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Blockbusting and the Challenges Faced by Black Families in Building Wealth through Housing in the Postwar United States

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Abstract

We study the historical impacts of blockbusting, i.e., large-scale racial turnover of urban neighborhoods in the 1950s-1970s orchestrated by realty professionals using aggressive and discriminatory practices. First, using property-level data in one neighborhood of Baltimore, Maryland, we find that new residents were charged high prices by blockbusters and had elevated foreclosure rates. Second, we analyze census tract data to estimate the effects of blockbusting on neighborhoods in large U.S. cities, comparing neighborhoods subjected to blockbusting to other similar neighborhoods. We find that blockbusting caused housing values to fall substantially by 1980.

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Introduction

In the late 1940s and early 1950s, an urgent demand for additional and better-quality housing emerged for Black residents in many American cities. A housing shortage resulted from the confluence of low housing construction during WWII, segregated housing, and influx of Black residents during the second wave of the Great Migration. In these conditions, a phenomenon known as blockbusting became fairly common over the next two decades. Blockbusters were real estate professionals who targeted existing, White-populated neighborhoods for wholesale turnover. Blockbusters stirred up panic among White residents by catalyzing existing racial prejudices and promoting the view that housing values would soon fall as Black families moved in. White residents sold their homes at lower prices, under pressure from blockbusters. Blockbusters then resold these homes at higher prices to Black residents, often with risky financing arrangements, profiting from the limited housing options and uneven access to credit of Black urban residents at the time. In this way, blockbusters had a role in orchestrating the rapid racial turnover of urban American neighborhoods in the 1950s and 1960s. This infamous discriminatory practice became less prevalent after it was outlawed nationally as part of the Fair Housing Act of 1968.¹

Many journalists and scholars have detailed the aggressive and discriminatory practices employed by blockbusters through exposés and textual evidence. Our study provides a new quantification of the effects of blockbusting through data analysis at two geographic levels. First, we analyze individual property transactions in one neighborhood in Baltimore, Maryland, known as Edmondson Village, the site of one of the most notorious instances of blockbusting in American history (Orser 1994). Second, we analyze a panel of neighborhoods, in 35 large American cities, that we identify as having been the sites of blockbusting from historical sources.

¹ The Fair Housing Act prohibits “For profit, to induce or attempt to induce any person to sell or rent any dwelling by representations regarding the entry or prospective entry into the neighborhood of a person or persons of a particular race, color, religion, sex, handicap, familial status, or national origin.” The code of federal regulations, § 100.85 Blockbusting, prohibits “(1) Engaging, for profit, in conduct (including uninvited solicitations for listings) which conveys to a person that a neighborhood is undergoing or is about to undergo a change in the race, color, religion, sex, handicap, familial status, or national origin of persons residing in it, in order to encourage the person to offer a dwelling for sale or rental. (2) Encouraging, for profit, any person to sell or rent a dwelling through assertions that the entry or prospective entry of persons of a particular race, color, religion, sex, familial status, or national origin, or with handicaps, can or will result in undesirable consequences for the project, neighborhood or community, such as a lowering of property values, an increase in criminal or antisocial behavior, or a decline in the quality of schools or other services or facilities.”

In Edmondson Village during the 1950s and 1960s, nearly every one of the 20,000 residents of this neighborhood moved out as the Black population climbed from virtually zero to 96 percent. We digitize all real estate transactions and mortgages in this neighborhood from 1954 to 1976.² We identify blockbusters in the data and find that they acquired about two-thirds of the properties in this neighborhood at some point. They resold these properties at a 55 percent markup on average, a price gap that community groups at the time called “the Black tax” (Activists Inc. 1971). Those who bought properties via blockbusters experienced a 17 percent foreclosure rate, compared to 2 percent among those who bought directly from prior residents. Moreover, we document how some of the new residents lost their properties because they had financed the purchases through risky installment contracts, which are the focus of many historical accounts of blockbusting.

Our panel data analysis of urban neighborhoods complements our case study of Edmondson Village with a comprehensive analysis of blockbusting in hundreds of census tracts across 35 cities. We identify episodes of blockbusting from textual sources largely in the 1950s and 1960s. We find that these reports of blockbusting tended to occur in neighborhoods adjoining existing Black population centers. To estimate the causal impact of blockbusting, we compare the outcomes in tracts that experienced blockbusting to other tracts that escaped blockbusting despite similarities to areas that did experience blockbusting. Our baseline specification compares tracts that experienced blockbusting with contiguous tracts that did not. Alternately, in case there are spillover effects from blockbusting neighborhoods to neighboring tracts, we also consider a donut specification using tracts that are not immediately contiguous as a comparison group. As another alternate, to mitigate concerns regarding ring-based spatial designs, we use a comparison group of tracts located in other neighborhoods in different parts of the same city with similar characteristics to the blockbusting neighborhoods. In each of these specifications, we show that the comparison group and blockbusting neighborhoods shared ex-ante characteristics that predict blockbusting. Econometrically, we use a propensity score

² These data contributions are preceded by similar work by a group of community activists known as Activists Inc. in Baltimore during the late 1960s and 1970s. Using a large team of volunteers, they used contemporary computers to assemble a data set of thousands of real estate transactions in Baltimore. A summary of their findings is reported in US Congress (1971), pp. 321-347. We have been able to locate a copy of one of the three reports they produced (Activists, Inc 1971), but not the other two or the underlying data. In addition, George, Hendley, Macnamara, Perez, and Vaca-Loyola (2019) analyze data on installment contracts in Chicago extracted from court filings.

weighting technique to weight the comparison tracts as a function of their propensity for being subjected to blockbusting.

Our analysis of census tracts suggests that blockbusting increased racial turnover and decreased housing values. Prior to the 1950s, we estimate parallel trends between comparison and treated tracts. In the decades following, housing values dropped substantially in neighborhoods subjected to blockbusting, reaching a trough of -16 percent in 1980. The share of Black individuals rises as well, peaking at +18 percent in 1980. In combination with the Edmondson Village data, our analysis suggests that blockbusting left new residents in poor financial shape, having paid high prices for housing that subsequently decreased in value, and experiencing high rates of foreclosure.

These results directly bear on the history of wealth building by Black families in the postwar period. Scholars in this literature tend to emphasize that, through housing market and housing finance discrimination, Black families were excluded from key wealth building opportunities. This paper highlights the poor conditions for wealth building that blockbusting created when many Black American families obtained homeownership for the first time.

Literature

To our knowledge, our study is the first to quantitatively estimate the effects of blockbusting across a large number of cities. However, our findings relate to several strands of literature in economics, history, and sociology.

First, blockbusting has been documented by many historians, sociologists, anthropologists, and other social scientists. Taylor (2019, pp. 48-50) provides an overview of blockbusting as a general phenomenon. Many other monographs detail blockbusting in specific cities. These include studies of Baltimore by Orser (1994); Chicago by Seligman (2005), Satter (2010), and Marchiel (2022); Cleveland by Michney (2017); Kansas City by Gotham (2002); Detroit by Sugrue (2005); Queens, NY by Rothman (1980); and Buffalo by Goldman (2010) and Kraus (2000). Legal scholars have also written about blockbusting, including Mehlhorn (1998), Moskovitz (1977), and Glassberg (1972).

Second, social scientists have studied the racial turnover of urban American neighborhoods in the 1950s and 1960s. These studies often describe this historical process as the result of uncoordinated decision-making of individual households. For example, Card et al.

(2007) and Davis et al. (2025) build upon the theoretical models of segregation and neighborhood change developed in Schelling (1969, 1971). Using data from the 1970-2000 decennial censuses, Card et al. (2007) find that once a neighborhood exceeds a certain minority population share, or “tipping point,” White residents are much more likely to leave in future years. Davis et al. (2025) focus on the spatial characteristics of this process, showing that it is driven by Black population growth and White population departure from central city neighborhoods on the outskirts of existing Black neighborhoods, and by White population growth in the suburbs. Our findings point to blockbusters as actors with a profit motive to coordinate and induce neighborhood change. Ouazad (2015) explores the profit motives of blockbusters with a theory-based approach.

Third, the racial turnover of American neighborhoods occurred not just in the 1950s and 1960s but also before WWII. Notably, Akbar et al. (2025) examines neighborhood racial change from 1930 to 1940 and associated change in home values and rents, finding that segregated housing markets led to both elevated rents and declining home values for Black households. The authors do not focus on real estate professionals intermediating the process.

Other studies have focused on the effects of historical institutional-based housing discrimination. A few examples of recent work in this area include Aaronson et al. (2021), Ali (2022), Fishback et al. (2023), and Sood and Ehrman-Solberg (2023).

Our study is also related to another strand of the economics literature that empirically estimates preferences for neighborhood racial and ethnic composition. Bayer et al. (2025) find that both Black and White homeowners are more likely to move after a new neighbor of a different race moves in next door. Caetano and Maheshri (2025) and Davis, Gregory, and Hartley (2024) estimate preferences for neighborhood racial composition. Both studies estimate strong preferences for neighbors of the same race which lead to increasing segregation, the speed of which depends on how expectations about future neighborhood composition are formed. Finally, our study relates to the literature on Black-White wealth inequality. Derenoncourt et al. (2023) constructs estimates of the racial wealth gap from 1860 to 2020 and focuses on its slow convergence. The blockbusting episode suggests that discrimination in the housing market has played a role in slowing that convergence. Binder et. al. (2025) investigates intergenerational mobility of housing wealth finding that White families show more intergenerational persistence and Black families experiencing stronger downward mobility of housing wealth. Using housing

transaction data from 1988-2013, Sakong (2026) finds that Black households are more likely to buy homes toward the peak of boom and sell homes when prices are closer to a trough. These differences may contribute to the racial wealth gap and are consistent pro-cyclical credit dynamics where lower wealth households gain access to credit in a boom and become more constrained during a bust.

Historical Context

During the Great Migration (1910-1970), six million Black Americans moved from Southern rural areas to urban cities in the Northern, Midwestern, and Western states, driving a surge in housing demand. The Great Migration is typically considered to have come in two waves, interrupted by the Great Depression. The second and somewhat larger wave occurred from 1940 to 1970. In this period, incoming Black residents faced a tight housing market in northern cities. Residential construction had been very limited during WWII. Furthermore, existing patterns of segregation meant that housing options for Black people were limited to select existing neighborhoods. These neighborhoods became increasingly overcrowded and expensive.

Segregation had more than one motivation. In part, many homeowners had taste-based desires to not live in mixed-race neighborhoods. However, homeowners were also motivated by economic considerations stemming from longstanding widespread beliefs that the entrance of Black families into a neighborhood would lead to lower property values (Laurenti, 1961; Akbar et. al., 2022).

Segregation was enforced in several ways: by the choices of realtors, mortgage lenders and homeowners; by threats of violence; and by racially restrictive covenants. In 1948, the latter of these constraints was largely lifted by the Supreme Court ruling in *Shelley v. Kramer* that federal courts could not enforce covenants that prohibited sales to certain racial groups. While the *Shelley v. Kramer* ruling potentially opened more neighborhoods to Black residents, other forces kept segregation in place. For example, real estate boards and lending institutions commonly had policies to not facilitate sales or loans to Black households in White neighborhoods. Nationally, the National Association of Real Estate Boards (NAREB) published a code of ethics in 1924 that advised against “introducing into a neighborhood a character of

property or occupancy, members of any race or nationality, or any individual whose presence will clearly be detrimental to property values in that neighborhood.” After the Supreme Court’s 1948 ruling, this code was changed to state “A Realtor should not be instrumental in introducing into a neighborhood a character of property or use which will clearly be detrimental to property values in that neighborhood.” Realtors stated that they knew the meaning remained the same (Helper 1969, p. 201).

Similar policies existed at local levels. In Chicago, for example, the Chicago Real Estate Board recommended as far back as 1917 that realtors only sell or rent homes to Black households on blocks that were adjacent to existing Black neighborhoods: “Inasmuch as more territory must be provided, it is desired in the interest of all, that each block shall be filled solidly and that further expansion shall be confined to contiguous blocks, and that the present method of obtaining a single building in scattered blocks, be discontinued.” (Helper 1969 quoting Chicago Real Estate Board Bulletin, XXV, No. 4 (April, 1917), 313) Similar practices continued to be in place in Chicago in the 1950s. According to Helper (1969), the policy of realtors in Chicago as of 1958 was still to only sell properties to Black families in an adjacent block to an existing, Black-populated block, and only when that existing block was fully inhabited by Black people. Alternatively, a “five family” rule was described in 1959 by a member of Chicago’s real estate board for Black realtors (US Commission on Civil Rights, 1959b, p. 741), i.e. that a realtor should not sell a property to a Black family unless five Black families already inhabited that area. Helper also reports interviews of real estate brokers from 1958 showing that brokers were hesitant to sell to Black households until two or three units on the block were occupied by Black people (pages 41-42). Altogether, these policies were intended to ensure that the formation of new Black-populated neighborhoods were restricted to the periphery of existing, Black neighborhoods, and could not leap-frog to other parts of the city.

These institutional and market features created strong profit motives for real estate professionals willing to break the patterns of segregation and codes of ethics by intermediating sales and financing for new Black residents in White neighborhoods. Indeed, some must have violated this policy for blockbusting to have occurred. Hirsch (1983) suggests that realtors’ degree of actual compliance with the Chicago realtors’ policy varied; some waited until 50 percent of a border area was Black, while “others utilized successively lower percentages.” Real estate professionals exploring such transactions found that some White households were willing

to sell at a discount. A wide mix of racial and economic motivations drove homeowners in these situations, as documented by Orser (1997), Satter (2010), Hirsch (1983), and Sugrue (2005). In addition, intermediaries found that Black households were willing to pay above market prices due to the dearth of property otherwise available to them. The lack of availability of mortgage financing for Black buyers also meant that real estate professionals could profit by financing these purchases either through a mortgage or through rent-to-own or “installment” contracts.

