# The Long Shadow of Housing Discrimination: Evidence from Racial Covenants*

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**Abstract**

Racial covenants in housing deeds were widely used in the United States during the early 20th century to segregate neighborhoods. In 1948, the Supreme Court decision in *Shelley v. Kraemer* made racial covenants unenforceable in court, stopping short of prohibiting them entirely. We surmise that even after losing their judicial enforceability, such covenants could have shaped segregation if they served as a focal point for coordination on an initial equilibrium in neighborhood formation, a context characterized by investment durability and path dependency. To test this hypothesis, we assemble novel parcel-level data on racial covenants and develop a quasi-experimental design that exploits delays between covenant execution and housing construction to isolate exposure to *enforceable* covenants at the time neighborhoods were built. Covenant enforceability indeed affected early-stage neighborhood formation in terms of housing characteristics, public infrastructure placement, and the composition of initial residents. These early differences proved durable: Enforceable covenants account for 6–24% of the observed neighborhood racial segregation from 1980 to 2020 and 3.1–4.4% of housing price differentials in the 21st century. We conclude that by coordinating beliefs and decisions during neighborhood formation, the law generated spatial inequality that has long outlived racial covenants themselves.

*Keywords*: Housing Discrimination, Racial Covenants, Segregation

*JEL*: R31, R52, N92, H23

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# 1. Introduction

Racial and ethnic segregation is ubiquitous and has well-documented negative effects on children's outcomes, upward mobility, and adult labor market outcomes (Durlauf, 2004; Chetty et al., 2016; Chyn and Katz, 2021). The observed segregation can be due to variation in underlying natural amenities (Lee and Lin, 2018) or to an initial shock or coordination that can have large and persistent effects on neighborhood development (Bleakley and Lin, 2012; Heblich et al., 2021). One way such coordination could arise is through laws and policies that coordinate beliefs and expectations among agents in the housing market about where racial and ethnic minorities are expected or permitted to live. When these expectations are formed in settings with durable investments and slow adjustment over time, the effects of discriminatory laws can persist even after their legal enforceability ends. This paper studies how a long-defunct legal mechanism for enacting segregation—racially exclusive covenants in housing contracts that could be enforced in a court of law—coordinated beliefs in the housing market during the neighborhood formation stage and shows that its effects on segregation and housing prices persisted long after the provisions themselves ceased to be judicially enforceable.

Racial covenants—common during the first half of the 20th century throughout the US—were property deed clauses prohibiting sale or rental to racial, religious, or ethnic minorities. By legally restricting who could purchase or occupy the property, such covenants facilitated segregation during a period of rapid city expansion, suburbanization, and housing construction. They offered a judicially enforceable means for developers, homeowners' associations, and local officials to coordinate and shape not only who could live where but also how neighborhoods were built and governed.

In 1948, the US Supreme Court ruled in *Shelley v. Kramer* that racial covenants would no longer be enforceable in court. The decision stopped short of outright prohibiting such covenants in sales deeds: The court left open to contracting parties the option to use racial covenants to coordinate behavior *privately* but stripped them of recourse to the courts for covenant violations (only in 1968 did the Fair Housing Act of 1968 outlaw such clauses completely). This unique feature of our setting offers a novel opportunity to isolate how the

possibility of *enforcement*, as distinct from legal provisions "on paper" (Basu, 2018), shapes strategic market behavior—an unresolved question in the law and economics literature.

Specifically, we conceptualize *Shelley* as disrupting the expectation of key actors in the housing market—developers, public officials, and initial residents—that they could rely on public enforcement in coordinating to exclude minorities precisely at the moment when many neighborhoods were being formed. Accordingly, decisions made during neighborhoods' construction and initial occupancy differed sharply depending on whether the racial covenants covering the housing in those neighborhoods were enforceable. These early differences, in turn, anchored neighborhood characteristics that persisted long after the exclusionary law itself had become obsolete.

Isolating the causal effects of a law's *enforceability*—as distinct from the letter of the law itself—is challenging because the two are rarely separately observable in a quasi-experimental setting. In addition, the racial covenants in our setting were not randomly distributed across space. Our empirical design allows us to address these issues by using the timing of the formation of neighborhoods with racial covenants with respect to 1948, the year the covenants lost their public enforceability. Early 20th century housing development often suffered long delays; developers frequently subdivided land and added racial covenants many years before homes started being built, finished being built, and were sold and occupied (Jackson, 1987). Accordingly, we can identify a set of otherwise-similar neighborhoods that were ex ante planned to be identical and had the same underlying natural quality but were covered to different degrees by *enforceable* covenants. Our sample thus consists of neighborhoods with varying proportions of housing units subject to enforceable covenants, where a higher proportion of the enforceable covenants exposure means that a larger portion of the neighborhood was built right *before* the 1948 decision that deprived housing market agents of a legally enforceable means of coordinating segregation.

We exploit this feature of the development process to isolate exposure to covenant enforceability at conjunctures when housing market agents made key decisions. By considering covenanted neighborhoods constructed just before the *Shelley* ruling with their counterparts constructed just after it, we can leverage quasi-random variation in exposure to enforceable covenants while holding fixed agents' endogenous decision to add covenants

to contracts in the first place. The exposure to enforceable covenants is the percentage (from 0% to 100%) of a neighborhood's covenanted housing that was built before *Shelley*. We further restrict attention to neighborhoods built around 1948 to minimize confounding time trends. The empirical analysis uses a novel parcel-level dataset detailing all racial covenants in Hennepin County (in the Minneapolis metro area) merged with decennial census data from 1940 to 2020 and county tax records. These data allow us to observe the year the covenants were executed, the timing of housing starts and completions, and subsequent neighborhood outcomes.

Guided by a conceptual framework of neighborhood formation, we examine how a loss of covenant enforceability could affect the decisions of three key types of agents in the housing market. First, developers could alter housing characteristics in response to the diminished ability to coordinate exclusion. Second, public officials could adjust investments in local public amenities if the anticipated composition or political influence of residents changed. Third, the composition of initial residents in the newly formed neighborhoods could shift in the absence of legal enforcement of racial homogeneity. Such responses would have occurred at or near the time of construction and initial occupancy, when neighborhoods were still malleable and expectations about future residents were being formed.

We find that neighborhoods with more unenforceable covenants had less desirable characteristics at their time of building: A reduction of one standard deviation (SD)—equivalent to a change of 33.8 percentage points (pp)—in exposure to enforceable covenants in a neighborhood during its formation corresponds to a difference of -3.2% in the area of new housing built and moderately fewer bathrooms and floors. Covenanted neighborhoods built before the loss of covenant enforceability also had interstates placed further away, which suggests that developers or local residents influenced their placement to avoid disamenity effects from the highways and their construction. In addition, the loss of covenant enforceability impacted the racial composition and racial attitudes of the initial residents: We find evidence of more racial integration by 1960 and higher vote shares for Democratic congresspeople in the racially charged 1964 and 1968 elections in covenanted neighborhoods built after the loss of covenant enforceability.

We next examine whether these early differences persisted over time. Neighborhoods are characterized by durable housing stock, long-lived public amenities, and slow residential turnover (Heblich et al., 2021). Moreover, subsequent movers, when choosing where to live, take into account existing neighborhood characteristics and resident composition in terms of both neighbors' race and racial attitudes. As a result, differences established during neighborhood formation can shape neighborhood trajectories long after the conditions that gave rise to an initial exclusionary equilibrium have passed.

We find that neighborhoods exposed to enforceable covenants during formation indeed exhibited persistent differences in racial composition, racial attitudes, and housing prices even many decades later. We measure racial composition in 1980–2020, racial attitudes in 2020, and housing prices in 2010–2019. We observe that a rise of 33.5 pp in the exposure to enforceable contracts in a neighborhood at formation corresponds, on average, to a decline of 0.6 pp in its percentage of Black residents and 0.3–1.1 pp in its percentage of residents of other races in 1980–2020; this effect represents 13–24% and 6–23% of the mean neighborhood percentage of Black and other minority residents, respectively, in these decades. Additionally, neighborhoods with the aforementioned degree of exposure of enforceable covenants at formation had 0.3–0.5 pp fewer housing units occupied by Black and other racial minority homeowners between 1980 and 2010 (an effect amounting to 13–29% of the average on this outcome), and 3.1–4.4% higher house values in the 21st century. We also find evidence of persistence in segregation of racial attitudes as expressed through minor civil disturbances after George Floyd's death in May 2020. We interpret these long-run effects of covenant enforceability as a summary reduced-form measure resulting from two mechanisms—differences in the built environment (housing and neighborhood quality), and homophily bias and inertia based on the initial composition of neighbors. These mechanisms interplay, however, and we cannot distinguish their relative roles.

**Related Literature:** The paper relates to the literature on the causes of observed racial or income segregation across neighborhoods (Bayer and Timmins, 2007; Christensen and Timmins, 2022; Rosenthal and Ross, 2015; Banzhaf and Walsh, 2013; Caetano and Maheshri, 2021). Much of this literature takes neighborhood quality as exogenous or abstracts from initial segregation. In contrast, we allow differences in the initial built environment and

initial segregation to arise endogenously through racial covenants, to persistent effect. We also complement Lee and Lin (2018) by disaggregating location quality effects into their components attributable to natural quality (Lee and Lin, 2018) and to the built environment in neighborhoods that were similar ex ante. More broadly, this paper relates to the extensive literature on how initial shocks shape spatial form (Redding and Sturm, 2008; Nunn and Puga, 2012; Dippel, 2014; Hanlon, 2017; Barjamovic et al., 2019).

This paper also falls within the economics of race literature. We build on the papers documenting the rise of racial segregation and racial homeownership gaps in the US during the first half of 20th century (Cutler et al., 1999; Collins and Margo, 2011; Logan and Parman, 2017). Racial covenants became ubiquitous in this period, but most related works are qualitative studies in law, history, and sociology (Rose and Brooks, 2016; Gordon, 2023; Brown and Smith, 2016; Rothstein, 2017), with quantitative analyses limited mainly by data constraints. Another stream looks at other, less overt instruments of segregation, such as discriminatory lending by the Federal Housing Authority (FHA) and Home Owners' Loan Corporation (HOLC) and inequality in credit access (Myers Jr, 1995; Aaronson et al., 2021; Fishback et al., 2022), zoning (Shertzer et al., 2016), and the placement of 19th-century railroads (Ananat, 2011). Racial covenants, in contrast, were openly aimed at segregation and often predated many of these policy instruments. In addition, unlike the papers above, our paper uses a setting with large-scale new developments in historically undeveloped areas to examine how the law facilitated coordination on initial exclusionary equilibria in neighborhood design, with consequences for contemporary spatial inequalities.

Finally, this paper adds to important innovations in the law and economics literature. First, we build on the literature on how the law and policies create focal points for selecting among multiple equilibria (Greif, 1994; Acemoglu et al., 2005). In particular, we study the role of coordination of beliefs through a legal instrument. Second, our research design falls within the literature using changes in laws and court rulings as quasi-experimental variation to study downstream effects (Brooks, 2011; Kucheva and Sander, 2014; Diamond et al., 2019; Aneja and Avenancio-León, 2019). However, we look not at illegality per se but at the *public enforceability* of legal provisions—a subtle and rarely observable distinction. That the *Shelley* decision specifically countenanced continued *private* coordination through

racial covenants allows us to isolate the contribution of the expectation of state enforcement, beyond the letter of the law itself, to coordinated outcomes—an empirical contribution that, to our knowledge, is unique in the literature.

## 2. Historical Background, Data, and Descriptive Evidence

Racial covenants addressed a collective action problem by legally preventing neighbors from selling homes to minorities at a markup, preventing future demographic tipping in neighborhoods (Schelling, 1969). Their enforcement depended on "injured parties," typically neighbors, suing violators. We document at least 30 such cases across various courts (Appendix Table D.1). Alongside litigation threats, collusion with real estate agents and violence likely played a role in enforcing these covenants (Christensen and Timmins, 2022). Figure 1 provides examples of racial covenants in sales deeds.

In the Minneapolis metro area, racial covenants were added mainly by real estate developers when they subdivided large swaths of undeveloped or agricultural land; the developers then advertised the covenants to enhance their developments' housing value.[1] Like many northern US metro areas, Hennepin County's European immigrant population exceeded its Black population before the Great Migration; thus, much of the exclusionary intent of these covenants fell on religious minorities such as Catholic and Jewish families and working-class immigrants from Eastern and Southern Europe, who fell outside the bounds of "Whiteness" as socially understood in that historical moment (Almagro and Sood, 2025). Because the discriminatory housing instruments used before racial covenants, such as racial zoning, are not part of the history of the Minneapolis metro area, our setting, featuring large-scale new developments built during the early to mid-20th century, is unburdened by path dependence from earlier historical housing discrimination.

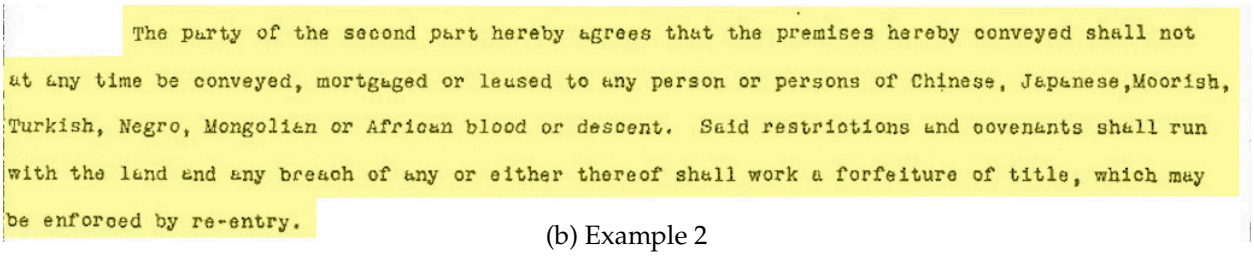
### *The rise and fall of racial covenant enforceability*

The first known racial covenant in the US can be traced to the late 19th century (Rose and Brooks, 2016), with the first in Hennepin County appearing in 1910. Figure 2 plots new housing construction on lots without (in blue) and with covenants (in red) in Hennepin County from 1910 to 1960, highlighting their proliferation especially after 1930.

