms.

It is also possible that most of the benefits for homeowner children come from increased housing wealth, which families can partially liquidate to finance higher education (Daysal, Lovenheim and Wasser, 2023; Lovenheim, 2011). I find that Houston parents who owned their homes in 2000 experienced a more than $80,000 increase in housing wealth post-reform, and show that these parents took efforts to liquidate some of this wealth by selling their homes and taking out second mortgages. Taking the estimate from Lovenheim (2011), who found that each $10,000 in housing wealth (in 2010 dollars) increases college-going by 0.7 percentage points, 72 percent of the 5.5 percentage point increase in college-going among homeowner children could theoretically be attributed to home equity rather than neighborhood or school effects.[37] I do not find any changes in parent earnings or overall economic self-sufficiency, suggesting these channels are not driving effects.

Meanwhile, younger children of renters were made worse off as a result of the housing reform. One possibility is that renter households differ from owners in unobservable ways that affect how their children respond to revitalization. However, another likely explanation is that these differences are due to negative exposure to cost-of-living increases and resulting disruptions. The negative effects are too large to only arise from direct displacement, since I estimate that only two percent of renter children left downtown Houston due to the housing reform. Scaling up the reduced-form by this "first stage" results in a point estimate that is implausibly large given prior estimates of the effects of moving itself on child outcomes (Hanushek, Kain and Rivkin, 2004; Schwartz, Stiefel and Cordes, 2017). Instead, this implies that the reform negatively disrupted children even in households who would have always remained in downtown Houston or would have always moved some time during childhood. Rojas-Ampuero and Carrera (2026) documented similar "disruption effects" from changes in neighborhood context and social network dissolution for children displaced from Chilean urban slums. In the context of urban gentrification, these disruption channels can occur for both stayer *and* leaver children, since stayers also experience large changes to their local social networks and lived environments and are exposed to increases in equilibrium rents.

Indeed, in Table C.13, I split the sample of younger children into those who did or did not move at least five miles from their original residence during childhood. These samples are partially endogenous since the reform affected moving decisions, though this bias is likely small since only a small share of households were induced to move due to the reform. Overall, the negative effects for renters are concentrated among

[37] After accounting for the difference in the year used to measure dollar levels (2010 vs 2024).

movers, especially for economic self-sufficiency, but they are present for stayers as well. This suggests that both mover and stayer households may have experienced increased instability and disruptions, but for stayer households these may have been partially offset by living in lower-poverty areas. For owners, effects are more positive for movers than stayers, providing additional evidence supporting the role of home equity shocks. For example, recall that Houston children of owners attended higher-quality schools (Table C.3), but downtown Houston schools did not improve (Table 2). Therefore, families may have leveraged their additional housing wealth to invest in moves to better school districts.

Multiple channels could explain the observed increases in crime among owner children. Police enforcement may have increased when downtown Houston became more affluent, due to pressure from new residents (Anderson, 2013; Verrilli, 2025).[38] However, the relative benefits from engaging in crime, particularly property crime, also increase when areas become more affluent (Kling, Ludwig and Katz, 2005). Lastly, disruptions to a neighborhood's social equilibrium could negatively impact the mental health of incumbent children (Dragan, Ellen and Glied, 2019); if being newly surrounded by affluence increased feelings of relative deprivation (Merton, 1938; Smith et al., 2012), then this could also explain increased criminal behavior. While I cannot fully distinguish potential mechanisms, the evidence points to an important role for increased enforcement.[39] This is because I observe increases in both violent and property crime, suggesting that greater economic returns are not the sole drivers. In addition, if social disruptions were the predominant driver, we would expect to see larger impacts among children of renters, since mover families may experience the largest psychological harms from gentrification (Dragan, Ellen and Glied, 2019).[40]

6 Policy Implications

I now consider the overall effects of Houston's lot size reform on incumbent households from the perspective of a local policymaker. The reform and resulting neighborhood change affected households in multiple ways. For children, it affected education and earnings. For parents, it did not affect contemporary earnings but did affect both housing costs and amenities in Houston's downtown. While all households could potentially benefit from improved amenities, increases in housing costs could be harmful for renters but potentially beneficial for homeowners. Quantifying overall effects requires placing these various benefits and costs on a single unified scale.

I estimate changes in the well-being of Houston parents and children in present day dollar-equivalent terms. For children, I estimate discounted changes in annual prime-age log earnings. For parents, I use a

[38] Increased rules enforcement can also occur in schools where fewer children are low-income (Chin, 2024), potentially explaining the increase in high school suspensions for these children.

[39] This mechanism is supported by informal conversations with Houston residents, who described community concerns about increased police enforcement in new housing developments.

[40] While I cannot rule out relative deprivation as an explanation, prior work has found limited evidence for this theory driving changes in criminal behavior in similar contexts (Clampet-Lundquist et al., 2011).

simple neighborhood choice model to calculate effects on household well-being in "compensating variation" terms, i.e., the dollar transfer that would make households as well off as in the absence of the reform. I derive sufficient statistics that allow me to bound the valuation households place on unobserved amenities, which I use to estimate compensating variation. Summing the parent and child effects together gives the total effect on incumbents.

Note that I focus on estimating effects for incumbent households with children. I do not estimate effects on new entrants who were drawn downtown by the new single-family construction. This means I likely understate the overall benefits of the housing reform, since by revealed preference these households are weakly better off from having moved downtown.[41] I also do not account for broader effects on Houston's macroeconomy, which may have benefited incumbent prime-age workers as I show in Table C.11. This means that these estimates likely understate the benefits of Houston's reform to the downtown region.

6.A Estimation Overview

I provide an overview of the estimation here, and refer the reader to Appendix F for more details.

Children. I estimate effects on children using the present discounted value of estimated log-earnings changes from Table C.12. Prime-age earnings are assumed to be realized between ages 35 and 57, consistent with Bailey, Sun and Timpe (2021), and discounted back to age 7 at a three percent annual rate. Given the estimated prime-age earnings in the control group of \$39,676 for owner children and \$35,529 for renter children,[42] this translates to an additional +\$2,682 per owner child and −\$2,059 per renter child. With approximately 0.8 children per household below the age of seven, this equates to gains of \$2,145 per owner household per year and losses of \$1,647 per renter household per year. Because these are reduced-form earnings estimates, they already incorporate childhood investments financed by parent home equity gains; I return to this below.

Renter parents. To estimate effects on parent well-being, I use a simple neighborhood choice model where families value consumption and local amenities. Beginning with renter parents, I assume that household i living in neighborhood j receives indirect utility that takes the following form:

$$V_{i,j} = \ln(y_i - R_{i,j}) + \gamma \ln(A_j) + \varepsilon_{i,j}, \tag{6}$$

where $y_i - R_{i,j}$ is the household's consumption (income minus rents), A_j is the neighborhood's level of common amenities, and $\varepsilon_{i,j}$ is a time-invariant idiosyncratic location preference draw from a Type-I extreme value distribution. The housing unit is rented from an absentee landlord who profits from rents

[41] See Mei (2022) for a more detailed discussion of the reform's effects on these new homeowners.

[42] Taking the sample means in Table C.5 and multiplying by 1.3 to account for life-cycle earnings growth.

but is assumed to not be another incumbent who has children. When households leave their original neighborhood, they pay a moving cost, denoted κ.

Estimating effects on parents requires distinguishing between households who stayed downtown and those who left due to the housing reform. Since nearly all renters leave their original 2000 housing unit within 10 years, I focus on long-distance moves that take households outside of the downtown core, proxied with moves of at least five miles while children are still living at home (see Section 5.D.1). In the language of the instrumental variables literature (Angrist, Imbens and Rubin, 1996), I characterize renters as "never-movers" (those who remain downtown with and without gentrification), "compliers" (those who leave downtown due to gentrification), and "always-movers" (those who move out of downtown regardless of gentrification). The population shares of these groups are 52 percent, 2 percent, and 46 percent, respectively.

When downtown Houston gentrified, both rents and amenities increased. Household i experiences a change from $(R_{i,0}, A_0)$ to $(R_{i,0} + \Delta R_i, A_0 + \Delta A)$. Never-mover renter households experience direct changes in utility due to gentrification. I characterize these changes using a compensating variation approach, estimating the dollar transfer (CV_i) that would make households equally well off before and after their neighborhood gentrifies:

$$\ln(y_i - R_{i,0} - \Delta R_i - \mathbf{CV_i}) + \gamma \ln(A_0 + \Delta A) = \ln(y_i - R_{i,0}) + \gamma \ln(A_0). \tag{7}$$

Taking a first-order approximation around the pre-reform equilibrium, the compensating variation for a never-mover household i is approximately:

$$CV_i^{NM} \approx \underbrace{\gamma \cdot (y_i - R_{i,0}) \frac{\Delta A}{A_0}}_{\text{WTP for amenities}} - \underbrace{\Delta R_i}_{\text{rent increase}} . \tag{8}$$

The scaling component of willingness-to-pay for amenities, $\gamma \frac{\Delta A}{A_0}$, is not observed, meaning CV_i is not directly observed.[43] We can instead express CV_i relative to the marginal household, who is approximately indifferent between staying and leaving. Representing the income of this marginal never-mover household as y^* and the compensating variation for this household as CV^*, I show in Appendix F that one can write the compensating variation for *any* never-mover household as:

$$CV_i^{NM} \approx \left(\frac{y_i - R_{i,0}}{y^* - R_0^*} \right) (\Delta R^* + CV^*) - \Delta R_i, \tag{9}$$

using the change in rents for marginal households to benchmark willingness-to-pay for amenities for all

[43] I show in Appendix F that we can bound $\gamma \cdot \frac{\Delta A}{A_0}$ between zero and 0.018. Assuming the percent change in amenities, $\frac{\Delta A}{A_0}$, is no less than the 7.4 percent increase in equilibrium rents, then γ lies between 0 and 0.24.

households. The average compensating variation across all never-movers is then:

$$\overline{CV}_R^{NM} \approx \left(\frac{\overline{y}_R^{NM} - \overline{R}_0}{y^* - R_0^*} \right) (\Delta R^* + CV^*) - \overline{\Delta R}. \tag{10}$$

Rather than obtain a point estimate for CV^*, I bound it between $[-\Delta R^*,\ 0]$. CV^* cannot be greater than zero, because this would imply that the household should have left the downtown previously to obtain higher utility in their outside option neighborhood.[44] In contrast, these households can be worse off post-reform even if they are indifferent between staying and leaving, for example if moving costs are high or other neighborhoods in the discrete choice set do not provide high utility values. However, the household cannot be worse off by more than the negative of the change in rents (ΔR^*). This lower bound occurs when the valuation of downtown amenity changes equals zero, which is a change in utility that is achievable for all households by simply not moving.[45]

Always-movers do not experience effects from the reform, since the reform did not affect neighborhoods outside of the downtown. Compliers, who were induced to move as a result of the reform, are weakly worse off depending on the quality of their preferred outside option neighborhood. By revealed preference, utility effects from moving are more negative than CV^*, since if households are better off than the marginal never-mover, they would not have moved. Because I observe that Houston renter households do not live in observably worse neighborhoods post-reform, I assume that mover households are no worse off than the full moving cost, $-\kappa$, i.e., their neighborhood choice set is sufficiently large that there exists at least one other neighborhood j' such that $V_{i,j'} = V_{i,0}$.

Owner parents. A key distinction between renters and owners is how they experience changes in housing costs following neighborhood revitalization. For a renter household, housing costs are dictated by the market rent, which increases with neighborhood amenities in equilibrium. For an owner household, in contrast, housing costs equal a fixed mortgage payment m_i anchored to the purchase price prior to the reform. I model the owner's effective budget constraint as:

$$c_i = y_i - m_i. \tag{11}$$

Owner households that remained downtown benefit from increased amenities, but there is no offsetting rent increase since m_i is fixed. As for renters, the average compensating variation can again be represented

44 Denoting the household's pre-reform utility from downtown Houston as V_0^*, their post-reform utility from downtown Houston as V_1^*, and their utility from the best outside option (net of moving costs) as $V_{j'}^* - \kappa$, having $CV^* > 0$ would imply that $V_1^* > V_0^*$. If the household is indifferent between staying and leaving, then $V_1^* \approx V_{j'}^* - \kappa$, meaning $V_{j'}^* - \kappa > V_0^*$.

45 Intuitively, under the envelope theorem, changes in utility for marginal households subject to a local change in rents and amenities should be small. The estimated lower bound of $CV^* = -\$509$ is also empirically reasonable given the distribution of moving costs estimated in previous studies of gentrification's effects (French, Gandhi and Gilbert, 2023).

in terms of the WTP for the marginal renter never-mover (up to a first-order approximation):

$$\overline{CV}_O^{NM} \approx \frac{\overline{y}_O^{NM} - \overline{m}}{y^* - R_0^*} \cdot (\Delta R^* + CV^*). \tag{12}$$

This is the same structure as the renter WTP term, scaled by owner disposable income rather than $\overline{y}_R^{NM} - \overline{R}_0$. Since there is no $-\Delta R_i$ to subtract for owners, $\overline{CV}_O^{NM} > 0$ unconditionally: unlike renters, all owners gain from the reform, because their housing costs are insulated from the amenity-driven rent increase.

Home equity shock. In addition to increased access to downtown amenities, owner parents also now have increased housing equity. They can leverage this in multiple ways, either to increase their own consumption or to directly invest in their children. Formally, I introduce a new per-period annuity, $h_i(A)$, that can be used either for consumption or for childhood investments. Let $\alpha_i \in [0, 1]$ denote the share of this annuity that parents consume each year, with the remaining $(1 - \alpha_i)$ share spent on their children. The parameter α_i therefore governs additional parent-side gain from the equity shock, beyond what is already incorporated in the estimated effects on children. I annuitize the observed equity increase over thirty years.

Owner "always-movers" can also benefit from the fact that their original home sold for more than it would have otherwise. I therefore assume that these families benefit from the same potential home equity as the stayers, also scaled by α_i. This contrasts with always-mover renter households, whose well-being is unchanged. I do not observe a change in the overall likelihood of staying within five miles for homeowners, and I therefore assume that the complier share for owners is zero.

Sufficient Statistics. The model implies that estimating average effects for renter and owner parents requires only the following statistics:

1. Income of marginal renter never-movers, y^*
2. Average income of inframarginal renter and owner never-movers, $\{\overline{y}_R^{NM}, \overline{y}_O^{NM}\}$
3. Baseline and change in rents paid by marginal renter never-movers R_0^*, ΔR^*
4. Average baseline rents $\overline{R}_0$ and mortgage payment $\overline{m}$, average changes in rents and home values for never-movers $\overline{\Delta R}$, $\overline{\Delta P}$
5. Moving costs κ
6. Interest rate r and time horizon T
7. Parental consumption share of the home equity shock, $\overline{\alpha}$

Table F.1 shows how these statistics allow estimation of average compensating variation for never-movers, compliers, and always-movers, for both renters and owners. I estimate $\overline{CV}$ at the lower and upper bound, based on the assumed value for CV^*. Table F.2 lists the values for each input parameter. I estimate the average income of never-mover renters and owners and the income of complier renters using standard instrumental variables approaches (Abdulkadiroğlu, Pathak and Walters, 2018; Angrist, Imbens and Rubin, 1996). I estimate the marginal renter household's income to be \$35,160 (in 2024 dollars) and the average income of never-mover renter and owner households to be \$55,800 and \$97,050, respectively. These income differences match the gradient predicted by the model, and imply that never-mover households have higher relative willingness-to-pay for amenities given the diminishing marginal utility of income.

Assumptions. The model makes three key simplifying assumptions. First, I assume that the valuation households place on neighborhood amenities, A_j, is constant. Assuming that γ is uncorrelated with income implies that richer households have a higher willingness-to-pay for increasing amenities, since their marginal utility of consumption is lower (Equation 8). This is consistent with a literature documenting that demand for urban amenities is greater among higher-income residents (Couture et al., 2024; Diamond, 2016). In addition, this assumption means that owner and renter households with the same income cannot value neighborhood characteristics such as school quality or restaurant access differently due to unobservable traits associated with their housing tenure choice. This permits me to estimate $\overline{CV}_O^{NM}$ using information on the marginal never-mover renter household.

Second, I assume that idiosyncratic neighborhood preferences are uncorrelated with household income. If $\varepsilon_{i,j}$ is positively correlated with income conditional on other observables, this would bias upwards the estimated compensating variation of never-movers (and vice versa), since the higher average income of never-movers would be attributed to gains from higher ΔA when it is simply due to higher $\varepsilon_{i,j}$.[46] Third, I assume that the relationship between utility changes from rents and amenity valuation is approximately linear, a reasonable assumption given the relatively small change in rents. Given the concavity of the utility function, this means I may slightly overstate household compensating variation.[47] The aggregate estimates below show that willingness-to-pay for amenities is a relatively small share of the total change in incumbent well-being, meaning the biases resulting from these assumptions are proportionately small.

6.B Effects on Overall Well-Being

Table 7 shows estimated changes in average well-being for Houston children and adults in both owner and renter households, as well as the average change across all households. I show these estimates assuming that marginal never-mover renters have a utility change of either zero or −\$509, the lowest value consistent with the model. This bounds average estimated compensating variation for never-mover renters between \$205 and −\$643 per year. The two percent of renters who moved more than five miles due to the reform

[46] I also make a related, benign assumption that the distribution of $\varepsilon_{i,j}$ is static, i.e., that it relates to fixed characteristics of neighborhoods rather than changes post-reform.

[47] The ratio of the second-order to first-order term is approximately $\frac{1}{2} \cdot \frac{\Delta A}{A_0}$. Assuming the percent change in amenities is no less than the 7.4 percent increase in equilibrium rents and no greater than the 17 percent equilibrium increase in resale prices for pre-existing homes, then the second-order term is between 3.7% and 8.5% as large as the first-order term.

experience average losses between \$1,250 and \$1,505 per year, equal to the midpoint of $[-\kappa, CV^*]$ under each scenario.

Owner households experience large improvements, since housing cost increases positively contribute to these families' home equity. At $\overline{\alpha} = 0.75$, parents consume 75 percent of the annuitized equity flow ($\overline{\alpha} \cdot \overline{h}(A) = 0.75 \times \$4{,}151 = \$3{,}113$ per year), with the remaining 25 percent reflected in the child welfare effects. I estimate average changes for never-mover owner parents at \$3,113 to \$4,643 per year, and changes for always-mover owner parents at \$3,113 per year. Overall, the average change for renter parents is between \$82 and −\$364 per year, while the average change for owner parents is between \$3,113 and \$4,214 per year.

Summing together with the effects on children, the reform increased the well-being of the average owner household with children by between \$5,259 and \$6,360 per year in dollar-equivalent terms and reduced the well-being of the average renter household with children by between \$1,565 and \$2,011 per year. Averaging across owner and renter households, who represent 43 and 57 percent of downtown households, respectively, this is a change of between +\$1,135 and +\$1,865 per incumbent household per year. Therefore, Houston's lot size reform increased the average well-being of downtown Houston households, but this average improvement masks strong distributional impacts, with aggregate gains of between \$76.2 million and \$92.2 million per year for owner households and losses of \$29.7 million to \$38.2 million per year for renter households.

6.C Accounting for Inequality Aversion

I consider how the conclusions would change if a local social planner has a social welfare function that places greater weight on less affluent households. Consider some arbitrary welfare weights, $\{\omega^O, \omega^R\}$, for owner and renter households, respectively. We can solve for the cutoff value for these relative welfare weights such that the reform no longer improved overall well-being. Setting the welfare-weighted sum to zero requires the renter-to-owner weight ratio to equal the ratio of aggregate owner gains to aggregate renter losses:

$$\frac{\omega^R}{\omega^O} = \frac{W^O_{agg}}{W^R_{agg}} = \frac{76.2M}{38.2M} \approx 2.0 \tag{13}$$

The reform therefore generates positive aggregate effects if the social planner values a marginal dollar transferred to a renter household at less than twice that of a marginal dollar transferred to an owner household (when $CV^* = -\$509$), or 3.1 times as much when $CV^* = 0$. These relative welfare weights are larger than one would predict based on the differences in household income alone, since owner households are not so much richer than renters. For example, using the "inverse-optimum" weights calculated by Hendren (2020), the U.S. income tax system values a marginal dollar transferred to households with the same income as Houston renters at 1.14 times that of a dollar transferred to households earning as much as

Houston owners. This means that the reform was overall beneficial for incumbents as long as a local social planner's relative preference for redistribution is less than 1.75 times as large as the preferences embedded in the tax code.

6.D Sensitivity

Table F.3 shows estimates under various parameter assumptions, including various moving costs, discount rates, and life-cycle earnings trajectories. Estimates are consistently positive. One exception is the assumed share of housing wealth consumed by parents, $\overline{\alpha}$, since if parents are investing most of this housing wealth in their children then any housing equity gains are increasingly "double-counted." Net welfare changes remain positive as long as parents consume at least 12 percent of their housing wealth.

6.E Potential for Pareto-Improving Transfers

Lastly, I consider whether feasible transfers exist that could make the reform Pareto-improving. Ensuring renter households are no worse off after the reform would require transfers of \$2,011 per renter household each year (under $CV^* = -\Delta R^*$). If these transfers came from just incumbent owner households, this necessitates an annual transfer of $\frac{\$38.2\text{M}}{14{,}500} \approx \$2{,}635$ per owner household. However, because the reform also benefited the more than 20,000 new entrant households who purchased small single-family homes downtown, we can also consider transfers from this larger group of policy "winners." Even with these additional households, one would need to tax approximately \$1,108 from each winner household to fully compensate loser households. This equals roughly one-third of the typical property tax bill in Houston,[48] suggesting policies of this type, while technically feasible, would be politically challenging to implement in practice.