It is important to note that not all intermediaries who served Black homebuyers were blockbusters. For example, Smithsimon (2022) describes a realtor who was accused of being a blockbuster because they showed a property in a White-populated neighborhood to a Black client. Blockbusting a neighborhood is not the same as efforts to serve a single client or to promote integration, which many contemporaries hoped could be accomplished through less disruptive processes. Contemporaries were well aware that these issues were not completely cut and dry. Violating the NAREB code, for example, could lead to an accusation of being a blockbuster, but abiding by the NAREB code meant actively preserving segregation and engaging in discrimination, a lose-lose situation in the eyes of many at the time. Some realtors tried to draw lines in the sand: an “illegitimate” speculator was one who would violate the code of ethics by breaking a block, in contrast to a “legitimate speculator” who would only participate after a neighborhood was already changing from White to Black residents but who would not induce such a change.

In our view, the key feature of a blockbuster was the intentional targeting of a White-populated neighborhood for wholesale racial turnover by making representations about the entry of Black residents into the neighborhood. Typically, blockbusters encouraged this turnover by stoking racial panic and by using aggressive sales practices. Moreover, blockbusters also typically exploited new residents with high prices and risky financing schemes. Realty company officials rarely self-identified as blockbusters, but when accused of being blockbusters they often defended themselves by observing that few other realtors or banks were willing to work with Black home-seekers or were willing to promote integration. Some blockbusters themselves were subject to discrimination as members of certain religions or ethnicities, factors which led them into the self-employment in the realty business in the first place and that left them with little interest in enforcing segregation for others.

Part 1: Edmondson Village, Baltimore

In this section we provide a detailed descriptive analysis of a particularly well-documented episode of blockbusting that occurred in the Edmondson Village neighborhood of Baltimore, MD. Our aim is to show how blockbusting resulted in a change in ownership of almost all of the housing units in the neighborhood in about 15 years, the extent that blockbusters intermediated transactions, the types financing that were used in the transactions, and the subsequent foreclosure rates depending on blockbuster intermediation and financing. This analysis is feasible (yet time-consuming) in Baltimore due to Maryland's land records, which can be accessed as pdfs of hand-written ledgers for time period that is our focus. The analysis in this section provides important context for understanding the results that we present in the next section which estimate the causal effects of blockbusting across a number of large cities.

Background

Edmondson Village is a neighborhood on the western side of Baltimore, developed largely in the 1920s and 1940s as brick rowhouses, the predominant historic Baltimore building style. Edmondson Village is somewhat geographically isolated, with a large park to the north, a stream to the east, and a cemetery to the southwest. This geographic isolation was in fact a feature that proponents of segregation once stressed as helpful to preserving segregation; geographic barriers were viewed as a natural way to prevent the “encroachment” of people of a different race or nationality who lived in a nearby neighborhood. Coming into the 1950s, Edmondson Village was nearly completely White-populated and was viewed by contemporary real estate professionals as a desirable place for investment. For example, in a study of Baltimore assessing mortgage lending risk made in the late 1930s by the Home Owners' Loan Corporation (HOLC), Edmondson Village was rated a “B”—the second highest rating out of four—and was noted to be a “fairly new residential section of homogeneous development” inhabited by residents with white collar jobs and not by Black or foreign-born people.³

Despite the neighborhood's geographic isolation, blockbusters found a toehold in the southeast corner around the middle of the 1950s. That corner was the closest to the core urban neighborhoods where most Black families lived, to the east. Blockbuster activity proceeded

³ The HOLC's maps reflect the way that contemporaries thought about neighborhoods and urban housing markets.

roughly northwest into the rest of the neighborhood over the course of the next decade. As a result, the racial composition of the neighborhood changed to an exceptional degree in the late 1950s and early 1960s. In 1950, Edmondson Village had vanishingly few non-white residents, less than 1 percent according to census data on the two tracts that comprised the neighborhood. By 1970, the population was 96 percent non-white. Altogether, therefore, more than 19,000 of the roughly 20,000 residents of this neighborhood in 1950 must have left — a truly spectacular degree of population turnover. By the time of the 1968 Fair Housing Act, the racial transition in Edmondson Village had been completed. Blockbusters that operated in Edmondson Village before 1968 did so at a time when their activities were not yet illegal under federal law.

Contemporaries were well aware of the role of blockbusters in inducing these changes in Edmondson Village. For example, one of the most well-known realty companies operating in Edmondson Village was owned by Morris Goldseker. Figure 1 shows an advertisement from 1959 in which Goldseker posed a racially charged question: “Changing Neighborhood?” A group of community activists estimated that Goldseker’s network of shell companies had purchased at some point over 1,700 properties across Baltimore (Activists, Inc., 1971). In 1969, these activists sued Goldseker’s company, accusing it of over-charging the Black buyers, using shell companies to effectively create monopoly power, and violating new federal laws.⁴

Figure 1: Advertisement by a Baltimore realty company described by contemporaries as engaging in blockbusting



⁴ The case was eventually dismissed in 1972 when the defendants ran out of funds to proceed. Goldseker died the following year and left a fortune to a foundation established in his name. Several years later, the Baltimore *Sun* published an exposé titled “The Riddle of Morris Goldseker’s Legacy” on February 5, 1978. The article explores how contemporaries tried to reconcile the “unscrupulous exploiter who got rich at the expense of the poor” with the act of devoting those riches in posterity to helping the poor and disadvantaged.

Data

To measure the scope and timing of blockbuster activity in Edmondson Village, we digitize all transactions and financing instruments covering a set of properties within Edmondson Village from 1954 to 1976. These data come from the Baltimore City land records, a public registry in which property owners and lenders record information on ownership and liens. We have digitized the transactions for 2,817 properties in the core of the neighborhood.

The scope of blockbuster activity was very large. We find that about two-thirds of these properties were acquired by realty companies at some point from 1954 to 1976. These properties are colored red in the map in Figure 2. To identify blockbusters, we assume that if a realty company purchased multiple properties in Edmondson Village in this period that they were acting as a blockbuster. We identify 300 blockbusters in this way. Given the well-known presence of blockbusters operating in Edmondson Village at this time, it is unlikely that a realty firm would be purchasing multiple properties while not participating in that activity. The large scale of many realty company's activity is itself highly indicative of blockbusting; for example, one firm alone purchased about 180 of the properties in Edmondson Village. That said, since blockbusters used networks of shell companies to prevent potential sellers from being able to recognize the scale of their activities, many of the approximately 300 realty companies we identify may have been affiliated with one another. In addition, we note that many of the realty companies we identify in the data are also listed in Orser (1994) as known or suspected blockbusters.

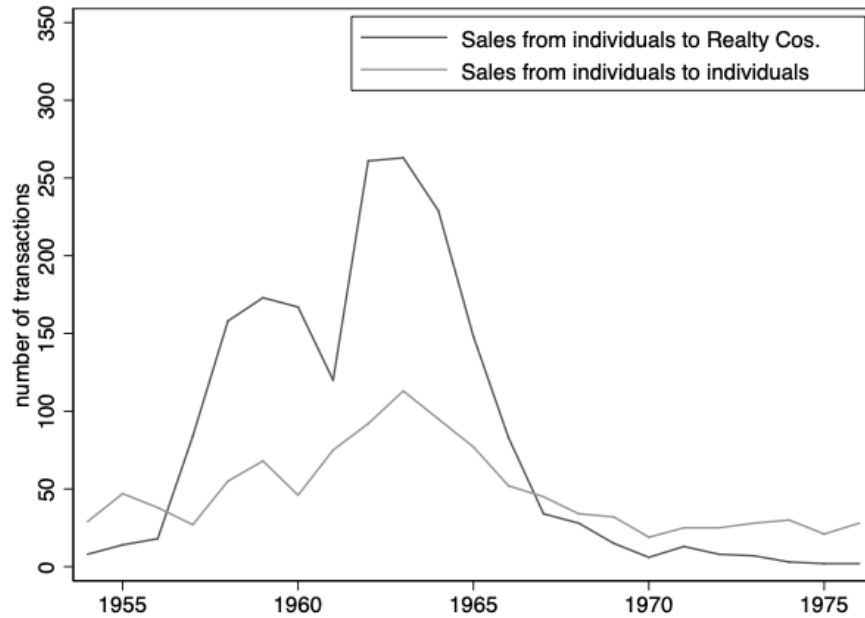
Figure 2: Map of realty company activity in Edmondson Village



Notes: Red-shaded properties were sold to realty companies (blockbusters), blue-shaded properties were sold directly to individuals, and gray-shaded properties were never sold during the period under study. White shaded parcels include properties not digitized as well as some nonresidential properties such as schools, churches, and small alley garage lots. Green areas are parks and cemeteries.

The timing of blockbuster activity was fairly compressed. Blockbusters were intensely active in Edmondson Village for about one decade, from the mid-1950s to the mid-1960s. A [video](#) of the timing of realty company activity is available in an accompanying publication (Bennett, Hartley, and Rose 2022). This temporal pattern is also shown in Figure 3. Purchases by realty companies peaked around 1963-1965 and declined steeply in the late 1960s.

Figure 3: Volume of sales by owners of properties in Edmondson Village



Of the remaining properties in Edmondson Village that were not sold to realty companies from 1954 to 1976, most were sold from one individual to another, and a small amount were never transacted, as shown in Table 6. If a property was sold exactly once between individuals, it is safe to assume that this sale involved a racial transition, given that the Black population of this neighborhood went from less than one percent in 1950 to 96 percent in 1970. Figure 3 shows the timing of sales between individuals; this timing largely tracks the timing of realty company activity. We omit from the graph properties that were sold multiple times between individuals. For such properties, it is difficult to know which sale commenced the ownership of a property by a Black family, since the race of property owners is not given in property records and we do not know of a feasible way of acquiring that information.

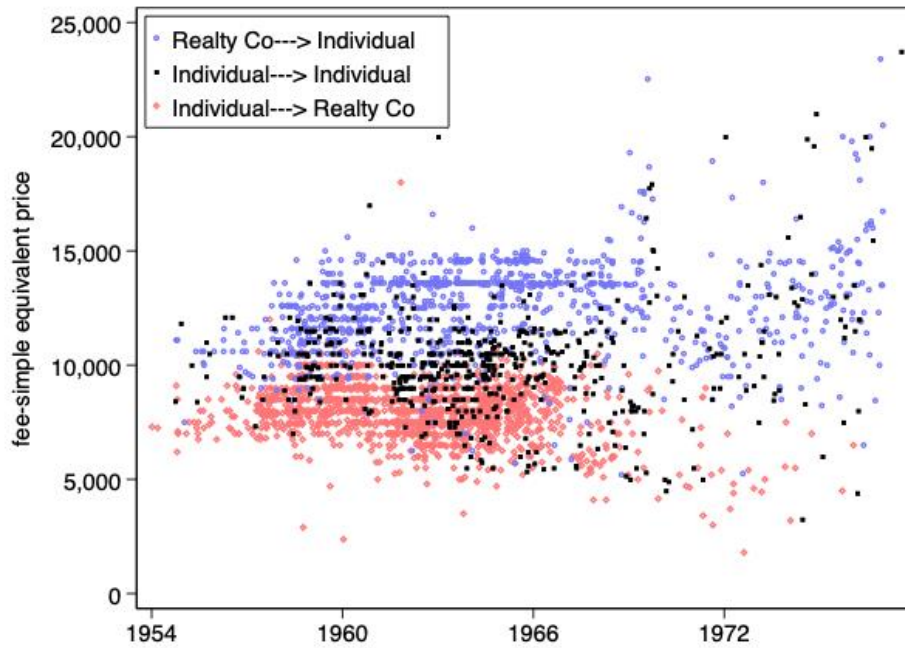
Table 1: Classification of ownership changes in Edmondson Village from 1954 to 1976

Type of ownership change	Number	Percent
Individual → Realty Co → Individual	1147	41%
Individual → Realty Co	693	25%
Individual → Individual, once	577	20%
Individual → Individual, multiple times	231	8%
No transactions	169	6%
Total	2817	100%

High prices

Blockbusters sold properties for high prices. In the short run, these sales may have temporarily lifted house values as measured by transaction prices. In the longer run, however, the high prices likely put financial burdens on new homeowners, leading to distressed sales or foreclosures and downward price pressure. Figure 4 shows the prices paid by realty companies to purchase properties (red dots) and the far higher prices at which they sold those properties (blue dots). This gap persists from the mid-1950s until the mid-1960s, at which point the supply of unsold properties in Edmondson Village was largely depleted and the variance of prices increased. Until that point, this figure shows that blockbusters had a remarkably iron grip on this neighborhood's housing market, controlling the flow of sales.