[1]See Appendix Figure D.1 for an example of one such advertisement.

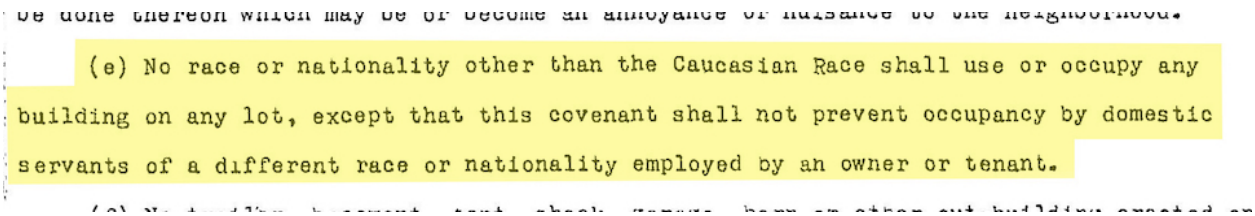
Figure 1: Examples of Racial Covenants

(a) Example 1



The party of the second part hereby agrees that the premises hereby conveyed shall not at any time be conveyed, mortgaged or leased to any person or persons of Chinese, Japanese,Moorish, Turkish, Negro, Mongolian or African blood or descent. Said restrictions and covenants shall run with the land and any breach of any or either thereof shall work a forfeiture of title, which may be enforced by re-entry.

(b) Example 2



(e) No race or nationality other than the Caucasian Race shall use or occupy any building on any lot, except that this covenant shall not prevent occupancy by domestic servants of a different race or nationality employed by an owner or tenant.

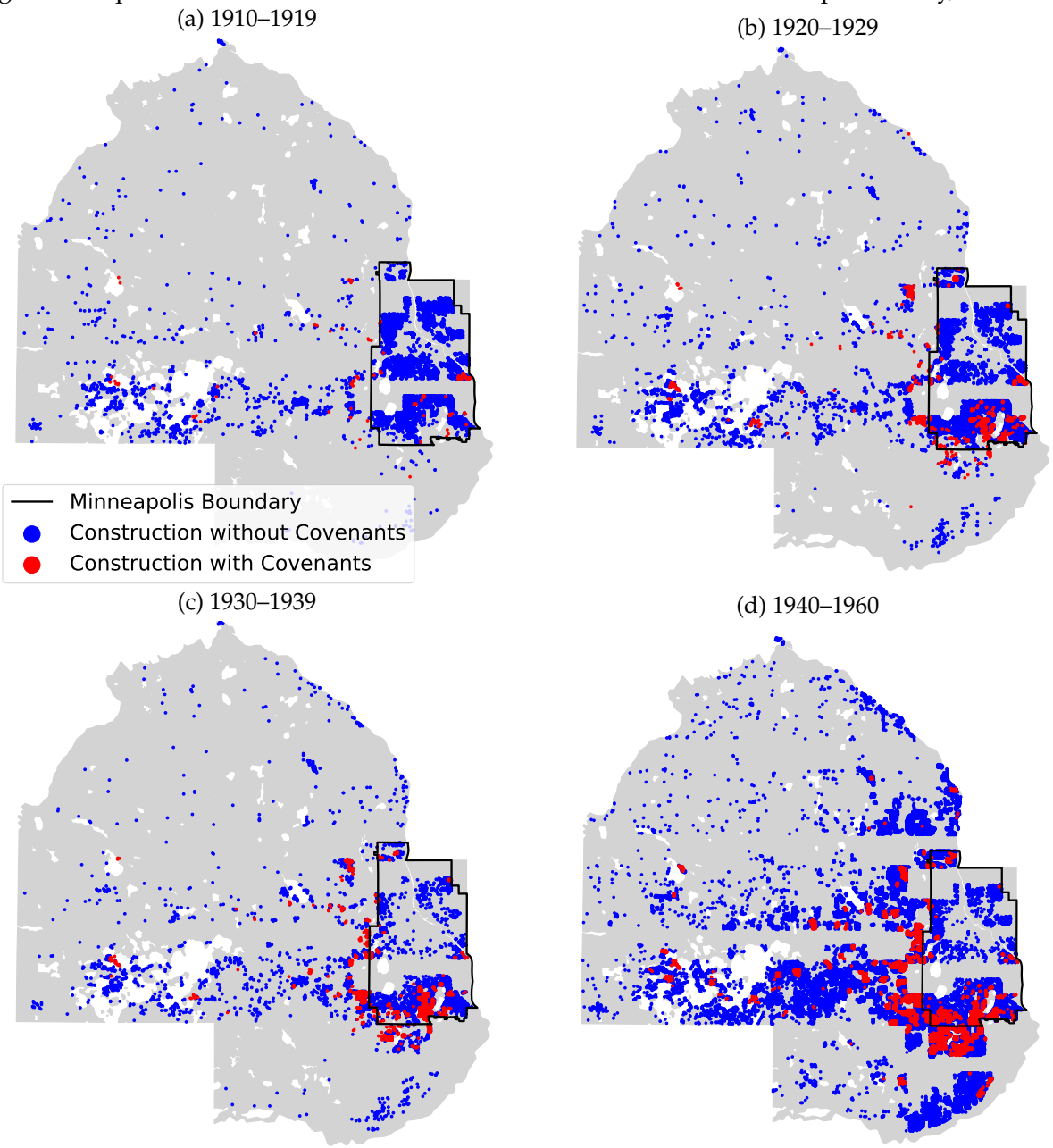
Note: This figure provides examples of racial covenants in property sales deeds in Hennepin County.

Four key historical events contributed to the proliferation of racial covenants as the primary tool for segregating urban and suburban areas throughout the US during the first half of the 20th century. First, amid international immigration, Black migration from the South, and rapid urbanization (Massey and Denton, 1993; Boustan, 2010; Shertzer and Walsh, 2019), many cities implemented municipal zoning ordinances in the 1910s to create separate residential areas for White and racial minority residents.[2] However, in 1917, the US Supreme Court ruling in *Buchanan v. Warley* prohibited municipalities from designing racially segregated neighborhoods, making the use of *private* racial covenants in housing developments more attractive (Lowe, 1948). Second, in 1924, the National Association of Real Estate Boards amended its charter to encourage the use of racial covenants (Jones-Correa, 2000). Third, in 1926, the Supreme Court reaffirmed their enforceability in *Corrigan v. Buckley* and upheld this decision in repeated subsequent challenges (Crooks, 1948). Finally, the federal government institutionalized the use of racial covenants when, in its 1938 underwriting manual, the FHA gave preference to mortgages associated with "subdivision regulations and suitable restrictive covenants" (FHA, 1938).

**Loss of legal enforceability:** In 1948, in *Shelley v. Kramer*—a 6–0 ruling from which three judges abstained because the deeds on their own homes featured racial covenants—

[2]Baltimore and Richmond enacted racial zoning ordinances by 1911; over 30 municipalities adopted such practices in the next few years (Ellen and Steil, 2019).

Figure 2: Expansion of New Construction and Racial Covenants in Hennepin County, 1910–1960




Note: This figure plots the new houses whose construction finished between 1910 and 1960 in Hennepin County. In blue are houses built on lots with no racial covenants. In red are houses built on lots with racial covenants. Racial covenants were added between 1910 and 1955 (see Appendix Figure A.2). The Minneapolis city boundary is indicated in black.

the Supreme Court deemed racial covenants no longer *legally enforceable*. This loss of enforceability did not make the writing of racial covenants illegal; that change occurred only decades later with the enactment of the Fair Housing Act of 1968. In Hennepin County, there was a sharp decline in the writing of this language after 1948, though some instances of covenant language continued to be added to subdivision maps until 1955 (see Appendix

Figure A.2 for a histogram of the number of racial covenants executed each year).

## 2.1 Data

We collect data from multiple sources to observe racial covenants and housing characteristics, ascertain residents' location decisions and racial attitudes, and observe neighborhood quality variables. For details on the data, see Appendix A.

**Racial covenants:** The data on racial covenants come from the Mapping Prejudice Project, a complete census of such covenants in Hennepin County. The data collection involved five individuals reviewing every sales deed and subdivision map for 1900–1960 to verify the presence of racial covenants and record the covenant restrictions and execution dates. Because of Hennepin County's well-maintained records and early use of typewritten sales deeds, the potential for errors in data collection was minimal.

**Housing value and characteristics:** We geocode the 21,973 racial covenants to present-day (2019) lots, where the baseline geography of lots comes from the Hennepin County tax assessor's office. From the same office, we obtain data on the 2019 assessed values of buildings based on home visits and recent sales benchmarks, along with the sales year and price (2010–2019) and house characteristics, including the year of construction end; lot size; built area; number of floors, bedrooms, and bathrooms; type of property; and year of remodeling (if any). Missing characteristics are supplemented with data from Zillow.

The year of housing construction end provided by the assessor's office indicates when the housing construction was completed. For 40% of the houses in our baseline sample, we also have the housing construction start year, sourced from historical building permit index cards at the Hennepin County Library. These cards show the month and year the first building permit was issued, marking the beginning of construction. The median and mode *actual construction time*—the difference between the construction start and end years—is one year, with a mean difference of 0.64 years (Appendix Table A.2), consistent with construction timelines for this period in the US (HUD, 1974). For the remaining 60% of the sample, we estimate the construction start year by subtracting one year from the housing construction end year provided by the assessor's office.

**Neighborhood racial composition and resident racial attitudes:** Neighborhoods are defined as the smallest geographical units for which decennial census data were collected

in a given year. In 1940, neighborhoods corresponded to enumeration districts (EDs); in 1960, census tracts; and from 1980 to 2020, census blocks.[3] The Integrated Public Use Microdata Series (IPUMS) does not provide geocoded data on 1940 EDs. Therefore, we collect, digitize, and geocode these data from the published census for Hennepin County. Neighborhood racial composition is the percent of residents or homeowners who are White, Black, or all remaining races ("other") in that neighborhood.[4]

Residents' racial attitudes during the 1960s are proxied by their voting patterns in the 1964 and 1968 congressional elections. We geocode and digitize the 1961 Minneapolis precinct and ward electoral map for the 1964 and 1968 elections from the Minnesota Historical Society. While historical electoral maps for other municipalities in the county were unavailable, for smaller municipalities without precincts or wards, we include the entire municipality in our analysis as an election unit. This approach allows us to analyze electoral data for approximately half of the housing units in the Minneapolis metro area. Voting data, which we also digitize, come from the Minnesota Legislative Reference Library. We proxy present-day racial attitudes by examining minor civil disturbances after George Floyd's death on May 25, 2020. The data come from the City of Minneapolis Office of Emergency Management, which sent out surveyors to report the exact location and scope of civil disturbance incidents during May 25–29, 2020.

**Land quality and local public amenities:** Data on land elevation, slope, and soil quality are from a 1953 survey conducted by the Natural Resources Conservation Service. Additionally, we use the 1899 geological map of Hennepin County in Almagro and Sood (2025) to identify submerged areas and bogs, indicative of the wetlands common in the county's lake geography. We also collect data on local public amenities planned and constructed either concurrently or soon after our baseline sample neighborhoods were subdivided and built. These data indicate proximity to highways planned and constructed during the 1950s and 1960s, zoning regulations first adopted in 1948, and lakes and their associated parkways, which were added concurrently with the neighborhood subdivision process

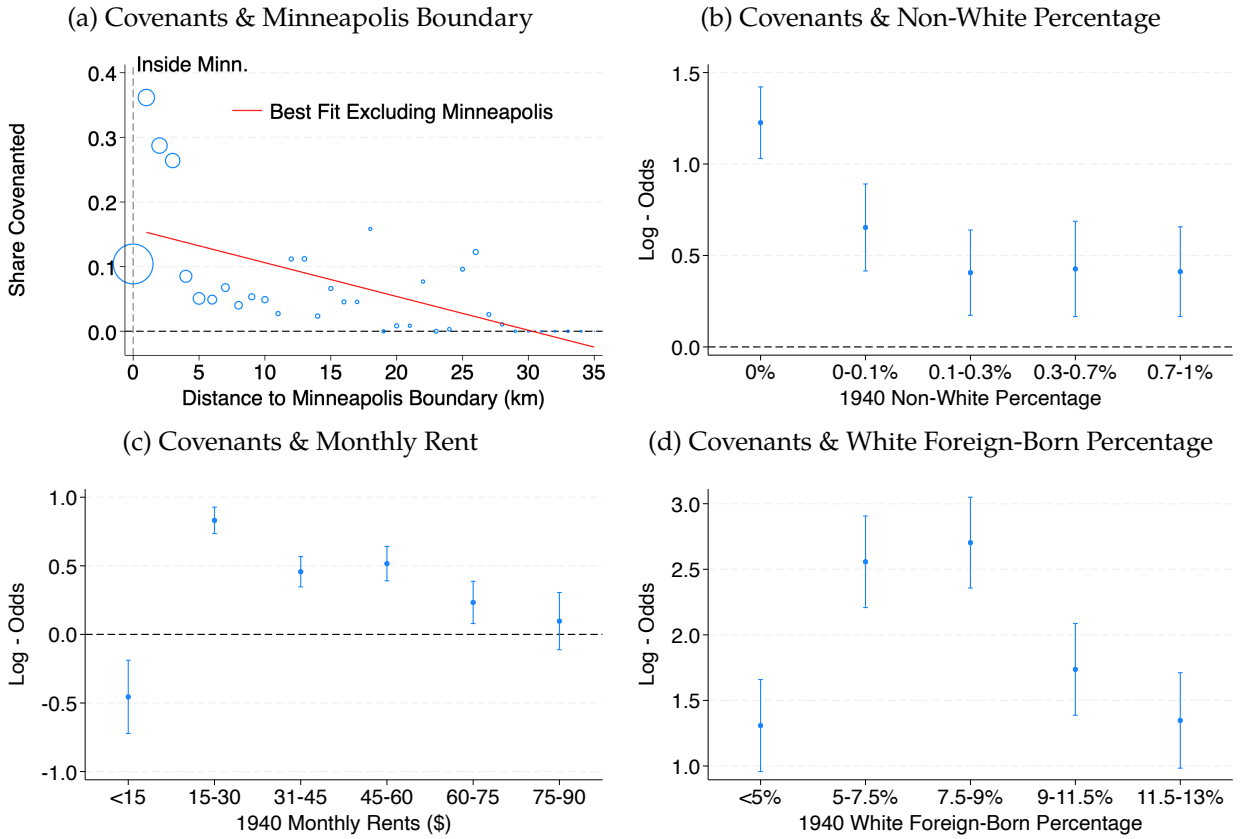
[3]We exclude 1970 from analysis because of noise in race statistics at the census block level for that year.