7 Conclusion

This paper examines a central question in urban economics and public policy: who benefits when neighborhoods improve? Focusing on a large housing reform undertaken in Houston in the late 1990s, I show that the resulting development and neighborhood change had disparate effects on incumbent children depending on whether their family owned or rented their home. The reform led some incumbent renter households to move to cheaper neighborhoods farther away, sacrificing improved neighborhood quality for lower cost of living. Homeowner households, who did not face this trade-off, remained in redeveloped

[48] Houston homeowners pay property tax rates of \$0.51919 per \$100 of assessed home value to the city of Houston, \$0.86830 per \$100 to the Houston Independent School District (HISD), and \$0.38529 per \$100 to Harris County (Harris County Tax Office, 2026). However, assessed values lie below home values due to various property tax exemptions: HISD allows homeowners to exempt \$140,000 of their home's value, and Harris County allows homeowners to exempt 20 percent of the home's value (the city of Houston offers no homestead exemption). For a home valued at \$277,800, the current median in Houston, a household would pay approximately \$3,495 per year in property taxes: \$1,442 to the city of Houston, \$856 to Harris County, and \$1,197 to HISD. This amounts to an effective rate of about 1.26 percent of the home's value.

areas at higher rates, and their children received more education and had improved early-career labor market outcomes, though they were more likely to be charged with crimes. Children of renters were instead harmed on average, with lower employment and earnings through their early 30s.

In this setting, where almost all households pay market rates for housing, I find that the benefits from neighborhood revitalization accrue to more advantaged incumbents who are more insulated from cost of living changes. This extends prior work on gentrification's effects on children by studying new outcomes, identifying distributional impacts that may be masked by average treatment effects, and providing evidence on key mechanisms underlying these effects (Baum-Snow, Hartley and Lee, 2023; Brummet and Reed, 2019; Chetty et al., 2026; Dragan, Ellen and Glied, 2019).

Even given the costs to renter children, Houston's minimum lot size reform improved the well-being of incumbents overall, given the large benefits for homeowner households. Because housing wealth increased and homeowner children had better outcomes, these households were made considerably better off due to the reform, more than enough to offset the negative effects on renters under reasonable preferences for redistribution. I also estimate small increases in the economic self-sufficiency of prime-age working adults, likely due to increased economic agglomeration. In addition, the reform benefited the more than 20,000 households who moved into new small single-family homes built downtown.

These findings have implications for policy design. I estimate that losses for renter households are large enough that financing Pareto-improving transfers via property taxes would be practically challenging. Instead, potential ways to reduce displacement of low-income renters in gentrifying neighborhoods include expanding access to housing vouchers or other rental assistance or incentivizing greater construction of mixed-income housing. Future work can provide more evidence on the effects of these policies for individuals and communities, helping equalize access to revitalized neighborhoods independent of circumstance.

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Table 1. Characteristics of New Small Single-Family Occupants

	Small SF Occupants	**Incumbent Households**
Any Children	0.086	0.304
White	0.628	0.320
Black	0.072	0.204
Hispanic	0.185	0.432
2000 Nbhd. Prop. Adults BA+	0.521	0.326
2000 Nbhd. Poverty Rate	0.078	0.192
Harris County Pre-Move	0.707	
Houston Pre-Move	0.332	
Downtown Houston Pre-Move	0.207	
Age Move In	37.9	
Homeowner Pre-Move	0.447	
Homeowner Post-Move	0.692	
N Individuals	33,500	456,640
N Households	20,000	175,928

Note: First column consists of households who moved into each new small single-family home built in downtown Houston after 1998, where small homes are those on lots smaller than 5,000 square feet. Second column consists of all households in downtown Houston in 2000 using public Decennial Census estimates. Estimates and sample sizes in first column have been rounded according to Census Bureau DRB rules. Count variables were subject to a small amount of noise injection to comply with Census Bureau disclosure requirements for small geographic areas. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0445.

Table 2. Effects of Downtown Houston's Minimum Lot Size Reform

Outcome	SDID Main	SDID w/ Rich Nbhds	DID Unweighted	Triple Difference
N Small SF Homes	7.05***	6.62***	8.77***	7.49***
	(1.09)	(1.03)	(2.35)	(2.69)
Housing Prices				
Median Rent	0.074***	0.064***	0.053**	0.045*
	(0.020)	(0.019)	(0.022)	(0.025)
Median Home Value	0.151***	0.155***	0.130***	0.149***
	(0.030)	(0.029)	(0.041)	(0.041)
Home Price Index (FHFA)	27.98***	27.73***	24.723***	23.633***
	(4.75)	(4.27)	(4.268)	(4.795)
Neighborhood Demographics				
Median HH Income	0.105***	0.112***	0.093**	0.158***
	(0.026)	(0.027)	(0.039)	(0.039)
Prop. Adults BA+	0.027***	0.025***	0.016	0.059***
	(0.010)	(0.009)	(0.016)	(0.017)
Poverty Rate	-0.041***	-0.037***	-0.036***	-0.048***
	(0.010)	(0.009)	(0.011)	(0.012)
Prop. White	0.005	-0.001	0.008	0.054***
	(0.009)	(0.009)	(0.016)	(0.017)
Neighborhood Amenities				
Restaurant Index (std.)	0.082***	0.087***	0.117***	0.018
	(0.017)	(0.016)	(0.010)	(0.011)
Pollution (PM2.5)	0.581***	0.581***	1.300***	0.363***
	(0.078)	(0.079)	(0.105)	(0.121)
School Characteristics				
Log School Spending Per Student	0.038	0.026		
	(0.034)	(0.032)		
Log Median Teacher Salary	0.004	0.004		
	(0.014)	(0.014)		
Median Teacher Tenure (years)	0.050	-0.002		
	(0.479)	(0.483)		
Prop. Students White	0.011	0.012		
	(0.010)	(0.011)		

Note: * p<0.1, ** p<0.05, *** p<0.01. Average treatment effects of Houston's 1998 housing reform on the downtown's housing supply, housing prices, demographics, and amenities. SDID estimates are from synthetic difference-in-differences models (Arkhangelsky et al., 2021), where standard errors are estimated by running 999 placebo regressions with untreated units only to estimate the model's underlying variance structure. The main sample excludes all neighborhoods with median household incomes above $150,000 in 2000; the second column includes all neighborhoods. The third column restricts the control set to neighborhoods selected to match Houston's pre-reform trajectory along three outcomes: the number of small single-family homes, the median rent, and the share of adults with a college degree. Following Sun, Ben-Michael and Feller (2025), I standardize these outcomes and estimate synthetic difference-in-differences models (Arkhangelsky et al., 2021). I then select neighborhoods in the top quartile of the average weight across these three measures. The last column estimates triple-differences models that include outer ring neighborhoods in both Houston and untreated cities, where the coefficients in the table are the triple-interaction term. Dollar values are in 2024 amounts. The FHFA's Home Price Index (HPI) is normalized to 100 in 1998. Standard errors in the last two columns are estimated using a clustered bootstrap at the neighborhood level with 999 replications. School variables only include elementary schools in Texas.

Table 3. US Census Sample Statistics, Children in Sun Belt Downtowns

	Children in Renter HHs		**Children in Owner HHs**	
	Houston	Other Cities	Houston	Other Cities
Child Characteristics (2000)				
Child Age	6.31	6.44	7.24	7.33
Child Female	0.489	0.490	0.490	0.492
Child White	0.059	0.083	0.182	0.228
Child Black	0.267	0.201	0.143	0.120
Child Hispanic	0.644	0.627	0.639	0.589
Any Adult Has BA	0.056	0.097	0.128	0.177
Poor	0.432	0.464	0.220	0.190
Own HH Income (2024$)	43,740	56,120	79,830	83,780
Household Characteristics (2000)				
N Residents in HH	5.307	5.351	5.660	5.701
N Children in HH	3.055	3.088	2.920	2.961
HH Head Female	0.473	0.528	0.290	0.322
Neighborhood Characteristics (2000)				
Prop. Adults BA+	0.159	0.129	0.191	0.171
Poverty Rate	0.264	0.288	0.213	0.216
Median Rent (2024$)	841.9	936.8	917.7	978.5
Median HH Income (2024$)	50,490	45,970	60,570	56,070
Unemployment Rate	0.197	0.124	0.096	0.098
Marriage Rate	0.392	0.358	0.447	0.396
N Children	31,500	289,000	23,500	163,000

Note: Sample consists of children living in downtown Houston or another large Sun Belt city's downtown in 2000. HHs = households, BA = bachelor's degree. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. The sample for the last three variables in the top panel (Any Adult Has BA, Poor, Own HH Income) includes only households who responded to the long-form census, a one in six random sample; these estimates use sampling weights. To preserve individual Census respondent privacy, Own HH Income is a "pseudo-median" defined as the average income among households very near the median. Count variables in columns 1 and 3 (downtown Houston) were subject to a small amount of noise injection to comply with Census Bureau disclosure requirements for small geographic areas. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092 and CBDRB-FY26-0347.

Table 4. Effects of Housing Reform on Neighborhood Exposure During Childhood

	Counterfactual Exposure				**Actual Exposure**			
	Prop. BA+	Poverty Rate	Log Median HH Income	Log Median Rent	Prop. BA+	Poverty Rate	Log Median HH Income	Log Median Rent
Panel A: All Children								
Children of owners (N = 1,757,000)								
Estimate	0.040***	-0.017**	0.164***	0.011	0.029***	-0.008*	0.096***	0.001
	(0.006)	(0.007)	(0.018)	(0.016)	(0.004)	(0.004)	(0.012)	(0.011)
Control Mean	0.138	0.245	10.780	6.876	0.158	0.216	10.900	6.933
Children of renters (N = 1,524,000)								
Estimate	0.051***	-0.050***	0.166***	0.038**	0.013***	-0.006*	0.021**	-0.016**
	(0.007)	(0.007)	(0.021)	(0.015)	(0.004)	(0.003)	(0.010)	(0.007)
Control Mean	0.142	0.300	10.620	6.787	0.149	0.234	10.840	6.931
Panel B: Younger Children								
Children of owners (N = 878,000)								
Estimate	0.054***	-0.024***	0.204***	0.022	0.033***	-0.010**	0.098***	-0.001
	(0.007)	(0.008)	(0.023)	(0.019)	(0.005)	(0.005)	(0.013)	(0.011)
Control Mean	0.148	0.253	10.770	6.910	0.174	0.212	10.930	6.988
Children of renters (N = 893,000)								
Estimate	0.062***	-0.061***	0.197***	0.052***	0.013***	-0.007**	0.023**	-0.018**
	(0.008)	(0.009)	(0.025)	(0.017)	(0.004)	(0.003)	(0.010)	(0.007)
Control Mean	0.151	0.304	10.620	6.813	0.157	0.229	10.870	6.976
Panel C: Older Children								
Children of owners (N = 878,000)								
Estimate	0.023***	-0.010*	0.118***	-0.001	0.024***	-0.007	0.097***	0.008
	(0.004)	(0.006)	(0.015)	(0.014)	(0.003)	(0.005)	(0.012)	(0.010)
Control Mean	0.129	0.238	10.800	6.844	0.143	0.219	10.870	6.881
Children of renters (N = 631,000)								
Estimate	0.033***	-0.033***	0.120***	0.017	0.012***	-0.006*	0.022**	-0.011
	(0.005)	(0.006)	(0.017)	(0.012)	(0.003)	(0.003)	(0.011)	(0.007)
Control Mean	0.126	0.293	10.630	6.745	0.136	0.242	10.800	6.859

Note: * p<0.1, ** p<.05, *** p<0.01. Younger = ages 0–7 in 2000, older = ages 8–14 in 2000. Dependent variable is the average neighborhood characteristic during childhood (all years post-2000 and up to age 17). Actual exposure uses the child's census tract of residence in each year, counterfactual exposure fixes their 2000 census tract. Dollar values are inflated to 2024 levels; BA = bachelor's degree. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions control for the lagged value of the outcome in 2000 and adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Sample sizes are identical across outcomes and exposure definitions within each row block. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092.

Table 5. Effect of Housing Reform on Educational and Labor Market Outcomes

	All	Owners	Renters
		All Children	
HC Index	0.023	0.039*	-0.018
	(0.017)	(0.024)	(0.022)
N	214,000	130,000	84,000
Control Mean	-0.222	-0.131	-0.287
Control SD	0.592	0.601	0.577
ESS Index	-0.007	0.024	-0.041
	(0.019)	(0.028)	(0.030)
N	214,000	130,000	84,000
Control Mean	-0.171	-0.106	-0.217
Control SD	0.628	0.626	0.625
		Younger Children	
HC Index	0.017	0.097**	-0.057
	(0.030)	(0.047)	(0.041)
N	66,500	37,000	29,500
Control Mean	-0.210	-0.122	-0.260
Control SD	0.563	0.566	0.556
ESS Index	0.002	0.142***	-0.112**
	(0.035)	(0.049)	(0.048)
N	66,500	37,000	29,500
Control Mean	-0.178	-0.121	-0.211
Control SD	0.588	0.576	0.592
		Older Children	
HC Index	0.020	0.029	0.001
	(0.021)	(0.025)	(0.030)
N	147,000	93,000	54,500
Control Mean	-0.229	-0.135	-0.303
Control SD	0.606	0.616	0.589
ESS Index	-0.012	-0.028	-0.002
	(0.024)	(0.036)	(0.034)
N	147,000	93,000	54,500
Control Mean	-0.167	-0.099	-0.221
Control SD	0.648	0.647	0.643

Note: * p<0.1, ** p<.05, *** p<0.01. Younger = 0-7 in 2000, Older = 8-14 in 2000. Human Capital (HC) Index averages the following variables, standardized nationally: years of schooling, high school / any college / bachelor's degree / post-bachelor's degree / management occupation. Economic Self-Sufficiency (ESS) Index inputs are: weeks worked last year, hours worked per week, wages, family income to poverty ratio, and the negative of public assistance income. Sample includes individuals who responded to the ACS between ages 24 and 33; I use ACS sampling weights. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092.

Table 6. Effect of Housing Reform on Criminal Justice Involvement

	Ever Charged:		Ever Convicted:		
	Felony	Misdem.	Felony	Misdem.	Incarcerated
Owners					
Estimate	0.009***	0.027***	0.009***	0.021***	0.004**
	(0.003)	(0.007)	(0.003)	(0.005)	(0.002)
N	1,757,000	1,757,000	1,757,000	1,757,000	1,757,000
Control Mean	0.096	0.180	0.072	0.128	0.030
Renters					
Estimate	-0.002	-0.005	0.003	0.003	0.001
	(0.003)	(0.008)	(0.003)	(0.005)	(0.002)
N	1,524,000	1,524,000	1,524,000	1,524,000	1,524,000
Control Mean	0.129	0.228	0.098	0.161	0.043

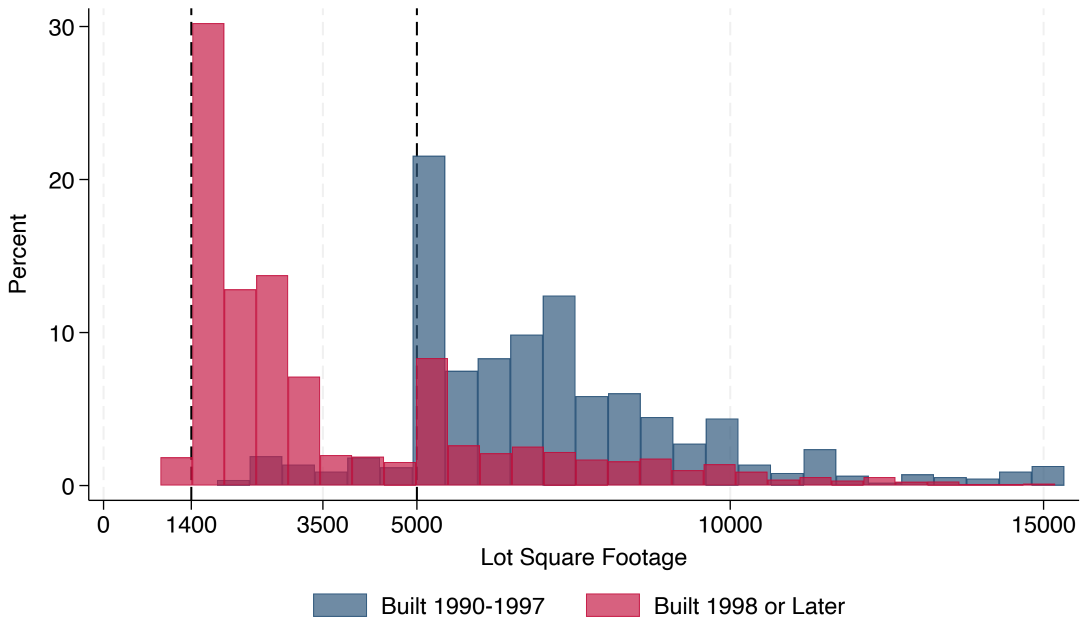
Note: * p<0.1, ** p<.05, *** p<0.01. Dependent variable is measured through age 24. Misdem. = misdemeanor. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092.

Table 7. Estimated Change in Household Well-Being

Group	Approach	N HHs	$CV^* = 0$		$CV^* = -\Delta R^*$	
			Per HH	Agg.	Per HH	Agg.
Children Ages 0–7						
Owner HHs	Disc. earnings	14,500	\$2,145	\$31.1M	\$2,145	\$31.1M
Renter HHs	Disc. earnings	19,000	−\$1,647	−\$31.3M	−\$1,647	−\$31.3M
Owner Parents						
Never-Movers	Comp. variation	10,440	\$4,643	\$48.5M	\$3,113	\$32.5M
Always-Movers	Comp. variation	4,060	\$3,113	\$12.6M	\$3,113	\$12.6M
Renter Parents						
Never-Movers	Comp. variation	9,880	\$205	\$2.0M	−\$643	−\$6.4M
Compliers	Comp. variation	380	−\$1,250	−\$0.5M	−\$1,505	−\$0.6M
Always-Movers	Comp. variation	8,740	\$0	\$0M	\$0	\$0M
Total: Owner HHs		14,500	\$6,360	\$92.2M	\$5,259	\$76.2M
Total: Renter HHs		19,000	−\$1,565	−\$29.7M	−\$2,011	−\$38.2M
Net Total			\$1,865	\$62.5M	\$1,135	\$38.0M

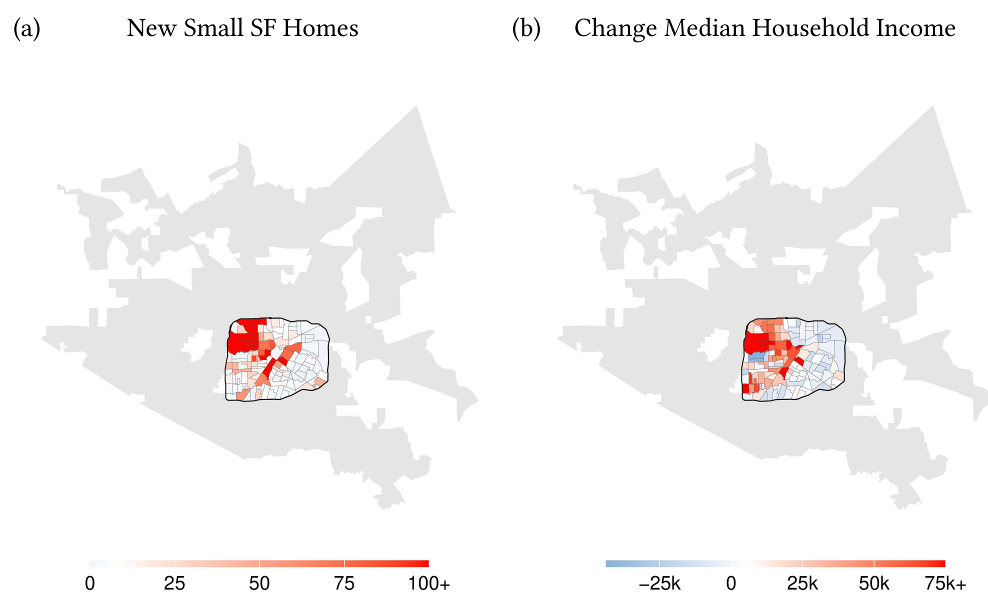
Note: Per-HH figures are annual (2024 dollars). $CV^* = 0$ assumes the marginal renter never-mover is welfare-neutral; $CV^* = -\Delta R^*$ sets the marginal household's compensating variation to the negative of the change in rents for the marginal household, the lowest value consistent with the model. Child rows reflect 0.8 children ages 0-7 per household. See Appendix F for details.

Figure 1. Distribution of Single-Family Lot Sizes in Downtown Houston



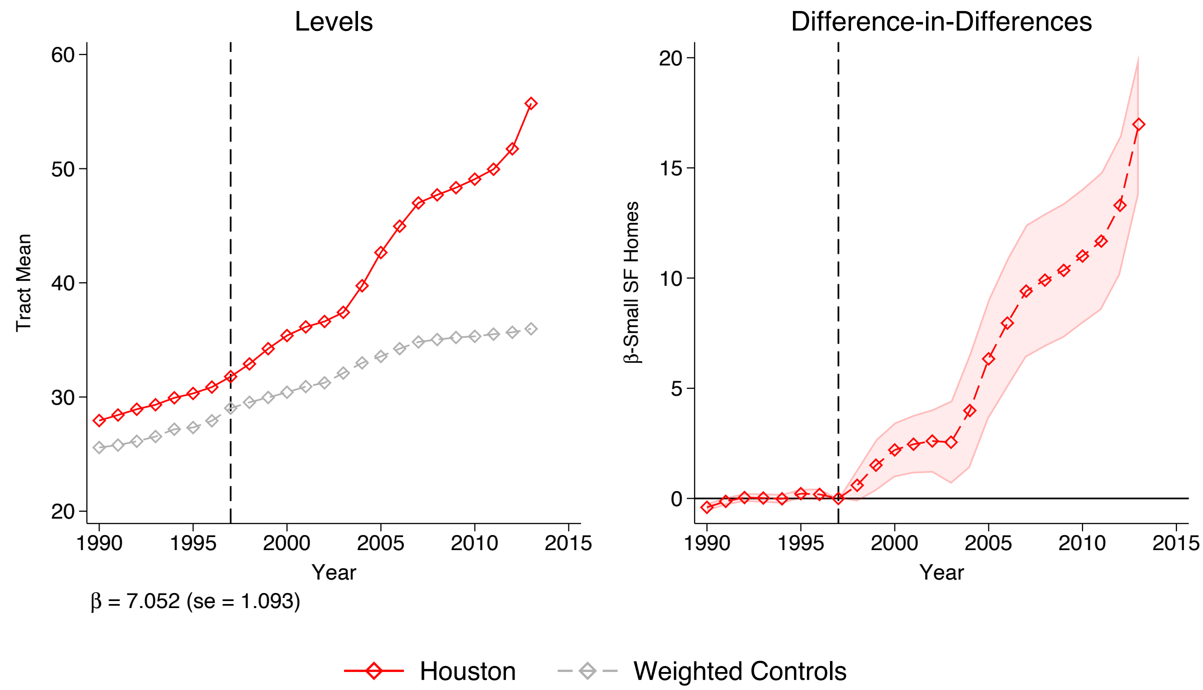
Note: Lot size distribution for single-family homes built in downtown Houston before and after the city's 1998 minimum lot size reform. Lot sizes and year of construction come from Corelogic property tax records. Figure omits lots above 15,000 square feet (2.9 percent of homes) for visual purposes.