Figure 4: Transaction Prices in Edmondson Village

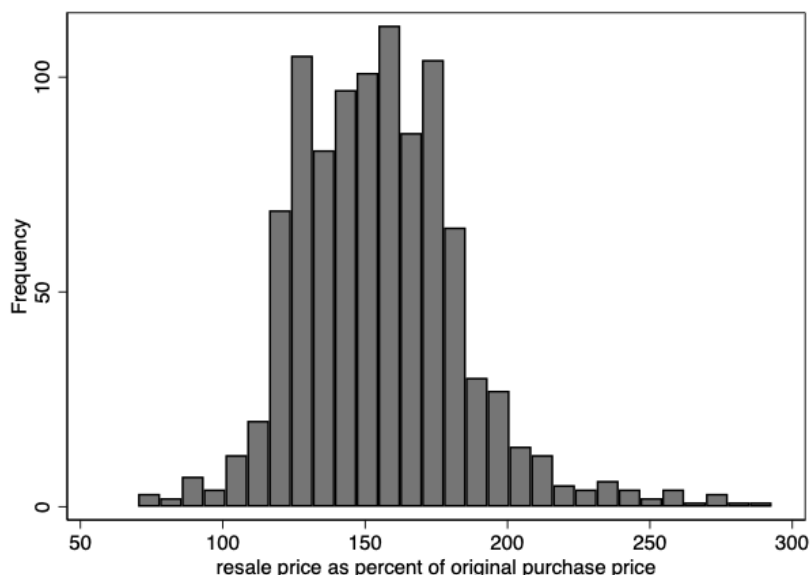


Sales between individuals include only properties that were transacted exactly once from 1954 to 1976, in an attempt to identify transactions that involved a racial transition.

For each property, we can link the purchase and sale of a property by a blockbuster and compute a markup. Overall, the average markup in the data is 55 percent. The distribution of these markups is shown in Figure 5. A community activist group in Baltimore, Activists, Inc., named this “The Black Tax,” the price paid by Black families to access better quality housing. To be clear, this markup is not necessarily all profit for the blockbuster, and indeed many realty companies noted that they had carrying costs from maintenance and financing.⁵

⁵ Here we have converted all prices into fee-simple equivalent prices. This is to account for ground rents, a financing mechanism peculiar to Baltimore (and some other limited parts of the US, including Pennsylvania and Hawaii, and some foreign countries as well). To this day, many Baltimore properties are owned subject to perpetual ground rents, requiring relatively small annual payments to the owner of the stream of ground rents. Ownership without such ground rents is known as fee-simple ownership, while ownership with ground rents is known as a leasehold estate. One way to think about these ground rents is as a financing mechanism, an infinite duration loan requiring only interest payments. Indeed, by state law these ground rents can be redeemed any time at a 6 percent capitalization rate, so that a \$60 ground rent could be extinguished with a payment of \$1000. In the data, about two-thirds of the original owners had no ground rent, but realty companies added ground rents to nearly every property they sold. For example, suppose a property was sold to a realty company for \$10,000 with no ground rent, and then resold for \$14,000 with an annual ground rent of \$60. The fee-simple equivalent price for the latter transaction would be \$15,000. Converting all prices to fee-simple equivalents allows for apples-to-apples comparisons of purchase prices, capitalizing the ground rent as part of the purchase price.

Figure 5: Distribution of markups between realty companies' sale and re-sale prices in Edmondson Village



In contrast, prices paid in transactions between individuals (and not involving realty companies) were much lower, shown in black squares in Figure 4. (These individual-to-individual transactions include only properties sold exactly once between individuals during the sample period, to limit to transactions in which presumably a White resident sold to a new Black resident.) Prices on these transactions usually lie roughly halfway between the sale and resale prices associated with blockbuster activity. The graph suggests that both seller and buyer gained from avoiding dealing with the blockbuster, as the seller generally received a higher price than they would have from a blockbuster, and the buyer a lower price. This raises the question of why so many others, especially on the sell-side, dealt with blockbusters. The answers suggested by Orser's (1994) study, among others, is a combination of racial prejudice, panic, and other blockbuster sales tactics.

We can analyze the prices more formally in a regression. Column 1 of Table 2 regresses the log sales prices paid to the initial owner on a dummy variable indicating whether a blockbuster is the purchaser. We find a -0.22 lower log price paid by blockbusters. We can add a couple of controls for other factors that might affect prices, to check whether the result is

driven by blockbusters purchasing less expensive properties. Keeping in mind that each block face of Edmondson Village tended to consist of identical brick row houses, we can add a block-face fixed effect and a dummy for whether a property is a corner unit (which tend to be more valuable given the extra natural light.) Adding these controls in column 2 makes little difference. In columns 3 and 4, we repeat this exercise by comparing the prices paid by new residents depending on whether they purchased from a blockbuster, finding a 0.23 to 0.25 log price higher price paid for properties purchased from blockbusters.

Table 2:

Sample:	(1) Sales by initial owner	(2) Sales by initial owner	(3) Purchases by new owner	(4) Purchases by new owner
Outcome variable:	log sale price	log sale price	log sale price	log sale price
1(blockbuster buys property)	-0.221*** (0.0108)	-0.204*** (0.0102)		
1(blockbuster sells property)			0.229*** (0.0111)	0.246*** (0.0112)
1(corner property)		0.0319*** (0.0112)		0.0201* (0.0120)
Constant	9.193*** (0.00994)	9.175*** (0.00907)	9.193*** (0.00994)	9.179*** (0.00934)
Fixed Effects		Block-face		Block-face
Observations	2,144	2,143	1,636	1,636
R-squared	0.216	0.504	0.248	0.498

Foreclosures

Foreclosure rates were extremely elevated in Edmondson Village. To understand the scale of foreclosures, first it is necessary to review the financing used by buyers to acquire properties in Edmondson Village. Buyers turned to both mortgage financing and installment contracts. We consider mortgage financing first and return later to installment contracts.

Foreclosure rates were exceptionally high on mortgages taken out after purchasing a property through a blockbuster. Table 3 shows particularly elevated rates of foreclosure on mortgages insured by the FHA or VA, of 23-27 percent. In contrast, foreclosure rates on mortgages taken out by new residents after buying from an individual (rather than a realty company) were far lower, around 1-3 percent. The underwriting of the mortgages arranged through blockbusters evidently was problematic.

Table 3: Foreclosure Rates by Financing Sources

Type of mortgage	Bought from Realty Co		Bought from Individual	
	Percent of mortgages	10-year foreclosure rate	Percent of mortgages	10-year foreclosure rate
VA	22%	23%	21%	1%
FHA	3%	27%	40%	3%
Conventional	75%	14%	39%	1%
Total	100%	17%	100%	2%

Note: sample restricted to properties bought before December 31, 1966, so that a 10-year foreclosure rate can be calculated.

The particularly high rates of foreclosure on FHA-insured and VA-insured mortgages may stem from a couple of factors. First, the FHA and VA allowed very high loan-to-value (LTV) ratios. Indeed, the LTVs of FHA and VA loans average 97 percent in the data, compared to 77 percent on conventional first mortgages (though higher leverage was possible through junior mortgages). If borrowers could not command the same re-sale prices that blockbusters commanded, they may have had trouble selling their properties to clear debts. Second, activists and Baltimore newspapers describe some fraudulent underwriting practices that particularly affected the VA program. Indeed, fraud appears to have been a serious problem in early VA-insured loans arranged by blockbusters in Baltimore. For example, in 1961, two realty company leaders were sentenced to two months each in federal prison for defrauding the VA. In league with a loan officer at a large Baltimore lender, these blockbusters encouraged buyers to make false statements about their income and debts and fabricated credit histories. The loan officer

apparently was known for never denying a loan.⁶ As a result of this fraud and its discovery, the volume of VA loans to buyers working with realty companies dropped precipitously in 1961.

The large amount of FHA- and VA-insured loans is somewhat surprising, since the FHA and VA are generally considered to have extended little insurance to Black borrowers before the late 1960s. FHA and VA insurance were highly desirable in this period because they required lenders to provide low interest rates and allowed very high LTVs. Edmondson Village evidently was not redlined by these programs, perhaps because it was outside of the core urban neighborhoods and was initially White-populated, even though it was filled with rowhouses that FHA underwriting guidelines generally discouraged. Overall, the participation of the FHA and VA in the racial turnover of these neighborhoods seems consistent with the account told by Kenneth Jackson (1985), though this account begins in 1966:

“In 1966 FHA drastically shifted its policies with a view toward making much more mortgage insurance available for inner-city neighborhoods. Ironically, the primary effect of the change was to make it easier for white families to finance their escape from areas experiencing racial change. At the same time, the relaxed credit standards for black applicants meant that home improvement companies could buy properties at low cost, make cosmetic improvements, and sell the renovated home at inflated prices approved by FHA. Many of the minority purchasers could not afford the cost of maintenance and FHA had to repossess thousands of homes. The final result was to increase the speed with which areas went through racial transformation and to victimize those it was designed to help. The only people to benefit were contractors and white, middle-class homeowners who were assisted in escaping from a distress position.” (pp. 214-215).

Installment contracts

⁶ Baltimore Sun, April 15, 1961, “Realty firm officials get terms, fines,” p. 32.

Installment contracts are a common feature of blockbusting episodes of the 1950s and 1960s. Installment contracts are not mortgages but rather an agreement to pay the sales price of a property in a series of installments. They are a particularly risky form of financing as they provide none of the statutory or common law protections afforded to mortgage borrowers. Installment contract buyers are essentially renters until they have paid off the entire debt, and thus can be evicted for missing a single payment. Moreover, upon eviction, such buyers lose all cumulated principal payments. As a result, previous scholars and activists have noted and castigated blockbusters for their use of installment contracts as a tool in exploiting new Black homeowners who lacked access to other forms of financing. Many activists charged blockbusters with misleading potential buyers by not conveying the risks associated with installment contracts or by leading buyers to believe they had mortgage loans.

Installment contracts are generally unobservable in property records because they do not involve the actual transfer of property deeds until all installments have been made, at which point it is the deed that can be observed rather than the prior installment contract. Therefore, activists and scholars have struggled somewhat to understand exactly how widespread their use was. Baltimore, however, in mid-1965 instituted a new city-level transfer tax and mandated that installment contracts be recorded and subjected to that tax. As a result, we can observe installment contracts after they begin appearing in land records in 1965. Some blockbusters appear to have only reluctantly paid them; many recorded instruments bear the words “paid under protest” penciled in at the top of the page. Though these recordings come in the late stages of the racial transition of Edmondson Village, they provide an unusual window into the use of installment contracts.

To gauge how widespread the use of installment contracts was, it is useful to focus on the brief period from mid-1965 to the end of 1966, before blockbuster activity fell off substantially. In this period, the data show that about 55 percent of 156 properties acquired by realty companies were subsequently sold via installment contracts. Most of these installment contracts were recorded fairly quickly after the acquisition, within a few months if not immediately. It is somewhat speculative to extrapolate backwards from these data, but the high level in this window generally suggests that the practice was quite common.

The foreclosure rate on installment contracts was quite elevated. 13 percent of these buyers involuntarily lost possession of their houses and, in the process, also lost the cumulative

sum of principal payments they had made. Several of these disposessions took place years after the initial contracts were struck, suggesting that the buyers lost very substantial amounts of cumulated principal payments. Otherwise, 35 percent of buyers eventually acquired their deeds, often by paying off the balance of the installment contract with a mortgage loan. Buyers could exercise this option at any time, and some did so right away (within 90 days) but others took years, with an average of about 4 years across all buyers. Lastly, the remaining 57 percent of installment contracts were still outstanding in 1976, 10-11 years after the agreements were struck. These buyers made weekly payments for years but never exercised the option to pay off the remaining balance through mortgage finance, a choice that's difficult to rationalize unless they faced severe limitations in access to conventional mortgage finance.

Part 2: Census tract analysis

We now turn to an analysis of census tracts in major U.S. cities, with the goal of identifying the impact of blockbusting on neighborhood housing markets.

Section A: Measuring Blockbusting

To identify neighborhoods that experienced blockbusting from the 1950s through the early 1970s we consult a large variety of sources—historical and modern news articles, books, and scholarly works. (Though blockbusting was illegal nationally after 1968, we nevertheless find reports of a limited number of cases afterwards. Blockbusting was also illegal in certain municipalities before 1968, though enforcement was mixed.) Focusing on the 60 largest cities as of 1950, we find narrative evidence of blockbusting in 46 of these cities.⁷

We identify neighborhoods where blockbusting occurred by evaluating whether narrative sources indicate activity by real estate investors who actively promoted racial panic among existing residents and who bought and resold houses to Black residents on a large scale. Racial turnover alone is not sufficient for an episode to be considered blockbusting, a point stressed by Smithsimon (2022). We also do not require any particular outcome to be associated with the episode, as not all blockbusting necessarily resulted in a wholesale population turnover.

⁷ The online appendix details the sources and methods we use to create this dataset. The 14 cities for which we did not find evidence of blockbusting are Akron, Birmingham, Fort Worth, Jacksonville, Jersey City, Providence, Salt Lake City, San Antonio, San Diego, Seattle, Saint Paul, Syracuse, Tulsa, and Worcester. Note that because we treat Saint Paul and Minneapolis as one city, Saint Paul's tracts remain as potential comparison areas.

We expect that our data collection methods underestimate the extent of blockbusting somewhat, given the inherent limitations in trying to find accounts, contemporary or ex post, of every blockbusting episode. First, some instances of blockbusting may not have been recorded or may have only been recorded in relatively obscure sources. However, if an episode of blockbusting was reported then we assume the account is correct. This data generation process will likely result in a type of measurement error known as one-sided misclassification. Even though we did not condition on impact in identifying episodes, journalists and scholars may have been less likely to report less impactful episodes. In the absence of heterogeneous treatment effects, one-sided misclassification means that our estimates may be viewed as a lower bound of the effects of blockbusting, because the control group may include areas where blockbusting occurred and had some effect. We loosely quantify the possible effects of one-sided misclassification in a robustness exercise. Second, there is also a chance that if there is heterogeneity in the degree to which a neighborhood is affected by blockbusting then we may only detect the more impactful episodes of blockbusting. In this case, our estimates may be thought of as capturing the effect of more salient or widespread blockbusting episodes. We investigated treatment heterogeneity by estimating specifications that allowed for our treatment effect to vary by the number of sources that we found mentioning blockbusting in a particular neighborhood (see online appendix for citations). While we found some evidence of larger increases in Black population share in neighborhoods with three or more citations to blockbusting, we didn't find larger effects in neighborhoods with more blockbusting citations⁸.