[4]The "White" census category includes all individuals of European descent, including those of Southern and Eastern European ancestry historically subject to ethnic discrimination by other White European groups. "Other" includes American Indian, Native Alaskan, Asian, Pacific Islander populations, and those classified as "other" by the census in a given census year. Appendix Table A.4 breaks down residents' race over time.

largely by 1947. The highway data are from the Federal Highway Administration; the information on lakes and their associated parkways is from the Minnesota Geospatial Commons. Parcel-level zoning regulation data are provided by Webster and Corey (2021).

## 2.2 Descriptive Evidence

Figure 3: Covenants in Transition Neighborhoods




Note: Panel (a) plots the share of covenanted houses among all new construction between 1910 and 1955 inside and outside Minneapolis (Minn.) against the distance from the city boundary in 1-kilometer (km) intervals. Panels (b), (c), and (d) plot the estimated log-odds (relative to the omitted base category, which is always the final bin) of addition of a racial covenant after 1940 from a logit specification whose independent variables are categorical and include the percentage of non-White residents, average monthly rent, and percentage of White foreign-born residents, respectively, all measured at the enumeration district level in 1940. For a companion figure with results for additional independent variables, refer to Appendix Figure C.1.

Using the years of housing completion and covenant execution (that is, when the covenant was added to the lot deed), we estimate that developer-imposed covenants make up 94.3% of all covenants, with 5.7% added later by homeowners. In this section, we show that covenants were primarily used in the neighborhoods likeliest to transition from being all White to racially and ethnically mixed, including lower-middle-class and middle-class neighborhoods and suburban neighborhoods along the Minneapolis city border.

Panel (a) in Figure 3 plots the share of racial covenants for houses completed between 1910 and 1955 against the distance from the Minneapolis city boundary in 1-kilometer (km) intervals. Racial covenants within Minneapolis ("Inside Minn.") are plotted at a distance of 0. The figure demonstrates significant coverage of covenants in suburbs within 5 km of the city boundary. The share of covenanted new housing steadily declines with distance from the city boundary. Racial covenants were thus primarily added in neighborhoods adjacent to Minneapolis to prevent urban-like racial and ethnic transitions in these suburbs.[5]

Panel (b) plots the log-odds from a logit specification (relative to the omitted last bin) on the likelihood of addition of a covenant in and after 1940 against the 1940 non-White population percentage at the ED level. Covenants were more prevalent in neighborhoods with minimal racial minority presence than in those with higher minority percentages. Panel (c) shows the log-odds of covenant addition in relation to 1940 monthly rents, revealing an inverted-U shape. Covenants were more common in lower- to mid-rent areas; higher-rent neighborhoods likely excluded minorities through pricing. In the lowest-rent neighborhoods, where minorities typically resided, the marginal likelihood of addition of covenants is negative. The inverted-U shape also characterizes the relation of covenanting with White foreign-born resident percentage (see Panel (d)) and neighborhood house values, income, population density, and unemployment rates (see Appendix Figure C.1).

# 3. Framework and Empirical Strategy

## 3.1 Framework of Neighborhood Formation

Our key question is what happens when a previously legally enforceable policy (in this case, racial covenants) loses public—but not private—enforceability and how this affects coordination on an initial equilibrium (here, neighborhood formation). To answer it, we first develop a framework mapping the loss of covenant enforceability to both early-stage responses linked to neighborhood formation and long-run outcomes arising from the variation in this initial equilibrium. The *Shelley* decision altered a legal focal point around which the beliefs of three types of agents in the housing market—developers,

[5]Preliminary evidence from racial covenant data for St. Louis County, Missouri, and Montgomery County, Maryland (suburban Washington, DC), similarly suggests that covenants were used in neighborhoods undergoing transitions to prevent demographic tipping (Gordon, 2023; Montgomery-County, 2023).

initial residents, and public officials—could be coordinated precisely at the moment neighborhoods were being formed. While the ruling could not change racist preferences and explicitly allowed agents to continue to coordinate on such preferences privately, it deprived them of the expectation of judicial support in doing so, with implications for their early-stage decisions. These early-stage responses shaping the neighborhood built environment (housing and neighborhood characteristics) and residential composition proved durable, anchoring neighborhood trajectories over subsequent decades.

#### 3.1.1 Early-Stage Responses to the Loss of Covenant Enforceability

**Developers:** Developers acquired virgin or agricultural land and subdivided it into lots. Developers also built houses on that land, decided the price at which to sell them to initial buyers, and determined the amount to invest in building—that is, the size and characteristics of each house (built area, number of floors, bedrooms, bathrooms, etc.). They also decided whether to attach racial covenants to the lot deeds.

If a developer believed that potential White buyers valued racial covenants (whether because they preferred not to have racial minority neighbors or because they thought future buyers would pay more for a house in an entirely White neighborhood), then those prospective White buyers would likely also pay more for each feature of a house "protected" by such covenants. In this case, racial covenants and housing characteristics could be complements if developers chose to add better characteristics to the housing if they felt more confident selling homes "protected" by covenants than selling uncovenanted units. Since developers jointly decided housing prices, housing characteristics, and whether to covenant, in response to the 1948 shock depriving them of covenant enforceability, developers could have altered the characteristics of the housing units they were building.

**Initial residents:** The second set of housing market agents whose decisions could have been affected by the loss of covenant enforceability were the buyers of the newly built houses around 1948. Before 1948, prejudiced White buyers priced out of the neighborhoods where high prices could control neighborhood demographics may have preferred to buy homes in covenanted neighborhoods. After 1948, such buyers could no longer expect covenant protection from the courts. They then faced either a lack of neighborhoods offering this "protection amenity" or the option to buy homes with potentially different

characteristics in other neighborhoods where they could coordinate privately to exclude minorities—whether formally through now-unenforceable covenants or informally through social norms or threats—with other prejudiced residents. If prejudiced buyers expected such private mechanisms would prove less effective than the law in coordinating agents on and preserving an exclusionary equilibrium, they would have had less incentive to concentrate in newly built neighborhoods with unenforceable covenants and would have been likelier to scatter throughout the metro area after covenant enforceability was lost.

**Public officials:** The last set of housing market agents who could have been affected by the loss of covenant enforceability were local and national public officials who decided on amenity provision in a neighborhood, such as zoning laws or placement of highways or parkways around lakes. Such officials' decisions may have been swayed by influential constituents such as developers seeking to boost the value of their lots by introducing high-quality amenities near their developments (Walker et al., 2022). If each marginal addition to public amenities increases real estate value by a fraction, developers would have allocated their lobbying efforts toward their higher-quality houses—those with more rooms, bathrooms, square footage, and perhaps attached racial covenants. Such lobbying would have coincided with the buying-up of land or subdivision of lots or during construction. Such lobbying efforts could have changed with the loss of covenant enforceability in 1948.

Another category of influential constituents was homeowners who had moved into covenanted neighborhoods before enforceability was lost in 1948. Since the presence of enforceable covenants allowed these homeowners to coordinate on type and attitudes in an exclusionary initial equilibrium, they may have been wealthier (having paid more for covenanted properties) and/or more prejudiced and correspondingly more effective or vocal in their demands of public officials than the homeowners scattered in neighborhoods formed without legally enforceable racial covenants post-1948.

#### 3.1.2 Persistence in Neighborhood Characteristics

We expect some durability in neighborhoods' built environment and resident composition—the characteristics likeliest to have been affected at neighborhood formation by the loss of covenant enforceability. First, while teardowns can and do happen and housing investment is a dynamic process, the vast majority of housing characteristics do not change much over

time. Additionally, neighborhood amenities such as proximity to highways, lakes, and parks remain relatively stable over time. While zoning, in contrast to highways, lakes, and parks, can in theory change over time, it rarely does (Kulka et al., 2026). Lee and Lin (2018) show that unchanging exogenous natural amenities can anchor neighborhoods, driving long-run segregation. The built environment can also anchor a neighborhood over time.

In addition to the neighborhood anchoring from the unchanging endogenous built environment, anchoring could have arisen from the segregation of residents across neighborhoods through initial coordination on racial covenants. Initial self-segregation by prejudiced White buyers could last generations through homophily bias and inertia (Heblich et al., 2021) if people lived in their houses for a long time. Indeed, Hennepin County records suggest that during the 1940s and 1950s, residents lived in their houses for, on average, 11.9 years.[6] Likewise, minority and unprejudiced White families were unlikely to have preferred neighborhoods populated with prejudiced neighbors. Real estate agents could also have engaged in discriminatory steering, which, in the presence of inertia, would reinforce segregation and house price differentials over time.

## 3.2 Empirical Strategy

Our empirical strategy must contend with the fact that racial covenants were not randomly distributed across space and were instead likely correlated with unobserved location quality, as shown in Figure 3. Accordingly, we define below measures of the *time to housing start* and *time to build* in the housing subdivision and construction process relative to the 1948 *Shelley* ruling and exploit variation in them to identify causal effects.

### 3.2.1 Timing of Neighborhood Formation and Sample Construction

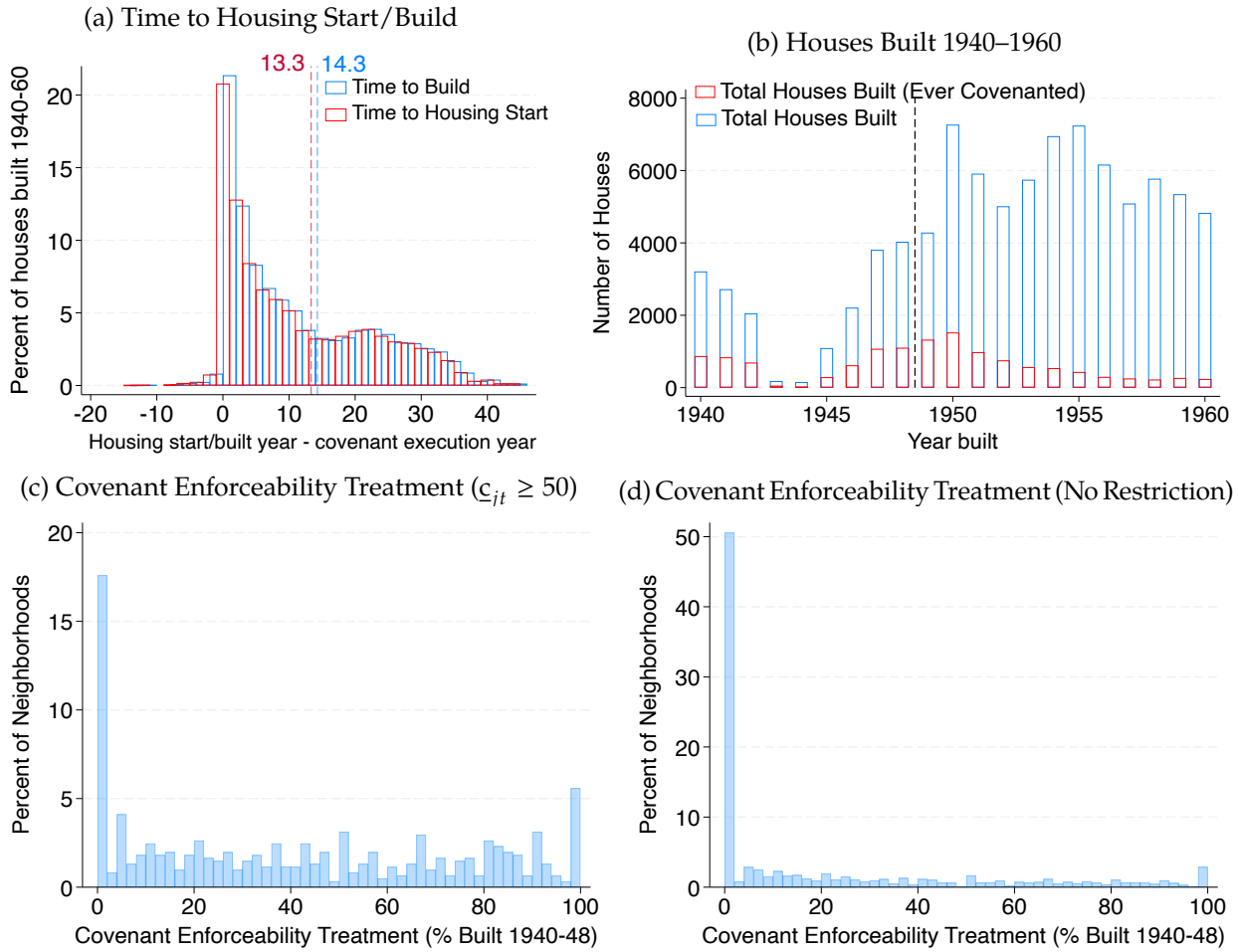
We begin with some definitions. The *covenant execution year* refers to the year a racial covenant was added to the lot deed, typically the year the lot was subdivided by the developer. The *year of housing start* is when housing construction began and the *year of construction end* when construction was completed and the house was ready for move-in.[7]

[6] For a random sample of 805 houses from our baseline sample of 11,740 houses, a search of historical county records shows that the mean and median difference between the first and second sale is 11.9 and 9 years, respectively. See Appendix Table A.1.

[7] From a random sample of historical county records, we estimate a mean difference between a house's first sale, i.e., move-in date, and year built of 0.13 years (0.0 median years). See Appendix Table A.1.

We define the *time to housing start* as the difference between the years of housing construction start and covenant execution. *Time to build* is the difference between the years of housing construction end and covenant execution. *Actual construction time* is difference between the construction start and end years.