Figure 2. Changes in Downtown Houston Neighborhoods Post-Housing Reform



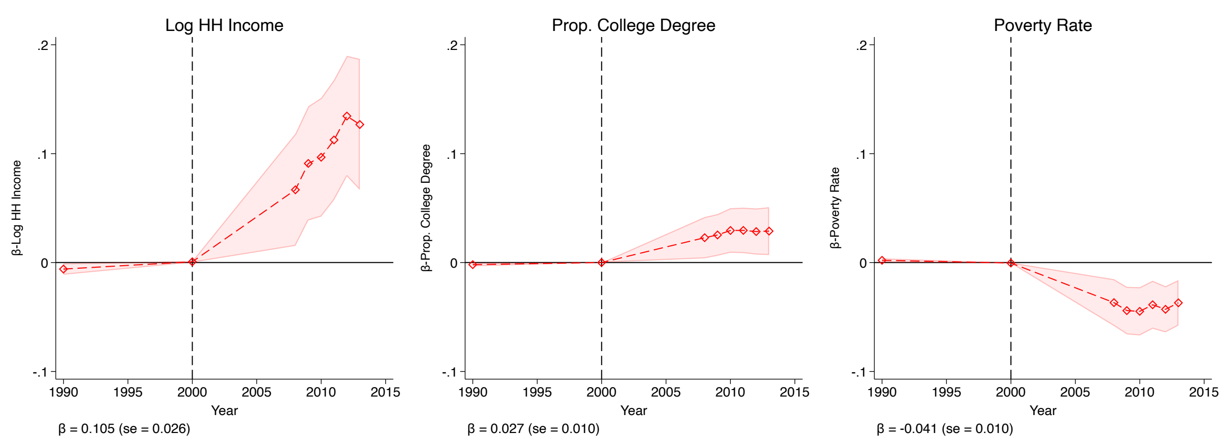
Note: Maps show downtown Houston census tracts (2010 boundaries), defined as those inside the I-610 highway loop. Grey tracts are those in the rest of Houston. Left panel shows tract-level counts of the change in detached single-family housing units smaller than 5,000 square feet from 1998-2013. Right panel shows the change in median household income from 2000 to 2013 (in 2024 dollars). Housing data are from CoreLogic deeds and tax records, income data are from the Decennial Census and American Community Survey.

Figure 3. Effect of Minimum Lot Size Reduction on Small Single-Family Housing Supply



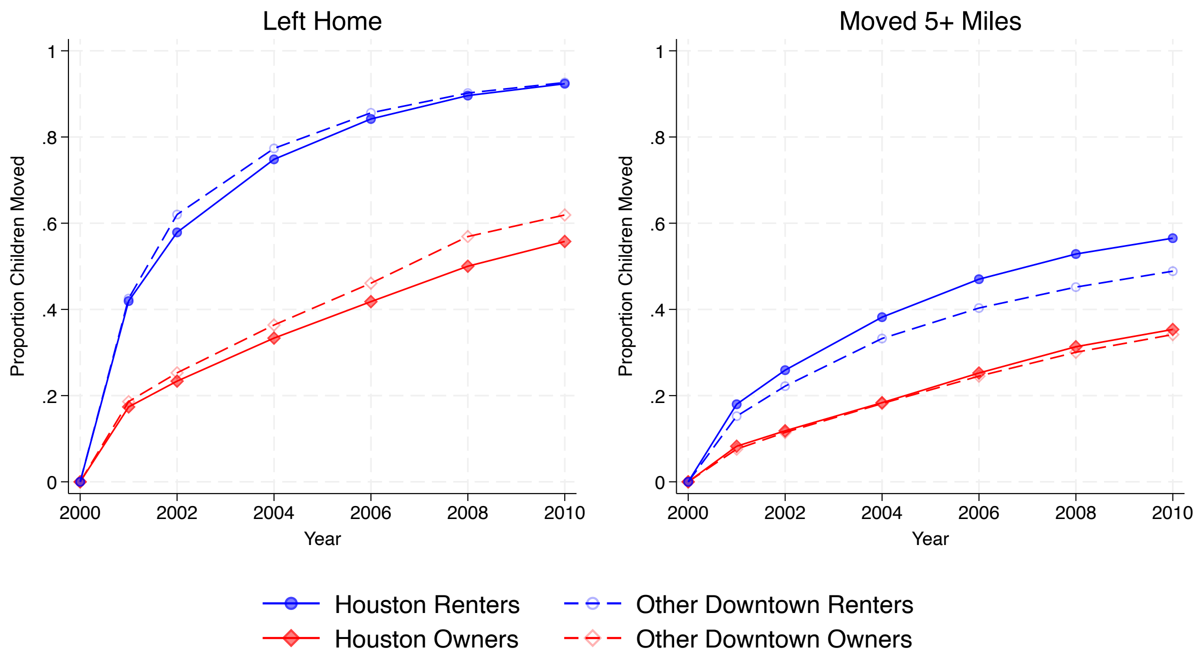
Note: Synthetic difference-in-differences estimates of the effect of Houston's 1998 minimum lot size reform on supply of small single-family (SF) homes in downtown Houston; small homes are those on lots smaller than 5,000 square feet. Standard errors are estimated by running 999 placebo regressions with untreated units only to estimate the model's underlying variance structure. Control line is a weighted average of downtown neighborhoods in the other largest cities in Sun Belt states, with weights selected at both the unit and time period level. See Arkhangelsky et al. (2021) for methodology details.

Figure 4. Effect of Minimum Lot Size Reduction on Neighborhood Demographics



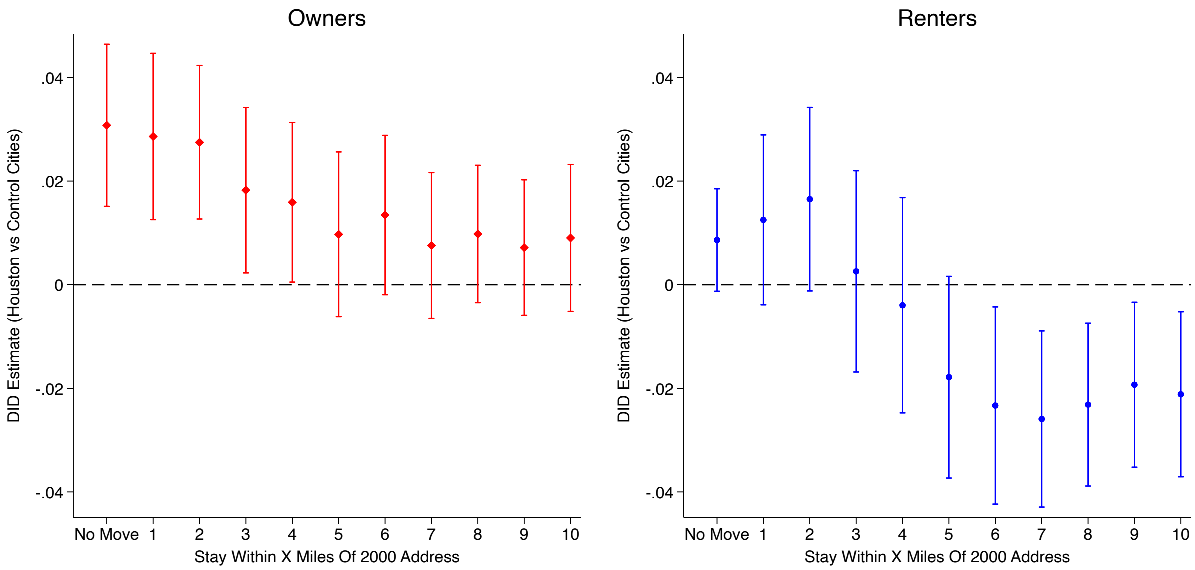
Note: Synthetic difference-in-differences estimates of the effect of Houston's 1998 minimum lot size reform on neighborhood demographics. Standard errors are estimated by running 999 placebo regressions with untreated units only to estimate the model's underlying variance structure. Control line is a weighted average of downtown neighborhoods in the other largest cities in Sun Belt states, with weights selected at both the unit and time period level. See Arkhangelsky et al. (2021) for methodology details.

Figure 5. Post-Reform Differences in Migration Rates by Housing Tenure



Note: Sample consists of children in households that owned or rented their homes living in downtown Houston or another large Sun Belt city in 2000. Control statistics are propensity-score weighted. Estimates have been rounded according to Census Bureau DRB rules; for Houston, estimates were subject to a small amount of noise injection to comply with Census Bureau disclosure requirements for small geographic areas. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092.

Figure 6. Effect of Housing Reform on Household Migration



Note: * p<0.1, ** p<.05, *** p<0.01. Dependent variable is an indicator for living within each mile distance of the child's original 2000 address, measured in the year the child was 17. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-0164.

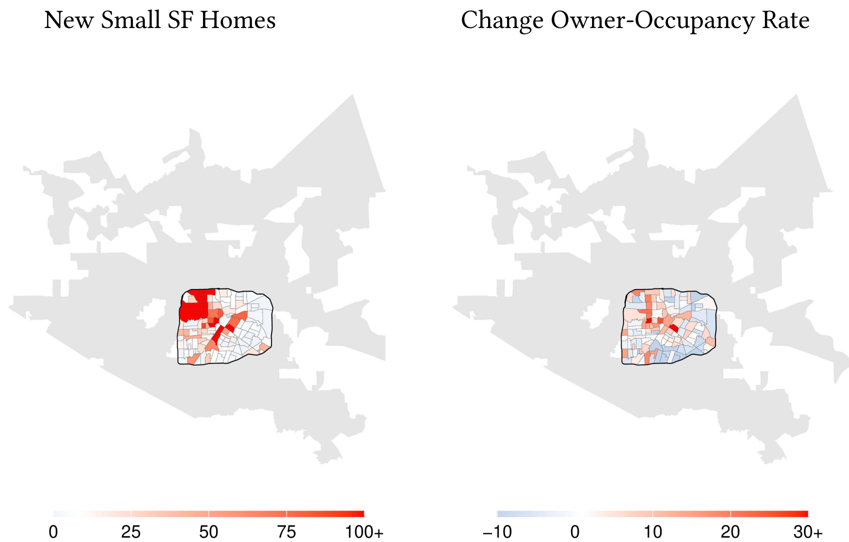
A Houston Descriptives

Table A.1. List of Control Cities

City
Albuquerque, NM
Atlanta, GA
Augusta, GA
Austin, TX
Bakersfield, CA
Colorado Springs, CO
Corpus Christi, TX
Dallas, TX
Denver, CO
El Paso, TX
Fresno, CA
Fort Worth, TX
Greensboro, NC
Jacksonville, FL
Las Vegas, NV
Los Angeles, CA
Miami, FL
Oklahoma City, OK
Phoenix, AZ
Raleigh, NC
Riverside, CA
Sacramento, CA
San Antonio, TX
San Diego, CA
San Francisco, CA
San Jose, CA
Stockton, CA
Tampa, FL
Tucson, AZ
Tulsa, OK

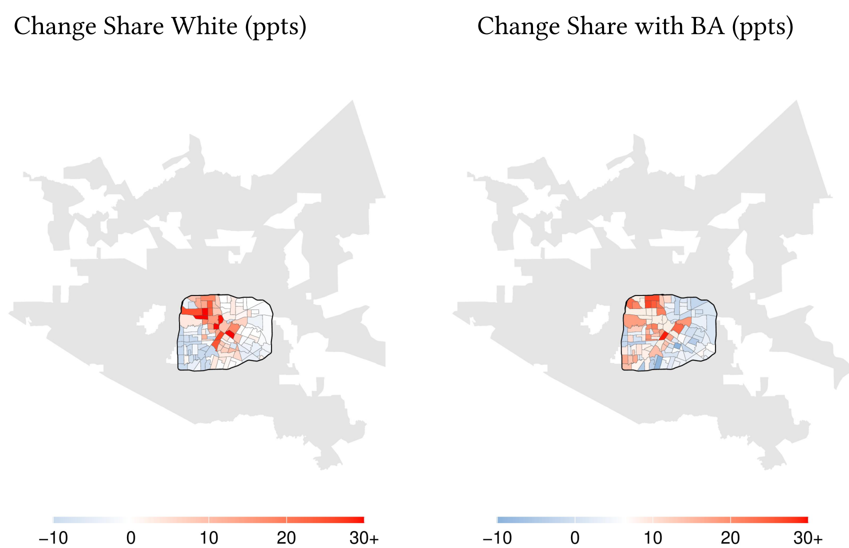
Note: Control cities located in Sun Belt states (Arizona, California, Colorado, Florida, Georgia, Nevada, New Mexico, North Carolina, Oklahoma, Texas) among the 100 largest in the US in 2000.

Figure A.1. Post-Reform Single-Family Housing Growth and Owner-Occupancy



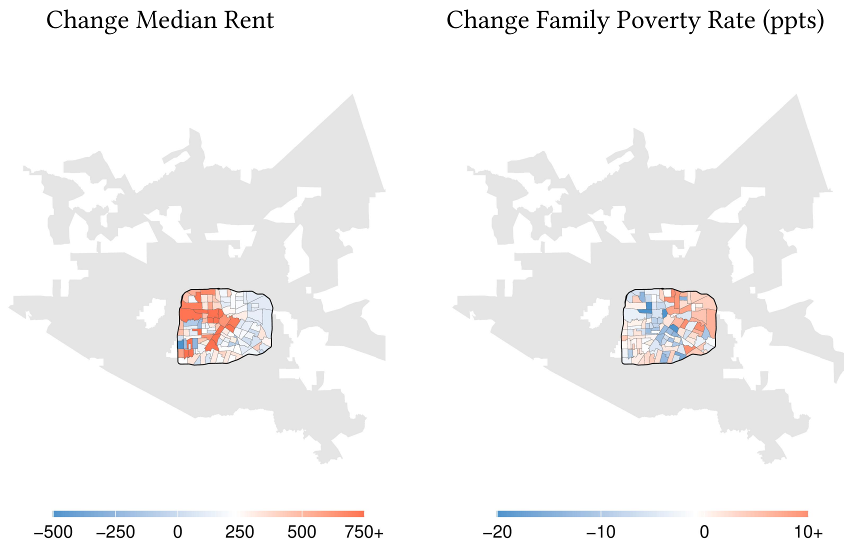
Note: Maps show downtown Houston census tracts (2010 boundaries), defined as those inside the I-610 highway loop. Grey tracts are those in the rest of Houston. Left panel shows tract-level counts of the change in detached single-family housing units smaller than 5,000 square feet from 1998-2013. Right panel shows the change in the proportion of housing units that are owner- rather than renter-occupied from 2000-2013. Data are from CoreLogic deeds and tax records and the U.S. Census and American Community Survey. In right panel, blue = below-median, red = above-median.

Figure A.2. Post-Reform Change in Neighborhood Demographics



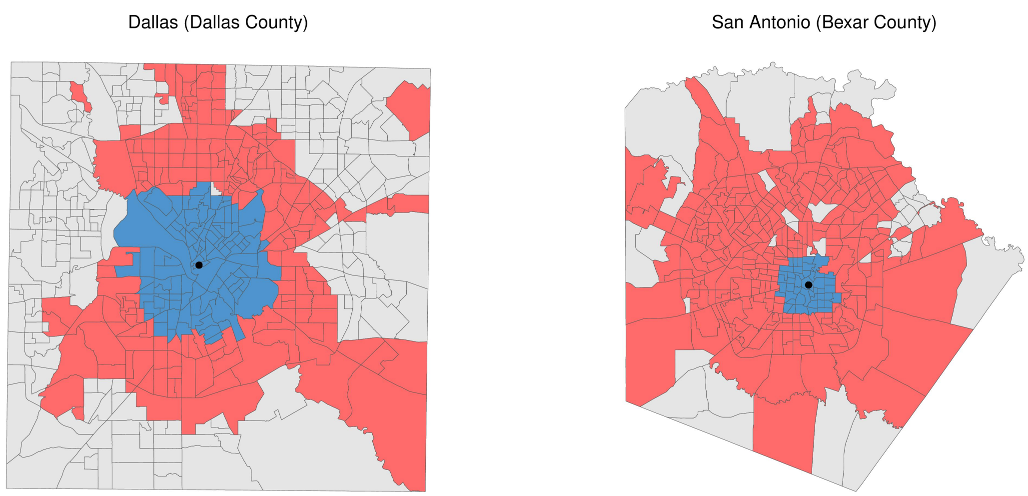
Note: Maps show downtown Houston census tracts (2010 boundaries), defined as those inside the I-610 highway loop. Grey tracts are those in the rest of Houston. BA = bachelor's degree. Data are from the U.S. Census and American Community Survey. Blue = below-median, red = above-median.

Figure A.3. Post-Reform Change in Neighborhood Rents and Poverty Rates



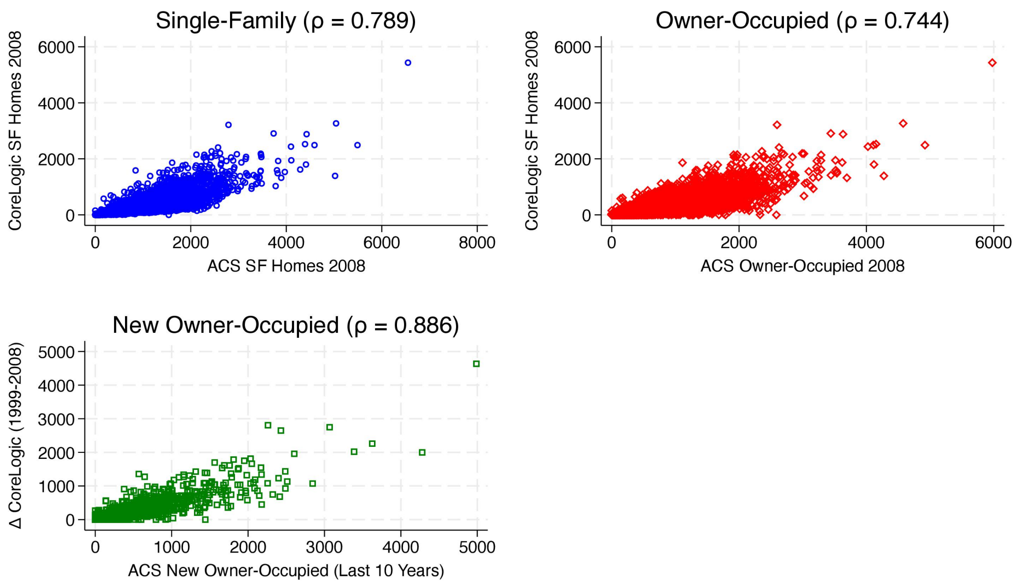
Note: Maps show downtown Houston census tracts (2010 boundaries), defined as those inside the I-610 highway loop. Grey tracts are those in the rest of Houston. Rents are in 2024 dollars. Data are from the U.S. Census and American Community Survey. Blue = below-median, red = above-median.

Figure A.4. Examples of Control City Urban Cores and Outer Rings



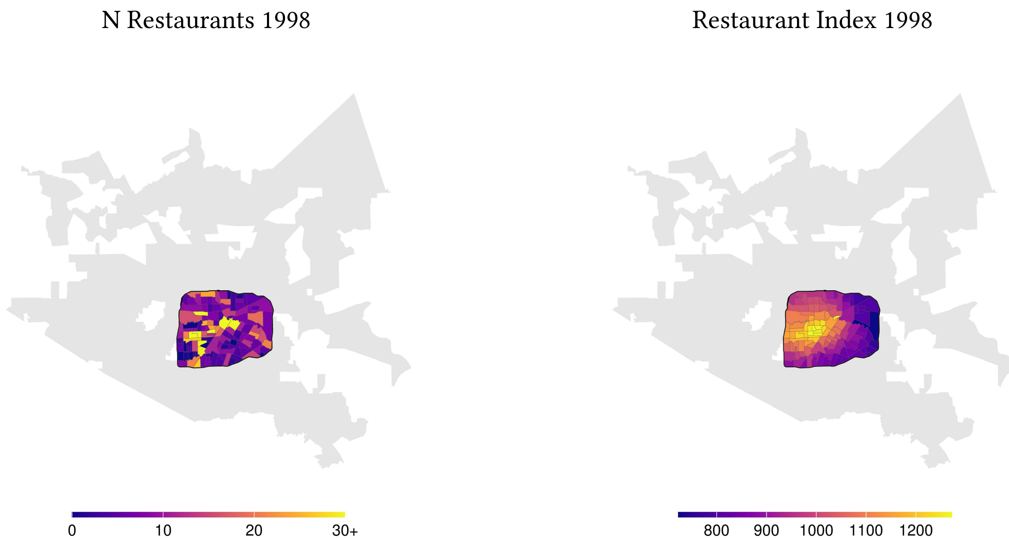
Note: Blue census tracts are the city's downtown neighborhoods, red tracts are the city's outer ring, grey tracts are other tracts in the county not part of the city. Downtown is defined as the set of tracts nearest to the city's central business district (the black dot) such that the downtown contains 17.5 percent of the city's total 2000 population, equivalent to the proportion of Houston residents living in neighborhoods treated by the 1998 minimum lot size reform. Population data come from the U.S. Census.