Section B: Census Tract Tabulations

From 1950 to 1990, our census tract data on decadal demographics and housing conditions comes from the decennial census. Tract boundaries change over time. To compare tracts over time, we use 1950 tract boundaries to create a panel of observations, creating tract relationship files between each decadal census tract boundaries and 1950 boundaries. Using these relationship files, we reweight the Census variables from each of the subsequent years to obtain estimated tabulations for 1950 tract equivalents.

⁸ Of the tracts in our sample where we find evidence of blockbusting, 52% have one citation, 20% have two citations, and 27% have 3 or more citations. We examined treatment heterogeneity by running specifications which added the interaction of census year and either a two-or-more citation indicator or a three-or-more citation indicator.

From 1910 to 1940, we geocode address information in the restricted access decennial full count U.S. Census data from IPUMS. This allows us to tabulate the microdata exactly to the 1950 tract boundaries for each of these earlier decades.

Section C: Linking neighborhood blockbusting to census tracts

We match the neighborhoods identified by narrative sources to census tracts by consulting geographic markers or neighborhood names provided by the historic sources. For neighborhood names, we use modern shapefiles provided by each city that delineate neighborhood boundaries as drawn today. The sources of these shapefiles are listed in the Appendix.

After we identify which neighborhoods experienced blockbusting in modern-day shapefiles, we overlay 1950 tract boundaries with the neighborhood shapefiles. We classify census tracts as having experienced blockbusting if more than one percent of their area is overlapped with a neighborhood where blockbusting was reported. Having a low threshold ensures that our comparison tracts will not contain neighborhoods where we have evidence that blockbusting occurred. As robustness checks, we tried using cutoffs of 15% and 50% overlap and found that for our baseline specification, the point estimates were quite similar for the main outcomes of Black population share and log home values, though the standard errors were slightly larger. In some cities, e.g. Chicago, neighborhoods are larger than census tracts, allowing for better classification (Figure 7). In other cities, e.g. Baltimore (Figure 6), neighborhoods are smaller than census tracts. In this case, by assigning an indicator for whether blockbusting occurred at the tract-level we may be overstating the geographic extent of blockbusting. However, in most cities census tracts have a smaller area than neighborhoods, on average.

Though we identify blockbusting in 46 cities, in our main analysis, we exclude eleven of these cities. Six are excluded because we could not identify a specific neighborhood in which the blockbusting occurred; four additional cities are excluded because they did not have tract boundaries yet in the 1950 Decennial Census or because we were unable to obtain 1950 Census tract data for them; and one final city is excluded because the reference to blockbusting

described an event prior to the 1950s.⁹ Thus, our final sample contains 35 cities. Table 4 lists these cities and the 613 census tracts in which we identified blockbusting as having occurred.

Table 4: Blockbusting tracts by city and decade

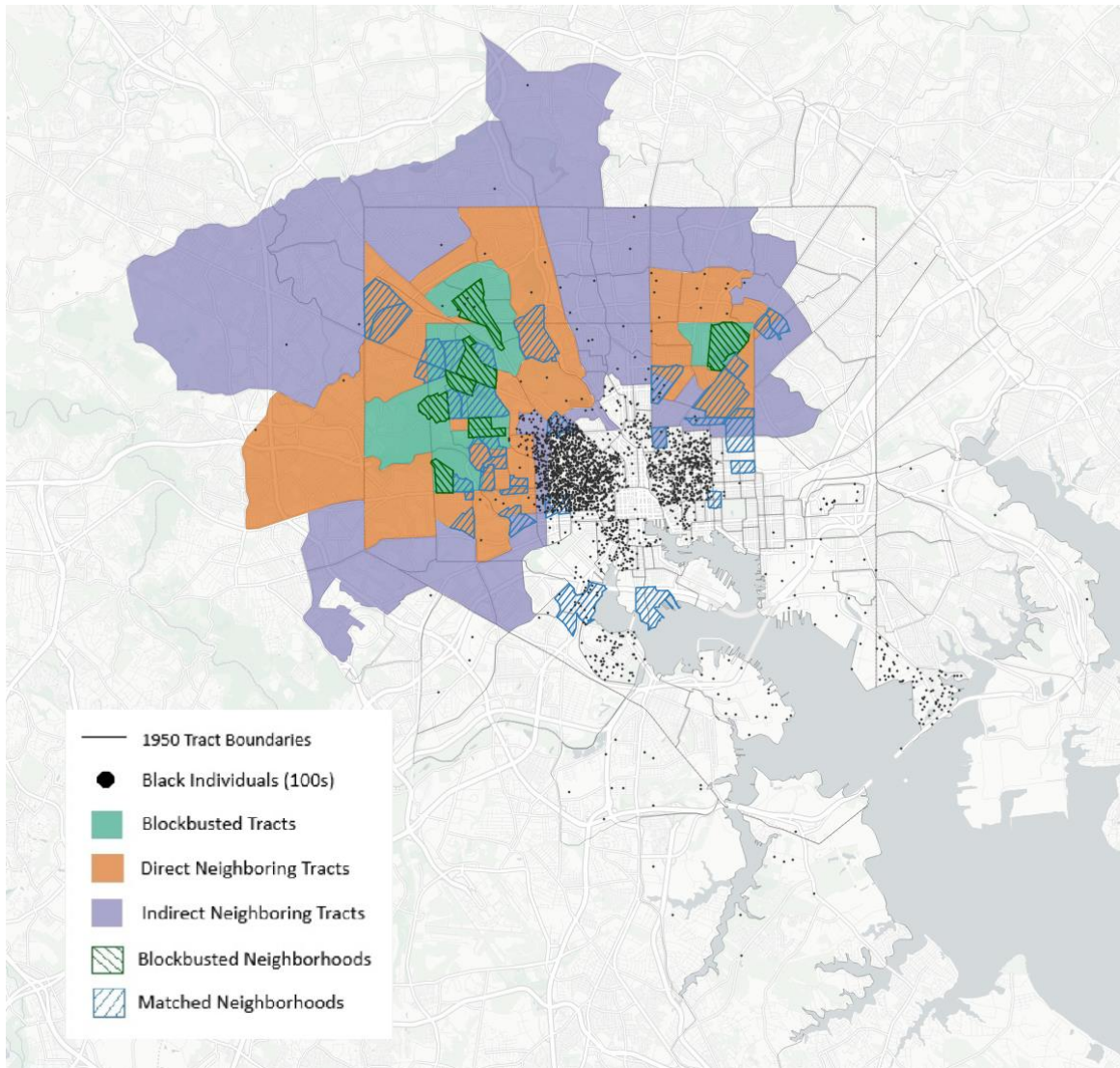
City	Tracts	Neighborhoods	Mean Neighborhood Size (square miles)	Mean Tract Size (square miles)
Atlanta	6	7	1.31	1.69
Baltimore	15	7	0.29	0.58
Boston	30	3	1.88	0.32
Buffalo	15	5	1.16	0.60
Chicago	129	13	3.00	0.28
Cincinnati	31	7	1.06	0.86
Cleveland	22	4	2.30	0.41
Columbus	4	1	6.16	5.03
DC	1	1	1.49	0.64
Denver	5	6	1.98	1.35
Detroit	10	7	0.67	0.38
Flint	4	6	0.56	1.59
Hartford	1	1	1.06	0.44
Houston	7	2	6.79	11.10
Indianapolis	5	1	4.07	1.36
Kansas City	31	41	1.30	0.88
Long Beach	2	19	0.42	0.87
Louisville	22	9	0.68	0.48
Memphis	2	2	5.21	1.24
Milwaukee	7	1	0.51	0.56
Minneapolis	8	1	0.66	0.48
NYC	146	13	1.55	0.12
New Haven	2	1	0.15	0.68
Norfolk	2	2	0.33	0.81
Oakland	1	1	0.36	0.74
Oklahoma City	1	1	68.99	1.77
Omaha	19	1	8.49	0.65
Philadelphia	33	10	0.90	0.35

⁹ The six cities in which we identified blockbusting as occurring, but in which we could not identify specific neighborhoods where that activity occurred, are Dallas, Dayton, Des Moines, Nashville, New Orleans, and Wichita. Grand Rapids and Youngstown had not yet been tracted. Los Angeles and Newark were tracted in 1940, but tract level data for Los Angeles were only partially available in 1950 and were not available at all for Newark in 1950. Miami is excluded because the reference to blockbusting is regarding the 1940s.

Pittsburgh	6	1	0.08	0.29
Portland	12	15	1.16	1.11
Richmond	6	5	0.42	0.81
Rochester	11	1	0.89	0.41
San Francisco	5	1	2.15	0.40
St Louis	8	2	0.71	0.51
Toledo	4	1	2.62	0.70

The census tracts that we identify account for about 10 percent of all census tracts that did not have a majority Black population in 1950 in these cities. This figure may be an overestimate if blockbusting only occurred in some of the tracts that comprised a neighborhood where we have found evidence of blockbusting. Alternatively, the 10 percent figure could be an underestimate of the extent of blockbusting if the historical record available to us does not comprehensively account for all such episodes.

Figure 6: Baltimore blockbusting



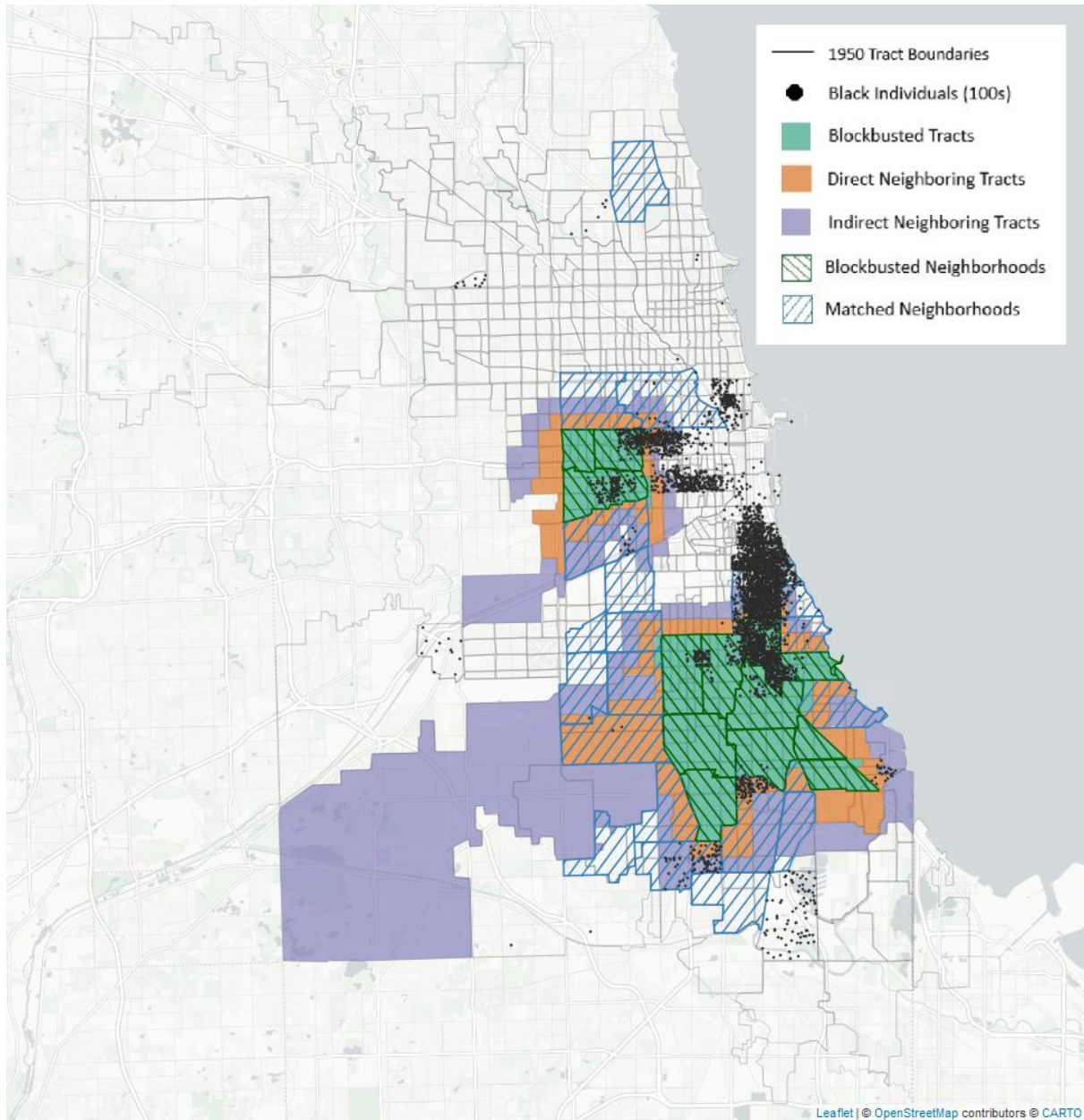
As an example, Figure 6 is a map of Baltimore indicating the Black population in 1950, the neighborhoods and tracts where blockbusting occurred, and our baseline and robustness comparison areas. The gray lines denote 1950 boundaries of census tracts. The downward-sloping green cross hatched polygons are neighborhoods we identify as having experienced blockbusting. They are overlaid upon census tracts shaded green that we use in our analysis. Direct neighboring tracts (our baseline comparison areas) are shaded in orange, and indirect neighboring tracts (our first set of robustness control areas which we also refer to as second ring tracts) are shaded in purple. The Black population in 1950 is represented by the dots, with every 100 Black individuals represented by one dot. Lastly, we identify the neighborhoods most similar to the neighborhoods that experienced blockbusting (our second robustness control

areas), they are indicated by upward-sloping blue crosshatching. In Baltimore, the neighborhoods are typically smaller than the 1950 census tracts.