Figure 4: Time to Housing Start/Build and Baseline Sample




Note: Panel (a) shows the difference between the year of housing start or build end minus the year of covenant execution, defined as *time to housing start* (in red) and *time to build* (in blue), for houses built between 1940 and 1960. The mean *time to housing start* is 13.34, and the mean *time to build* is 14.29. Panel (b) shows in blue the total number of housing units built and in red housing units ever covenanted built from 1940 to 1960. The dashed line at 1948 represents the *Shelley* ruling. Panel (c) shows the distribution of the covenant enforceability treatment, $(\%\text{built} \leq 1948)_{jt}$ from Equation 1, where the sample is restricted to neighborhoods in which at least 50% of the housing units had covenants ($\%\text{cov houses}_{jt} \geq \underline{c}_{jt} = 50$) and Panel (d) the distribution of the treatment with no restriction on the share of units with covenants ($\%\text{cov houses}_{jt} \geq \underline{c}_{jt} = 0$) for the 2020 census blocks sample. See Appendix Figure C.1 for the treatment distribution across housing units.

The difference between the years of covenant execution and housing start/construction end was especially large in the first half of the 20th century, as cities expanded rapidly into virgin land and farmlands, speculative developers often subdividing land and adding

racial covenants long before urban and suburban boundaries reached these developments (Jackson, 1987). In Panel (a) of Figure 4, we plot both the *time to housing start* (in red) and the *time to build* (in blue) in our sample; 45.4% and 53% of houses were started and finished within ten years of covenant execution. However, many construction starts and ends were much later, and the mean *time to housing start* is 13.3 and *time to build* 14.3 years.[8]

We exploit the *time to housing start* and *time to build* in our empirical strategy and restrict our baseline sample to neighborhoods in which at least half of the housing built was covenanted. Because of *Shelley*, only neighborhoods and houses whose construction had started or ended by 1948 could benefit from legally enforceable covenants; for housing whose construction started or finished later, any racial covenants in the land deeds were no longer defensible in court. Correspondingly, we can set neighborhoods' exposure to treatment on the basis of their coverage of enforceable covenants, benefiting from quasi-random variation while accounting for selection in the choice to covenant.[9]

To avoid confounding time trends, we also restrict the analysis to houses and neighborhoods built around the 1948 ruling. The baseline sample includes houses and neighborhoods whose building started or ended between 1940 and 1960. Panel (b) of Figure shows in red the 12,941 covenanted houses whose building ended between 1940 and 1960 and in blue the total number of houses built between 1940 and 1960, 89,050.

#### 3.2.2 Margins of Housing Market Agent Responses

Whether we use the year of housing start and *time to housing start* or the year of construction end and *time to build* relative to the 1948 *Shelley* ruling in our empirical strategy to ascertain whether agents' actions were affected by the loss of racial covenant enforceability depends on which type of agent we are studying, as we explain below.

**Developers:** We use the year of housing start and *time to housing start* to study developers' decisions since the loss of enforceability is plausibly likelier to have shaped developers' decisions to adjust housing characteristics before construction began than after it had

[8]For a small number of observations, the covenant was executed *after* construction start and end, as indicated by negative values in Figure 4 Panel (a).

[9]Because we aim to isolate the coordinative role of beliefs about state enforcement from that of the covenants themselves, our sample includes only covenanted properties. An analysis comparing covenanted and noncovenanted neighborhoods faces endogeneity in the choice to covenant, but we conduct one in Appendix Section C.1 and compare its results with the baseline results in Section 6.

been completed. Potential margins of response among this group include the housing characteristics more amenable to adjustment before construction: log built area and numbers of bedrooms, bathrooms, and floors. In contrast, lot size, determined at the time of subdivision, would have proved significantly harder to adjust, and thus we use this measure in a falsification test to show that covenanted units and neighborhoods built right before and right after 1948 were planned ex ante to be similar.

**Initial residents:** We use the year of construction end and *time to build* in our analysis of the effects on initial residents of the treated neighborhoods. As margins of response, we consider a) racial segregation at census tract level in 1960 and b) residents' racial attitudes in the 1960s. Data on race are available in the decennial census, and we infer residents' racial attitudes from precinct-level vote shares for Democrats, whose party advanced desegregation in the 1960s (Carmines and Stimson, 1989), in the 1964 and 1968 congressional races, when the civil rights movement was at its peak.[10] We focus on congressional vote shares instead of presidential, as Hubert Humphrey, a popular Minnesotan, was the vice-presidential and presidential candidate in 1964 and 1968, respectively.

**Public officials:** To study public officials' margins of response, we focus on three types of local public amenities added at land subdivision, during construction, or after construction end. These include a) distance to interstate highways, b) zoning regulations, and c) distance to lakes and their associated parkways.

*Highways*: Beyond the possibility of demolition, construction of a highway through a neighborhood can create disamenities from noise, pollution, and neighborhood segmentation (Brinkman and Lin, 2024), incentivizing influential developers and residents to attempt to sway public officials regarding highway placement. We measure highway amenities in terms of the Euclidean distance and commute time to the highway. Hennepin County developed and released its interstate highway plans in 1957 (Ramer and Walker, 2025).[11] Highway construction began in 1958 and continued until the late 1960s. Appendix Figure B.2 and Section B.2 show that much but not all of the 1957 plan was actually built and

[10] The 1964 Civil Rights Act removed differential voter registration requirements by race and banned employment discrimination on the basis of race. The 1968 Civil Rights Act included the Fair Housing Act, banning any type of racial discrimination in the housing sector or housing lending.

[11] A few highway plans by the state existed before 1957 but were never publicly released or built. Appendix Section B.2 details the planning and construction of highways.

document protests by residents that led to the scrapping of plans to build highways through their neighborhoods. Since public officials' decisions on where to plan and actually build highways would have turned on the dates of construction end and move-in, we use the year of construction end and *time to build* to test for a response on this margin.[12]

*Zoning regulations:* Comprehensive zoning regulations were first adopted in Hennepin County in 1948, starting with the city of Minneapolis, with suburban municipalities adopting their own comprehensive zoning laws in the 1950s–1970s.[13] As measures of coordination on an exclusionary equilibrium through zoning, we consider single-family-only and minimum lot size regulations. Municipal officials' zoning decisions were likely influenced by both local developers and residents, depending on whether the neighborhood zoning code was planned and adopted before construction began or ended. Accordingly, we conduct our analysis using both the the year of housing start and *time to housing start* to account for developers' influence on local officials and vice versa and the year of construction end and *time to build* to account for residents' influence on local officials.

*Lakes and parkways:* Parts of lakes were dredged and massive parkways built around hundreds of lakes starting in 1883. As documented in Appendix Section B.4, most of this dredging and parkway construction had happened by 1947. Walker et al. (2022) provide historical evidence of developer influence on local public officials regarding the dredging and construction of parkways and document that this influence was exercised at the time when land lots were subdivided and covenants added. Accordingly, this amenity was likely not affected by *Shelley*, and we use the Euclidean distance to lakes and their parkways in a falsification test to show that lots and covenanted neighborhoods built right before and right after 1948 were ex ante planned to be similar. For this (exogenous to our setting) variable, whether we use the year of housing start or construction end does not matter.

**Long-run outcomes:** Since persistence of an exclusionary equilibrium would stem in part from decisions made by housing market agents long after 1948, we use the year of construction end and *time to build* for this analysis. To study persistent effects, we look at a) neighborhoods' racial composition between 1980 and 2020, b) racial attitudes of residents

[12]Houses whose construction started between 1957 and 1960 could also have been affected on this margin, but our analysis with a restricted 1940–1956 sample shows this was not the case (see Section 4.3).

[13]See Appendix Section B.3 for a timeline of zoning regulation adoption.

in 2020, and c) house sales prices between 2010 and 2019 and assessed values in 2019.

To assess current racial attitudes, we analyze minor civil disturbances, such as graffiti and broken windows, that occurred in the four days after George Floyd's death on May 25, 2020.[14] We hypothesize that areas with racially progressive attitudes will show a higher number of these disturbances. Progressive political cultures often promote greater civic engagement, protests, and visible expressive disorder, such as graffiti (Tarrow, 2022). Unlike organized protests, minor disturbances are usually scattered, occurring near offenders' homes (Rossmo, 2025). To ensure that these disturbances are influenced solely by variations in covenant enforceability—and not by any unobservable factors that might correlate with the location of these incidents—we restrict our analysis to events from the early days of the 2020 protest movement, when likely only locals were involved, and exclude prominent locations that drew participants from outside the neighborhood. In addition, we focus only on minor civil disturbances, as locals may have expressed their views in less disruptive ways to protect their neighborhoods (Brooks et al., 2024).

## 3.3 Empirical Models

To study the effect of loss of covenant enforceability on the early-stage responses of three types of agents and outcomes relevant to persistence over time, we analyze empirical models at neighborhood and house levels. For a neighborhood $j$, either a census block over the years $t$=1980–2020 or an electoral precinct in $t = 1964, 1968$, the model is given by[15]:

$$\begin{aligned} Y_{jt} = {} & \alpha_0 + \alpha_1(\%\text{built/start} \leq 1948)_{jt} + \alpha_2(\%\text{cov houses})_{jt} \\ & + \alpha_3(\text{mean time to build/start})_{jt} + \alpha_4 X_{jt} + \eta_{k(j)t} + \epsilon_{jt} \quad \text{if } \%\text{cov houses}_{jt} \geq \underline{c}_{jt} \end{aligned} \tag{1}$$

The variable $(\%\text{built/start} \leq 1948)_{jt}$ denotes the covenant enforceability treatment, equal to 100 if all houses in the neighborhood started or finished building (depending on which

[14] White voters' electoral behavior since the 1960s is not a good measure of their racial attitudes because of secular changes in the nature of partisanship in the intervening decades and because present-day elections do not focus directly on race relations, in contrast to those during the civil rights era (Morales, 1999).

[15] The decision to covenant was made at the development level by the developer, but the smallest geographical unit for which public census and vote share data are available is the census block and precinct, respectively. An analysis at the census block level closely resembles a development-level analysis since the 50th percentile development is 2.8 times larger than the median census block in our sample. Therefore, our census block–level analysis is often more granular than the development-level analysis.

dependent variable we are considering) between 1940 and 1948. We restrict the baseline sample to neighborhoods where at least 50% of the houses were started or finished building between 1940 and 1960 to ensure that we consider only neighborhoods largely developed around the 1948 ruling. We also restrict at baseline to neighborhoods at least 50% covenanted (%cov houses$_{jt} \geq \underline{c}_{jt} = 50$). Doing so allows us to control for selection into covenanting.[16] Panels (c) and (d) of Figure 4 plot the covenant enforceability treatment variable with %cov houses$_{jt} \geq \underline{c}_{jt} = 50$ and without any %cov houses$_{jt}$ restriction, respectively.

Our key parameter of interest, $\alpha_1$, quantifies the local average treatment effect (LATE) arising from covenant enforceability.[17] The treatment is continuous, ranges from 0 to 100 across the neighborhoods, and measures the intensity of racial covenant enforceability across neighborhoods. $\alpha_1$ quantifies the effects of ex post temporary covenant enforceability—that is, of the forward-looking expectation of legal enforcement of contract provisions the court would later make publicly unenforceable.

As our dependent variables, $Y_{jt}$, we use land and location quality or historical built environment measures for balance tests to ensure that there were no underlying or historical differences across the intensity of covenant enforceability treatment of neighborhoods, local public amenities (distance to highways or lakes and associated parkways, zoning), Democrat vote share in the 1964 and 1968 congressional races, percentage of residents or owner-occupied housing units categorized by race from 1980 to 2020, or the total number of minor civil disturbances in a census block after George Floyd's killing.

We use (%cov houses)$_{jt}$ to control for the neighborhood's covenanted property share (above the %cov houses$_{jt} \geq \underline{c}_{jt} = 50$ restriction) to rule out effects of developers' own ideology and decisions and/or beliefs about *buyers'* ideology beyond the mechanism of coordination through the law that we study. (mean time to build/start)$_{jt}$ controls for the neighborhood mean *time to housing start* or *time to build* (depending on the dependent variable), which could be correlated with unobserved neighborhood quality. When the dependent variables are the percentages of residents or owner-occupied housing units

[16] %cov houses$_{jt}$ accounts for all covenants, as virtually all were executed by 1948 (see Appendix section A.4). In Section 6, we limit the sample to neighborhoods at least 75% built between 1940 and 1960, with $\underline{c}_{jt} \geq 75$. See Appendix B.5 for further details on our neighborhood sample construction.

[17] Note that LATEs may differ from the aggregate, metro-wide effects of racial covenanting, which our localized methodology cannot identify.

categorized by race from 1980 to 2020, $X_{jt}$ controls for house values and observable house characteristics, ensuring that our long-run estimates are not driven by differential affordability or housing characteristic preferences across races. We also include tract–year fixed effects, $\eta_{k(j)t}$. These control for unobserved broader neighborhood quality, ensuring that our identification is based on the variation in covenant enforceability treatment across census blocks or electoral precinct within the same tract.[18]

When the dependent variables are housing characteristics or house values, we use the house-level model, given by:

$$\begin{aligned} Y_{ijt} = \alpha_0 + \alpha_1(\%\text{built/start} \leq 1948)_{jt} + \alpha_2(\%\text{cov houses})_{jt} + \alpha_3(\text{log time to build/start})_{ijt} \\ + \eta_{k(j)t} + \delta_t + \epsilon_{ijt} \;\text{ if }\; \%\text{cov houses}_{jt} \geq \underline{c}_{jt} \;\&\; 1940 \leq \text{year built}_i \leq 1960 \end{aligned} \tag{2}$$

As before, the key variable of interest is $(\%\text{built/start} \leq 1948)_{jt}$, a continuous treatment quantifying the effect of racial covenant enforceability on the dependent variables ($Y_{ijt}$).[19] As in Equation 1, we restrict the baseline sample in this model to census blocks where at least 50% of the housing was built between 1940 and 1960 and ($\underline{c}_{jt} \geq 50$). Additionally, house $i$ must have been built between 1940 and 1960 in the baseline sample.

The dependent variables are characteristics for house $i$ in neighborhood $j$ (log lot size, log built area, and numbers of bedrooms, bathrooms, and floors) and housing values, including log assessed values (2019) and log sale prices (2010–2019).[20] For the first set of dependent variables above, we use $(\%\text{start} \leq 1948)_{jt}$ and control for log *time to housing start* for the housing unit, while for the second set, we use $(\%\text{built} \leq 1948)_{jt}$ and control for log *time to build*.[21] As before, we control for $(\%\text{cov houses})_{jt}$. $\eta_{k(j),2020}$ denotes 2020 census tract fixed effects. $\delta_t$ denotes sale-year fixed effects, when applicable.