Figure A.5. Neighborhood Correlation Between CoreLogic and Census Housing Counts



Note: Top-left figure shows the tract-level correlation between single-family homes identified in CoreLogic and the number of single-family detached homes in the corresponding American Community Survey 5-year estimates. Top-right figure shows the neighborhood-level correlation with the number of owner-occupied housing units. Bottom-left figure shows the correlation between the change in housing units in CoreLogic from 1999-2008 and the number of owner-occupied homes built in the last 10 years from the 2006-2010 ACS 5-year estimates. Figure includes all neighborhoods in large Sun Belt cities.

Figure A.6. Comparing Restaurant Counts vs Smoothed Index

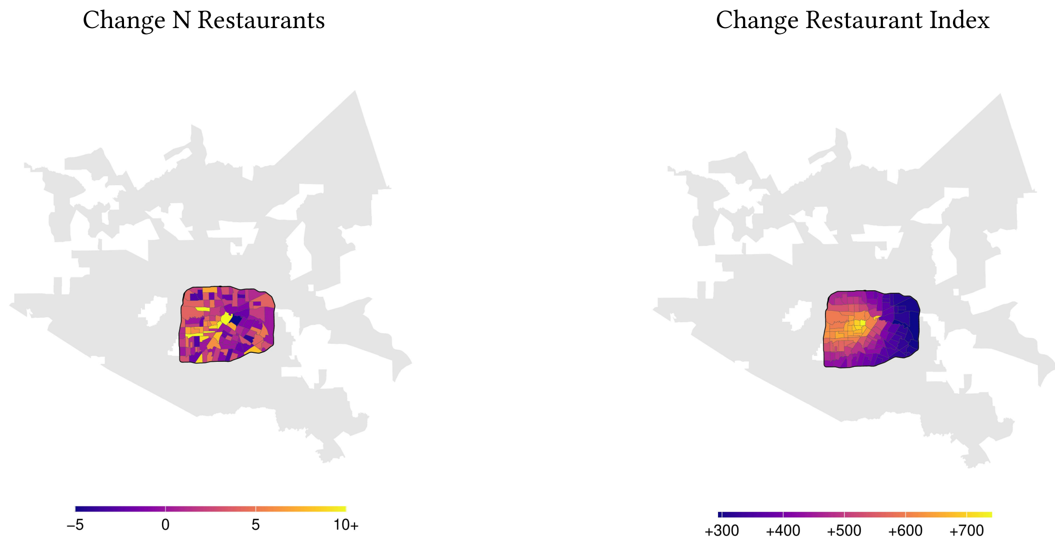


Note: Maps show downtown Houston census tracts (2010 boundaries), defined as those inside the I-610 highway loop. Grey tracts are those in the rest of Houston. Number of restaurants in downtown Houston census tracts in 1998, using establishment counts from Data Axle. Restaurants are identified from 6-digit NAICS codes following Cook (2023). Smoothed restaurant index assumes a CES utility function, where households value increased concentration of restaurants within each neighborhood and increased access to other nearby restaurant markets:

$$A_r = \sum_{n=1}^{\mathcal{N}} \frac{a_n}{d_{r,n}}, \quad \text{where} \quad a_n = \left(\sum_{f \in \mathcal{F}} \left(\sum \#_n(f = 1) \right)^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}$$

Here, each category of goods f represents a type of restaurant (cafe, bar, full-service, limited-service) and the accessibility of restaurants in neighborhood r is determined by its distance from neighborhood n, $d_{r,n}$. I use $\sigma = 6.5$, estimated in Couture et al. (2024).

Figure A.7. Post-Reform Change in Houston Restaurant Access



Note: Maps show downtown Houston census tracts (2010 boundaries), defined as those inside the I-610 highway loop. Grey tracts are those in the rest of Houston. Left panel shows the change in the number of restaurants in each census tract from 1998-2013, right panel shows the change in each tract's smoothed restaurant access, described above.

B SDID Robustness and Extensions

B.1 Alternative Specifications

I explore multiple potential threats to identification. First, I show that estimates are not driven by my choice to exclude pre-gentrified neighborhoods from the sample (Table 2). Second, one challenge with interpreting SDID estimates for multiple outcomes is that the control group changes with each regression since I weight neighborhoods to match outcome-specific pre-trends. Given this, I also identify a single set of untreated census tracts that approximately matches Houston's pre-reform trends along multiple outcomes. Using this control group, I then estimate *unweighted* difference-in-differences regressions using these neighborhoods as the control group.

I identify these control neighborhoods following the advice in Sun, Ben-Michael and Feller (2025). I standardize three main outcomes capturing changes in neighborhood housing supply, housing demand, and neighborhood demographics—the number of small single-family homes, median rents, and the share of residents with a college degree—and estimate SDID models to generate neighborhood-specific weights for each of these outcomes. I average these weights across the three outcomes within control units and select the set of neighborhoods in the top quartile of this composite weight.[1]

The unweighted difference-in-differences estimates are overall very similar to the SDID models (Table 2). As I show in Figure B.4, the parallel trends assumption is generally satisfied in this control set, including for outcomes not used in the selection process such as the median household income, poverty rate, and home prices (however, it is not satisfied for the White population share). Lending credence to the algorithm's selection process, neighborhoods in other Texas cities represent 30 percent of neighborhoods in this selected sample, compared to only 22 percent of the full sample, and two of the top three cities with the greatest representation are Dallas and San Antonio (the third is Phoenix; each represents approximately 9.2 percent of all control neighborhoods).

I also restrict my sample of control cities in two ways to account for different potential confounders (Table B.1). I compare downtown Houston to just the largest 10 cities in the Sun Belt,[2] since housing demand responses may differ for larger versus smaller cities (Monte, Porcher and Rossi-Hansberg, 2023). In addition, because I include observations from many states, I may be overlooking important state-level factors driving Houston's downtown evolution during the 2000s, such as Texas' relative resilience during the Great Recession (Thompson, 2010). I re-estimate the SDID models restricting the comparison group to just the other largest cities in Texas.[3] Estimates are very similar.

[1] Note that I estimate these models using neighborhoods in both downtown and outer ring areas, which enables me to use the same set of control neighborhoods in both these estimates and the triple-difference estimates described in Section B.2. Estimates are similar if I select neighborhoods based on models using only downtown neighborhoods.

[2] Los Angeles, Phoenix, Dallas, San Antonio, San Diego, San Jose, San Francisco, Jacksonville, Austin, Fort Worth.

[3] Dallas, San Antonio, Austin, Fort Worth, El Paso, and Corpus Christi.

The MLS reduction was also not the only policy aimed at revitalizing Houston's downtown during the late 1990s and early 2000s. The two most significant of these were the introduction of Tax Increment Reinvestment Zones (TIRZs), a tax instrument designed to encourage private development (Greenbaum and Landers, 2014); and the construction of the city's first light rail transit (LRT) line in 2004.[4] These reforms may have been partially responsible for the downtown's revitalization rather than the MLS reduction. I am unable to fully disentangle their effects, since there could be equilibrium interactions between public, commercial, and housing development. However, these other initiatives were relatively concentrated geographically, as more than half of all TIRZ land area was located in ten percent of downtown census tracts and the original LRT line only covered five percent of downtown tracts. Houston's TIRZ program was also famously mismanaged, as the city lost track of tens of millions of dollars in revenues from the program, resulting in more than $10 million in interest debt (Elliott and Morris, 2017; Morris and Elliott, 2017).

Table B.2 shows that small single-family housing growth is associated with presence of a TIRZ, but this exposure explains relatively little of all post-reform construction, with a regression R-squared of 0.035. Light rail transit stops are uncorrelated with small single-family housing growth. I also institute a stricter test by dropping these neighborhoods entirely and re-estimating the SDID models.[5] Estimates, shown in Table B.1, are almost entirely unchanged. One exception is that when omitting TIRZs, the increase in the share of adults with a college degree attenuates and, while remaining positive, loses statistical significance.

B.2 Accounting for Changes in Houston's Outer Ring

While the housing reform only affected a small region of Houston, it was instituted during a period of large demographic changes affecting the city as a whole. Houston's total population grew by 26 percent between 2000 and 2013, but its poor and Hispanic population grew by more than 40 percent while its White population shrank. If these broader migration trends differed from those in other large Sun Belt cities, then this would bias estimates of the reform's effects on neighborhood demographics. Because these trends are generally opposite to those associated with revitalization, the bias would likely point towards zero.

I test whether this is the case by comparing coefficients from standard DID models with those from triple-differences models, where I include outer ring neighborhoods (which were not subject to the housing reform) as an additional difference. If the coefficient from a standard DID model is similar to the triple-

[4] In more recent years, the city also greatly expanded its downtown parks network through the Bayou Greenways initiative and construction of other parks. The city's green space initiatives are unlikely to drive these effects, since they did not begin in earnest until the late 2000s and early 2010s, after the focal period in this study. Moreover, the actual amount of new green space was marginal; as Figure B.5 shows, the total share of downtown Houston's land area comprised of green space was effectively unchanged between 2001 and 2015, while the share of land containing buildings increased.

[5] I omit neighborhoods with at least 41 percent of their land area covered by a TIRZ, corresponding to the 90th percentile of the distribution. As Figure B.6 shows, these tracts comprise half of all TIRZ land area. I do not omit all tracts with *any* TIRZs because more than half of all tracts have some TIRZ presence, usually with small overlap area.

interaction coefficient in these models, then this suggests that city-wide trends are not biasing estimates of the reform's effects (Olden and Møen, 2022). Because synthetic triple-differences estimators are not yet standard, I do this comparison using the unweighted DID approach described in Section B.1.[6] The last column in Table 2 presents the triple-interaction ATT estimates, which are very similar to the DID models. Only two estimates differ: both the share of residents with a college degree and the White population share increase more in the triple-difference. However, as Figure B.4 shows, I am unable to match pre-trends for these outcomes, unlike the other outcomes.

B.3 Testing for Amenity Changes

Many revitalized neighborhoods experience endogenous increases in amenity quality (Couture et al., 2024). This occurs for a number of reasons: high-income or college-educated households spend more on consumption amenities such as restaurants but also frequently demand more local public services or police enforcement (Feigenbaum and Hall, 2015; Verrilli, 2025), all of which can improve local quality of life. Houston's particular reform could have increased school funding, if higher home values increased property tax revenues. Alternatively, if downtown Houston became more congested due to greater density, this could cause greater air pollution.

I use CoreLogic parcel-level deeds data to estimate DID regressions of the change in home sale prices in downtown Houston relative to other cities, before versus after the reform, to measure overall changes in the value of downtown Houston's amenities (Baum-Snow and Duranton, 2025; Bayer, Ferreira and McMillan, 2007). Importantly, I restrict to homes that were built prior to 1998. These models, by also including year fixed effects, therefore isolate the change in housing demand from differences in quality between units built before and after 1998. I estimate models that include property fixed effects, restricting to homes that were sold multiple times, both before and after 1998. Because homes that were sold multiple times are a selected sample, I also expand the sample to all homes sold in Houston and other cities and control for observable home characteristics including the number of bedrooms and bathrooms.

Estimates, shown in Table B.3, show that pre-existing homes in downtown Houston experienced larger increases in home prices than homes in other downtowns. The ATT equals 28.5 log points when I control for property observable characteristics and 17.0 log points when I include property fixed effects. Overall, this suggests a marked increase in demand for downtown Houston housing, indicative of improving amenities.

However, I detect only small changes in observable amenities as a result of the reform. I estimate effects on restaurant access, air pollution, and school characteristics. These measures are relatively coarse due in part to data limitations; for example, while I can measure changes in restaurant quantity, quality changes may be unobserved. I observe a small increase in the density of restaurants in downtown Houston (Table 2),

[6] Note that for downtown neighborhoods, these models use the same control units as the unweighted DID models.

equivalent to a 0.08 standard deviation increase.[7] PM2.5 concentration was higher in downtown Houston by 0.6 additional micrograms per cubic meter. This difference is small, falling within the EPA's acceptable margin of measurement error for air quality (U.S. Environmental Protection Agency, 2006).

I do not find effects on school quality inputs, with small and statistically insignificant changes in school funding per pupil, teacher experience, and teacher salaries. The lack of effects is perhaps surprising, since 90 percent of Houston school funding comes from local property tax revenues (Houston Independent School District, 2021),[8] and home values increased post-reform. However, because Houston allocates school funding based on student enrollments, any increased revenue from downtown homes would be redistributed across all schools. Given the downtown's small size relative to the city, the overall effect was muted. I also do not observe an increase in the White enrollment share, perhaps indicative of the fact that less than ten percent of the households who moved into newly constructed homes had children (Table 1).

B.4 Testing for Equilibrium Spillovers

I also study whether the downtown housing reform generated changes to Houston's housing market *outside* of the downtown. Because housing costs are set in equilibrium, it is possible that the price effects observed in the downtown spilled over to other nearby areas (Brueckner, 1998). While this is a common concern when studying geographically targeted policies (Jardim et al., 2022), spillovers are *a priori* less likely in this setting for two reasons. First, downtown Houston is a large area in an absolute sense, but is small compared to the city as a whole, containing 17 percent of its total population. The city's untreated outer ring areas were, on their own, large enough to comprise the 5th largest city in the U.S. in 2000. Second, Interstate 610, a multi-lane highway, encircles the downtown; the "Inner Loop", as it is known, presents a meaningful physical and noise disamenity to surrounding areas (see Figure B.8). Because it separates the treated area from the rest of the city, this likely impedes spatial spillovers (Caro, 1974).[9]

I test for spillovers by comparing changes in neighborhood characteristics on either side of the I-610 highway loop. In these boundary discontinuity regressions (Jardim et al., 2022; Lee and Lemieux, 2010), if we observe no difference in characteristics on either side of the highway, this implies that rent and amenity effects crossed the border. I present these estimates in Figure B.9.[10] Standard errors increase due

[7] The triple-differences estimate is smaller, but this is in part because of how I construct the access measure, smoothing restaurant counts across nearby neighborhoods. This means that increased downtown restaurant density mechanically increases restaurant access in outer ring areas.

[8] Public schools in Texas are mostly funded by a flat property tax set at the municipal level; in Houston, for example, this district tax was 1.38 percent of the property's value in 1998 (City of Houston Controller's Office, 1999). This tax rate changed only slightly during the post-reform study period (Texas Education Agency, 2024), though it has now fallen to approximately 0.86 percent (Harris County Tax Office, 2026).

[9] This view is supported by conversations with Houston residents and policymakers, who discuss the "Inner Loop" as a distinct entity from the rest of Houston.

[10] I include all neighborhoods regardless of their 2000 income due to sample size restrictions. However, estimates are similar (albeit less precise) if I restrict to neighborhoods with 2000 median incomes below $150,000.

to the smaller neighborhood sample size, but estimates are generally very similar to the main estimates. Neighborhoods just inside the I-610 loop saw increased construction of single-family homes; increases in rents and home values; increases in median household incomes and the share of residents with a college degree; and decreases in poverty rates.

The border discontinuity tests rule out meaningful local spillovers, but it is possible that spillovers could operate at a larger geographic level. For example, I cannot directly test whether families moving to Houston from other places chose to locate in outer ring neighborhoods because the downtown became more expensive. However, recall that the triple-differences estimates in Table 2 that incorporate outer ring neighborhoods were very similar to the difference-in-differences estimates.

In addition, given the relative size of the two areas, it is unlikely that migration spillovers drove the overall demographic changes in the outer ring. Non-treated parts of Houston contain 4.5 times as many households as treated parts, meaning migration responses between the two areas would need to be very large to be reflected in population changes in these outer ring areas. For example, between 2000 and 2013, the number of poor families living in the outer ring grew by 41,400. Meanwhile, in 2000 the *total* number of poor families living in the treated area was only 15,800. I estimate a marginal effect on out-migration from the downtown of less than 1,000 incumbent households, of whom approximately half were poor, meaning the reform contributed only approximately one percent of these newly poor households. Similarly, the reform attracted approximately 2,500 new households from these outer ring neighborhoods to the downtown to buy new small single-family homes, a direct migration effect of less than half a percent.

B.5 Tables and Figures

Table B.1. Robustness of Synthetic Difference-in-Difference Estimates

	Main Model	Largest 10 Control Cities	Only TX Control Cities	Drop High TIRZ Tracts	Drop LRT Tracts
N Small SF Homes	7.05***	6.44***	5.61***	6.45***	6.44***
	(1.09)	(1.19)	(0.80)	(1.12)	(1.12)
Housing Prices					
Median Rent	0.074***	0.065***	0.065***	0.059***	0.063***
	(0.020)	(0.019)	(0.025)	(0.020)	(0.021)
Median Home Value	0.151***	0.115***	0.120***	0.128***	0.142***
	(0.030)	(0.030)	(0.039)	(0.030)	(0.030)
Home Price Index (FHFA)	27.98***	23.23***	13.96*	29.60***	27.03***
	(4.75)	(5.02)	(8.20)	(5.10)	(4.90)
Neighborhood Demographics					
Median HH Income	0.105***	0.113***	0.127***	0.076***	0.086***
	(0.026)	(0.026)	(0.029)	(0.029)	(0.028)
Prop. Adults BA+	0.027***	0.026***	0.036**	0.015	0.020**
	(0.010)	(0.010)	(0.015)	(0.010)	(0.010)
Poverty Rate	-0.041***	-0.041***	-0.034***	-0.035***	-0.039***
	(0.010)	(0.009)	(0.012)	(0.010)	(0.010)
Prop. White	0.005	0.012	0.005	-0.001	0.005
	(0.009)	(0.009)	(0.013)	(0.010)	(0.009)
Neighborhood Amenities					
Restaurant Index (std)	0.082***	-0.001	0.296***	0.055***	0.058***
	(0.017)	(0.018)	(0.019)	(0.013)	(0.013)
Pollution (PM2.5)	0.581***	0.773***	0.319***	0.581***	0.614***
	(0.078)	(0.051)	(0.040)	(0.084)	(0.082)
Log School Spending Per Student	0.038		–	0.044	0.040
	(0.034)		–	(0.033)	(0.034)
Log Median Teacher Salary	0.004		–	0.001	-0.001
	(0.014)		–	(0.014)	(0.014)
Median Teacher Tenure (years)	0.050		–	-0.014	0.054
	(0.479)		–	(0.472)	(0.487)
Prop. Students White	0.011		–	0.012	0.013
	(0.010)		–	(0.010)	(0.010)

Note: * p<0.1, ** p<0.05, *** p<0.01. Average treatment effects of Houston's 1998 housing reform on the downtown's housing supply, housing prices, demographics, and amenities. "Main SDID" estimates are from synthetic difference-in-differences models (Arkhangelsky et al., 2021), where standard errors are estimated by running 999 placebo regressions with untreated units only to estimate the model's underlying variance structure. The main sample excludes all neighborhoods with median household incomes above $150,000 in 2000. The second column restricts the donor pool to just the 10 largest cities other than Houston. The third column restricts the donor pool to just cities in Texas. The fourth column drops Houston neighborhoods with high concentration of tax increment reinvestment zones (TIRZs); the cutoff is the 90th percentile, equivalent to at least 41% of land area covered by a TIRZ. The fifth column drops Houston neighborhoods with any light rail transit (LRT) stops built in 2004. Dollar values are in 2024 amounts. The FHFA's Home Price Index (HPI) is normalized to 100 in 1998. School variables only include elementary schools in Texas.

Table B.2. Downtown Houston Policy Exposure and Housing Supply Changes, 1998-2013

	Change N Small SF Homes	Change N Small SF Homes	Change N Small SF Homes
Prop. Tract in TIRZ	61.879**		66.475**
	(29.329)		(32.649)
N Light Rail Stops		1.001	-4.523
		(5.192)	(5.601)
Constant	26.276***	33.938***	26.376***
	(6.021)	(6.848)	(6.085)
Dependent Variable Mean	34.089	34.089	34.089
R-Squared	0.035	0.0001	0.038
N	113	113	113

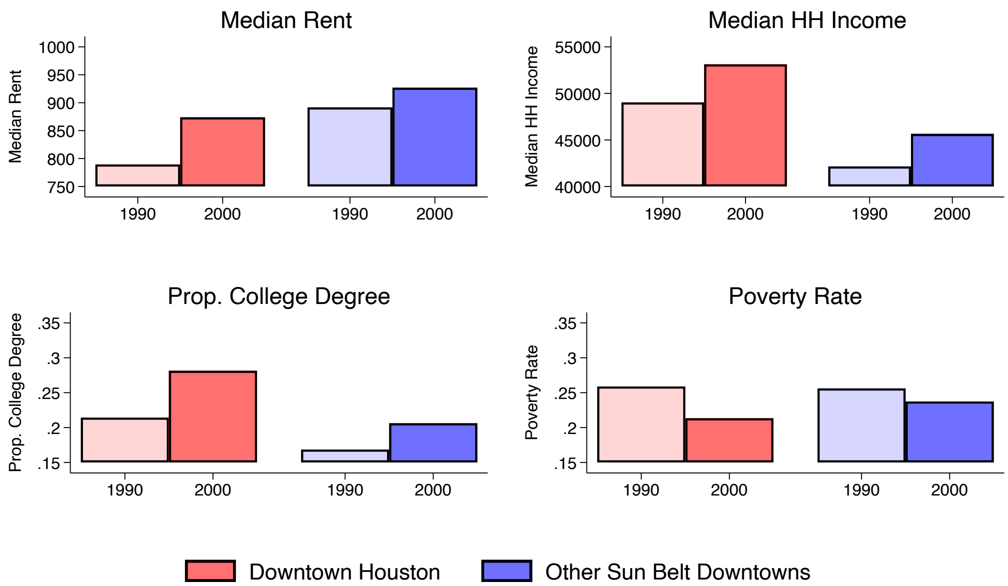
Note: * p<0.1, ** p<0.05, *** p<0.01 (robust standard errors). The sample consists of census tracts in downtown Houston (inside the I-610 highway loop), corresponding to the area affected by the city's 1998 minimum lot size reform. Change in small single-family homes is measured from 1998-2013; small homes are those on lots smaller than 5,000 square feet. SF = single-family. TIRZ = tax increment reinvestment zone, measured as the share of the tract in 1998. LRT = light rail transit stops (2004 line).