In Baltimore, we identify blockbusting in 15 out of 134 census tracts (11 percent) that did not have a majority Black population in 1950. This includes the one tract that comprised Edmondson Village, as well as tracts in some nearby neighborhoods known as Forest Park, Ashburton, Liberty Heights, Walbrook, and Windsor Hills, and some in other parts of the city such as Montebello, Northwood, and Ellwood Park. We use the deed and mortgage records discussed earlier to check whether we are capturing every neighborhood with blockbusting in Baltimore. In those data, after 1965 we are able to identify the use of installment contracts. We find that the geographic pattern of installment contract usage overlaps very closely with the neighborhoods where blockbusting occurred as identified from narrative sources.

As another example, in Chicago, we identified blockbusting in 130 out of 700 census tracts (19 percent) that did not have a majority Black population in 1950. This large share of tracts is consistent with many historical discussions of widespread blockbusting in Chicago, including infamous episodes in neighborhoods such as Lawndale and Englewood. Figure 2 shows a map of the 1950 census tract boundaries for Chicago. The map shows two contiguous areas of blockbusting activity, one on the West Side and the other on the South Side.

Figure 7: Chicago blockbusting



Summary Statistics

Table 5 displays characteristics of the census tracts in our samples in 1950. We limit the sample to tracts that did not have a majority Black population in 1950.

Table 5: Descriptive statistics in 1950

	Full Sample				Restricted Sample (Blockbusting & Neighboring Tracts) (Unweighted)				Restricted Sample (Blockbusting & Neighboring Tracts) (Weighted)	
Variables (1950)	Mean	SD	Difference*	P-Value	Mean	SD	Difference*	P-Value	Difference*	P-Value
Log Rent	6.191	0.320	0.008	0.565	6.193	0.308	0.010	0.586	0.007	0.730
Log House Value	11.59	0.396	-0.041	0.016	11.561	0.360	-0.021	0.317	-0.001	0.970
Share Black	0.036	0.087	0.008	0.024	0.041	0.090	0.006	0.271	-0.001	0.911
Home Ownership Rate	0.457	0.241	-0.006	0.590	0.462	0.236	-0.020	0.143	-0.003	0.877
Vacancy Rate	0.023	0.038	-0.004	0.012	0.021	0.029	-0.003	0.085	0.000	0.977
Population Density (thousand people per square mile)	24.09	24.82	3.431	0.001	24.000	22.694	6.546	0.000	0.829	0.611
Distance from Nearest Majority Black Tract (miles)	2.653	2.243	-1.254	0.000	1.758	1.524	-0.414	0.000	-0.055	0.616
Distance from Nearest Highway (miles)	0.988	0.825	0.225	0.000	1.179	0.846	0.008	0.865	-0.010	0.865
Share of Area HOLC Rated “D”	0.238	0.353	0.048	0.001	0.245	0.356	0.075	0.000	-0.001	0.971
Foreign-Born Population Share	0.135	0.083	-0.001	0.750	0.124	0.082	0.020	0.000	0.002	0.690
Distance from Nearest Public Housing Building built before 1970 (miles)	3.227	7.110	-1.438	0.000	2.511	5.804	-1.138	0.001	-0.438	0.494
Share of Area covered by Urban Renewal Project	0.055	0.199	0.026	0.002	0.079	0.235	-0.003	0.846	0.025	0.161
Observations		4693				1162			1162	
Cities		35				35			35	

The left panel of Table 5 displays in the first two columns the mean and standard deviation of key characteristics for the full set of non-majority Black population tracts; the next column is the difference in means between the blockbusting tracts and the other tracts within city limits in the sample. We also report the p-value from a statistical test of the difference in means. The middle panel displays the same statistics but limits the sample to only tracts where blockbusting occurred and tracts that are adjacent to those tracts. Finally, the right panel uses the same sample as the middle panel, but we reweight the sample using inverse probability weights designed to make the blockbusting and neighboring tracts more comparable in terms of their 1950 characteristics. We describe the estimation of the weights in the next section.

In the full sample (left panel) the difference in means tests show that the blockbusting tracts had roughly 4 percent lower home values, a 0.8 percentage point higher Black population share, a 0.6 percentage point lower homeownership rate, a 0.4 percentage point lower vacancy rate, roughly 3,400 more residents per square mile, were 1.25 miles closer to the nearest majority Black tract, were 0.23 miles farther from the nearest interstate highway, had 5 percentage points more area located in a neighborhood that received a “D” grade in Homeowners’ Loan Corporation residential security maps that were created in the mid-to-late 1930s, had a 0.1

percentage point lower Foreign Born Share.¹⁰ In addition, blockbusting tracts were closer to public housing (a large share of which was built during the 1950-1970 period), and had 2.6 percentage points more land area covered by urban renewal projects (also largely constructed during the 1950-1970 period).¹¹

The fact that blockbusting tracts tended to be near tracts with a majority Black population share in 1950 is consistent with historical sources which emphasize that population movement of Black residents tended to happen especially in border areas around existing Black neighborhoods. For example, a report on Chicago housing conditions in 1959 stated that the only places that the city's Black residents were able to find additional housing was at the periphery of existing Black neighborhoods, "through the purchase of adjacent white homes and the transition of the block or neighborhood" from White to Black residents. These border neighborhoods were also the most common sites of racial violence in the 1950s (US Commission on Civil Rights 1959a, p. 434).¹²

The tracts that are adjacent to blockbusting tracts (middle panel) appear to be a slightly better comparison group, with fewer statistically significant differences in 1950 characteristics. However, the blockbusting and comparison samples look most comparable once the inverse probability weights have been applied (right panel).

The restricted sample tracts including blockbusting and adjacent tracts have 1950 characteristics that are quite similar to the full sample. Unsurprisingly, the restricted sample tracts are closer on average to the nearest majority Black population tract at 1.8 miles rather than 2.7 miles for the full sample. Interestingly, these tracts have a slightly lower vacancy rate than the full sample.

¹⁰ Three cities in our sample (Minneapolis, Portland, and Rochester) did not have any Census tracts with a Black population share above 50% in 1950. For these cities, we use the distance to the nearest Census tract with a Black population share above 35%. We calculate the share of the area of each of the 1950 Census tracts in our sample that fall in neighborhoods that were assigned grades of A, B, C, and D on the Homeowners Loan Corporation residential security maps created in the 1930s. We obtained shape files of the georeferenced maps from the Mapping Inequality website from the University of Richmond's Digital Scholarship Lab (Nelson et. al. 2021).

¹¹ We calculate public housing location and numbers of units using HUD's database ([link](#)). The HUD database contains records of public housing buildings that existed on the day that we accessed it in October 2021. To focus on the period of interest we have dropped buildings that were constructed in 1970 or later. We find similar results if we do not drop these buildings. We calculate the share of the area of each of the 1950 Census tracts in our sample that fall within urban renewal project areas. We obtained shapefiles of georeferenced urban renewal project maps from the Renewing Inequality website from the University of Richmond's Digital Scholarship Lab (Digital Scholarship Lab 2019).

¹² This pattern is consistent with the method of maintaining racial segregation that the Chicago Real Estate Board espoused in 1917, which we quote in footnote 4.

Characteristics of tracts that were subjected to blockbusting

Next, we investigate how the economic and demographic characteristics of Census tracts in 1950 correlate with blockbusting occurring in the tract over the next two and a half decades. We use predictions from these estimates to form inverse probability weights to help balance the sample. Table 6 presents probit model estimates where the dependent variable is $1\{Blockbusting_{i,c}\}$ an indicator of whether the Census tract experienced blockbusting and the explanatory variables are the set of 1950 Census tract characteristics presented in Table 2.¹³ Specifically, we estimate

$$(1) \quad 1\{Blockbusting_{i,c}\} = \beta x_{i,c} + \alpha_c + \epsilon_{i,c}$$

where $1Blockbusting_{i,c}$ is an indicator of whether blockbusting occurred in tract i , in city c , α_c is a city fixed effect, $x_{i,c}$ are the characteristics of the tract in 1950, β is a vector of coefficients to estimate, and $\epsilon_{i,c}$ is an error term. The first column presents results using the full sample of census tracts and the second column presents results for the restricted sample where the comparison group is limited to tracts that are adjacent to blockbusting tracts. In the first column, the coefficient of -0.36 on distance from the nearest majority Black tract implies that tracts one mile farther away from a majority Black tract are roughly 36 percentage points less likely to experience blockbusting, signaling a connection between spatial proximity to a majority Black neighborhood and the likelihood of experiencing blockbusting. In the second column, there are several coefficients of note. The coefficient on distance to the nearest majority Black tract is lower in magnitude, compared to the first column, implying that the decrease in the probability of blockbusting is likely non-linear, falling more slowly with distance to the nearest majority Black tract in the neighboring tracts sample than in the full sample.

¹³ We do not include distance to public housing and share covered by urban renewal projects to avoid controlling for endogenous regressors since a large share of public housing and urban renewal projects were put in place in the 1950-1970 period. Reassuringly, the rightmost column of Table 2 reveals no statistically significant differences in means even for these characteristics that were not included in the propensity score model.

Table 6: Predicting neighborhoods that experienced blockbusting from 1950 characteristics

VARIABLES (1950)	Full Sample	Restricted Sample (Unweighted)
Distance from Majority Black tract (miles)	-0.362*** (0.0894)	-0.0832 (0.0599)
Share Black	0.239 (0.561)	0.680 (0.463)
Home Ownership Rate	0.740*** (0.240)	0.489 (0.341)
Log Rent	0.638 (0.448)	0.571 (0.453)
Log House Value	-0.258 (0.181)	-0.279 (0.235)
Vacancy Rate	-1.102 (1.045)	-0.996 (0.764)
Population Density (thousands of people per square mile)	0.00114 (0.00208)	0.00349 (0.00295)
Distance to Nearest Highway (miles)	0.215 (0.160)	0.0403 (0.0805)
Share of Area HOLC Rate "A"	-0.595 (0.536)	-0.979 (0.642)
Share of Area HOLC Rate "B"	0.227 (0.186)	0.488*** (0.186)
Share of Area HOLC Rate "C"	0.0470 (0.189)	0.150 (0.173)
Share of Area HOLC Rate "D"	0.163 (0.179)	0.482** (0.201)
Northern European Population Share	-0.222 (1.817)	4.329 (2.964)
Eastern European Population Share	0.319 (1.000)	1.143 (0.801)
Southern European Population Share	0.462 (0.929)	1.671 (1.269)
Other Foreign-Born Population Share	-5.984 (4.580)	-3.918 (6.180)
Constant	-1.981 (2.419)	-0.997 (1.938)
Observations	5,586	1,331
Cities	35	35
R-Squared	0.179	0.0823

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Log Rent and Log House Value adjusted for inflation

Empirical Approach

Our basic empirical strategy is to run panel regressions to trace out the effects of blockbusting over time on a set of outcome variables. We estimate the following specification

$$(2) \quad y_{i,c,t} = \sum_{\tau=1930, \tau \neq 1950}^{1990} \gamma_{\tau} 1\{Blockbusting_{i,c}\} 1\{t = \tau\} + \delta_i + \varepsilon_{i,c,t}$$

where $y_{i,c,t}$ represents an outcome in tract i , city c , and year t . $1\{Blockbusting_{i,c}\}$ is an indicator for whether blockbusting occurred in tract i in city c . γ_{τ} is a tract fixed effect, and $\varepsilon_{i,c,t}$ is an error term. For all of the estimates that follow, standard errors are clustered at the city-level to allow for correlation of the error terms within cities and across decades.

Results

We focus on decadal outcomes from 1960 through 1990, using data from 1930 through 1950 to estimate pre-treatment trends. In the appendix, for a subset of the tracts in our sample, we extend the analysis further back in time using data from 1910 and 1920. In this section, using our baseline specification, we examine a number of different outcomes, including the Black population share, population density, log rents, log home values, and home ownership rates.

Figures 8 through 12 plot the coefficients from three different sets of estimates: unweighted estimates from the full sample (blue line), unweighted estimates from the restricted sample (orange line), and inverse probability weighted estimates from the restricted sample (red line). The figures show 95% confidence intervals for the latter estimates (red bars). The weighted estimates from the restricted sample are also presented in Appendix Table 1.

Figure 8 plots our estimates of the effect of blockbusting on the Black population share over time. The estimates using the full sample attribute about a 15 percentage point increase in the Black population share to blockbusting by 1960, which rises to over 40 percentage points by 1980 and 1990. The full sample uses all of the non-majority Black census tracts in the neighborhoods where we did not find narrative evidence of blockbusting as a comparison group for the blockbusting neighborhoods. One potential concern is that we may be comparing blockbusting tracts to distant tracts in other parts of the city that were on different trajectories.