[18] When the dependent variable is the 1964 and 1968 congressional Democratic vote share, neighborhood $j$ is an electoral precinct, and we use 1980 census tract fixed effects. For concordance between precincts and census tracts, we match a precinct to the census tract that the majority of its residents belonged to.

[19] Panels (e) and (f) of Appendix Figure C.1 plot the variation in the covenant enforceability treatment with $\%\text{cov houses}_{jt} \geq \underline{c}_{jt} = 50$ and without any $\%\text{cov houses}_{jt}$ restriction, respectively, at the housing unit level.

[20] Sale prices reflect the market price; only 40% of all houses in the sample were for sale from 2010 to 2019. Conversely, assessed values are available for all houses but can be biased (Berry, 2021). We present the results for both measures and discuss their relative strengths in Section 5.3.

[21] For the first set of dependent variables, we also control for the type of property (e.g., single-family, townhouse). For the second set, we add property type, housing characteristics, and public amenities controls only for robustness and additional results in Sections 5.3 and 6.

We are also interested in the potential for nonlinearity in the effects of loss of covenant enforceability on individual properties in neighborhoods with different degrees of treatment exposure—especially with respect to housing values, as covenanting only one's own property restricts what the owner can do with it, reducing its option value. Accordingly, we modify the house-level model in Equation 2 as follows:

$$
\begin{aligned}
Y_{ijt} = \alpha_0 + \alpha_1 \mathbb{1}\{\text{built} \leq 1948\}_i + \sum_{m=0}^{4} \beta_m \mathbb{1}\{\%\text{built} \leq 1948\}_{mj,2020} + \alpha_2 (\%\text{cov houses})_{jt} \quad (3) \\
+ \alpha_3 (\text{log time to build})_{ijt} + \eta_{k(j),2020} + \delta_t + \epsilon_{ijt} \\
\text{if} \quad \%\text{cov houses}_{j,2020} \geq \underline{c}_{j,2020} \;\; \& \;\; 1940 \leq \text{year built}_i \leq 1960
\end{aligned}
$$

The dummy variable $\mathbb{1}\{\text{built} \leq 1948\}_i$ is equal to 1 if the house itself was built between 1940 and 1948. $\alpha_1$ measures the effect of the covenant enforceability treatment on the value of that house. $\mathbb{1}\{\%\text{built} \leq 1948\}_{mj,2020}$ provides five discrete levels $m$ of covenant enforceability treatment in each census block $j$, where $m = 0$ or 0% denotes no treatment. $m = 1$ denotes treatment of 1–25% of the census block with enforceability coverage and $m = 4$ enforceability coverage of 76–100%. We use a similar model to study nonlinearity in the persistence of the segregation effects over 1980–2020 (see Section 5.1).

## 3.4 Identification and Conditions for Validity

Identification requires that, conditional on the presence of covenants in a neighborhood, the timing of neighborhood formation relative to 1948 be orthogonal to unobserved neighborhood quality and that the 1948 ruling was unanticipated by agents in the housing market. Here, we show that these conditions hold.

### *Balance of location quality and built environment by treatment intensity*

To test whether the underlying quality and pre-1940 built environment of the sample neighborhoods are statistically similar at varying treatment intensities, we use Equation 1.[22] Table 1 shows the parameter estimate $\alpha_1$ from Equation 1 for the baseline sample of neighborhoods, where the dependent variables are the land and location quality and 1940

[22] We exclude the $(\text{mean time to build/start})_{jt}$ controls, as the dependent variables are measured at their values pre-1940, before any construction had begun or ended.

built environment measures. Standard errors clustered at the census tract level appear in parentheses (column 2, with associated $p$-values in column 3); robust standard errors are in brackets (column 4).

Table 1: Balance of Location Quality and Built Environment by Treatment Intensity

| Dependent Variable | $\alpha_1$ | s.e. (cluster) | $p$-value (cluster) | s.e. [robust] | $N$ | $R^2$ | Mean of Dep. Variable |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel A: Land & Location Quality* | | | | | | | |
| Distance to CBD (km) | -0.002 | (0.003) | 0.550 | [0.003] | 735 | 0.884 | 861.37 |
| Elevation (feet) | 0.031 | (0.020) | 0.129 | [0.015] | 735 | 0.933 | 879.56 |
| Average Slope ($^o$) | 0.001 | (0.002) | 0.717 | [0.002] | 735 | 0.579 | 3.84 |
| Slope Min-Max | 0.0005 | (0.002) | 0.841 | [0.002] | 735 | 0.577 | 3.81 |
| Depth to Wet Soil | -0.045 | (0.032) | 0.172 | [0.020] | 735 | 0.650 | 9.65 |
| Water in Soil | -0.045 | (0.032) | 0.172 | [0.020] | 735 | 0.650 | 9.65 |
| Flood Probability | 0.00001 | (0.0001) | 0.911 | [0.0001] | 735 | 0.246 | 0.01 |
| Ponding Frequency | -0.007 | (0.005) | 0.210 | [0.006] | 735 | 0.416 | 0.95 |
| 1899 Percentage Wetlands | -0.017 | (0.025) | 0.503 | [0.021] | 735 | 0.240 | 4.90 |
| Development Subdivision Year | 0.023 | (0.025) | 0.354 | [0.017] | 732 | 0.622 | 1929.59 |
| Time to Build | -0.108*** | (0.028) | <0.001 | [0.018] | 732 | 0.614 | 20.84 |
| Time to Housing Start | -0.109*** | (0.028) | <0.001 | [0.018] | 732 | 0.615 | 19.90 |
| *Panel B: Pre-1940 Built Environment* | | | | | | | |
| Population Density | 1.870 | (1.273) | 0.146 | [0.910] | 735 | 0.864 | 1102.81 |
| Percentage White | 0.00004 | (0.0001) | 0.547 | [0.00004] | 735 | 0.969 | 99.95 |
| Percentage Black | 0.00002 | (0.00003) | 0.614 | [0.00002] | 735 | 0.971 | 0.03 |
| Percentage Other | -0.0001 | (0.0001) | 0.384 | [0.00004] | 735 | 0.868 | 0.01 |
| Percentage White Foreign-Born | 0.001 | (0.001) | 0.325 | [0.001] | 735 | 0.964 | 7.41 |
| Unemployment Rate | -0.001 | (0.003) | 0.695 | [0.002] | 735 | 0.858 | 6.82 |
| Average Rent | -0.018 | (0.018) | 0.330 | [0.014] | 735 | 0.974 | 46.59 |
| Average House Value | -1.422 | (2.265) | 0.532 | [1.037] | 735 | 0.889 | 4662.02 |
| Average Income | 0.099 | (0.160) | 0.540 | [0.148] | 735 | 0.856 | 626.45 |

Note: Column 1 displays parameter estimate $\alpha_1$ from Equation 1. The dependent variables in Panel A are various land and location quality measures and in Panel B pre-1940 built environment measures. Standard errors clustered at the census tract level appear in parentheses in column 2, their associated $p$-values in column 3, and robust standard errors in brackets in column 4. In Panel A, CBD refers to the central business district, and km represents kilometers. The dependent variables in Panel B are calculated from 1940 census enumeration district data. Rents, house value, and income are presented in 1940 US dollars. The sample for both panels is restricted to census blocks where at least 50% of the housing was built between 1940 and 1960 and at least 50% had racial covenants ($\underline{c}_{jt} \geq 50$). The mean of the dependent (Dep.) variables is displayed in column 7. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

As shown in Panel A, neighborhoods have similar distances to the central business district (CBD) across treatment intensities. Additionally, the elevation (average and difference

between maximum and minimum slope), water in soil, depth to wet soil, and both flood probability and ponding frequency (all measured at 1953) are statistically similar across the baseline sample blocks that received different intensities of covenant enforceability treatment. Land elevation, slope, etc., are unlikely to have changed significantly between 1940 and 1953, but we also look for—and fail to find—any statistical differences across census blocks in the percentage of submerged areas and bogs (wetlands) from the digitized 1899 map, well before any development took place in our sample neighborhoods. Because developments within the baseline sample of census blocks were subdivided in statistically identical years, both *time to housing start* and *time to build* are statistically different across treatment intensities by design, hence why in Equations 1, 2, and 3 we control for the respective variable, depending on our outcome variable of interest.

The pretreatment built environment characteristics in the sample neighborhoods must also be similar across treatment intensities. In Panel B, the built environment measures from the 1940 census—population density; percentages of White, Black, and Other residents; unemployment rate; average rent; average self-reported house value; and average income—are statistically similar regardless of treatment intensity.[23]

### *Anticipation effects*

The US Supreme Court agreed to hear *Shelley* in October 1947 and issued its ruling in June 1948. Our empirical strategy requires no anticipation of the outcome of *Shelley* on the part of developers, public officials, or residents. This possibility is worrisome because the ruling came at a time when the advocacy of the National Association for the Advancement of Colored People (NAACP) was gaining momentum after World War II (WWII).

However, first, *Shelley* came five years before the liberal constitutional revolution of the Warren court began in 1953 (Horwitz, 1993). Second, at least 30 cases before *Shelley* at the district, state, and national levels had tried and failed to overturn the legality of racial covenants either state- or nationwide (see Appendix Table D.1).[24] *Shelley* was unexpected because contemporary observers believed that the Court's would follow its 1926 ruling in

[23]The 1940 census data are at the ED level, and the dependent variables are calculated with the mean ED value for the census blocks in our baseline sample.

[24]Two cases had voided racial covenants for specific houses or neighborhoods. However, neither struck down the enforceability of racial covenants city-, state-, or nationwide. A 1892 US Circuit Court ruling striking down the use of racial covenants, *Gandolfo v. Hartman*, "was never followed, probably because it was not

*Corrigan v. Buckley* upholding the enforceability of racial covenants (Jones-Correa, 2000; Rothstein, 2017). To address lingering concerns about anticipation, we run falsification checks in Section 4 and remove observations from the years leading up to *Shelley* (1947 and 1948) from our analysis in Section 6. Neither test indicates anticipation effects.

# 4. Evidence on Early-Stage Responses

In this section, we empirically show how the loss of covenant enforceability affected the decisions and actions of the three types of agents in the housing market and how their responses on along these margins could have shaped neighborhood formation.

## 4.1 Developer Decisions

The loss of covenant enforceability under *Shelley* shaped developers' decisions and in turn the characteristics of their housing at construction. The estimates from Equation 2 for the baseline sample of neighborhoods are in Table 2 Panel A, with standard errors clustered at census block–year level in parentheses. To ensure that remodeling over time does not bias the estimated results, we further restrict the baseline sample to only unremodeled houses.[25] A 1 SD increase in the covenant enforceability treatment in a neighborhood—equivalent to a 33.8 pp increase in the number of houses built between 1940 and 1948 in a covenanted neighborhood—corresponds, on average, to a 3.2% increase in the built-up area of houses (column 6). The same treatment amount corresponds, on average, to a modest increase of 0.03 bathrooms and 0.06 floors (1.9% and 4.8% of the respective means; column 7) but no statistical difference in the number of bedrooms. Thus, in response to the loss of covenant enforceability, developers adjusted and reduced housing size, indicating that racial covenants were complements to housing quality within census tracts.[26]

**Falsification test:** We conduct a falsification test using lot size, determined when the land was subdivided—well before construction. We find no statistical difference in the log lot

consistent with established interpretations of the Fourteenth Amendment" (Crooks, 1948). The 1940 US Supreme Court ruling in *Hansberry v. Lee* struck down only the covenants in the plaintiff and defendant's own development—not state- or nationwide—because the case was adjudicated as a *res judicata* violation of the due process clause of the 14th amendment (Kamp, 1986).

[25] This is true for all housing characteristics other than lot size, which is difficult to adjust over time.

[26] Differences in *time to housing start* across treatment intensities could plausibly reflect unobserved housing quality differences across houses and neighborhoods; the baseline estimates in Table 2 Panel A control for log time to housing start, but in Appendix Table C.1, the estimated parameter $\alpha_3$ is almost never significant.