Table B.3. Price Effects of Houston Reform on Existing Homes

	Hedonic	Repeat-Sales
Houston × Post-Reform	0.285***	0.170***
	(0.040)	(0.046)
Log Property Sq. Ft.	-0.016	
	(0.029)	
Building Sq. Ft.	0.784***	
	(0.042)	
N Rooms	-0.057***	
	(0.012)	
N Bedrooms	0.021**	
	(0.009)	
N Bathrooms	0.220***	
	(0.021)	
Central Heat	0.712***	
	(0.057)	
Public Sewage Connection	-0.320***	
	(0.079)	
Garage	0.314***	
	(0.027)	
Basement	0.322***	
	(0.088)	
A/C	-0.216***	
	(0.068)	
Property FEs		X
Year FEs	X	X
Houston FE	X	
R-Squared	0.332	0.776
N	373,663	251,991

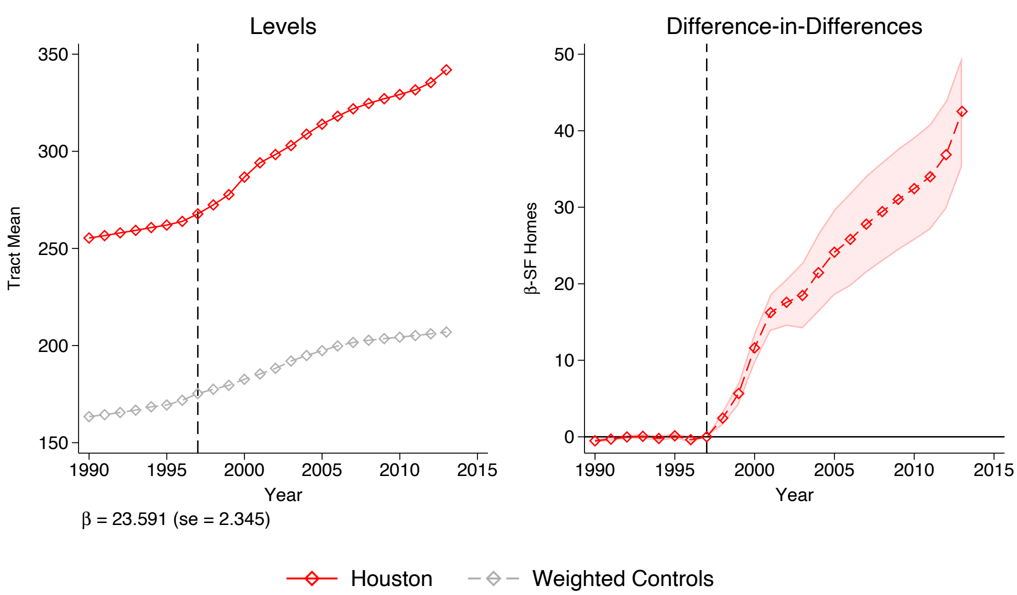
Note: * p<0.1, ** p<0.05, *** p<0.01 (standard errors clustered at tract level). Sample consists of single-family homes in downtowns of large Sun Belt cities that were built prior to 1998. First column contains all home sales in these cities from 1996-2013; second column only contains sales for properties with multiple owners during this period. Sales prices come from county deeds records from the commercial data provider CoreLogic.

Figure B.1. Differences in Downtown Houston Demographics in 1990 and 2000



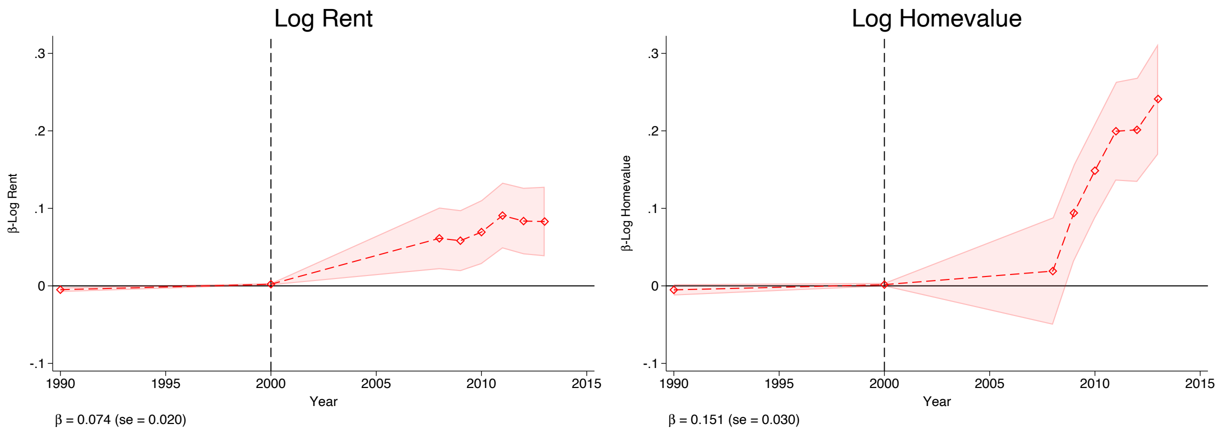
Note: Figure shows average neighborhood characteristics in downtown Houston and other large Sun Belt downtowns in 1990 and 2000. Characteristics come from the Decennial Census tract-level estimates.

Figure B.2. Effect of Minimum Lot Size Reduction on Overall Single-Family Housing Supply



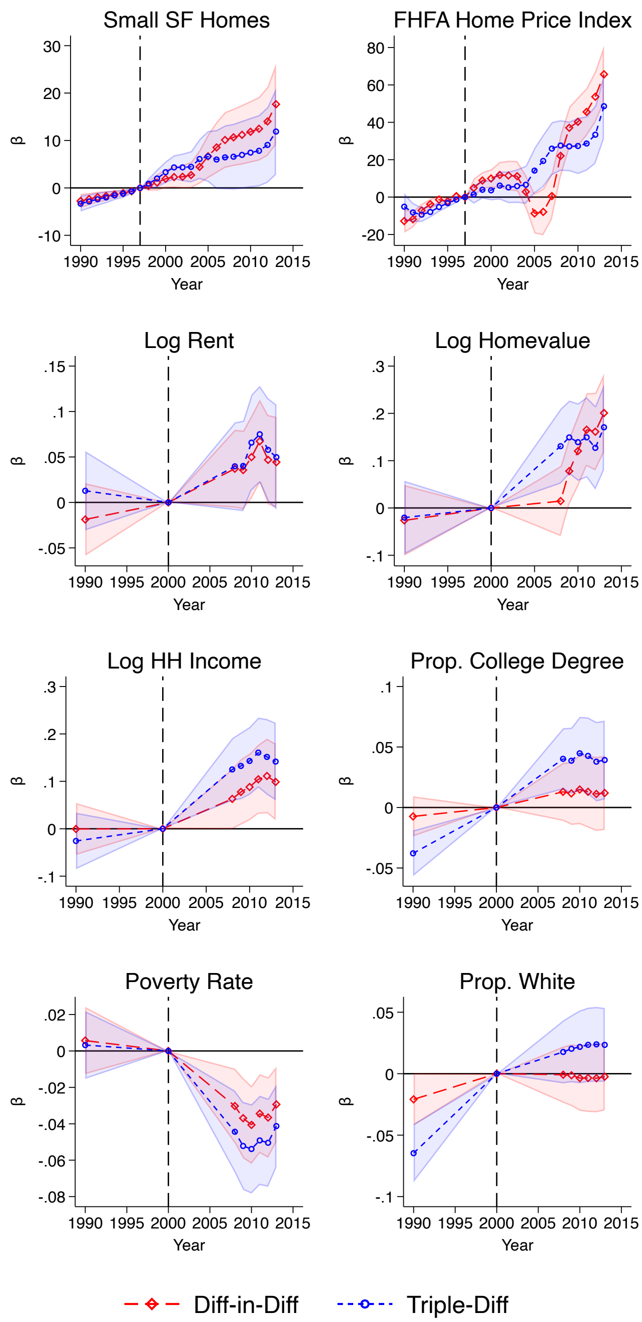
Note: Synthetic difference-in-differences estimates of the effect of Houston's 1998 minimum lot size reform on supply of single-family (SF) homes in downtown Houston. Standard errors are estimated by running 999 placebo regressions with untreated units only to estimate the model's underlying variance structure. Control line is a weighted average of downtown neighborhoods in the other largest cities in Sun Belt states, with weights selected at both the unit and time period level. See Arkhangelsky et al. (2021) for methodology details.

Figure B.3. Effect of Minimum Lot Size Reduction on Housing Costs



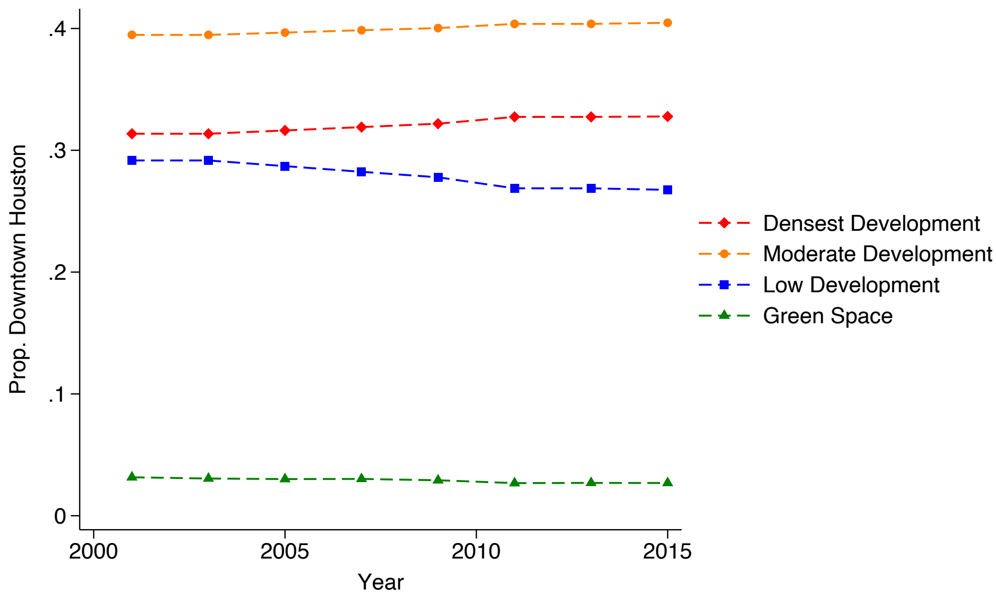
Note: Synthetic difference-in-differences estimates of the effect of Houston's 1998 minimum lot size reform on neighborhood housing costs. Median rents and home values are in 2024 dollars and come from the Decennial Census and ACS. The FHFA's Home Price Index (HPI) is normalized to 100 in 1998. Standard errors are estimated by running 999 placebo regressions with untreated units only to estimate the model's underlying variance structure. Control line is a weighted average of downtown neighborhoods in the other largest cities in Sun Belt states, with weights selected at both the unit and time period level. See Arkhangelsky et al. (2021) for methodology details.

Figure B.4. Effect of Minimum Lot Size Reduction, Unweighted Regressions



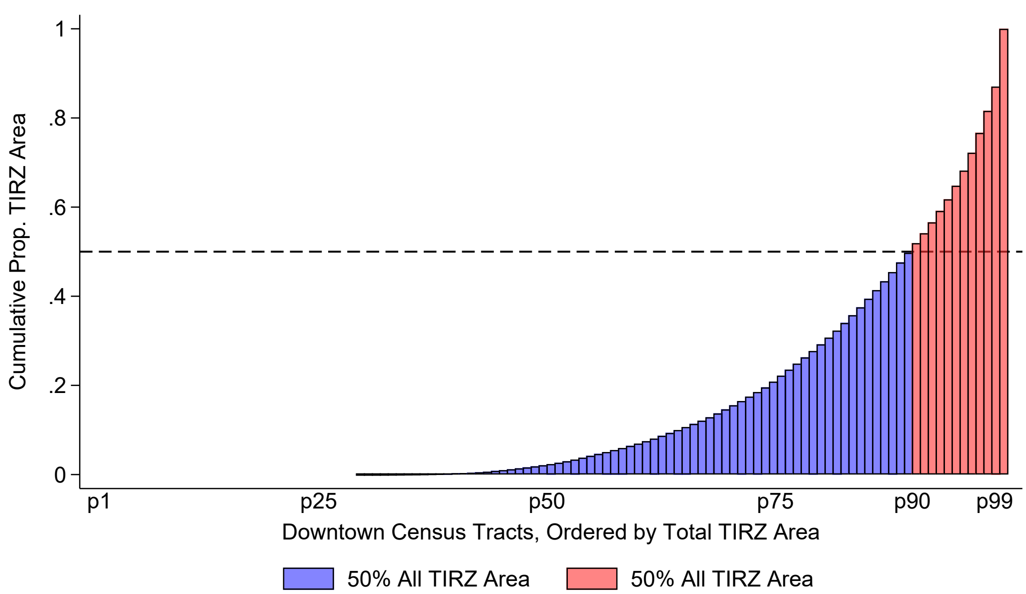
Note: Difference-in-differences and triple-differences estimates of the effect of Houston's 1998 minimum lot size reform on neighborhood characteristics. Estimates compare downtown Houston neighborhoods to a selected sample of neighborhoods in other Sun Belt downtowns. These control neighborhoods are selected to match Houston's pre-reform trajectory along three outcomes: the number of small single-family homes, the median rent, and the share of adults with a college degree. Following Sun, Ben-Michael and Feller (2025), I standardize these outcomes and estimate synthetic difference-in-differences models (Arkhangelsky et al., 2021). I then select neighborhoods in the top quartile of the average weight across these three measures. Dollar values are in 2024 amounts. The FHFA's Home Price Index (HPI) is normalized to 100 in 1998. Standard errors are estimated using a clustered bootstrap at the neighborhood level with 999 replications.

Figure B.5. Change in Downtown Houston Land Coverage Post-Reform



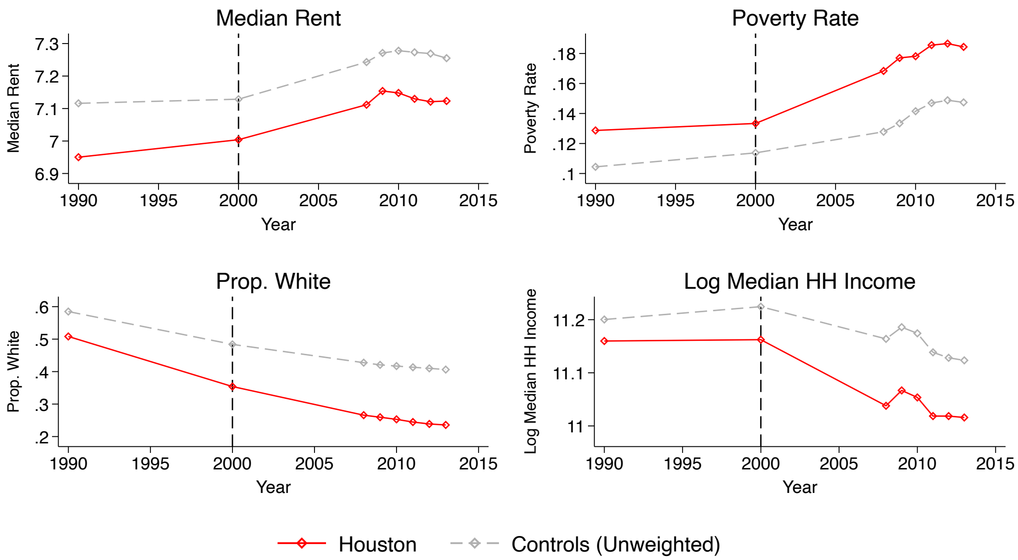
Note: Share of Houston's downtown land area fitting various development categories. Data come from the National Land Cover Database (NLCD), which uses satellite imagery to categorize the presence of various land surfaces. "Densest development" refers to land where at least 80 percent of the ground surface is covered by built structures. "Moderate development" refers to places with 50-79 percent building coverage. "Low development" refers to places with 0-49 percent coverage, and "green space" refers to places with grass, forest, or pasture coverage.

Figure B.6. Downtown Houston Cumulative Share of Tracts in Tax Increment Reinvestment Zone



Note: Bars showing the share of downtown Houston located in a tax increment reinvestment zone (TIRZ), with census tracts ranked by the share of land area covered by a TIRZ. The red bars show that 50 percent of TIRZ land area was located in just 10 percent of tracts.

Figure B.7. Demographic Trends in Outer Ring Houston and Control Neighborhoods

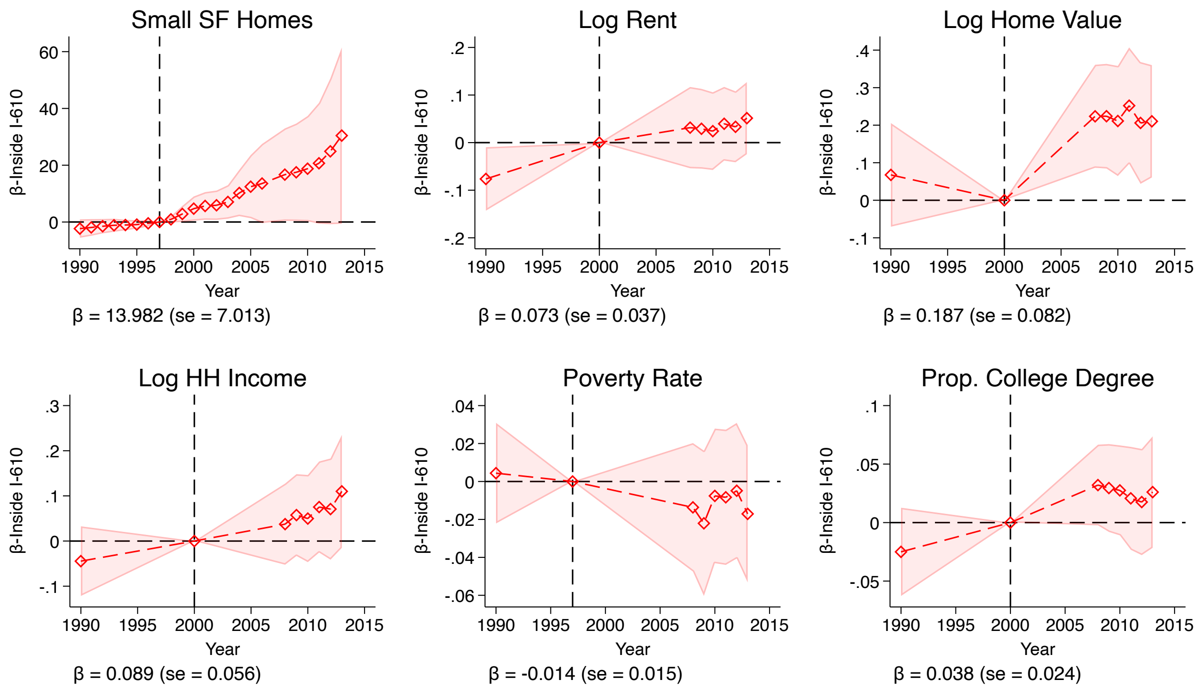


Note: Figure shows average neighborhood characteristics in outer ring (non-treated) Houston neighborhoods and those in other large Sun Belt cities. Characteristics come from the Decennial Census and ACS tract-level estimates.

Figure B.8. Houston's I-610 Highway Loop

Note: Photo credit: Mark Mulligan (staff photographer, Chron). Published July 20, 2021.

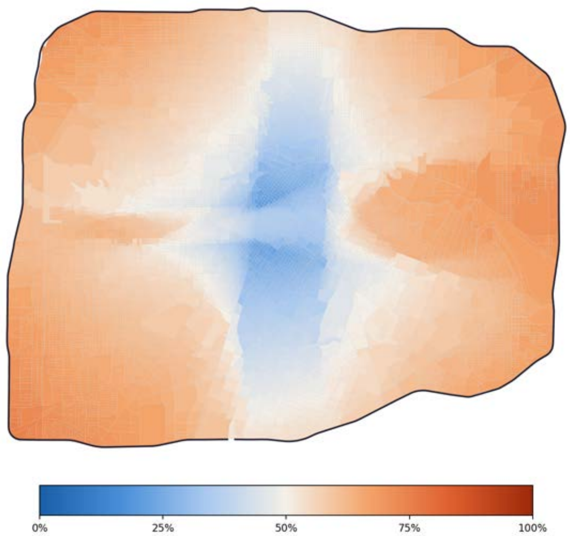
Figure B.9. Border Discontinuity Regressions at Houston's I-610 Highway Loop



Note: Event study plots comparing changes in neighborhood characteristics on either side of Houston's I-610 border, which separates the treated and untreated parts of the city. Standard errors are estimated using a clustered bootstrap at the neighborhood level with 999 replications.

C Effects of Neighborhood Revitalization on Children

Figure C.1. Probability of Leaving Inner Loop With a Five-Mile Move



Note: Figure plots census blocks in Houston, shaded by the probability that a move of at least five miles would result in the individual leaving Houston's Inner Loop. Probabilities are calculated by simulating 180 evenly spaced directions from each block's centroid, then computing the share of five-mile endpoints that fall outside the I-610 boundary. The population-weighted average across all Inner Loop 2000 census blocks is 55.3 percent.