The restricted sample mitigates this concern by using only tracts that are adjacent to the blockbusting neighborhoods. The restricted sample shows a smaller effect of blockbusting on the Black population share (orange line). These effects are reduced a bit further when the restricted sample is weighted using inverse probability weights (red line). These estimates show that blockbusting is associated with an increase in the Black population share of 10 percentage points in 1960 which rises to about 18 percentage points by 1980 and then falls slightly to about 16 percentage points in 1990. Furthermore, our estimates for 1930 and 1940 are economically small in magnitude and are statistically indistinguishable from zero, indicating that the blockbusting neighborhoods had similar trends in Black population share as the comparison tracts (whether all tracts or the restricted tracts and whether or not the restricted tracts are weighted to be more comparable to the blockbusting tracts or not). The lack of differential pre-trends lends support for the assumption that the trajectories in blockbusting and comparison tracts would have evolved similarly had blockbusting not occurred. It should be noted that the propensity score and inverse probability weights are estimated using only 1950 characteristics, not levels or trends from 1930 or 1940. For this reason, it is reassuring to see a lack of pre-existing trend from 1930 through 1950.

Figure 8: Estimated effect of blockbusting on Black population share

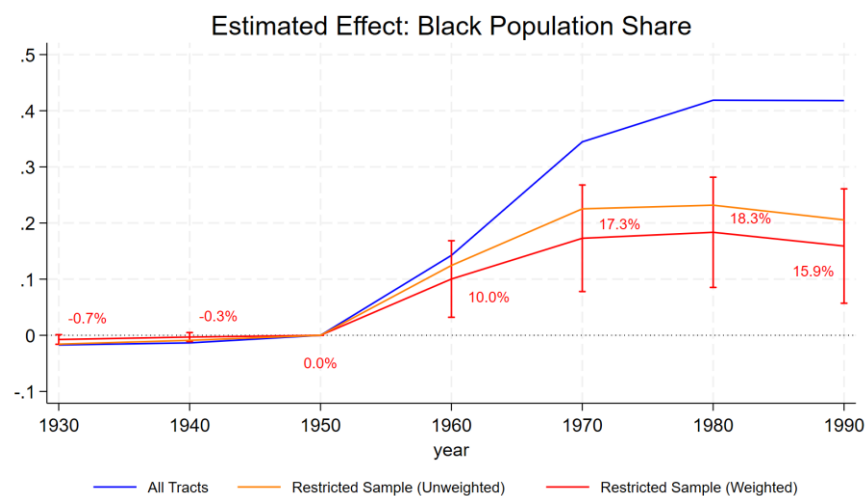


Figure 9 plots our estimates of the effect of blockbusting on population density. Our preferred specification, the weighted version of the restricted sample, shows that blockbusting increased population density by about 1.1 thousand people per square mile in 1960 and 1970

(though not statistically different from zero). This effect falls back to zero by 1980 and falls a bit further (though remains statistically indistinguishable from zero) in 1990.

Figure 9: Estimated effect of blockbusting on population density

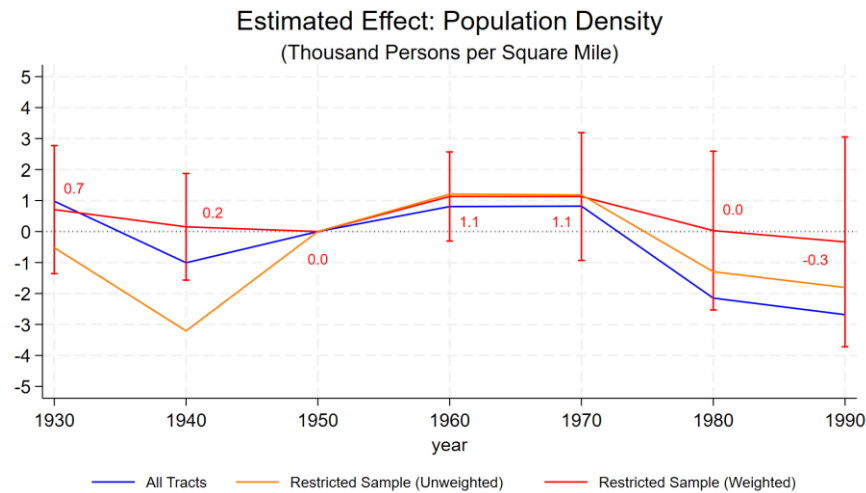
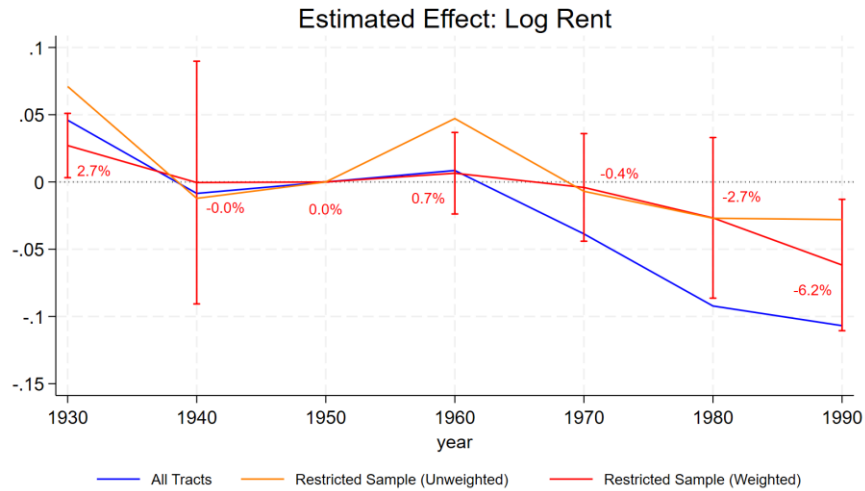


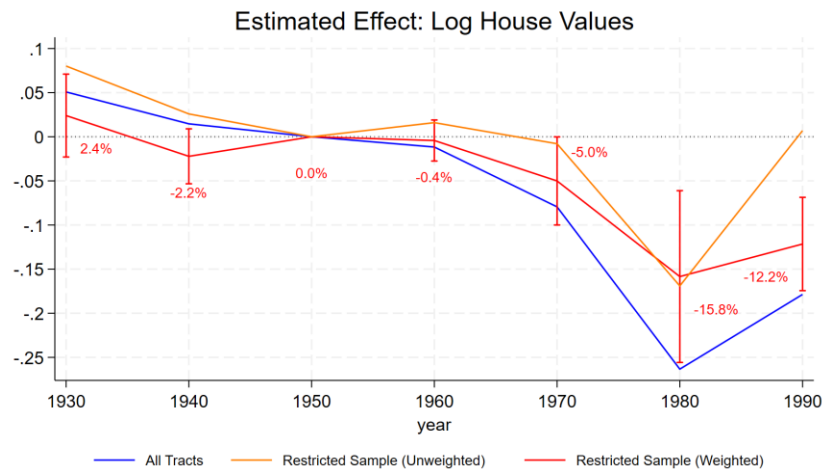
Figure 10 shows the effect of blockbusting on the log of average rents. Our preferred estimate shows a slight decrease in rents in 1970 of 0.4 percent, though not statistically significant. This effect increases in magnitude to a statistically significant -6.2 percent by 1990. The figure also shows a marginally statistically significant 2.7% higher rent in 1930. While this could be seen as slight evidence of a pre-trend in rent, it is reassuring that the point estimate for 1940 is zero. It is also worth noting that the robustness exercises that follow the main results do not show signs of a pre-trend in log rent (or any other outcome).

Figure 10: Estimated effect of blockbusting on rents



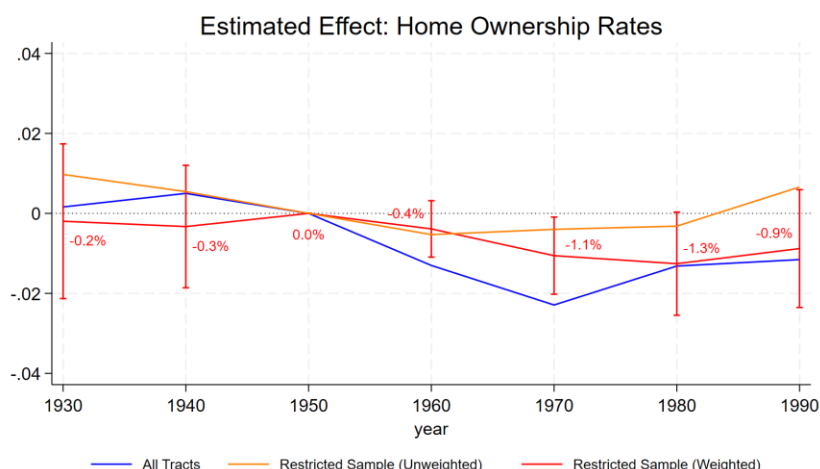
The effect of blockbusting on home values (Figure 11) shows a more dramatic decline than for rents, although with a bit of a rebound in 1990. Blockbusting is associated with a 0.4 percent drop in home values in 1960. This effect increases to a statistically significant –15.8 percent by 1980 as the population density falls back to its pre-blockbusting level. The effect of blockbusting decreases in magnitude to about 12 percent by 1990.

Figure 11: Estimated effect of blockbusting on house values



Finally, blockbusting has a statistically discernable impact on the home ownership rate (Figure 7) in 1970. The point estimates of our preferred specification indicate that a 1.1 percentage point drop in the home ownership rate can be attributed to blockbusting in 1970, and a marginally statistically significant –1.3 percentage points in 1980.

Figure 12: Estimated effect of blockbusting on home ownership rates



Overall, blockbusting led to a higher Black population share, an initial increase in population density that faded over time, and lower housing values and lower rents in 1980 and 1990. These results are consistent with the idea that housing values eventually came under pressure in blockbusting neighborhoods, given the impact of foreclosures and possibly depreciation due to homeowners being unable to afford maintenance on their properties.

Robustness

In this subsection we show that our main results are robust to extending our pre-trend estimates back to 1910 and altering the comparison group to either a farther ring of tracts to avoid contamination from spillover effects, or a set of neighborhoods with similar characteristics to the blockbusting neighborhoods rather than a ring of tracts. We also consider misclassification error and an estimate that is robust to within-city general equilibrium effects, using a comparison group of neighborhoods in cities without blockbusting that had similar characteristics to neighborhoods that did experience blockbusting in cities where blockbusting occurred.

Our first robustness exercise is to supplement our panel dataset with data from the 1910 and 1920 decennial censuses and to re-estimate our baseline specifications using this longer panel. These results are shown in Appendix A1. For this exercise, we use population-based outcome variables since they are available in every census and measured in a consistent manner. The other limitation of extending the sample back to 1910 is that only 816 of the 1,156 tracts in our baseline sample have data going all the way back to 1910. Figure A1 Panel A shows our estimates of the effect of blockbusting on the Black population share. The pre-trend estimates

show point estimates that are very close to zero with small confidence intervals that all include zero. The estimates from 1960-1990 are quite similar to our baseline results shown in Figure 8. Figure A1 Panel B shows our estimates of the effect of blockbusting on population density. As is the case with our baseline results for population density, none of the estimates are significantly different from zero. Reassuringly, the point estimates in the pre-period are all very close to zero.

Our second robustness exercise is motivated by a potential concern that neighboring tracts without blockbusting may be contaminated by spillover effects from the blockbusting tracts. Instead of using the tracts adjacent to blockbusting neighborhoods as the control group we use the next ring out, the tracts that are adjacent to the adjacent tracts (which we refer to as the indirect neighboring tracts or second ring tracts) of the treated tracts as a comparison group. These are the tracts with purple shading in Figures 6 and 7. With the exception of the 1960 estimate, we find even larger increases in black population share (Appendix Figure A2 Panel A) than for our baseline comparison group, consistent with the possibility that these indirect neighboring tracts mitigate contamination of the comparison group by spillovers that may occur when using the direct neighboring tracts as a comparison group. Blockbusting causes larger increases in population density in 1960 and 1970 using this comparison group, becoming marginally statistically significant at 2.4 percentage points in 1970 (Appendix Figure A2 Panel B). The effect on rents shows a similar decrease in point estimates over time as the baseline comparison group, but with larger confidence intervals. The effects on home values appear quite similar although they are slightly more negative than for the baseline comparison group, but do not appear until 1980 and 1990. Finally, there is no measurable effect on the homeownership rate. Overall, the results are robust to switching to the indirect neighbor comparison group with slight movements in statistical significance providing evidence that spillover effects are unlikely to bias our baseline estimates. Reassuringly, the pre-trend estimates are economically small and not statistically distinguishable from zero.

Our third robustness exercise is motivated by the concern raised in Pollmann (2020) that when estimating the effects of a spatial treatment, comparison groups formed using rings around the treated units may yield biased estimates. The idea is that if the placement of the treatment in space is plausibly exogenous conditional on characteristics of the treated place, then a good comparison would be another shape like the treatment group with similar characteristics rather

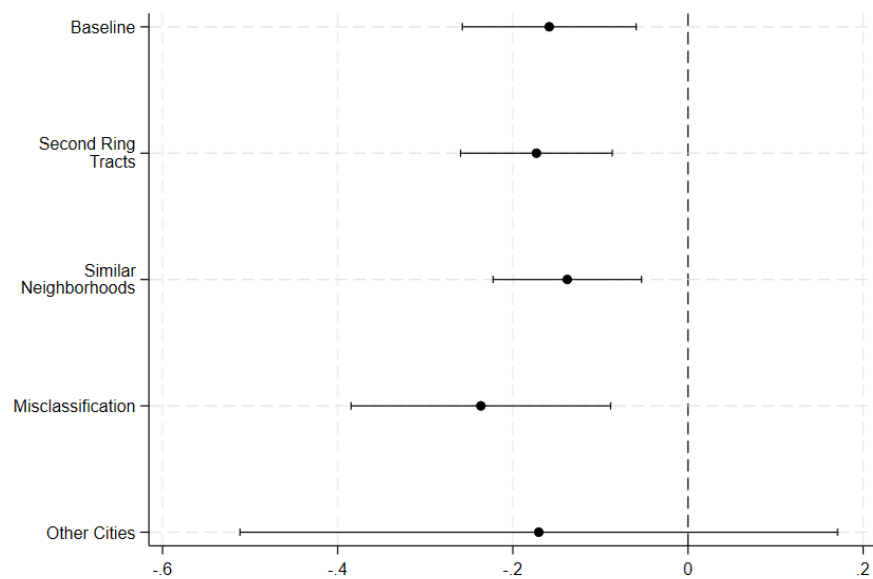
than a ring. Pollmann's solution is to search for sets of spatial units that border each other with similar characteristics using a neural net approach adapted from digital image processing.