Table 2: Early-Stage Responses of Different Agents

| Dependent Variable | $\alpha_1$ | s.e. (cluster) | s.e. (robust) | $N$ | $R^2$ | Effect of 1 SD increase | Mean of Dep. Variable |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel A: Developer Responses* | | | | | | | |
| Log Built Area | 0.0009** | (0.0004) | [0.0002] | 5,642 | 0.513 | 3.16 | – |
| No. Bedrooms | 0.0007 | (0.0005) | [0.0004] | 5,876 | 0.144 | – | 3.08 |
| No. Bathrooms | 0.0008* | (0.0005) | [0.0003] | 5,876 | 0.352 | 0.03 | 1.60 |
| No. Floors | 0.002*** | (0.0002) | [0.0001] | 5,769 | 0.200 | 0.06 | 1.25 |
| *Falsification:* Log Lot Size | -0.0003 | (0.0002) | [0.00009] | 10,261 | 0.737 | – | – |
| *Panel B: Voting by Residents of Newly Built Neighborhoods* | | | | | | | |
| Percentage of DFL Vote (1964) | -0.098** | (0.043) | [0.046] | 56 | 0.942 | -1.92 | 54.06 |
| Percentage of DFL Vote (1968) | -0.095** | (0.035) | [0.041] | 54 | 0.944 | -1.88 | 51.96 |
| *Panel C: Public Official Responses* | | | | | | | |
| Dist. Highways (m) | 1.593*** | (0.544) | [0.407] | 732 | 0.965 | 53.23 | 1,041 |
| Travel Time to Highway (mins) | 0.004*** | (0.001) | [0.001] | 732 | 0.991 | 0.15 | 19.30 |
| Share Single-Family | 0.024 | (0.028) | [0.017] | 732 | 0.961 | – | 71.06 |
| Min. Lot Size (sq ft) | 0.767 | (1.057) | [2.454] | 725 | 0.859 | – | 7,926 |
| *Falsification:* Dist. Lakes & Parkways (m) | -0.370 | (0.499) | [0.375] | 732 | 0.836 | – | 1,007 |

Note: This table presents parameter $\alpha_1$ (column 1) from Equations 2 (Panel A) and 1 (Panels B and C). In Panel A, the dependent variables are housing characteristics, in Panel B the percentage of votes for Minnesota's Democratic-Farmer-Labor Party (DFL) candidates in the 1964 and 1968 congressional races, and in Panel C local public amenities, including the Euclidean distance (dist.) in meters (m) and travel time in minutes (mins) to highways, share of single-family-only zoned area, minimum lot size in square feet (sq ft), and distance to lakes and associated parkways in meters. Standard errors clustered at the 2020 census block level (Panel A), 1980 census tract level (Panel B), and 2020 census tract level (Panel C) appear in parentheses (column 2). Robust standard errors are in brackets (column 3). The sample of census blocks is restricted to census blocks or precincts where at least 50% of the housing was built between 1940 and 1960 and at least 50% had racial covenants ($\underline{c}_{jt} \geq 50$) for Panels A and C and greater than 0% had racial covenants ($\underline{c}_{jt} > 0$) for Panel B. The sample for Panel A is additionally restricted to houses built between 1940 and 1960 and those never remodeled in terms of log built area or numbers of bedrooms, bathrooms, or floors; there is no remodeling restriction when the dependent variable is log lot size. The effect of a 1-standard-deviation (SD) increase in covenant enforceability intensity for statistically significant variables is given in column 6. The means of the non-log dependent (Dep.) variables are given in column 7. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

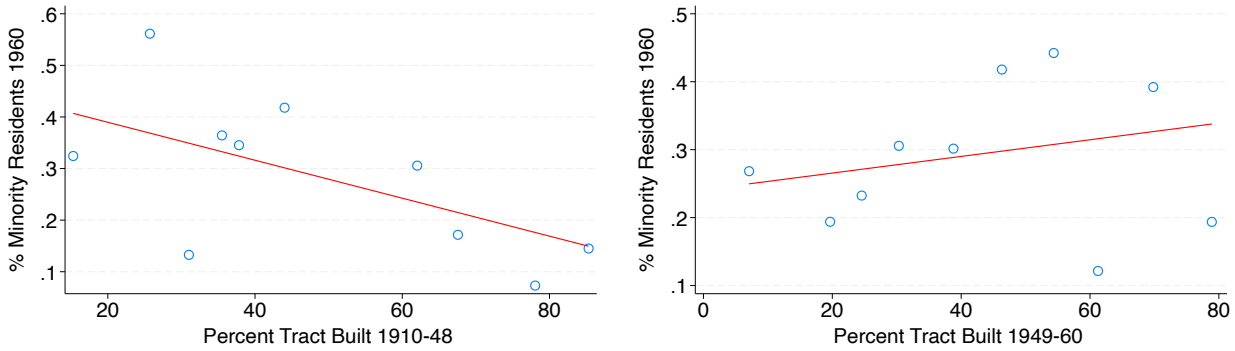
size across houses with different treatment intensity, underscoring that the neighborhoods in our majority-covenanted sample built before 1948 were ex ante designed to be similar to those built after.

## 4.2 Neighborhoods' Early Resident Composition

In this section, we provide evidence on the effect of the loss of covenant enforceability on the decisions of residents of the newly built covenanted neighborhoods, considering both

racial composition and racial attitudes. First, we test for racial differences among initial occupants of the newly built neighborhoods by treatment intensity. Since population data by race are not available at census block level for 1960, we define neighborhoods at the coarser census tract level.[27] Figure 5 plots the percentage of racial minority residents in 1960 against the percentage of census tracts built from 1910 to 1948 (left) and from 1949 to 1960 (right). To accommodate the coarser grid of analysis, we restrict our attention to census tracts with at least 20% covenanted housing. There is an inverse relationship between the percentage of tracts built between 1910 and 1948 and the tract-level percentage of racial minority residents in 1960.[28] Conversely, the relationship between the percentage of houses built between 1949 and 1960 (post-*Shelley*) and the tract-level racial minority resident share is positive. The available data for 1960 are too coarse for us to establish causal effects with the neighborhood-level model in Equation 1, but the figure suggests some differences in racial composition among the 1960 occupants of covenanted tracts primarily built between 1910 and 1948 and those primarily built between 1949 and 1960, with the latter being more racially diverse.

Figure 5: Percentage of Minority Residents (1960 Census Tracts)




Note: The left panel shows the percentage of racial minority residents (measured at census tract level) in 1960 plotted against the percentage of the tract built from 1910 to 1948. The right panel shows the same comparison for percentage of the tract built from 1949 to 1960. This applies to census tracts in which over 20% of the housing was covenanted.

Second, as highlighted in Section 3.2.1, we infer racial attitudes from precinct-level vote shares for Democratic (Minnesota's Democratic-Farmer-Labor Party, DFL) candidates in

[27] While block-level data were collected for both the population and housing census in 1960, only the housing census information is still available.

[28] We do not expect to observe complete racial segregation at tract level because the local racial minority population in 1960 was too small for this to have occurred.

the 1964 and 1968 congressional races. In Table 2 Panel B, a 1 SD increase in treatment in a voting precinct, equivalent to 19.9 pp, corresponds to, on average, a 1.92 pp and 1.88 pp fall in the voting percentage for Democratic congressional candidates in 1964 and 1968—a moderate fall of 3.55% and 3.62% in the mean Democratic vote percentages in 1964 and 1968 (column 7).[29] Figure 5 and Table 2 Panel B together suggest that residents who bought and lived in covenanted neighborhoods built by 1948 were White and more racially prejudiced than their counterparts in covenanted neighborhoods built after 1948.

## 4.3 Local Public Amenity Responses

*Shelley* marginally affected the decisions and actions of national and local public officials, particularly decisions on where to place interstate highways in the 1960s. A 1 SD (33.5 pp) increase in covenant enforceability intensity in a neighborhood corresponds to highway placement on average 53.2 m further away—a 5.1% increase in the mean Euclidean distance (Table 2 Panel C). That neighborhoods more exposed to enforceable covenants sit further from highways implies they enjoy reduced barrier, noise, and pollution disamenity effects. But highways also shorten commutes (Brinkman and Lin, 2024), and this amenity gain comes at the cost of longer commutes: A 1 SD increase in treatment intensity lengthens commutes by 0.15 mins (9 seconds). However, the amenity value of this 9-second difference is minuscule relative to that of a 53.2 m distance from a highway: Moretti and Wheeler (2025) find that noise and pollution effects peak within 100 m of a highway. For zoning, we find no statistical differences from treatment from 1948 through the 1970s in terms of either the share of area zoned for single-family use only or minimum lot size.

**Falsification test:** As highlighted in Section 3.2.1, lake alterations and associated park development coincided with subdivision and covenanting. The null differences in these amenities across treatment intensities thus serve as another falsification test, demonstrating that covenanted neighborhoods built before and after 1948 were ex ante similar in design.[30]

[29] Again, we use the lower cutoff of $(\%\text{cov houses})_{jt} > \underline{c}_{j,1960} = 0\%$ because precincts are much larger than census blocks. Standard errors clustered at 1980 census tract level appear in parentheses.

[30] The baseline specifications for highways consider the 1940–1960 timeline, but since highway plans were publicly released in 1957, we truncate our robustness sample period to 1940–1956 and find effects statistically similar to baseline (Appendix Table C.10). In addition, for zoning regulations and distance to lakes and parkways, we use $(\%\text{built} \leq 1948)_{jt}$ and control for *time to build*. When we use $(\%\text{start} \leq 1948)_{jt}$ and *time to housing start* as control, we again find results statistically similar to baseline (Appendix Table C.10).

In summary, we find evidence that the loss of covenant enforceability under *Shelley* affected the decisions of developers to a larger extent and local and national public officials to a smaller extent, with consequences for neighborhoods' built environment. Suggestive and empirical evidence also indicates that both the racial makeup and racial attitudes of residents in the 1960s varied with neighborhoods' exposure to enforceable covenants.

# 5. Long-Run Outcomes

This section studies the long-run consequences of *Shelley* for racial segregation, racial attitudes, and housing values, tracing how initial differences in the neighborhood built environment and residential composition continued to shape neighborhood outcomes decades later. The effects measured here are a composite of the early responses of developers, public officials, and initial residents to *Shelley*, their persistence over time, and decisions made by agents in the housing market between the mid-20th century and today.

## 5.1 Racial Segregation 1980–2020

The treatment had a long-run effect on racial segregation in residency and ownership. The estimated parameter $\alpha_1$ from Equation 1 is shown in Table 3 for the baseline sample of neighborhoods, with the dependent variables being the percentages of White, Black, and other racial minority residents over 1980–2020 and of owner-occupied housing units by race over 1980–2010.[31] Standard errors clustered at census tract–year level are in parentheses.

A 1 SD (33.5 pp) increase in treatment intensity in a neighborhood corresponds, on average, to a 0.4–1.5 pp increase in the percentage of White residents within the neighborhood from 1980 to 2020. This effect accounts for approximately 0.4–1.7% of the mean neighborhood White resident percentage between 1980 and 2020 (the number in column 6 divided by that in column 7 in Table 3). In contrast, a 1 SD increase in treatment intensity corresponds to a 0.6 pp lower Black resident percentage and 0.3–1.1 pp lower percentage of residents of other races, an effect explaining 13–24% and 6–23% of the mean neighborhood Black and other minority resident percentages, respectively, between 1980 and 2020. Thus, treatment has exerted economically meaningful effects on resident segregation by race over the past five decades.

[31] The Census Bureau did not release block-level statistics on owner-occupied housing units by race for 2020 (Ruggles, 2024).

The impact on segregation in homeownership by race is also significant and persistent. A 1 SD increment in treatment intensity corresponds to a 1.5–2.5 pp increase in the numbers of White owner-occupied housing units, a 0.3 pp fall in Black owner-occupied units, and a 0.3–0.5 pp fall in units occupied by owners of other racial minorities between 1980 and 2010.[32] These effects account for 1.7–3%, 23%, and 13–29% of the average neighborhood White, Black, and other racial minority shares of owner-occupied housing units, respectively, over 1980–2010. A comparison of the results in Panels A–C and Panels D–F shows that the persistent effects are slightly more substantial in percentage terms for segregation in ownership than for segregation in residency, which affects both renters and homeowners. This finding is expected, as homeownership is a more persistent outcome than renting.[33]

For the five-decade period, the estimated coefficients are statistically similar across all census years, except for a statistical increase in absolute terms in the estimated parameters for the Black resident percentage for 1990–2000.[34] The rise in the segregation effects between 1990 and 2000 could be due to the overall increase in the county population or rise in the Black population share during that period (see Appendix Table A.4). In addition, although the estimated parameters are almost always statistically similar across the five census years, the average persistent impact as a share of the mean degree of racial segregation in residency and homeownership decreases over time for Black and other racial minorities (as can be seen from dividing the number in column 6 by that in column 7 in Table 3). The effects on White resident and homeowner segregation remained relatively consistent

[32] We cannot estimate the impact on 1980 housing units occupied by owners of other racial minorities because this group accounted for only 3% of the county's population in 1980 and there was no variation in its homeownership rates across the neighborhoods in the baseline sample. Additionally, the census lacks a complete race breakdown for owner-occupied units for all census blocks, resulting in fewer observations for 1980. Note also that the dependent variable means (column 7) should not sum to 100 across all races when we consider homeownership segregation (Panels D–F), as we calculate the baseline measure by dividing the race-specific number of owner-occupied housing units by the total number of owner-occupied and renter-occupied housing units in the neighborhood. See Section 6 for alternative measures.

[33] In Appendix Figure C.2, the effects on White and other racial minority residents and homeowners are primarily driven by neighborhoods where 51–75% of the units were built in 1940–1960, while the effects on Black residents and homeowners are primarily driven by neighborhoods where 76–100% of the units were built in 1940–1960. For both homeowners and residents, the effects on other racial minorities arise mostly from effects on Asians and Pacific Islanders; see Appendix Table C.2. For this table, we show only the effects on American Indians and Asians and Pacific Islanders, as these are the only two groups in the other racial minority category with data for each of the census years over 1980–2020.

[34] The $p$-values from the Hausman test for differences in parameter estimates $\alpha_1$ over time are mostly greater than 0.1, except for the Black resident percentage for 1990–2000, which has a $p$-value of 0.032.