Table C.1. Balance Tests for Pre-Reform Differences in Child Outcomes

	Human Capital	Teen Births		Any Criminal Charges				
	HS Grad	Any Birth	N Births	Violent	Property	Drug	DUI	Non-Drug
Owners								
Estimate	0.008	0.007	0.005	−0.004	0.003	0.019***	−0.001	0.011
	(0.019)	(0.007)	(0.010)	(0.003)	(0.004)	(0.004)	(0.001)	(0.008)
N	60,000	281,000	281,000	579,000	579,000	579,000	579,000	579,000
Control Mean	0.859	0.165	0.208	0.030	0.052	0.046	0.005	0.108
Renters								
Estimate	−0.025	−0.013	−0.006	0.001	0.004	0.017***	−0.001	0.005
	(0.026)	(0.009)	(0.013)	(0.003)	(0.006)	(0.005)	(0.001)	(0.008)
N	35,500	205,000	205,000	398,000	398,000	398,000	398,000	398,000
Control Mean	0.792	0.216	0.292	0.041	0.076	0.061	0.005	0.146

Note: * p<0.1, ** p<.05, *** p<0.01. Table shows a series of balance tests comparing the outcomes of children who were ages 15–19 in 2000 in downtown Houston to control children. All outcomes are measured through age 19, meaning they were measured no later than 2004. Sample for the first column includes individuals who responded to the ACS; columns 2–3 only contain female children. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-0164.

Table C.2. Effects of Housing Reform on Moving During Childhood

	N Moves During Childhood
Children of Owners	
Estimate	0.037
	(0.032)
N	1,757,000
Control Mean	1.965
Children of Renters	
Estimate	0.078*
	(0.044)
N	1,524,000
Control Mean	3.973

Note: * p<0.1, ** p<.05, *** p<0.01. Dependent variable is measured between the year 2000 and when the child turns 18. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P3114-R13261.

Table C.3. Effect of Housing Reform on School and Peer Characteristics

	Prop. White	Prop. Black	Prop. Hispanic	Log Spending Per Pupil	Tenure (p50)	Salary (p50)	Math VA	Reading VA
Predicted Owners								
Estimate	-0.022***	-0.013	0.022*	0.070	0.787***	0.016*	0.016***	0.019***
	(0.008)	(0.009)	(0.011)	(0.049)	(0.236)	(0.009)	(0.006)	(0.006)
N	66,763	66,763	66,763	66,763	66,756	66,756	66,763	66,763
Control Mean	0.094	0.141	0.748	8.379	6.429	10.688	0.016	-0.012
Predicted Renters								
Estimate	-0.002	0.022**	-0.027***	-0.110***	0.058	0.010***	0.017***	0.005
	(0.003)	(0.009)	(0.009)	(0.025)	(0.134)	(0.004)	(0.006)	(0.005)
N	129,947	129,947	129,947	129,947	132,114	132,114	129,902	129,902
Control Mean	0.048	0.331	0.604	8.499	6.569	10.705	0.027	0.034

Note: * p<0.1, ** p<.05, *** p<0.01. Dependent variable is the average value of the school characteristic during K-12. Sample includes children in Texas public schools who entered Kindergarten between the 1997-98 through 2000-01 school years, and attended one of the largest school districts in Texas in Kindergarten (Houston, Dallas, San Antonio, Austin, El Paso, Fort Worth, Corpus Christi). Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions control for the level of each covariate in the child's first year in school and adjust for differences in child covariates (school cohort, gender, race/ethnicity, free/reduced-price lunch status, immigrant status, limited English proficiency, special education), neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent), and school covariates (school spending per student and reading/math test score value-added). Value-added is estimated following the approach in Kane and Staiger (2008). Standard errors are clustered at the school level using a stratified block bootstrap (999 replications).

Table C.4. Effects on Components of Human Capital Index, Younger Children

	Yrs School	HS	Any College	BA+	Post-BA	Mgmt. Job
Children of Owners						
Estimate	0.54**	0.044	0.054	0.055	0.010	-0.017
	(0.22)	(0.029)	(0.033)	(0.040)	(0.015)	(0.026)
N	37,000	37,000	37,000	37,000	37,000	37,000
Control Mean	14.40	0.926	0.656	0.293	0.010	0.039
Children of Renters						
Estimate	-0.21	-0.021	-0.017	0.017	-0.010**	-0.027**
	(0.19)	(0.022)	(0.038)	(0.033)	(0.005)	(0.013)
N	29,500	29,500	29,500	29,500	29,500	29,500
Control Mean	13.89	0.890	0.552	0.201	0.005	0.026

Note: * p<0.1, ** p<.05, *** p<0.01. Sample includes individuals who responded to the ACS between ages 24 and 33 and were ages 0-7 in 2000; I use ACS sampling weights. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092.

Table C.5. Effects on Components of Economic Self Sufficiency Index, Younger Children

	Wks Work	Hrs Work	Earnings	Inc./Pov. Ratio	Pub Asst
Children of Owners					
Estimate	2.51	3.66***	-411	72.0***	-232.4**
	(1.84)	(1.14)	(2,482)	(23.3)	(94.2)
Control Mean	38.9	32.0	30,520	337.2	138.7
N	37,000	37,000	37,000	36,500	37,000
Children of Renters					
Estimate	-3.34*	-3.17**	-6,056***	-31.9*	-60.8
	(1.86)	(1.41)	(1,732)	(16.9)	(95.5)
Control Mean	37.7	30.5	27,330	284.9	218.3
N	29,500	29,500	29,500	28,000	29,500

Note: * p<0.1, ** p<.05, *** p<0.01. Sample includes individuals who responded to the ACS between ages 24 and 33 and were ages 0-7 in 2000; I use ACS sampling weights. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092.

Table C.6. Effect of Housing Reform on Criminal Justice Involvement by Age

	Ever Charged: Felony	Ever Charged: Misdem.	Ever Convicted: Felony	Ever Convicted: Misdem.	Incarcerated
			Younger Children		
Children of Owners					
Estimate	0.001	0.016**	0.002	0.012**	0.000
	(0.003)	(0.007)	(0.002)	(0.005)	(0.001)
N	878,000	878,000	878,000	878,000	878,000
Control Mean	0.063	0.136	0.043	0.085	0.013
Children of Renters					
Estimate	-0.007**	-0.016*	-0.003	-0.007	-0.002
	(0.003)	(0.008)	(0.003)	(0.005)	(0.002)
N	893,000	893,000	893,000	893,000	893,000
Control Mean	0.096	0.183	0.067	0.120	0.023
			Older Children		
Children of Owners					
Estimate	0.016***	0.035***	0.014***	0.026***	0.007**
	(0.005)	(0.009)	(0.005)	(0.007)	(0.003)
N	878,000	878,000	878,000	878,000	878,000
Control Mean	0.127	0.222	0.101	0.169	0.046
Children of Renters					
Estimate	0.007	0.016	0.012***	0.022***	0.004
	(0.005)	(0.010)	(0.004)	(0.008)	(0.003)
N	631,000	631,000	631,000	631,000	631,000
Control Mean	0.182	0.299	0.148	0.227	0.075

Note: * p<0.1, ** p<.05, *** p<0.01. Younger = 0-7 in 2000, Older = 8-14 in 2000. Dependent variable is measured through age 24. Misdem. = misdemeanor. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092.

Table C.7. Effect of Housing Reform on Criminal Justice Involvement by Charge Type

	Any Charge	Violent	Property	Drug	DUI
	Younger Children				
Children of Owners					
Estimate	0.011*	0.003	0.000	0.007*	-0.002
	(0.006)	(0.003)	(0.004)	(0.004)	(0.002)
N	878,000	878,000	878,000	878,000	878,000
Control Mean	0.158	0.045	0.063	0.062	0.019
Children of Renters					
Estimate	-0.013	-0.009**	-0.017***	-0.008*	-0.002
	(0.008)	(0.004)	(0.005)	(0.004)	(0.001)
N	893,000	893,000	893,000	893,000	893,000
Control Mean	0.211	0.073	0.100	0.087	0.018
	Older Children				
Children of Owners					
Estimate	0.032***	0.023***	0.019***	0.007	0.004*
	(0.007)	(0.004)	(0.005)	(0.004)	(0.002)
N	878,000	878,000	878,000	878,000	878,000
Control Mean	0.259	0.075	0.112	0.106	0.034
Children of Renters					
Estimate	0.012	0.010***	0.002	0.008	0.002
	(0.009)	(0.004)	(0.006)	(0.006)	(0.002)
N	631,000	631,000	631,000	631,000	631,000
Control Mean	0.346	0.121	0.173	0.148	0.029

Note: * p<0.1, ** p<.05, *** p<0.01. Younger = 0-7 in 2000, Older = 8-14 in 2000. Dependent variable is measured through age 24. Misdem. = misdemeanor. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092.

Table C.8. Effect of Housing Reform on Criminal Justice Involvement, By Gender

	Ever Charged:		Ever Convicted:		
	Felony	Misdem.	Felony	Misdem.	Incarcerated
			Children of Renters		
Male					
Estimate	-0.002	-0.005	0.006	0.005	0.001
	(0.005)	(0.009)	(0.004)	(0.007)	(0.003)
N	777,000	777,000	777,000	777,000	777,000
Control Mean	0.201	0.304	0.160	0.229	0.076
Female					
Estimate	-0.003	-0.006	-0.001	0.000	-0.001
	(0.003)	(0.008)	(0.002)	(0.004)	(0.001)
N	747,000	747,000	747,000	747,000	747,000
Control Mean	0.053	0.147	0.033	0.090	0.008
			Children of Owners		
Male					
Estimate	0.018***	0.032***	0.018***	0.035***	0.012***
	(0.005)	(0.009)	(0.004)	(0.007)	(0.003)
N	897,000	897,000	897,000	897,000	897,000
Control Mean	0.154	0.253	0.120	0.190	0.053
Female					
Estimate	0.001	0.022***	0.000	0.006	-0.004***
	(0.003)	(0.007)	(0.002)	(0.005)	(0.001)
N	860,000	860,000	860,000	860,000	860,000
Control Mean	0.037	0.105	0.024	0.065	0.006

Note: * p<0.1, ** p<.05, *** p<0.01. Dependent variable is measured through age 24. Misdem. = misdemeanor. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092.

Table C.9. Effect of Housing Reform on Girls' Birth Outcomes

	Any Children	Teen Mother	Single Mother
Children of Owners			
Estimate	0.010	0.010*	0.020***
	(0.007)	(0.006)	(0.007)
N	860,000	860,000	860,000
Control Mean	0.423	0.212	0.266
Children of Renters			
Estimate	0.000	-0.005	0.002
	(0.007)	(0.005)	(0.006)
N	747,000	747,000	747,000
Control Mean	0.503	0.272	0.350

Note: * p<0.1, ** p<.05, *** p<0.01. Dependent variable is measured through age 24. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092.

Table C.10. Effect of Housing Reform on Parent Outcomes

	ESS Index	Wage	Home Value	Rent	Own Home	Mortgage	2nd Mortgage
Renter Parents							
Estimate	-0.007	-1,958	-	-2.29	-	-	-
	(0.026)	(1,898)	-	(39.27)	-	-	-
N	63,000	63,000	-	21,500	-	-	-
Control Mean	-0.238	35,240	-	1,225	-	-	-
Owner Parents							
Estimate	-0.010	-1,660	81,360***	-	-0.046*	-0.027	0.033***
	(0.032)	(2,159)	(13,840)	-	(0.028)	(0.032)	(0.008)
N	83,000	82,500	46,500	-	48,000	48,000	48,000
Control Mean	-0.005	50,950	332,200	-	0.776	0.621	0.039

Note: * p<0.1, ** p<.05, *** p<0.01. Economic Self-Sufficiency (ESS) Index inputs are: weeks worked last year, hours worked per week, wages, family income to poverty ratio, and the negative of public assistance income. Sample includes individuals with children in the main analysis sample who responded to the ACS while their children were younger than 18; I use ACS sampling weights. Estimates for household variables (homeownership, home values, rents) are estimated at the household level rather than the individual level. Doubly robust difference-in-differences (DR-DID) regressions containing adults in downtown or outer ring neighborhoods, with outer ring individuals included to net out baseline city-level differences in outcomes. Regressions adjust for differences in individual covariates (age in 2000, race/ethnicity, gender, and human capital), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092 and CBDRB-FY26-0164.

Table C.11. Effect of Housing Reform on Adult Economic Self-Sufficiency

Age in 2000	15-19	20-29	30-44	45-54
Owners				
ESS Index				
Estimate	0.049	0.034	0.064***	-0.025
	(0.038)	(0.020)	(0.018)	(0.053)
N	58,500	179,000	319,000	23,000
Control Mean	-0.118	-0.026	0.026	-0.040
HC Index				
Estimate	0.014			
	(0.037)			
N	58,500			
Control Mean	-0.156			
Renters				
ESS Index				
Estimate	0.012	0.010	0.069***	-0.058
	(0.040)	(0.017)	(0.018)	(0.071)
N	35,000	290,000	227,000	9,100
Control Mean	-0.234	0.027	-0.142	-0.375
HC Index				
Estimate	0.016			
	(0.037)			
N	35,000			
Control Mean	-0.303			

Note: * p<0.1, ** p<.05, *** p<0.01. Economic Self-Sufficiency (ESS) Index inputs are: weeks worked last year, hours worked per week, wages, family income to poverty ratio, and the negative of public assistance income. Human Capital (HC) Index averages the following variables, standardized nationally: years of schooling, high school / any college / bachelor's degree / post-bachelor's degree / management occupation. The HC Index is reported only for the 15-19 cohort, whose outcomes are measured between the ages of 24 and 33; at older ages human capital is treated as a pre-determined covariate and included as a control. Sample includes individuals who responded to the ACS; I use ACS sampling weights. Doubly robust difference-in-differences (DR-DID) regressions containing adults in downtown or outer ring neighborhoods, with outer ring individuals included to net out baseline city-level differences in outcomes. Regressions adjust for differences in individual covariates (age in 2000, race/ethnicity, gender, and human capital), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY26-092.

Table C.12. Estimated Change in Permanent Earnings

Variable	BST Estimate	β Renters	β Owners
HS	0.138	-0.021	0.044
BA+	0.152	0.017	0.055
Post-BA	0.0166	-0.010	0.010
Mgmt. Job	0.249	-0.027	-0.017
Yrs Schooling	0.0304	-0.21	0.54
Wks Work	0.0352	-3.34	2.51
Hrs Work	0.0199	-3.17	3.66
Est. Δ Log Wages		**-0.194**	**0.188**
[Lower, Upper]		$[-0.378, -0.010]$	$[0.007, 0.369]$

Note: Estimates in second column are from Bailey, Sun and Timpe (2021) who use NLSY79 to predict log-wages between ages 35-57 with inputs to HC and ESS indices. Lower and upper bounds use minimum and maximum from each estimate's 90% confidence interval. See Tables C.4 and C.5 for details on the estimates in the third and fourth columns. Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092.

Table C.13. ACS Estimates by Household Mover Status

	Moved 5+ Miles	Didn't Move 5+ Miles
	Children of Owners	
HC Index	0.117*	0.040
	(0.068)	(0.045)
N	13,000	24,000
Control Mean	-0.117	-0.124
Control SD	0.568	0.564
ESS Index	0.145**	0.104
	(0.067)	(0.065)
N	13,000	24,000
Control Mean	-0.099	-0.134
Control SD	0.590	0.624
	Children of Renters	
HC Index	-0.025	-0.071
	(0.059)	(0.068)
N	16,000	13,500
Control Mean	-0.234	-0.285
Control SD	0.539	0.570
ESS Index	-0.174**	-0.027
	(0.068)	(0.060)
N	16,000	13,500
Control Mean	-0.178	-0.251
Control SD	0.616	0.633

Note: * p<0.1, ** p<.05, *** p<0.01. Sample includes children ages 0-7 in 2000. Moving 5+ miles is defined based on the distance between the child's address at age 17 and their address in 2000. Human Capital (HC) Index averages the following variables, standardized nationally: years of schooling, high school / any college / bachelor's degree / post-bachelor's degree / management occupation. Economic Self-Sufficiency (ESS) Index inputs are: weeks worked last year, hours worked per week, wages, family income to poverty ratio, and the negative of public assistance income. Sample includes individuals who responded to the ACS between ages 24 and 33. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P3114-R13261.

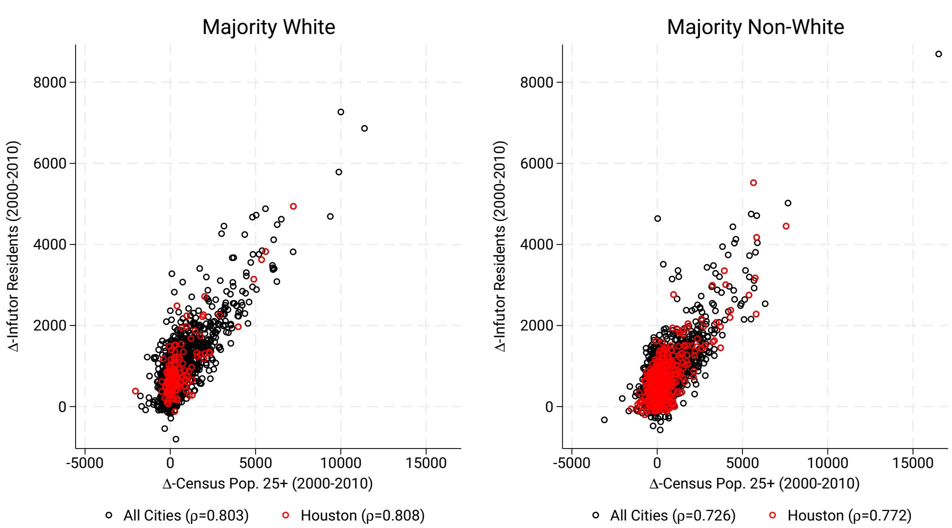
D Addressing Differential Migration Concerns with Infutor Data

The Census data do not allow me to measure household migration prior to 2000, meaning these estimates may partially reflect underlying differences in migration patterns of downtown Houston households that are unrelated to the reform. I use commercial address data from Infutor, which measures residential locations prior to 2000, to investigate this possibility.

Infutor compiles historical address information for nearly all American adults from a large number of sources, including USPS change-of-address forms, property deeds, voter registrations, and magazine subscriptions. Household moves are measured more accurately for high-SES households, who are over-represented in these data sources. However, I still capture a significant proportion of moves originating in areas with different minority or college-educated shares (see Figures D.1 and D.2). See Asquith, Mast and Reed (2023) and Diamond, McQuade and Qian (2019) for other uses of the data to study migration.

I identify all households living in the sample cities in 2000 and track their address histories before and after this year. Figure D.3 plots migration rates analogous to those in Figure 5, comparing the likelihood that individuals lived at their 2000 address in any year from 1997 to 2010, applying inverse probability weighting to address differences in observable characteristics between Houston and other cities.[1] The figure shows that housing mobility leading up to 2000 was very similar in Houston and other cities. Similar to the Census analyses, I do not observe large differences in migration rates (measured as leaving one's 2000 address) post-reform.

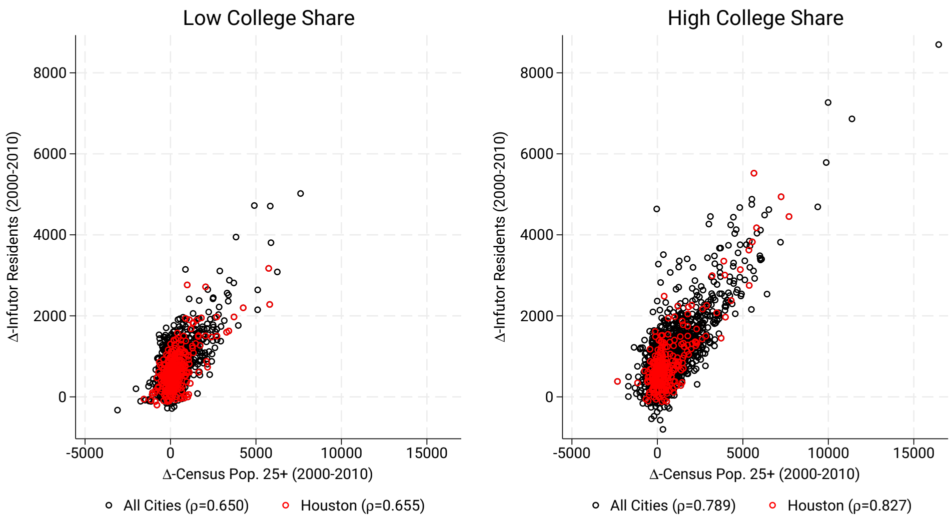
Figure D.1. Change in Infutor and Census Population (2000-10), by Neighborhood Race



Note: Census tract population counts include individuals 25 or older to match Infutor's population frame. "All Cities" includes all Sun Belt cities among the 100 largest in the U.S. in 2000.