In our application, a convenient way of implementing Pollman's idea is to use neighborhoods to provide a way of grouping spatial units (Census tracts) that are near each other. This fits well with our measure of treatment (blockbusting) which is also measured at the neighborhood-level. We start by estimating the propensity of neighborhoods to experience blockbusting by running a probit using the means and standard deviations of the same 1950 tract characteristics that we use in our tract-level propensity score estimation. Next, we identify a set of nearest-neighbor comparison neighborhoods with the closest propensity scores to the blockbusting neighborhoods. We calculate the propensity for blockbusting by neighborhood by aggregating the census tract characteristics to the neighborhood-level by area-weighting. Then, we use nearest neighbor propensity score matching to find the five most similar neighborhoods to each treated neighborhood. For Chicago and Baltimore, the selected nearest neighborhoods are the blue crosshatched neighborhoods shown in Figures 6 and 7. After selecting the group of comparison neighborhoods, we use the following methodology to classify the tracts as belonging to a treated neighborhood or comparison neighborhood. First, if more than one percent of the tract overlaps with a treated neighborhood, the tract is classified as treated. With the remaining tracts, if more than one percent of the tract area overlaps with a "nearest matched" neighborhood, we classify it as a comparison neighborhood.

The results of the third robustness exercise look quite similar to those of the second robustness exercise. The one exception is that the effect of blockbusting on the homeownership rate (Appendix Figure A3) shows a slightly larger statistically significant decrease of 1.6 percentage points in 1970. This result is in line with our baseline result which shows a decrease of 1.1 percentage points in homeownership rate in 1970. Again, the similarity of these results to our baseline results gives us some reassurance that using the tracts that are adjacent to the blockbusting neighborhoods as a comparison group is providing a reasonable counterfactual to outcomes that may have occurred in the blockbusting neighborhoods had blockbusting not occurred. Furthermore, the pre-trend estimates are economically small and not statistically distinguishable from zero. An important note for this third robustness exercise is that the true standard errors are likely larger. The standard errors that we report do not take the sampling

variation in estimating the propensity score at the neighborhood-level to find comparison neighborhoods with similar characteristics as the blockbusting neighborhoods into account.

Figure 13: Robustness checks: estimated effect of blockbusting on 1980 log housing values under different specifications



For ease of comparison, Figure 13 plots the estimated effect of blockbusting on 1980 log housing values for our baseline specification and four robustness specifications. The first estimate is from the baseline, and the second and third estimates are from the second and third

robustness exercises discussed above.¹⁴ The fourth estimate is from an additional robustness exercise which examines the possible effects of bias due to one-sided misclassification on our estimates. One-sided misclassification arises due to the fact that we are measuring the occurrence of blockbusting from narrative sources. When we find a written record of blockbusting having occurred in a neighborhood, we can be fairly sure that it did occur. However, in neighborhoods where we do not find an account of blockbusting, there is some chance that it still may have occurred. Aigner (1973) shows that if we know the error rate then an unbiased estimate of the effect of blockbusting can be found by dividing the OLS estimates by one minus the error rate. While we do not have information regarding the true error rate, we try a type of bounding exercise. If we assume that all neighboring tracts where the Black population share grew by more than 50 percentage points from 1950 to 1980 actually experienced blockbusting, then our error rate would be 33 percent. Specification 4 of Figure 8 shows the results, which are $1/(1-0.33) = 1.49$ times larger in magnitude than specification 3. The standard errors are also 1.49 times larger. While the framework in Aigner (1973) does not allow for treatment effect heterogeneity, we have some empirical evidence that this is not a large concern. As we noted above, our estimates of the effects of blockbusting on neighborhood home values do not scale with the number of citations that we found discussing blockbusting in the neighborhood.

The fifth estimate shown in Figure 13, is from a robustness exercise aimed at avoiding bias due to within-city general equilibrium effects. Instead of using comparison tracts from cities where blockbusting occurred, we form a comparison group using Census tracts from eight large cities where we did not find evidence of blockbusting. To find an appropriate comparison¹⁵, we use the estimates from the full sample probit (shown in Table 6) to predict the propensity of blockbusting occurring in tracts in the non-blockbusting cities. We form a comparison group using all tracts with a blockbusting propensity above 0.1. Using these comparison tracts from cities without evidence of blockbusting, we find a larger magnitude point estimate, roughly - 35%, with a large standard error.

¹⁴ We cannot extend the housing value analysis back to 1910 (the first robustness exercise) as home values were not reported in the 1910 and 1920 decennial censuses.

¹⁵ The comparison group contains the following cities (and number of tracts): Akron (30), Birmingham (14), Fort Worth (24), New Orleans (35), Providence (1), San Diego (1), Seattle (9), and Syracuse (9).

Appendix Figure A4 shows the results of re-estimating our baseline specification for two subsamples of our data: tracts in neighborhoods where we found evidence that blockbusting occurred in the 1950s and tracts in neighborhoods where we found evidence that blockbusting occurred in the 1960s. Of the 688 tracts in neighborhoods where we found evidence of blockbusting, 30% occurred in the 1950s, 48% in the 1960s, and 5% in the 1970s. We were unable to determine the decade when blockbusting occurred for 15% of the tracts. When we re-estimate using the 1950s and 1960s blockbusting sub-samples, we first limit the sample to the tracts that experienced blockbusting in the 1950s. To find a comparison group, we take all tracts that are adjacent to this set of tracts that did not experience blockbusting. Next, we re-estimate the propensity score for this subsample and form inverse probability weights. Then, we re-estimate the effects of blockbusting on the subsample using these subsample-specific weights. We do the same thing for the set of tracts that experienced blockbusting in the 1960s. It is a possible for a tract to be used as a comparison for both the 1950 blockbusting subsample and the 1960 blockbusting subsample. In practice only 22 comparison group tracts are included in both groups out of 246 comparison group tracts for the 1950 blockbusting sample and 378 comparison group tracts for the 1960 blockbusting subsample. In Appendix Table 4, the estimates for the Black population share corroborate that there is information content in the narrative sources regarding the decade that blockbusting occurred. The subsample of tracts that experienced blockbusting in the 1950s show a marked increase in Black population share by 1960, while the subsample that experienced blockbusting in the 1960s does not show a significant increase in Black population share until 1970. In Appendix Figure 4 the estimates for home values show a drop of 2% by 1960 for neighborhoods where blockbusting occurred in the 1950s and a drop of 2.8% by 1970 in neighborhoods where blockbusting occurred in the 1960s. The drop in home values increases in the following decade in both cases. In the neighborhoods that experienced blockbusting in the 1950s home values continue to drop through 1980 and then remain low in 1990, while the neighborhoods that experienced blockbusting in the 1960s see a slight rebound in 1990.

Finally, Appendix Figure A5 shows our baseline specification of the effect of blockbusting on home values with bootstrapped standard errors which have been stratified by city to account for the somewhat small number of cluster units (35 cities) and sampling error due to the estimation of the first stage probit. The standard errors are slightly larger, but the effects

in 1980 and 1990 remain statistically significant. Overall, the similarity of results in these robustness checks lend confidence that our preferred estimates using the weighted blockbusting adjacent tracts are robust to adjusting our estimates to handle bias due to spillover effects from the blockbusting tracts, bias due to the ring-based empirical design, bias due to one-side measurement error, and bias due to within-city general equilibrium effects.

Discussion: Blockbusting in Context

In this section, we discuss the place of blockbusting in the broader history of housing and public policy in the 1950s and 1960s.

Our first topic is how blockbusting interacted with other developments in urban neighborhoods that had disparate impacts on Black residents. For example, a question that might arise is whether the effects that we attribute to blockbusting might be driven instead by “White flight” due to public school desegregation. There is a literature that links the departure of White families from central cities to the suburbs to school desegregation that occurred after the Supreme Court found segregated schools to be unconstitutional in their ruling in *Brown vs. Board of Education of Topeka* (347 US 483) in 1954.¹⁶ Baum-Snow and Lutz (2011) point out that while many smaller school districts desegregated in the 1960s, large school districts located in central cities were slower to desegregate. In this regard, the timing of school desegregation in the large cities in our sample is likely to be in the 1970s rather than in the 1950s and 1960s when we observe the bulk of the blockbusting activity. Furthermore, our empirical strategy of comparing blockbusting neighborhoods to adjacent neighborhoods in the same city means that the effect of school desegregation is likely to impact both blockbusting and comparison neighborhoods similarly.

Another potential concern is that our results could be driven by disamenities due to the interstate highway systems that were built in the 1950s and 1960s. Brinkman and Lin (2022) estimate large welfare costs of interstate disamenities for residents living near the new highways. However, the rightmost panel of Table 5 provides evidence that the neighborhoods that experienced blockbusting were, on average, just as close to the nearest interstate as the adjacent

¹⁶ See Logan, Zhang, and Oakley (2017) and Baum-Snow and Lutz (2011) and the literature which they cite.

control neighborhoods. In fact, the blockbusting tracts are slightly farther from the nearest interstate, though the differences in distance are not statistically significant.¹⁷

Table 5 provides evidence on the relationship between blockbusting and two other mid-century public policies: the construction of large-scale public housing developments in the 1950s and 1960s and urban renewal projects from the mid-1950s through the mid-1970s. Both policies stemmed from the Housing Act of 1949. Since these policies were concurrent with our outcomes of interest, we consider them as endogenous regressors and thus do not include them when estimating the propensity score or include them as controls in OLS estimates. Even so, Table 5 reveals that while blockbusting tracts are closer to public housing and overlap more with urban renewal projects in the full sample (left panel), these differences are much smaller and less often statistically significant in the unweighted restricted sample (middle panel), and not at all statistically significant in the inverse probability weighted restricted sample (right panel). These findings show that concurrent public housing construction and urban renewal may have had important interactions with blockbusting, but that our inverse probability weighting approach is adequately controlling for these interactions using tract characteristics from 1950 or earlier.

Finally, how much did blockbusting contribute to the racial wealth gap? Our preferred estimate from the tract-level analysis is that blockbusting caused home values to drop by 14% from 1950 to 1980. To get a sense of the return on an investment in a newly constructed neighborhood where FHA mortgages were easier to obtain, we calculate mean home values in Census tracts where half or more of the housing units in 1960 had been built over the past 20 years. These newly constructed (likely suburban) neighborhoods fared much better; their property values appreciated from a mean of \$150,000 to \$180,000, a 20% return. Our rough estimate of the contribution of blockbusting to the racial wealth gap is to compare our estimate of -14% to this +20% return on investment for White households buying in new construction areas using FHA mortgages. However, these estimates do not take differential costs of financing into account. George et. al. (2019) analyzed contract sales in Chicago and found that after taking

¹⁷ We construct distance to the nearest interstate using ESRI's data on the present-day location of interstates (available here: <https://hub.arcgis.com/maps/esri::usa-freeway-system/about>). However, the results are similar and also not statistically different for blockbusting and adjacent neighborhoods if we use the distance to the nearest planned interstate from the 1955 "Yellow Book" plan for 50 U.S. cities. Neither planned nor constructed interstates are statistically closer to blockbusting neighborhoods than adjacent neighborhoods. We use the log of the distance to the nearest constructed interstate in Table 2 since two of our sample cities (Norfolk, VA and New Haven, CT) are missing from the Yellow Book data.

markups into account and higher financing costs compared to an FHA mortgage that Black households were paying on average an extra \$72,000 on a mean home valued at roughly \$120,000 (in 2019 dollars). In addition to differences in financing costs, it is important to note that our property level analysis shows that blockbusting is associated with high rates of foreclosure, and our back of the envelope exercise also does not take into account further losses due to foreclosure. On the other hand, not all Black households were directly affected by blockbusting and thus the contribution to the overall racial wealth gap should be scaled by the share of households affected.

Conclusion

Many scholars have argued that blockbusters employed aggressive, predatory, and discriminatory practices. These accounts typically focus on single cities and rely on anthropological or textual evidence. In this paper, we have sought to complement these deep narratives by gathering new data to understand the nature of blockbusting and its impact on housing values. Using data that show extensive activity by blockbusters in one neighborhood in Baltimore, we find that blockbusters sold properties to new residents at high prices and that their practices resulted in highly elevated foreclosure rates. Our analysis of census tracts across major US cities suggests that areas with blockbusting had significantly lower relative housing values by 1980. Using data from one neighborhood in Baltimore, we suggest that the downward pressure on housing prices came from the onerous financial positions in which blockbusters left new Black residents. The results, therefore, depict the challenges faced by new Black homeowners in building wealth through housing at the moment many likely became homeowners for the first time.