Table 3: Racial Segregation 1980–2020

| | $\alpha_1$ | s.e. (cluster) | s.e. [robust] | $N$ | $R^2$ | Effect of 1 SD increase | Mean of Dep. Variable |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel A Dependent Variable: Percentage of White Residents* | | | | | | | |
| 1980 | 0.012*** | (0.004) | [0.005] | 830 | 0.151 | 0.41 | 97.84 |
| 1990 | 0.017** | (0.007) | [0.007] | 830 | 0.191 | 0.56 | 96.42 |
| 2000 | 0.034*** | (0.012) | [0.012] | 795 | 0.330 | 1.15 | 92.48 |
| 2010 | 0.043** | (0.019) | [0.018] | 791 | 0.416 | 1.45 | 87.28 |
| 2020 | 0.038* | (0.020) | [0.022] | 731 | 0.428 | 1.27 | 77.17 |
| *Panel B Dependent Variable: Percentage of Black Residents* | | | | | | | |
| 1980 | -0.003 | (0.002) | [0.003] | 830 | 0.127 | – | 0.71 |
| 1990 | -0.002 | (0.003) | [0.004] | 830 | 0.220 | – | 1.41 |
| 2000 | -0.018*** | (0.006) | [0.007] | 795 | 0.308 | -0.60 | 2.54 |
| 2010 | -0.017* | (0.010) | [0.011] | 791 | 0.325 | -0.58 | 4.48 |
| 2020 | -0.005 | (0.013) | [0.014] | 731 | 0.327 | – | 5.03 |
| *Panel C Dependent Variable: Percentage of Other Minority Residents* | | | | | | | |
| 1980 | -0.009** | (0.004) | [0.004] | 830 | 0.131 | -0.30 | 1.45 |
| 1990 | -0.015*** | (0.006) | [0.006] | 830 | 0.127 | -0.50 | 2.16 |
| 2000 | -0.016 | (0.010) | [0.010] | 795 | 0.205 | – | 4.97 |
| 2010 | -0.026* | (0.014) | [0.013] | 791 | 0.323 | -0.88 | 8.24 |
| 2020 | -0.033* | (0.018) | [0.018] | 731 | 0.341 | -1.10 | 17.80 |
| *Panel D Dependent Variable: Percentage of White Owner-Occupied Housing Units* | | | | | | | |
| 1980 | 0.007 | (0.018) | [0.020] | 649 | 0.240 | – | 92.22 |
| 1990 | 0.046** | (0.018) | [0.021] | 830 | 0.274 | 1.53 | 89.20 |
| 2000 | 0.064*** | (0.019) | [0.022] | 795 | 0.273 | 2.16 | 88.73 |
| 2010 | 0.074*** | (0.025) | [0.025] | 791 | 0.346 | 2.48 | 83.52 |
| *Panel E Dependent Variable: Percentage of Black Owner-Occupied Housing Units* | | | | | | | |
| 1980 | 0.000 | (0.000) | [0.000] | 649 | 0.503 | – | 0.19 |
| 1990 | -0.002 | (0.002) | [0.003] | 830 | 0.182 | – | 0.78 |
| 2000 | -0.010** | (0.005) | [0.005] | 795 | 0.277 | -0.34 | 1.45 |
| 2010 | -0.007 | (0.006) | [0.005] | 791 | 0.202 | – | 1.84 |
| *Panel F Dependent Variable: Percentage of Other Minority Owner-Occupied Housing Units* | | | | | | | |
| 1990 | -0.009** | (0.003) | [0.003] | 830 | 0.110 | -0.29 | 1.00 |
| 2000 | -0.013** | (0.006) | [0.006] | 795 | 0.149 | -0.45 | 2.16 |
| 2010 | -0.014* | (0.008) | [0.008] | 791 | 0.196 | -0.45 | 3.59 |

*Notes:* This table displays the parameter estimate $\alpha_1$ (column 1) from Equation 1. The dependent variables are resident percentages by race (Panels A–C) and percentages of owner-occupied housing units by race (Panels D–F) over time. The housing characteristics $X_{jt}$ include the mean log of lot size and built area, mean number of bedrooms and bathrooms, and the share of houses with fewer than or equal to 1.75 floors in a census block. Standard errors clustered at census tract level are in parentheses (column 2). Robust standard errors in brackets (column 3). The sample of census blocks is limited to those where at least 50% of the block was built between 1940 and 1960 and at least 50% had racial covenants ($\underline{c}_{jt} \geq 50$). Column 6 is the effect of a 1-standard-deviation (SD) increase in intensity of the covenant enforceability treatment for statistically significant variables. Column 7 shows means of the dependent (Dep.) variables. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

between 1990 to 2020. Thus, similarly to the effects of the HOLC (Aaronson et al., 2021), the impact of the loss of racial covenant enforceability on racial segregation began to decay

only in the 21st century, especially for other racial minorities.

While the effects on other racial minorities prevailed throughout the past five decades, the effects on Black residents and owners appeared only in the 21st century. From 1980 to 2000, the percentages of Black and other racial minority residents in the metro area grew similarly (Appendix Table A.4); however, the mean percentage of Black residents increased from 0.7% to 2.5%, while that of other racial minorities rose from 1.5% to 5% in our baseline sample (Table 3 column 7). This suggests that during the 1980s and 1990s, Black residents mostly lived in neighborhoods outside our sample of covenanted neighborhoods in historically lower-middle- and middle-income areas—a market segment that became widely accessible in pricing for Black residents and homeowners only recently.

## 5.2 Racial Attitudes in 2020

Next, we infer racial attitudes in the present from minor civil disturbances after George Floyd's death in 2020. Table 4 Panel A indicates that a 1 SD increase in treatment intensity in a census block, equivalent to 32.4 pp, corresponds, on average, to 0.005 fewer minor civil disturbances—approximately 31.3% of the mean total number of minor civil disturbances in a neighborhood in May 2020. We treat this evidence as suggestive because we use the lower cutoff of $(\%\text{cov houses})_{jt} > \underline{c}_{j,2020} = 0\%$ given the small variation in this outcome when we restrict the sample to neighborhoods that were at least 50% covenanted.

## 5.3 Long-Run Housing Value Effects

The loss of covenant enforceability had a persistent effect on house values in the 21st century. Table 4 Panel B shows the estimated coefficient from Equation 2. Standard errors clustered at the 2020 census block level are in parentheses. A 1 SD increase in treatment intensity in a neighborhood, equivalent to 33.4 pp, corresponds to, on average, 3.1% higher 2019 assessed values and 4.4% higher sales prices in 2010–2019. That the estimated assessed value effects are slightly smaller than the estimated sale price effects implies that the difference across treatment intensity is smaller for the former than for the latter, in line with the findings from Avenancio-León and Howard (2022) that the assessed values of Black- and Hispanic-owned houses (less likely to have been subject to higher-intensity treatment) are higher than those of White-owned houses (likelier to have been subject to

Table 4: Long-Run Racial Attitudes and Housing Values

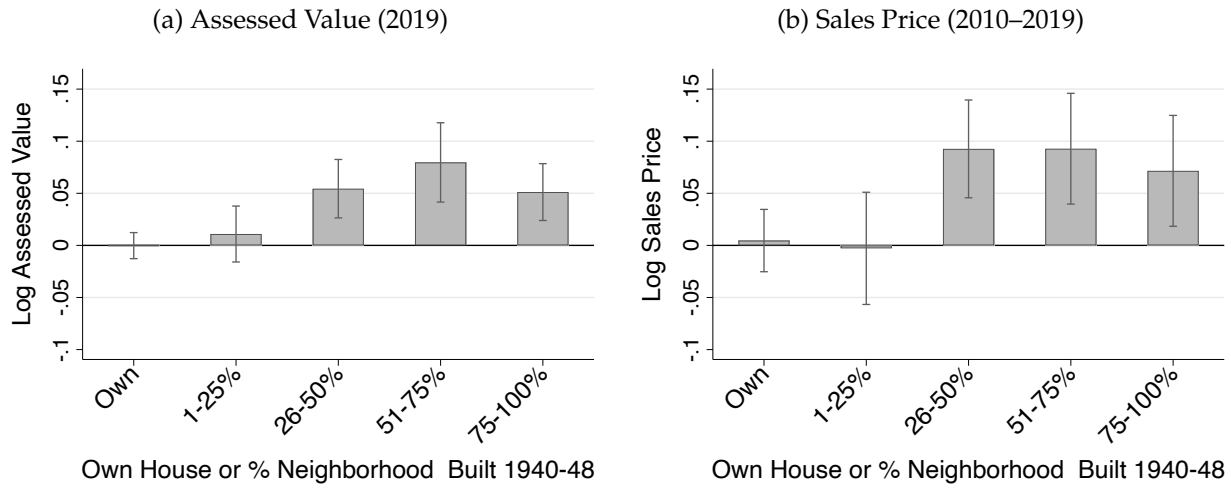
| | $\alpha_1$ | s.e. | $N$ | $R^2$ | Effect of 1 SD increase | Mean of Dep. Variable |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Racial Attitudes in 2020* | | | | | | |
| Total Minor Civil Disturbances | -0.0002*** | 0.00005 | 1,296 | 0.339 | -0.005 | 0.016 |
| *Panel B: Long-Run House Value Effects* | | | | | | |
| Log Assessed Value (2019) | 0.0009*** | (0.0002) [0.0001] | 11,438 | 0.733 | 3.089 | – |
| Log Sales Price (2010—2019) | 0.0013*** | (0.0003) [0.0003] | 4,610 | 0.414 | 4.367 | – |

*Notes:* This table displays the parameter estimate $\alpha_1$ (column 1) from Equations 1 in Panel A and 2 in Panel B. For Panel A, the dependent variable is the number of minor civil disturbances reported after George Floyd's death in 2020 and includes controls for socioeconomic status (median income, college education), share of the population under 18, median house value, and average block-level house characteristics (log lot size, log built area, numbers of bedrooms, bathrooms, and floors). Panel B dependent variables are log assessed value (2019) and log sales price (2010–2019); standard errors clustered at census block level appear in parentheses and robust standard errors in brackets (column 2). The sample of census blocks is restricted to those where at least 50% of the housing was built between 1940 and 1960. Additionally, the sample in Panel A includes no restriction on the covenanted housing share ($\underline{c}_{jt} \geq 0$). In Panel B, the sample includes only blocks with least 50% covenanted housing ($\underline{c}_{jt} \geq 50$). The effect of a 1-standard-deviation (SD) increase in covenant enforceability treatment intensity for statistically significant variables is given in column 5. The means of non-log dependent (Dep.) variables are reported in column 6 * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

higher-intensity treatment). Thus, though the assessed value estimates include all houses in the baseline sample, they may be biased and serve as a conservative estimate of the 21st-century house value differential across treatment intensities.

When we compare the effects on segregation with those on housing values, the impact on housing values is evidently greater in magnitude. This is primarily because a wide range of factors influence house prices. Even after we control for house characteristics and local public amenities, a 1 SD increase in treatment intensity in a neighborhood corresponds, on average, to 2.3% higher 2019 assessed values and 3.4% higher 2010–2019 sales prices (see Appendix Table C.3 Panels A and B; further discussion in Appendix C.2). Thus, differences in these observables do not fully account for the persistent house value differentials in the 21st century due to the loss of enforceability. The remaining variations in house values may stem from unobservable housing characteristics that we cannot control for and from segregation by residents' race and racial attitudes, as detailed in Sections 5.1 and 5.2.

Figure 6: Nonlinear House Value Effects




Note: This figure plots the parameter estimate $\alpha_1$ (Own) and $\beta_m$ for $m$ = 1–25%, 26–50%, 51–75%, 76–100%, where the omitted category is $m$ = 0%, from Equation 3. The dependent variables are log assessed value (2019) and log sales price [2010–2019]. Standard errors are clustered at census block level, and 90% confidence intervals are reported. The sample of census blocks is restricted to those where at least 50% of the housing was built between 1940 and 1960 and at least 50% was covenanted ($\underline{c}_{jt} \geq 50$).

The average house value effects of 3.1–4.4% also mask nonlinearities in the externalities of heavy covenanting. Figure 6 plots the parameter estimates $\alpha_1$ (Own) and $\beta_m$ from Equation 3 for different treatment intensities ($m$ = 1–25%, 26–50%, 51–75%, and 76–100%, where the omitted category is $m$ = 0%). The figure shows that an enforceable covenant only on *one's own* house did not affect its value: The value of racial covenant enforceability arose only when the majority of one's neighbors' covenants were also enforceable. If only 1–25% of a neighborhood was treated compared to 0%, there is no statistically significant effect on 21st-century house values. However, if 26–50%, 51–75%, and over 75% of a neighborhood was treated, assessed values rise by 6.0%, 8.6%, and 5.5%, respectively, over those in neighborhoods with 0% treatment, and sales prices increase by 9.1%, 9.9%, and 7.4%, respectively.

# 6. Robustness and Additional Results

**Robustness of housing characteristics results:** At baseline, we restricted the sample to only unremodeled houses, except in the analysis of lot size, as this characteristic is harder to adjust over time. To ensure that this choice does not drive the results on housing characteristics, we remove the remodeling restriction and find statistically similar

results (see Appendix Table C.1 Panel A). Additionally, to address concerns that housing construction followed construction trends or the 1940–1960 window is too wide, we restrict the sample to a narrower window (housing built in 1944–1953), where the $(\%\text{built} \leq 1948)_{jt}$ in Equation 2 refers to either at least 50% built between 1940 and 1960 (Table C.1 Panel B) or 1944 and 1953 (Table C.1 Panel C). The results are statistically similar to baseline.

**Placebo tests:** There could be a threat to identification if houses built in the 1940s differed systematically from those built in the 1950s, potentially because of WWII or stylistic changes, confounding our treatment intensity measure. To test this threat, we run a placebo test on neighborhoods built during the 1940–1960 period but not covenanted. In particular, we estimate Equations 1 and 2 for the sample of neighborhoods where 0% or at most 10, 25, or 35% were covenanted ($\%\text{cov houses}_{jt} \leq \underline{c}_{jt} = 0, 10, 25, 35$). Table 5 Column Group I shows only a few statistically significant differences in racial segregation over time or house values in noncovenanted neighborhoods built between 1940 and 1960. In fact, as the percentage of covenanted houses in low-covenanted neighborhoods rises from at most 10% to at most 35%, some effects start becoming statistically significant, as we would expect (Appendix Tables C.3 Panels I–K and C.7 Column Groups I–III.)

One might also be concerned that simply comparing uncovenanted neighborhoods $\%\text{cov houses}_{jt} \leq \underline{c}_{jt} = 0$ with those that were majority covenanted is not the ideal experiment as these neighborhoods obviously differ. Thus, we use an entropy-balancing framework (Basri et al., 2021), which assigns weights to the placebo sample to align it with the baseline sample based on population, share of adults over 18, and share with high school diplomas at the census block group level. Again, we find very few statistically significant differences in racial segregation in the sample of uncovenanted neighborhoods with the entropy-balanced sample (Appendix Table C.7 Column Group IV).

If houses built in the 1940s were simply larger than those from the 1950s because of changing styles or post-WWII factors, this could confound the baseline effects in Table 2. To test this, we estimate the house characteristic effects $\%\text{cov houses}_{jt} \leq \underline{c}_{jt} = 0, 10$ for houses built in 1940–1960 and 1944–1953. Table C.4 shows no differences in the number of bathrooms or built area, but both lot size and the number of bedrooms were smaller in the 1940s than in the 1950s in the sample of neighborhoods with little or no covenanting.

This suggests that all the houses in our sample were designed the same and that the drop in house size after 1948 came from the loss of covenant enforceability, in contrast to the broader trend of houses being smaller in the 1940s than the 1950s.[35]

**Placebo time periods:** We also examine whether placebo tests simulating alternative years of the *Shelley* decision yield similarly persistent effects, which could indicate that we are merely capturing differences between the 1940s and 1950s at baseline. The placebo time periods are 1920–1940 (with 1930 as the year of the "*Shelley*" decision), 1927–1947 (1935, respectively), 1928–1948 (1936), 1945–1964 (1954), 1949–1969 (1957), 1950–1970 (1958), 1951–1971 (1959), and 1952–1972 (1960). Appendix Table C.9 shows only 4 statistically significant $\alpha_1$ estimates out of 136 regressions on racial segregation and house price differentials.