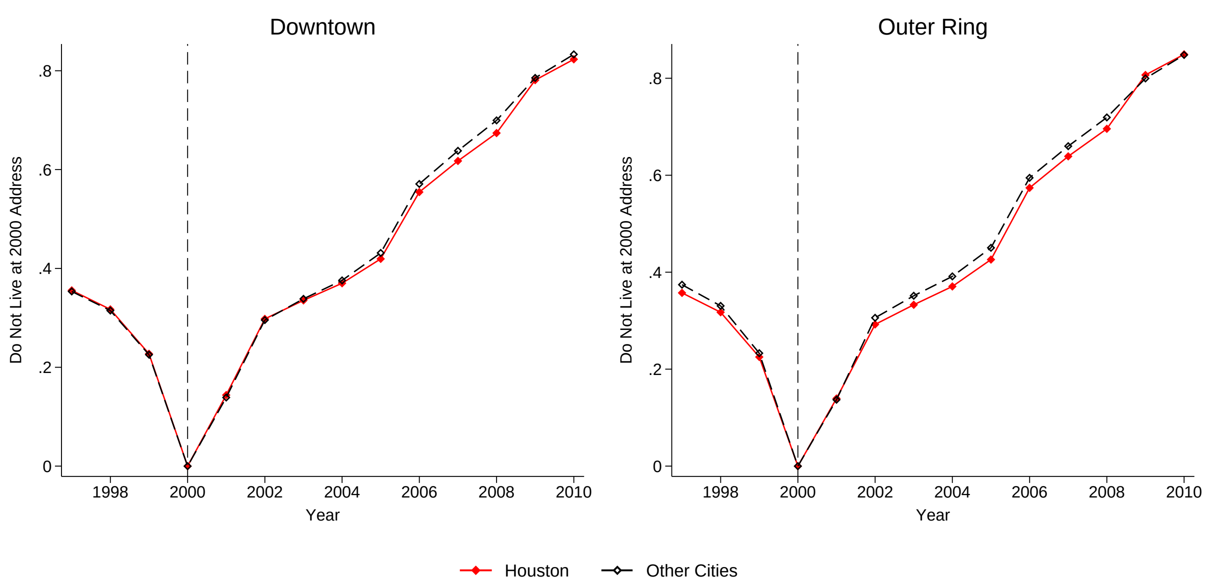
[1] I define weights based on housing tenure, race, age, and household size.

Figure D.2. Change in Infutor and Census Population (2000-10), by Neighborhood College-Educated Share



Note: Census tract population counts include individuals 25 or older to match Infutor's population frame. "All Cities" includes all Sun Belt cities among the 100 largest in the U.S. in 2000. High and low college-educated share is based on the 2000 sample median.

Figure D.3. Migration Patterns Pre-Reform in Houston and Control Cities



Note: Figure plots the probability that an individual lived anywhere other than their 2000 address in any year from 1997 to 2010, in Houston or other Sun Belt cities included as controls in the analysis. Commercial address data come from Infutor. I use propensity scores to weight individuals in control cities, where I include predicted race/ethnicity, age, tenure (renter vs owner), and household size as covariates. Tenure status comes by linking CoreLogic deeds records to Infutor addresses and matching names in the two datasets to identify homeowners.

E Robustness of Estimated Effects on Child Outcomes

Table E.1 studies how estimates differ when I do not use standard ACS sampling weights. If the survey's sampling scheme differs systematically across the cities in this sample, and in particular if the ACS differentially samples downtown versus outer ring areas in Houston relative to other places, then using the standard weights would produce biased estimates. I estimate models that do not use any ACS weights, as well as models that construct sample-specific weights to match downtown Houston's population distribution within age-sex-race bins. Estimates are very similar across these approaches for both owners and renters; note that the human capital index results for renter children attenuate and remain statistically insignificant.

In Tables E.2-E.4, I study the sensitivity of results to various specification changes. I first consider whether estimates are sensitive to the exact set of control cities used by restricting to just the ten largest control cities and to just cities in Texas. Next, I drop 2020 survey responses from the economic self-sufficiency regressions to test whether the COVID-19 pandemic had a differential impact on either downtown Houston's labor market or Census survey operations in treated neighborhoods.

Lastly, I also study whether migration from New Orleans after Hurricane Katrina affects the estimates. More than 100,000 evacuees from New Orleans settled at least temporarily in the Houston metropolitan area, with some remaining permanently (McIntosh, 2008). Prior work has found that this influx of migrants led to small negative impacts on incumbent worker wages and neighborhood home prices (Daepp, Hsu et al., 2023; McIntosh, 2008), but had negligible effects on student test scores (Imberman, Kugler and Sacerdote, 2012). Because most evacuees moved to outer ring Houston neighborhoods (Imberman, Kugler and Sacerdote, 2012), this could potentially bias *upwards* the estimated effects of the reform, if the Katrina shock worsened the outcomes of Houston households in untreated outer ring neighborhoods. To test for this possibility, I use the MAF-ARF to calculate the number of households who moved from New Orleans Parish between 2005 and 2006 to each census tract, as a share of the population. I then control for this share in regressions.[1]

As the tables show, the estimated effects on human capital and economic self-sufficiency are not sensitive to the exact set of control cities or the income of neighborhoods included, nor are the economic self-sufficiency estimates driven by differential impacts of the COVID-19 pandemic on downtown Houston. The results also do not reflect negative spillover effects from the influx of evacuees from New Orleans after Hurricane Katrina. The same is generally true for the criminal justice estimates, which show increases in felony and misdemeanor charges for owner children and no change or small decreases in charges for renter children. However, the increase in teen or single parenthood for female owner children is not robust across alternative samples and specifications.

I also show that the divergence in effects for younger owner and renter children is present in Texas

[1] I operationalize this control using an indicator for tracts that were above the 90th percentile of Houston's evacuee distribution share. The large majority of all Katrina evacuees moved to these tracts.

administrative data as well. These data measure outcomes for nearly all children who attended Texas public schools, and include outcomes that are not available in Census data such as test scores and school discipline. However, the lack of student address data means I do not directly observe household homeownership status in 2000. I instead compare children who I predict to be likely homeowners or renters based on a simple model described in Section 3.D.[2]

Table E.5 shows these estimates. Overall, conclusions are qualitatively the same: younger children of predicted Houston homeowners had better educational outcomes and early-career economic outcomes. I even estimate a positive effect on earnings here, which I did not observe in the ACS, and estimated increases in high school graduation are larger. Children of predicted renters, in contrast, are less likely to graduate high school or attend college and spend fewer quarters employed. Note that I also observe an increase in suspensions for children of homeowners but not renters, consistent with the patterns of criminal justice involvement for these groups.

[2] I include children who entered Kindergarten in the 1998 through 2001 school years, meaning they were four to seven years old in 2000; I omit younger cohorts because I do not observe their locations prior to entering Kindergarten, meaning the sample would likely contain households who moved into Houston post-reform.

Table E.1. Robustness of ACS Estimates to Alternative Weighting

	Main	No Weights	Internal Weights
		Children of Owners	
HC Index	0.097**	0.109***	0.112***
	(0.047)	(0.031)	(0.030)
N	37,000	37,000	37,000
Control Mean	-0.122	-0.107	-0.119
Control SD	0.566	0.590	0.590
ESS Index	0.142***	0.075**	0.080**
	(0.049)	(0.037)	(0.038)
N	37,000	37,000	37,000
Control Mean	-0.121	-0.131	-0.139
Control SD	0.576	0.597	0.598
		Children of Renters	
HC Index	-0.057	0.015	-0.001
	(0.041)	(0.030)	(0.031)
N	29,500	29,500	29,500
Control Mean	-0.260	-0.265	-0.279
Control SD	0.556	0.580	0.579
ESS Index	-0.112**	-0.090**	-0.095**
	(0.048)	(0.035)	(0.038)
N	29,500	29,500	29,500
Control Mean	-0.211	-0.229	-0.244
Control SD	0.592	0.610	0.613

Note: * p<0.1, ** p<.05, *** p<0.01. Sample includes children ages 0-7 in 2000. No Weights column does not use any ACS weights. Internal Weights column constructs sample-specific weights such that the ACS sample matches the race, sex, and age distribution of the full sample. Human Capital (HC) Index averages the following variables, standardized nationally: years of schooling, high school / any college / bachelor's degree / post-bachelor's degree / management occupation. Economic Self-Sufficiency (ESS) Index inputs are: weeks worked last year, hours worked per week, wages, family income to poverty ratio, and the negative of public assistance income. Sample includes individuals who responded to the ACS between ages 24 and 33. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-092.

Table E.2. Robustness of ACS Estimates to Alternative Samples

	Main	w/ Rich Nbhds.	Largest 10 Control Cities	Only TX TX Control Cities	Control Katrina Exposure	Drop 2020 Responses
			Children of Owners			
HC Index	0.097**	0.067	0.054	0.103**	0.097**	–
	(0.047)	(0.041)	(0.047)	(0.050)	(0.047)	–
N	37,000	42,000	22,000	12,000	37,000	–
Control Mean	-0.122	-0.115	-0.154	-0.173	-0.122	–
Control SD	0.566	0.567	0.563	0.569	0.566	–
ESS Index	0.142***	0.135***	0.097*	0.127**	0.132***	0.114***
	(0.049)	(0.044)	(0.050)	(0.050)	(0.048)	(0.044)
N	37,000	42,000	22,000	12,000	37,000	33,500
Control Mean	-0.121	-0.118	-0.126	-0.205	-0.122	-0.124
Control SD	0.576	0.616	0.602	0.619	0.613	0.613
			Children of Renters			
HC Index	-0.057	-0.050	-0.051	-0.074	-0.057	–
	(0.041)	(0.040)	(0.047)	(0.049)	(0.041)	–
N	29,500	30,000	18,500	8,400	29,500	–
Control Mean	-0.260	-0.260	-0.237	-0.294	-0.260	–
Control SD	0.556	0.556	0.565	0.527	0.556	–
ESS Index	-0.112**	-0.101**	-0.101**	-0.130***	-0.118**	-0.160***
	(0.048)	(0.047)	(0.051)	(0.048)	(0.049)	(0.053)
N	29,500	30,000	18,500	8,400	29,500	26,500
Control Mean	-0.211	-0.215	-0.184	-0.220	-0.215	-0.213
Control SD	0.592	0.626	0.607	0.589	0.626	0.638

Note: * p<0.1, ** p<.05, *** p<0.01. Sample includes children ages 0-7 in 2000. Rich Nbhds. column includes children in neighborhoods with baseline median incomes above $150,000. Largest 10 Control Cities column only includes control children in Los Angeles, Phoenix, Dallas, San Antonio, San Diego, San Jose, San Francisco, Jacksonville, Austin, and Fort Worth. Control Katrina Exposure column includes an additional regression indicator variable flagging neighborhoods with high shares of new Katrina evacuees (above the 90th percentile of Houston's distribution). Human Capital (HC) Index averages the following variables, standardized nationally: years of schooling, high school / any college / bachelor's degree / post-bachelor's degree / management occupation. Economic Self-Sufficiency (ESS) Index inputs are: weeks worked last year, hours worked per week, wages, family income to poverty ratio, and the negative of public assistance income. Sample includes individuals who responded to the ACS between ages 24 and 33. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY26-092 and CBDRB-FY26-P3114-R13261.

Table E.3. Robustness of Criminal Justice Estimates to Alternative Samples

	Main	w/ Rich Nbhds.	Largest 10 Control Cities	Only TX TX Control Cities	Control Katrina Exposure
			Children of Owners		
Felony Charge	0.009***	0.009***	0.004	0.006*	0.004
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
N	1,757,000	1,946,000	1,058,000	634,000	1,757,000
Control Mean	0.096	0.096	0.095	0.081	0.098
Misdemeanor Charge	0.027***	0.014*	0.017***	0.017**	0.018***
	(0.007)	(0.008)	(0.006)	(0.007)	(0.006)
N	1,757,000	1,946,000	1,058,000	634,000	1,757,000
Control Mean	0.180	0.182	0.187	0.164	0.191
			Children of Renters		
Felony Charge	-0.002	-0.002	-0.009**	0.004	-0.013***
	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)
N	1,524,000	1,544,000	962,000	506,000	1,524,000
Control Mean	0.129	0.130	0.119	0.102	0.125
Misdemeanor Charge	-0.005	-0.008	-0.021***	-0.002	-0.017*
	(0.008)	(0.009)	(0.007)	(0.006)	(0.009)
N	1,524,000	1,544,000	962,000	506,000	1,524,000
Control Mean	0.228	0.229	0.219	0.190	0.224

Note: * p<0.1, ** p<.05, *** p<0.01. Rich Nbhds. column includes children in neighborhoods with baseline median incomes above $150,000. Largest 10 Control Cities column only includes control children in Los Angeles, Phoenix, Dallas, San Antonio, San Diego, San Jose, San Francisco, Jacksonville, Austin, and Fort Worth. Control Katrina Exposure column includes an additional regression indicator variable flagging neighborhoods with high shares of new Katrina evacuees (above the 90th percentile of Houston's distribution). Dependent variable is measured through age 24. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY26-092 and CBDRB-FY26-P3114-R13261.

Table E.4. Robustness of Birth Estimates to Alternative Samples

	Main	w/ Rich Nbhds.	Largest 10 Control Cities	Only TX TX Control Cities	Control Katrina Exposure
			Children of Owners		
Any Children	0.010	0.001	0.000	-0.002	-0.004
	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)
N	860,000	953,000	518,000	310,000	860,000
Control Mean	0.423	0.417	0.443	0.435	0.419
Teen Birth	0.010*	0.006	0.011	0.003	0.005
	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)
N	860,000	953,000	518,000	310,000	860,000
Control Mean	0.212	0.210	0.225	0.222	0.209
Single Parent	0.020***	0.011*	0.000	0.004	0.004
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
N	860,000	953,000	518,000	310,000	860,000
Control Mean	0.266	0.263	0.274	0.268	0.262
			Children of Renters		
Any Children	0.000	-0.003	0.000	-0.006	0.000
	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)
N	747,000	756,000	471,000	248,000	747,000
Control Mean	0.503	0.505	0.504	0.502	0.501
Teen Birth	-0.005	-0.007	-0.004	-0.007	-0.006
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
N	747,000	756,000	471,000	248,000	747,000
Control Mean	0.272	0.273	0.277	0.278	0.269
Single Parent	0.002	0.001	-0.001	-0.002	0.005
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
N	747,000	756,000	471,000	248,000	747,000
Control Mean	0.350	0.352	0.349	0.343	0.349

Note: * p<0.1, ** p<.05, *** p<0.01. Rich Nbhds. column includes children in neighborhoods with baseline median incomes above $150,000. Largest 10 Control Cities column only includes control children in Los Angeles, Phoenix, Dallas, San Antonio, San Diego, San Jose, San Francisco, Jacksonville, Austin, and Fort Worth. Control Katrina Exposure column includes an additional regression indicator variable flagging neighborhoods with high shares of new Katrina evacuees (above the 90th percentile of Houston's distribution). Dependent variable is measured through age 24. Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (age in 2000, race/ethnicity, and gender), household covariates (the number of residents, number of children, household head age in 2000, and whether the household head was female), and neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent, and a criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023)). Standard errors are clustered at the census tract level using a stratified block bootstrap (999 replications). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY26-092 and CBDRB-FY26-P3114-R13261.

Table E.5. Effect of Housing Reform on Outcomes, Measured in Texas Administrative Data

	7th Grade Reading	7th Grade Math	9th Grade Reading	9th Grade Math	HS Suspensions	HS Grad	Any College	AA Enroll	BA Enroll	Prop. Qtrs. Employed	Avg. Earnings
Predicted Owners											
Estimate	0.074**	0.011	0.007	-0.011	0.299***	0.095***	0.072**	0.031	0.026	0.068**	2,054*
	(0.032)	(0.057)	(0.035)	(0.054)	(0.070)	(0.023)	(0.031)	(0.025)	(0.033)	(0.027)	(1,060)
N	53,587	53,637	52,760	52,255	66,763	66,763	66,763	66,763	66,763	66,763	66,763
Control Mean	-0.248	-0.221	-0.143	-0.179	0.705	0.707	0.465	0.405	0.175	0.528	19,093
Predicted Renters											
Estimate	0.040	0.042	0.025	-0.012	-0.055	-0.010	-0.021	-0.022*	-0.025*	-0.024**	-95
	(0.028)	(0.028)	(0.027)	(0.032)	(0.035)	(0.010)	(0.017)	(0.012)	(0.014)	(0.010)	(365)
N	108,627	108,687	107,481	105,965	132,501	132,501	132,501	132,501	132,501	132,501	132,501
Control Mean	-0.372	-0.373	-0.254	-0.303	0.953	0.693	0.466	0.394	0.204	0.568	17,096

Note: * p<0.1, ** p<.05, *** p<0.01. Sample includes children in Texas public schools who entered Kindergarten between the 1997-98 through 2000-01 school years, and attended one of the largest school districts in Texas in Kindergarten (Houston, Dallas, San Antonio, Austin, El Paso, Fort Worth, Corpus Christi). Doubly robust difference-in-differences (DR-DID) regressions containing children in downtown or outer ring neighborhoods, with outer ring children included to net out baseline city-level differences in outcomes. Regressions adjust for differences in child covariates (school cohort, gender, race/ethnicity, free/reduced-price lunch status, immigrant status, limited English proficiency, special education), neighborhood covariates measured in 2000 (share White, share with a college degree, marriage rate, unemployment and poverty rate, log median household income and rent), and school covariates (school spending per student and reading/math test score value-added). Value-added is estimated following the approach in Kane and Staiger (2008). I find no evidence of differential sample attrition from the employment and earnings data among treated students compared to control students. Standard errors are clustered at the school level using a stratified block bootstrap (999 replications).

F Modeling Effects on Household Well-Being

This section provides more details on how I estimate changes in well-being of Houston parents and children due to the reform. The reform and resulting neighborhood change affected households in multiple ways. For children, it affected education and earnings. For parents, it did not affect contemporary earnings but did affect both housing costs and amenities in Houston's downtown. While all households could potentially benefit from improved amenities, increases in housing costs could be harmful for renters but potentially beneficial for homeowners. Quantifying overall effects requires placing these various benefits and costs on a single unified scale.

F.1 Estimation Overview

Children. I estimate effects on children using the present discounted value of estimated log-earnings changes from Table C.12. Prime-age earnings are assumed to be realized between ages 35 and 57, consistent with Bailey, Sun and Timpe (2021), and discounted back to age 7 at a three percent annual rate. Given the estimated prime-age earnings in the control group of $39,676 for owner children and $35,529 for renter children,[1] this translates to an additional +$2,682 per owner child and −$2,059 per renter child. With approximately 0.8 children per household below the age of seven, this equates to gains of $2,145 per owner household per year and losses of $1,647 per renter household per year. Because these are reduced-form earnings estimates, they already incorporate childhood investments financed by parent home equity gains; I return to this below.

Renter parents. To estimate effects on parent well-being, I use a simple neighborhood choice model where families value consumption and local amenities. Beginning with renter parents, I assume that a household i living in neighborhood j receives indirect utility that takes the following form:

$$V_{i,j} = \ln(y_i - R_{i,j}) + \gamma \ln(A_j) + \varepsilon_{i,j} \tag{F.1}$$

where $y_i - R_{i,j}$ is the household's consumption (income minus rents), A_j is the neighborhood's level of common amenities, and $\varepsilon_{i,j}$ is a time-invariant idiosyncratic location preference draw from a Type-I extreme value distribution. The housing unit is rented from an absentee landlord who profits from rents but is assumed to not be another incumbent who has children. When households leave their original neighborhood, they pay a moving cost, denoted κ.

Estimating effects on parents requires distinguishing between households who stayed downtown and those who left due to the housing reform. Since nearly all renters leave their original 2000 housing unit within 10 years, I focus on long-distance moves that take households outside of the downtown core, proxied

[1] Taking the sample means in Table C.5 and multiplying by 1.3 to account for life-cycle earnings growth.

with moves of at least five miles while children are still living at home (see Section 5.D.1). In the language of the instrumental variables literature (Angrist, Imbens and Rubin, 1996), I characterize renters as "never-movers" (those who remain downtown with and without gentrification), "compliers" (those who leave downtown due to gentrification), and "always-movers" (those who move out of downtown regardless of gentrification). The population shares of these groups are 52 percent, 2 percent, and 46 percent.

When downtown Houston gentrified, both rents and amenities increased. Household i experiences a change from $(R_{i,0}, A_0)$ to $(R_{i,0} + \Delta R_i, A_0 + \Delta A)$. Never-mover renter households experience direct changes in utility due to gentrification. I characterize these changes using a compensating variation approach, estimating the dollar transfer (CV_i) that would make households equally well off before and after their neighborhood gentrifies:

$$\ln(y_i - R_{i,0} - \Delta R_i - \mathbf{CV_i}) + \gamma \ln(A_0 + \Delta A) = \ln(y_i - R_{i,0}) + \gamma \ln(A_0) \tag{F.2}$$

Taking a first-order approximation around the pre-reform equilibrium, the compensating variation for a never-mover household i is approximately:

$$CV_i^{NM} \approx \underbrace{\gamma \cdot (y_i - R_{i,0}) \frac{\Delta A}{A_0}}_{\text{WTP for amenities}} - \underbrace{\Delta R_i}_{\text{rent increase}} \tag{F.3}$$

The scaling component of willingness-to-pay for amenities, $\gamma \frac{\Delta A}{A_0}$, is not observed, meaning CV_i is not directly observed. We can instead express CV_i relative to the marginal household, who is approximately indifferent between staying and leaving. Representing the income of this marginal never-mover household as y^*, the compensating variation for this household is then:

$$CV^* \approx \gamma \cdot (y^* - R_0^*) \frac{\Delta A}{A_0} - \Delta R^* \tag{F.4}$$

This means we can solve for $\gamma \cdot \frac{\Delta A}{A_0}$ as:[2]

$$\gamma \cdot \frac{\Delta A}{A_0} = \frac{\Delta R^* + CV^*}{y^* - R_0^*} \tag{F.5}$$

Plugging into Equation F.3, we can write CV_i^{NM} for *any* never-mover household as:

$$CV_i^{NM} \approx \left(\frac{y_i - R_{i,0}}{y^* - R_0^*} \right) (\Delta R^* + CV^*) - \Delta R_i \tag{F.6}$$

The average compensating variation across all never-movers is then:

$$\overline{CV}_R^{NM} \approx \left(\frac{\overline{y}_R^{NM} - \overline{R}_0}{y^* - R_0^*} \right) (\Delta R^* + CV^*) - \overline{\Delta R} \tag{F.7}$$

[2] Given the estimated values for $\{\Delta R^*, y^*, R_0^*\}$ and the bounds on CV^*, we can bound $\gamma \cdot \frac{\Delta A}{A_0}$ between zero and 0.018. Assuming the percent change in amenities, $\frac{\Delta A}{A_0}$, is no less than the 7.4 percent increase in equilibrium rents, then γ lies between 0 and 0.24.