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Figure A1: 1910 Extension

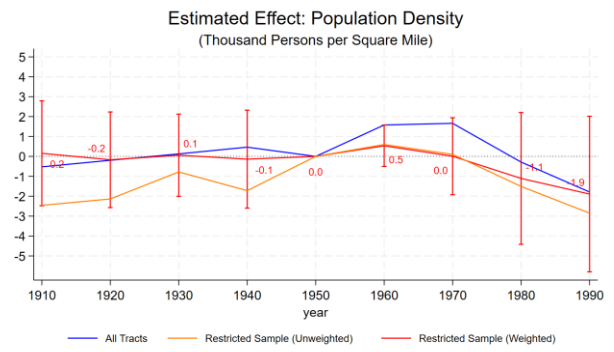
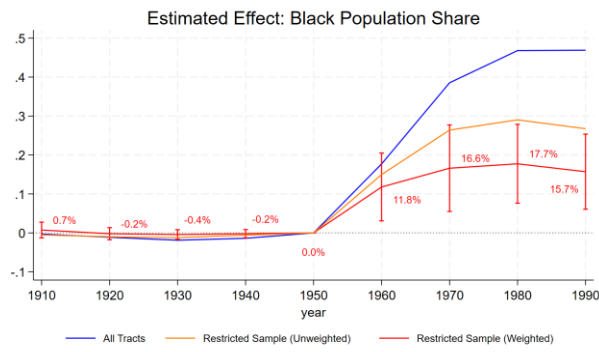


Figure A2: Indirect Neighbors (Second Ring Tracts) Specification



Figure A3: Similar Neighborhoods Specification

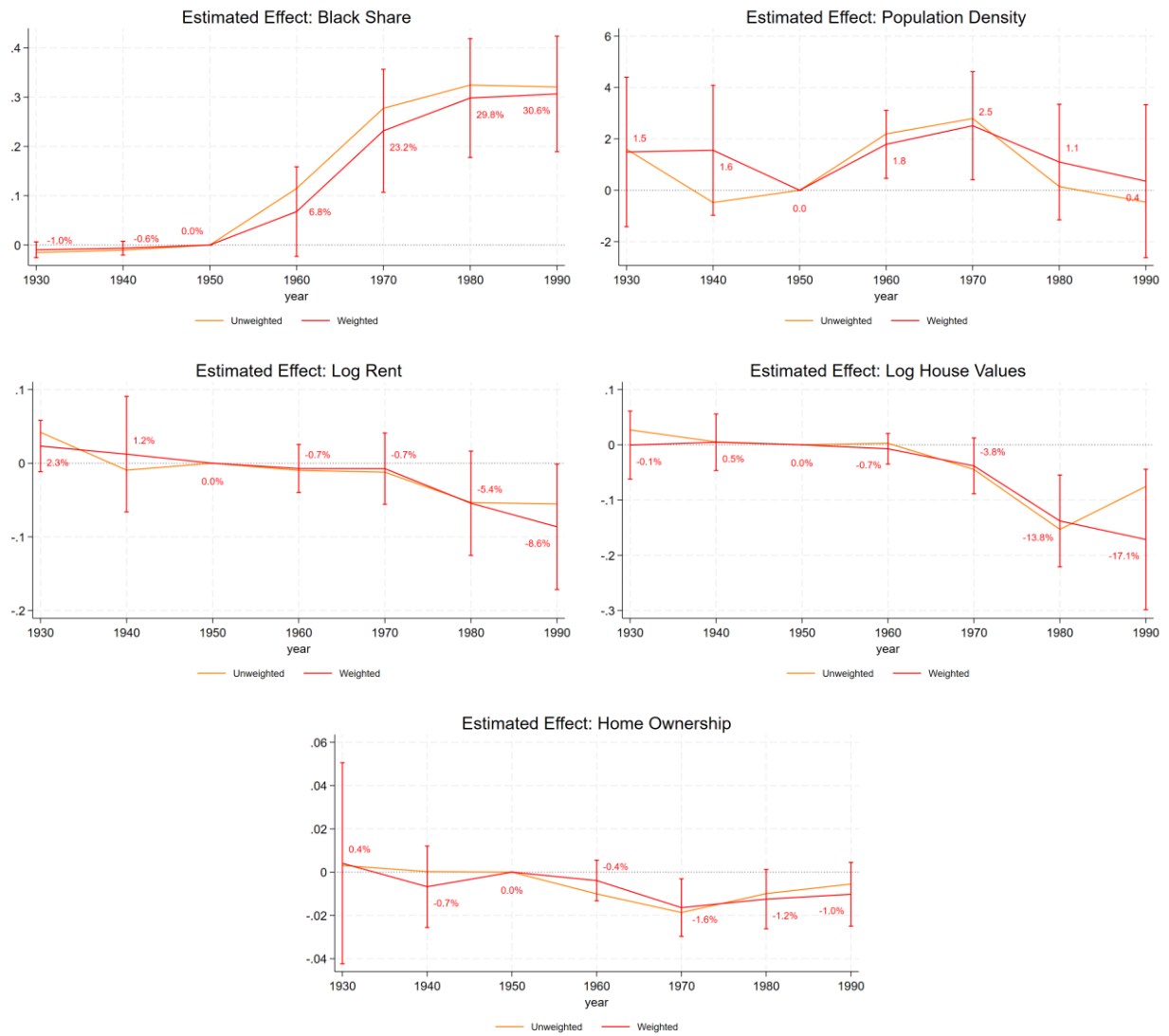


Figure A4: Blockbusting Decade Specification

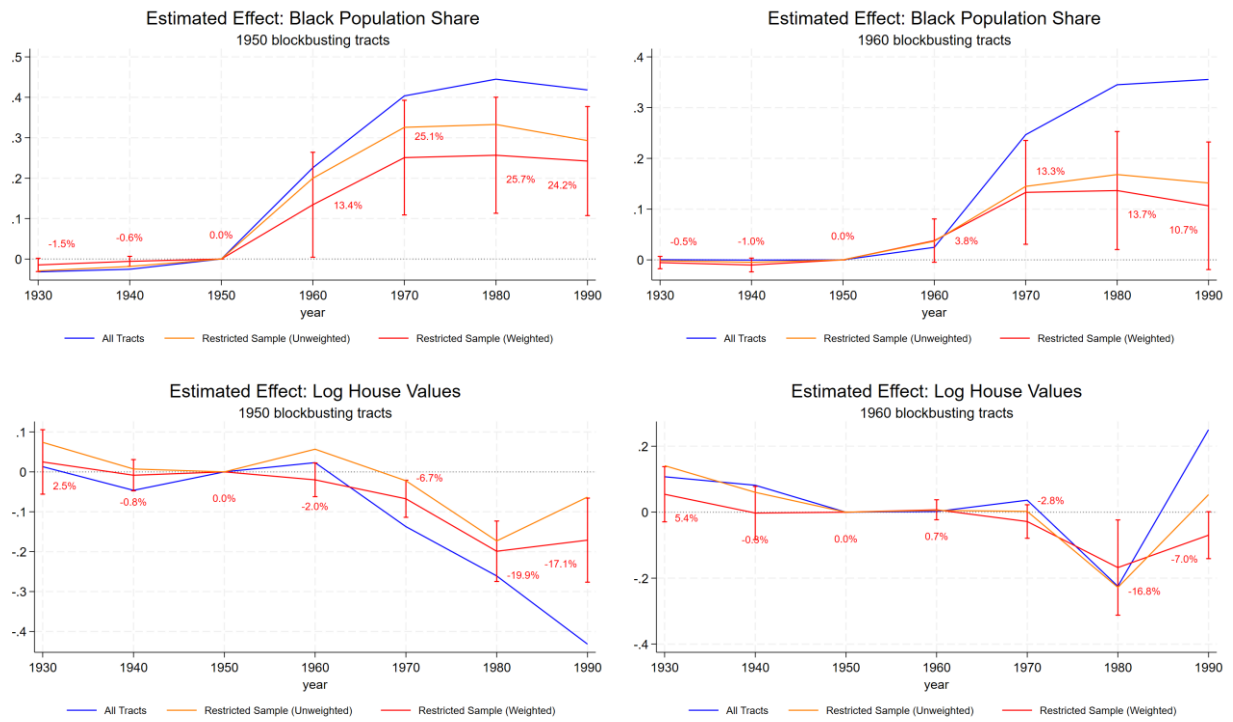


Figure A5: Wild Bootstrapped Clustered Standard Errors

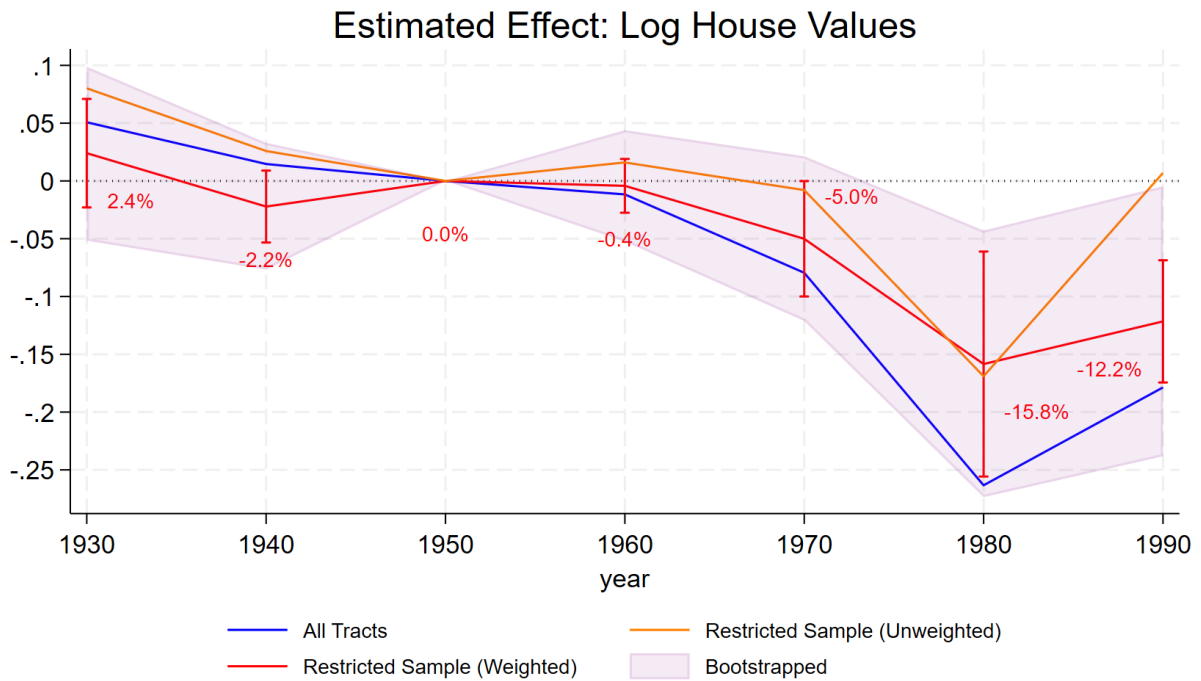


Table A1: Baseline Estimates

	(1)	(2)	(3)	(4)	(5)
	Black Population Share	Population Density	Log Mean Rent	Log Mean House Value	Home Ownership Rate
Blockbusting (1930)	-0.0075 (0.004)	0.7076 (1.055)	0.0271* (0.012)	0.0240 (0.024)	-0.0020 (0.010)
Blockbusting (1940)	-0.0031 (0.004)	0.1518 (0.878)	-0.0004 (0.046)	-0.0221 (0.016)	-0.0033 (0.008)
Blockbusting (1950)	- (.)	- (.)	- (.)	- (.)	- (.)
Blockbusting (1960)	0.1003** (0.035)	1.1315 (0.734)	0.0065 (0.015)	-0.0042 (0.012)	-0.0039 (0.004)
Blockbusting (1970)	0.1728** (0.048)	1.1289 (1.052)	-0.0041 (0.020)	-0.0500 (0.025)	-0.0106* (0.005)
Blockbusting (1980)	0.1835*** (0.050)	0.0304 (1.307)	-0.0267 (0.030)	-0.1584** (0.050)	-0.0126 (0.007)
Blockbusting (1990)	0.1590** (0.052)	-0.3343 (1.727)	-0.0618* (0.025)	-0.1215*** (0.027)	-0.0088 (0.008)
Cities	35	35	35	35	35

Online Appendix: Blockbusting and the Challenges Faced by Black Families in Building Wealth through Housing in the Postwar United States

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December 16, 2022

Abstract

This online appendix outlines the data collection procedure for determining whether and in which neighborhoods and decades blockbusting occurred in the 60 most populous cities in the United States as of 1950.

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A1: Blockbusting Data Collection Procedure

Our process for identifying blockbusting activity within a city consisted of four steps.

- 1) We began with a simple Google search for “Blockbusting in [city]”. We carefully examined the first three pages of results. This yielded many of the most well-known instances of blockbusting, as reviewed in academic journals, books, newspaper articles, urban history websites, dissertations, and other sources.
- 2) As a second step, we expanded the Google search to include alternative terms, including “panic peddling,” “panic selling,” and “white flight.” Note that these terms are distinct from blockbusting, but discussions of those subjects often bring up sources that also discuss blockbusting.
- 3) The third step was to search for “Blockbusting in [city]” and alternative terms in Google Books. Again, we carefully examined the first three pages of results.
- 4) Finally, we searched for “Blockbusting in [city]” and alternative terms on Google Scholar, and examined the first three pages of results.

When we identified a potential textual source, we looked for evidence of blockbusting, the specific neighborhoods where it occurred, and the years in which it occurred. Blockbusting can be identified either by name or through the description of blockbusting tactics. It is difficult to verify these reports of blockbusting, but given that the practice was legal until 1968, we take most plausible accounts at face value, with the only exceptions being instances of conflicting accounts, which garnered closer examination. Reference to racial turnover within a neighborhood alone was not treated as a sufficient indicator for us to conclude that blockbusting activity had occurred.

A2: Blockbusting Occurrence Sources by City and Neighborhood

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- Lake Montebello

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