Estimating $\overline{CV}_R^{NM}$ requires knowing CV^*. Rather than obtain a point estimate, I bound it between $[-\Delta R^*,\ 0]$. CV^* cannot be greater than zero, because this would imply that the household should have left the downtown previously to obtain higher utility in their outside option neighborhood.[3] In contrast, these households can be worse off post-reform even if they are indifferent between staying and leaving, for example if moving costs are high or other neighborhoods in the choice set do not provide high utility values. However, the household cannot be worse off by more than the negative of the change in rents $(-\Delta R^*)$. This lower bound occurs when the valuation of downtown amenity changes equals zero, which is a change in utility that is achievable for all households by simply not moving.[4]

Always-movers do not experience effects from the reform, since the reform did not affect neighborhoods outside of the downtown. Compliers, who were induced to move as a result of the reform, are weakly worse off depending on the quality of their preferred outside option neighborhood. By revealed preference, utility effects from moving are more negative than CV^*, since if households are better off than the marginal never-mover, they would not have moved. Because I observe that Houston renter households do not live in observably worse neighborhoods post-reform, I assume that mover households are no worse off than the full moving cost, $-\kappa$, i.e., their neighborhood choice set is sufficiently large that there exists at least one other neighborhood j' such that $V_{i,j'} = V_{i,0}$.

Owner parents. A key distinction between renters and owners is how they experience changes in housing costs following neighborhood revitalization. For a renter household, housing costs are dictated by the market rent, which increases with neighborhood amenities in equilibrium. For an owner household, in contrast, housing costs equal a fixed mortgage payment m_i anchored to the purchase price prior to the reform. I model the owner's effective budget constraint as:

$$c_i = y_i - m_i. \tag{F.8}$$

Owner households that remained downtown benefit from increased amenities, but there is no offsetting rent increase since m_i is fixed. As for renters, the average compensating variation can again be represented in terms of the WTP for the marginal renter never-mover (up to a first-order approximation):

$$\overline{CV}_O^{NM} \approx \frac{\overline{y}_O^{NM} - \overline{m}}{y^* - R_0^*} \cdot (\Delta R^* + CV^*). \tag{F.9}$$

This is the same structure as the renter WTP term, scaled by owner disposable income rather than $\overline{y}_R^{NM} - \overline{R}_0$.

[3] Denoting the household's pre-reform utility from downtown Houston as V_0^*, their post-reform utility from downtown Houston as V_1^*, and their utility from the best outside option (net of moving costs) as $V_{j'}^* - \kappa$, having $CV^* > 0$ would imply that $V_1^* > V_0^*$. If the household is indifferent between staying and leaving, then $V_1^* \approx V_{j'}^* - \kappa$, meaning $V_{j'}^* - \kappa > V_0^*$.

[4] Intuitively, under the envelope theorem, changes in utility for marginal households subject to a local change in rents and amenities should be small. The estimated lower bound of $CV^* = -\$509$ is also empirically reasonable given the distribution of moving costs estimated in previous studies of gentrification's effects (French, Gandhi and Gilbert, 2023).

Since there is no $-\Delta R_i$ to subtract for owners, $\overline{CV}_O^{NM} > 0$ unconditionally: unlike renters, all owners gain from the reform, because their housing costs are insulated from the amenity-driven rent increase.

Home equity shock. In addition to increased access to downtown amenities, owner parents also now have increased housing equity. They can leverage this in multiple ways, either to increase their own consumption or to directly invest in their children. Formally, I introduce a new per-period annuity, $h_i(A)$, that can be used either for consumption or for childhood investments. Let $\alpha_i \in [0, 1]$ denote the share of this annuity that parents consume each year, with the remaining $(1 - \alpha_i)$ share spent on their children. The parameter α_i therefore governs additional parent-side gain from the equity shock, beyond what is already incorporated in the estimated effects on children. I annuitize the observed equity increase over thirty years.

Owner "always-movers" can also benefit from the fact that their original home sold for more than it would have otherwise. I therefore assume that these families benefit from the same potential home equity as the stayers, also scaled by α_i. This contrasts with always-mover renter households, whose well-being is unchanged. I do not observe a change in the overall likelihood of staying within five miles for homeowners, and I therefore assume that the complier share for owners is zero.

Sufficient Statistics. The model implies that estimating average effects for renter and owner parents requires only the following statistics:

1. Income of marginal renter never-movers, y^*
2. Average income of inframarginal renter and owner never-movers, $\{\overline{y}_R^{NM}, \overline{y}_O^{NM}\}$
3. Baseline and change in rents paid by marginal renter never-movers R_0^*, ΔR^*
4. Average baseline rents $\overline{R}_0$ and mortgage payment $\overline{m}$, average changes in rents and home values for never-movers $\overline{\Delta R}$, $\overline{\Delta P}$
5. Moving costs κ
6. Interest rate r and time horizon T
7. Parental consumption share of the home equity shock, $\overline{\alpha}$

Table F.1 shows how these statistics allow estimation of average compensating variation for never-movers, compliers, and always-movers, for both renters and owners. I estimate $\overline{CV}$ at the lower and upper bound, based on the assumed value for CV^*. Table F.2 lists the values for each input parameter. I estimate the average income of never-mover renters and owners and the income of complier renters using standard instrumental variables approaches (Abdulkadiroğlu, Pathak and Walters, 2018; Angrist, Imbens and Rubin, 1996); see Section F.3. I estimate the marginal renter household's income to be $35,160 (in 2024 dollars) and the average income of never-mover renter and owner households to be $55,800 and $97,050, respectively. These income differences match the gradient predicted by the model, and imply that never-mover households have higher relative willingness-to-pay for amenities given the diminishing marginal utility of income.

Assumptions. The model makes three key simplifying assumptions. First, I assume that the valuation households place on neighborhood amenities, A_j, is constant. Assuming that γ is uncorrelated with income implies that richer households have a higher willingness-to-pay for increasing amenities, since their marginal utility of consumption is lower (Equation 8). This is consistent with a literature documenting that demand for urban amenities is greater among higher-income residents (Couture et al., 2024; Diamond, 2016). In addition, this assumption means that owner and renter households with the same income cannot value neighborhood characteristics such as school quality or restaurant access differently due to unobservable traits associated with their housing tenure choice. This permits me to estimate $\overline{CV}_O^{NM}$ using information on the marginal never-mover renter household.

Second, I assume that idiosyncratic neighborhood preferences are uncorrelated with household income. If $\varepsilon_{i,j}$ is positively correlated with income conditional on other observables, this would bias upwards the estimated compensating variation of never-movers (and vice versa), since the higher average income of never-movers would be attributed to gains from higher ΔA when it is simply due to higher $\varepsilon_{i,j}$.[5] Third, I assume that the relationship between utility changes from rents and amenity valuation is approximately linear, a reasonable assumption given the relatively small change in rents. Given the concavity of the utility function, this means I may slightly overstate household compensating variation.[6] The aggregate estimates below show that willingness-to-pay for amenities is a relatively small share of the total change in incumbent well-being, meaning the biases resulting from these assumptions are proportionately small.

F.2 Sensitivity

Table F.3 shows estimates under various parameter assumptions. I first vary the discount rate in Panel A, and then assume different life-cycle earnings growth trajectories in Panel B; these enter the estimates by affecting the returns to home equity and children's earnings in adulthood. Next, in Panel C I allow moving costs κ to vary from \$1,000 to \$5,100, the upper end of the range estimated in French, Gandhi and Gilbert (2023). Lastly, in Panel D I vary the share of increased housing equity consumed by the parent rather than re-invested in their children.

The average household's welfare is positive across both bounds in nearly all specifications. Across various discount rates, average welfare ranges from \$965 to \$2,239 per year, and across various life-cycle

[5] I also make a related, benign assumption that the distribution of $\varepsilon_{i,j}$ is static, i.e., that it relates to fixed characteristics of neighborhoods rather than changes post-reform.

[6] The ratio of the second-order to first-order term is approximately $\frac{1}{2} \cdot \frac{\Delta A}{A_0}$. Assuming the percent change in amenities is no less than the 7.4 percent increase in equilibrium rents and no greater than the 17 percent equilibrium increase in resale prices for pre-existing homes, then the second-order term is between 3.7% and 8.5% as large as the first-order term.

earnings growth trajectories from $1,134 to $1,866. Varying moving costs has minimal effects on aggregate estimates, since the share of renter households who were displaced is only two percent. Lastly, as parents consume a smaller share of the increase in housing wealth, they benefit less from the reform. However, for most values of $\overline{\alpha}$ the average welfare change remains positive: only once parents consume less than 12 percent of their new equity does the lower bound become less than zero.

F.3 Estimating Marginal Stayer and Never-Mover Incomes

I estimate the average income of marginal stayer and never-mover households using methods from the instrumental variables (IV) literature. Here, a "marginal" stayer household is defined as a renter household who is approximately indifferent between staying and leaving downtown Houston when exposed to gentrification. I proxy for their characteristics using those of "complier" households who moved out of downtown Houston due to the housing reform and untreated compliers living in other Sun Belt downtowns. Because less than two percent of households are compliers, their characteristics likely closely match those of households on the margin.

Abdulkadiroğlu, Pathak and Walters (2018) show that, under the standard instrumental variables assumptions in Abadie (2002), one can estimate the characteristics of treated and untreated compliers using an IV regression with the characteristic of interest on the left-hand side, where both the dependent variable and the instrument are interacted with treatment status. Formally, for any measurable function $g(Y_i, X_i)$, given a binary treatment M_i and a binary instrument Z_i where Z_i is independent of both potential outcomes and treatments $\{Y_i(0), Y_i(1), M_i(0), M_i(1)\}$, and the instrument satisfies monotonicity (i.e., $M_i(1) \geq M_i(0),\ \forall\ i$):

$$E[g(Y_i(1), X_i) \mid M_i(1) > M_i(0)] = \frac{E[g(Y_i, X_i)M_i \mid Z_i = 1] - E[g(Y_i, X_i)M_i \mid Z_i = 0]}{E[M_i \mid Z_i = 1] - E[M_i \mid Z_i = 0]} \tag{F.10}$$

meaning the average value of g for treated compliers can be estimated using a Wald (1940) IV estimand where the dependent variable is g interacted with M_i and Z_i is used to instrument for M_i. The same is true for untreated compliers, whose income can be estimated with an IV regression where the left-hand side containing g is interacted with $(1 - M_i)$ and Z_i is used to instrument for $(1 - M_i)$.

I use this approach to estimate the average income of treated and untreated compliers. Here, the endogenous regressor M_i is an indicator for whether the household moved at least five miles before their youngest child turned 18, which I use as a proxy measure for leaving downtown Houston that can be measured consistently across cities. As discussed in Section 5.A, exposure to the housing reform is only randomly assigned in a difference-in-differences model, where I include households in untreated areas of Houston and analogous outer ring areas of other Sun Belt cities to account for baseline city-level differences in migration rates. I therefore estimate Equation F.10 using the following two-stage least squares regression:

$$\begin{aligned} \text{HHInc} \times M_i &= \tau \cdot \widehat{M_i} + \gamma_1 \mathbb{1}\{H_i = 1\} + \delta_1 \mathbb{1}\{D_i = 1\} + \beta_1 X_i + \varepsilon_i \\ M_i &= \phi \mathbb{1}\{H_i = 1, D_i = 1\} + \gamma_2 \mathbb{1}\{H_i = 1\} + \delta_2 \mathbb{1}\{D_i = 1\} + \beta_2 X_i + \epsilon_i \end{aligned} \tag{F.11}$$

where I instrument for migration decision M_i using the interaction between living in Houston and living in the treated downtown core and include fixed effects for living in Houston and living in a downtown core of any city, as well as the vector of controls X_i used to adjust for differences in household demographics and baseline neighborhood characteristics in the paper's main estimating regressions. I estimate this regression using one observation per household, averaging child characteristics within the household when they differ across children. I use the analogous regression to estimate the income of control compliers:

$$\begin{aligned} \text{HHInc} \times (1 - M_i) &= \tau \cdot \widehat{(1 - M_i)} + \gamma_1 \mathbb{1}\{H_i = 1\} + \delta_1 \mathbb{1}\{D_i = 1\} + \beta_1 X_i + \varepsilon_i \\ (1 - M_i) &= \phi \mathbb{1}\{H_i = 1, D_i = 1\} + \gamma_2 \mathbb{1}\{H_i = 1\} + \delta_2 \mathbb{1}\{D_i = 1\} + \beta_2 X_i + \epsilon_i \end{aligned} \tag{F.12}$$

Under random assignment, τ is identified by both regressions but in finite samples the estimates from these two regressions may not be equivalent. I therefore average them to obtain the overall average income of compliers.

I measure pre-reform income using self-reported household income in the 2000 long-form Decennial Census. The long-form is a random sample of approximately one in six households, which corresponds to approximately 3,200 incumbent renter households in downtown Houston (Table 7). Because the complier share is very small (two percent, as shown in Figure 6), sampling error can lead to large, unrealistic estimates of complier income for such a small estimation sample. Given this, I instead predict household income in the entire sample by training a regression model on the sample of long-form respondents, using the vector of covariates in Equation F.11. Table F.4 contains the coefficients from this model, which I estimate separately for renters and owners to allow covariate values to flexibly predict income. I then predict income among all individuals and use this predicted value in a regression that includes the full sample of households.[7] The IV regressions have a first-stage F-statistic of 9.3.

Estimating the average income of never-mover households is straightforward. These are households that do not move in the presence of treatment, meaning I simply estimate the average (predicted) income of households who were living in downtown Houston pre-reform and did not move at least five miles while a child was living at home.

[7] The correlation between actual and predicted income among long-form respondents is 0.6.

Table F.1. Compensating Variation by Household Type

	Prop. HHs	Change in Household Welfare
Renters		
Never-Movers	52%	$\left(\frac{\overline{y}_R^{NM}-\overline{R_0}}{y^*-R_0^*}\right)(\Delta R^* + CV^*) - \overline{\Delta R}$
Compliers	2%	$\frac{1}{2}(-\kappa + CV^*)$
Always-Movers	46%	0
Owners		
Never-Movers	72%	$\frac{\overline{y}_O^{NM}-\overline{m}}{y^*-R_0^*} \cdot (\Delta R^* + CV^*) + \alpha \cdot \frac{r}{1-(1+r)^{-T}} \cdot \Delta P$
Compliers	0%	–
Always-Movers	28%	$\alpha \cdot \frac{r}{1-(1+r)^{-T}} \cdot \Delta P$

Note: Table summarizes the estimating equation for incumbent household compensating variation, depending on whether they owned or rented their home in 2000. Never-movers are households who prefer remaining in downtown Houston after it gentrifies. Compliers are households who prefer to leave the downtown due to the reform. Always-movers would have left the downtown regardless. CV^* is the compensating variation of the marginal household who is indifferent between staying and leaving downtown Houston post-reform and y^* is the marginal household's income. $\{\overline{y}_R^{NM}, \overline{y}_O^{NM}\}$ is the average income of never-mover renters and owners, respectively. R_0^* and ΔR^* are the baseline rent and rent increase of the marginal never-mover, while $\overline{R_0}$ and $\overline{\Delta R}$ are the corresponding averages across never-movers. ΔP is the change in home prices, $\overline{m}$ is the average mortgage payment, r is the average interest rate, and T is the number of years over which the household discounts. κ is the moving cost paid by compliers, whose welfare is the midpoint of $[-\kappa,\ CV^*]$. See Table F.2 for parameter values.

Table F.2. Sufficient Statistics: Calibration

Parameter	Description	Baseline Value	Source
ΔR	Annual rent increase	$7.4\% \cdot R_0$	Table 2
ΔP	Home price increase	$81,360	Table C.10
r	Discount rate	3%	Assumed
T	Tenure horizon	30 years	Assumed
κ	Incremental moving cost	$2,500	French, Gandhi and Gilbert (2023)
α	Parent consumption share of equity	0.75	Assumed
$\bar{m}$	Annual mortgage payment	$12,088	Decennial Census; Freddie Mac (2026)
y^*	Marginal renter income	$35,160	Estimated from 2000 Census long-form responses
$\bar{y}_R^{NM}$	Avg. never-mover renter income	$55,800	2000 Census long-form responses
$\bar{y}_O^{NM}$	Avg. never-mover owner income	$97,050	2000 Census long-form responses
R_0^*	Marginal renter's annual rent	$6,881	2000 Census long-form responses
$\overline{R}_0$	Avg. never-mover's annual rent	$8,693	2000 Census long-form responses

Note: All dollar values in 2024 dollars. Mortgage payment based on Houston's pre-reform average home value of $170,793 and a 30-year term, the 2000 average mortgage interest rate of 8.05%, 20% down payment. Estimates and sample sizes in the bottom panel have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P3114-R13261.

Table F.3. Welfare Effects Under Alternative Parameters

	Owner HH	Renter HH	Average HH
Baseline	[5,259, 6,360]	[−2,011, −1,565]	[1,135, 1,865]
Panel A: Discount rate r (baseline = 0.03)			
r=0.02	[5,830, 6,932]	[−2,748, −2,302]	[965, 1,695]
r=0.05	[5,016, 6,118]	[−1,168, −722]	[1,509, 2,239]
Panel B: Life-cycle earnings scaling (baseline = 1.30)			
$\times$ 1.10 (flat profile)	[4,929, 6,030]	[−1,758, −1,312]	[1,136, 1,866]
$\times$ 1.60 (steep profile)	[5,754, 6,855]	[−2,391, −1,945]	[1,134, 1,864]
Panel C: Moving cost κ (baseline = −2,500)			
$\kappa = -1{,}000$	[5,259, 6,360]	[−1,996, −1,550]	[1,144, 1,874]
$\kappa = -5{,}100$	[5,259, 6,360]	[−2,037, −1,591]	[1,121, 1,850]
Panel D: Parental consumption share $\overline{\alpha}$ (baseline = 0.75)			
$\alpha = 0.10$	[2,560, 3,662]	[−2,011, −1,565]	[−32, 697]
$\alpha = 0.25$	[3,183, 4,285]	[−2,011, −1,565]	[237, 967]
$\alpha = 0.50$	[4,221, 5,322]	[−2,011, −1,565]	[686, 1,416]
$\alpha = 1.00$	[6,296, 7,398]	[−2,011, −1,565]	[1,585, 2,314]

Note: Annual welfare change (2024 dollars) for the average owner household, renter household, and incumbent household with children under alternative parameter assumptions. Estimates shown as brackets [$CV^* = -\Delta R^*$, $CV^* = 0$], where $CV^* = 0$ assumes the marginal renter never-mover is welfare-neutral and $CV^* = -\Delta R^*$. The average-household column weights owner and renter households by population shares.

Table F.4. Predicting Household Characteristics from 2000 Census Long-Form

	Owners: HH Income	Renters: HH Income	Renters: Monthly Rent
N Residents 2000	3211***	3077***	19.85***
	(82.71)	(88.13)	(1.368)
N Children 2000	-3966***	-4133***	-16.11***
	(110.4)	(102.7)	(1.682)
HH Head Age	130.7***	115.2***	1.364***
	(15.65)	(15.97)	(0.2593)
HH Head Female	-8858***	-5564***	-45.15***
	(369.5)	(299.6)	(4.779)
Female (children mean)	-132.8	-528.5*	-2.459
	(330.1)	(313.1)	(4.993)
Black (children mean)	-6455***	-5328***	-140.7***
	(628.4)	(558.9)	(9.431)
Hispanic (children mean)	-10200***	-8108***	-163.5***
	(447.5)	(480.1)	(8.048)
Other Race (children mean)	-5061***	-5488***	-108.2***
	(615.3)	(636.3)	(11.54)
Age in 2000 (children mean)	147.4***	216***	2.621***
	(38)	(39.03)	(0.6122)
Prop. Adults White (2000)	-4689***	1687	-98.99***
	(1076)	(1031)	(18.12)
Prop. Adults BA+ (2000)	47050***	11350***	468.2***
	(2365)	(2199)	(42.11)
Unemployment Rate (2000)	12600**	17640***	31.13
	(5919)	(4523)	(84.22)
Poverty Rate (2000)	24300***	-9613***	212***
	(4490)	(3308)	(76.01)
Log Median HH Income (2000, 2024$)	34870***	10050***	91.61***
	(1818)	(1348)	(33.94)
Log Median Rent (2000, 2024$)	-1978**	4846***	838.9***
	(990.7)	(1139)	(27.33)

Continued on next page

Table F.4 – continued from previous page

	Owners: HH Income	Renters: HH Income	Renters: Monthly Rent
Marriage Rate (2000)	-36140***	-1437	-129.5***
	(2394)	(1761)	(39.76)
CJ Index (2000)	-1265***	-322.9***	-26.19***
	(117)	(106.1)	(1.782)
Downtown	-620.7	-122.5	-7.257
	(609.7)	(471.4)	(7.912)
Constant	-312700***	-110600***	-5800***
	(17580)	(12130)	(323.7)
R-Squared	0.265	0.171	0.416
Weighted N	439,000	361,000	331,000
Raw N	57,500	45,500	41,500

Note: * p<0.1, ** p<.05, *** p<0.01. Sample includes households with children who responded to the 2000 Census long-form survey, with one observation per household. Regressions are weighted using the survey's household sampling weights. Standard errors are clustered at the census tract level. HH = household; Prop. = proportion; BA+ = bachelor's degree or higher; CJ Index = criminal justice index capturing tract-level exposure to individuals with criminal justice involvement defined as in (Finlay, Mueller-Smith and Street, 2023). Estimates and sample sizes have been rounded according to Census Bureau DRB rules. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-P3114-R13261.