**Spatial spillovers:** At baseline, we compared census blocks within census tracts to control for unobserved location quality. However, if the spatial spillovers from racial covenants extended beyond tracts, the choice of tract fixed effects could attenuate or amplify the baseline estimates. Accordingly, we estimate Equations 1 and 2 for the baseline sample of neighborhoods with fixed effects for zip codes, which cover, on average, 6.5 times more housing units than census tracts. In Table 5 Column Group II, the estimates with zip code fixed effects are overall statistically similar to baseline—if anything, slightly attenuated in the case of segregation in homeownership (see Appendix Table C.5 Column Group III for robustness of the ownership results). This downward bias is consistent with our finding that racial covenants were applied in transition lower-middle- and middle-income regions of likely lower-to-intermediate quality within the larger metropolitan area.

**Alternative house and neighborhood samples:** The baseline sample is restricted to census blocks where at least 50% of the lots were covenanted and at least 50% of the houses were built between 1940 and 1960. To ensure that the 50% cutoff points do not drive the baseline results, we use a more conservative sample of census blocks where at least 75% of the houses were built between 1940 and 1960 and at least 75% of the houses were covenanted ($\underline{c}_{jt} \geq 75$). This restriction shrinks the baseline sample by approximately one-third. In

[35]The number of floors in the 1940s is larger than that in the 1950s in both our sample and the placebo sample. To ensure that this difference does not drive the results, we control in the baseline segregation specification for all housing characteristics, including number of floors. The house value estimates controlling for house characteristics (including number of floors) are also statistically significant (Appendix Table C.3 Panel A).

Table 5: Robustness: Long-Run Racial Segregation and Housing Value Effects

| Samples / Models | (I) Placebo Test ($\underline{c}_{jt} = 0$) | | | | (II) $\eta_{k(j)}$: ZIP Code FE | | | (III) ≥75% built 1940–1960 & $\underline{c}_{jt} \geq 75$ | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | $\alpha_1$ | s.e. | $N$ | $R^2$ | $\alpha_1$ | s.e. | $R^2$ | $\alpha_1$ | s.e. | $N$ | $R^2$ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| *Panel A Dependent Variable: Percentage of White Residents* | | | | | | | | | | | |
| 1980 | 0.006 | (0.004) | 5,304 | 0.755 | 0.012*** | (0.003) | 0.229 | 0.013** | (0.005) | 555 | 0.154 |
| 1990 | 0.004 | (0.004) | 5,448 | 0.753 | 0.018*** | (0.006) | 0.156 | 0.018* | (0.010) | 558 | 0.191 |
| 2000 | 0.004 | (0.007) | 5,334 | 0.760 | 0.036*** | (0.012) | 0.264 | 0.022 | (0.014) | 513 | 0.308 |
| 2010 | 0.016** | (0.008) | 5,226 | 0.669 | 0.040* | (0.021) | 0.273 | 0.043* | (0.022) | 483 | 0.368 |
| 2020 | 0.018* | (0.011) | 4,754 | 0.624 | 0.039* | (0.019) | 0.292 | 0.015 | (0.022) | 430 | 0.475 |
| *Panel B Dependent Variable: Percentage of Black Residents* | | | | | | | | | | | |
| 1980 | -0.005 | (0.003) | 5,304 | 0.822 | -0.002 | (0.003) | 0.326 | -0.007* | (0.004) | 555 | 0.179 |
| 1990 | -0.001 | (0.003) | 5,448 | 0.826 | -0.004 | (0.004) | 0.220 | -0.003 | (0.005) | 558 | 0.256 |
| 2000 | -0.002 | (0.004) | 5,334 | 0.761 | -0.020** | (0.009) | 0.245 | -0.015* | (0.008) | 513 | 0.292 |
| 2010 | -0.003 | (0.006) | 5,226 | 0.607 | -0.014 | (0.010) | 0.234 | -0.020* | (0.011) | 483 | 0.268 |
| 2020 | -0.016*** | (0.006) | 4,754 | 0.591 | 0.0004 | (0.010) | 0.251 | -0.004 | (0.015) | 430 | 0.311 |
| *Panel C Dependent Variable: Percentage of Other Minority Residents* | | | | | | | | | | | |
| 1980 | -0.001 | (0.003) | 5,304 | 0.269 | -0.010*** | (0.002) | 0.067 | -0.006 | (0.004) | 555 | 0.137 |
| 1990 | -0.002 | (0.004) | 5,448 | 0.298 | -0.014** | (0.006) | 0.077 | -0.015** | (0.007) | 558 | 0.148 |
| 2000 | -0.002 | (0.005) | 5,334 | 0.425 | -0.016* | (0.009) | 0.137 | -0.007 | (0.012) | 513 | 0.203 |
| 2010 | -0.013** | (0.006) | 5,226 | 0.419 | -0.025 | (0.015) | 0.192 | -0.023 | (0.018) | 483 | 0.310 |
| 2020 | -0.002 | (0.010) | 4,754 | 0.369 | -0.039*** | (0.012) | 0.208 | -0.011 | (0.020) | 430 | 0.407 |
| *Panel D Dependent Variable: House Values* | | | | | | | | | | | |
| Log Ass. Value | -0.0002 | (0.0002) | 53,975 | 0.608 | 0.0007 | (0.0006) | 0.589 | 0.0005** | (0.0002) | 6,764 | 0.776 |
| Log Sales Price | 0.0004 | (0.0003) | 21,653 | 0.408 | 0.0013* | (0.0007) | 0.348 | 0.0010** | (0.0004) | 2,609 | 0.423 |

Note: This table plots the parameter $\alpha_1$ (column 1) from Equation 1 in Panels A–C and Equation 2 in Panel D. In Panels A–C, the dependent variables are the percentages of residents by race over time, and standard errors are clustered at census tract level. In Panel D, the dependent variables are log assessed value (2019) and log sales price (2010–2019), and standard errors are clustered at census block level. The first sample of census blocks (Column Group I) is restricted to census blocks where at least 50% of the housing was built between 1940 and 1960 and 0% had racial covenants ($\underline{c}_{jt} = 0$). Column Group II uses the baseline sample with zip code fixed effects (FE) ($\eta_{k(j)}$) and standard errors clustered at the zip code level. The last sample of census blocks (Column Group III) is restricted to census blocks where at least 75% of the houses were built between 1940 and 1960 and at least 75% had racial covenants ($\underline{c}_{jt} \geq 75$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 and Appendix Table C.5 Column Group IV, the estimates on racial segregation in residency and homeownership and house values are again statistically similar to baseline,

albeit with larger standard errors due to a smaller sample size.[36]

**Alternative ownership measures:** At baseline, we calculated the percentage of owner-occupied housing units across different racial groups by dividing the race-specific number of owner-occupied units by the total number of neighborhood occupied units (both owned and rented) within a census block over time. Our first alternative measure divides the race-specific number of owner-occupied units by the total number of units, including vacant ones. Appendix Table C.5 Column Group I shows these effects are statistically similar to baseline, so vacant units do not drive the results. The second alternative measure divides the race-specific number of owner-occupied units by only the number of owner-occupied units within the census block, which may yield different estimates from baseline if homeownership rates vary significantly across space or time. For the Black and other racial minority categories, the estimates are similar to baseline, but those for White owner-occupied housing units shrink by approximately half (see Appendix Table C.5 Column Group II). This finding implies that racial covenants had a less persistent effect on White homeowners but persistently affected the choice of owning versus renting for White residents in the covenanted neighborhoods built between 1940 and 1960.

**Additional controls:** Our baseline estimates could be driven by not only the persistent effect of covenant enforcement but also the differences in socioeconomic status (SES) across racial groups. Accordingly, we add controls for population and population share over 18 at census block level and population share with more than a high school diploma and median income at census block group level to Equations 1 and 2 in the baseline models.[37] These SES controls produce no statistical change in the baseline results (Appendix Tables C.3 Panel C and C.6 Column Group I). We also control for *time to housing start* in addition to *time to build* in the baseline models for long-run outcomes and find statistically similar effects (Appendix Tables C.3 Panel D and C.6 Column Group II). One may also be concerned that the language in racial covenant contracts—specifically, which and how many racial and ethnic groups are targeted (Appendix Figure B.1), affects our baseline results. However,

[36] Additionally, when we restrict the sample to a narrower window (housing built in 1944–1953), where $(\%\text{built} \leq 1948)_{jt}$ in Equation 2 refers to at least 50% built between 1940 and 1960, we find that our results are statistically similar to baseline house value differentials (Table C.3 Panel H).

[37] We do not include education and income statistics for the 1980 model as they are reported only at tract level and thus are already accounted for by the tract–year fixed effects.

when we control for the total number of racial groups listed in the covenant language in the segregation and housing value models, we observe effects statistically similar to baseline (Appendix Tables C.3 Panel G and C.6 Column Group V). Last, the baseline model for segregation controls for house characteristics but not housing values, as we do not have those for 1980 and 1990. In Appendix Table C.8, we control for median housing values at block group level for 2000–2020 and sales prices for 2010 and 2020. The estimates remain statistically similar to baseline.[38]

**Anticipation effects:** Recall from Section 3.4 that to rule out anticipation effects, we redefined our treatment intensity variable to capture the percentage of the neighborhood built between 1940 and 1946 or 1947 and considered only neighborhoods at least 50% built between 1940 and 1946 or 1947 and between 1949 and 1960. The sample for Appendix Tables C.3 Panels E and F and C.6 Column Groups III and IV excludes both 1947 and 1948 or just 1948 and yields results overall statistically similar to baseline.

**Property teardown rates:** The data on house build year are from 2019 tax assessor records. However, if teardown rates vary with treatment intensity, then the treatment effect could be positively or negatively confounded by this measurement error. Accordingly, we study teardown rates over time in American Housing Survey (AHS) data. While these lack the exact year when houses were built, the year-of-construction categories coincide well with the build years of our sample neighborhoods. We plot the difference in the teardown rate for houses built in 1940–1949 and 1950–1959 from 1974, the first year of the AHS, for Minneapolis metro area in Appendix Figure C.3. While we cannot completely rule out that houses with racial covenants in the 1940–1949 and 1950–1959 categories differed in teardown rates, as the AHS categories include both noncovenanted properties (not included in our baseline sample) and covenanted properties, the fact that the $t$-test shows the teardown difference is not statistically different from zero for any sample year reduces the possibility of a teardown bias in our estimation.

**Alternative empirical design:** The baseline empirical design restricts attention to majority-covenanted neighborhoods (to control for the endogenous choice to covenant) and uses

[38] One may be concerned that a correlation between the covenant enforceability treatment and (mean time to build/start)$_{jt}$ drives our key results; however, omitting the (mean time to build/start)$_{jt}$ control yields results comparable to all the baseline estimates (untabulated results available upon request).

the 1948 ruling and 1940–1960 timeline to construct the treatment intensity—an approach that, we argue, delivers the cleanest causal estimates of the impact of loss of covenant enforceability. An alternative empirical design might instead compare covenanted neighborhoods built by 1948 with covenanted neighborhoods built after 1948 and uncovenanted neighborhoods built between 1940 and 1960, but we believe this does not adequately control for unobserved differences between neighborhoods with and without enforceable covenants. Nevertheless, we find effects mostly similar to baseline with this design, which we offer as additional suggestive evidence (see Appendix Section C.1).

# 7. Conclusion

Rational choice models in economics and law posit that the law regulates societally (sub)optimal economic behavior through incentives and models discrimination as an inefficiency arising from market frictions (Becker, 2010). Game-theoretic models in economics and law, in contrast, characterize laws as setting focal points, aligning beliefs and expectations to help market agents coordinate on a given equilibrium among multiple possible equilibria, including discriminatory ones (Basu, 2017). Our historical setting lets us test the effects of a shock to beliefs about public enforcement of a discriminatory law—albeit with explicit allowance for continued private discrimination—within the latter framework. We find that without the force of the courts behind them, racial covenants lost their power to tightly coordinate discrimination in the housing market.

Specifically, we exploit the time to housing start and time to build and a 1948 US Supreme Court ruling that made racial covenants unenforceable in court. By combining new microdata on racial covenants from individual parcels in the Minneapolis metro area with census and assessor data, we obtain quasi-experimental variation at the individual house and neighborhood levels in the enforceability of racial covenants. We find that differences in early responses of developers, public officials, and initial residents to the loss of enforceability anchored the sample neighborhoods over time such that 6–24% of the observed neighborhood racial segregation from 1980 to 2020 and 3.1–4.4% of the house price differentials in the 21st century can be causally linked to racial covenants. In sum, in a context characterized by long-lasting investments and gradual adjustments over time, a

historical discriminatory law fostered coordination on an exclusionary initial equilibrium that persisted long after the courts ceased to enforce that law.

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# The Long Shadow of Housing Discrimination: Evidence from Racial Covenants

*by Aradhya Sood and Kevin Ehrman-Solberg*

ONLINE APPENDIX

# A. Data Appendix

## A.1 Hennepin County Assessor Data

The baseline housing data come from 2019 Hennepin County GIS parcel files, which include 435,765 parcels, 418,953 of them residential. Each parcel is located by a geographical polygon. All parcels have an assessed market value for tax purposes. Detailed information on building characteristics, last sale date, and sale price is available for 315,414 parcels. For those with missing data, we use Zillow to supplement the information.[1] We deflate all prices to base year 2019 using the monthly historical consumer price index (CPI).

## A.2 Hennepin County Historical Records

Table A.1: Historical County Records for a Random Sample of Houses

| | Mean | Median | $N$ |
|---|---|---|---|
| $\Delta$ First Sale Year − Build Year | 0.13 | 0.0 | 2,982 |
| $\Delta$ Resale Year − First Sale Year | 11.9 | 9.0 | 805 |

Note: This table shows the mean and median differences between the first sale year and build year and between resale year and first sale year for a sample of houses built between 1940 and 1960, based on the Hennepin County historical sale register.

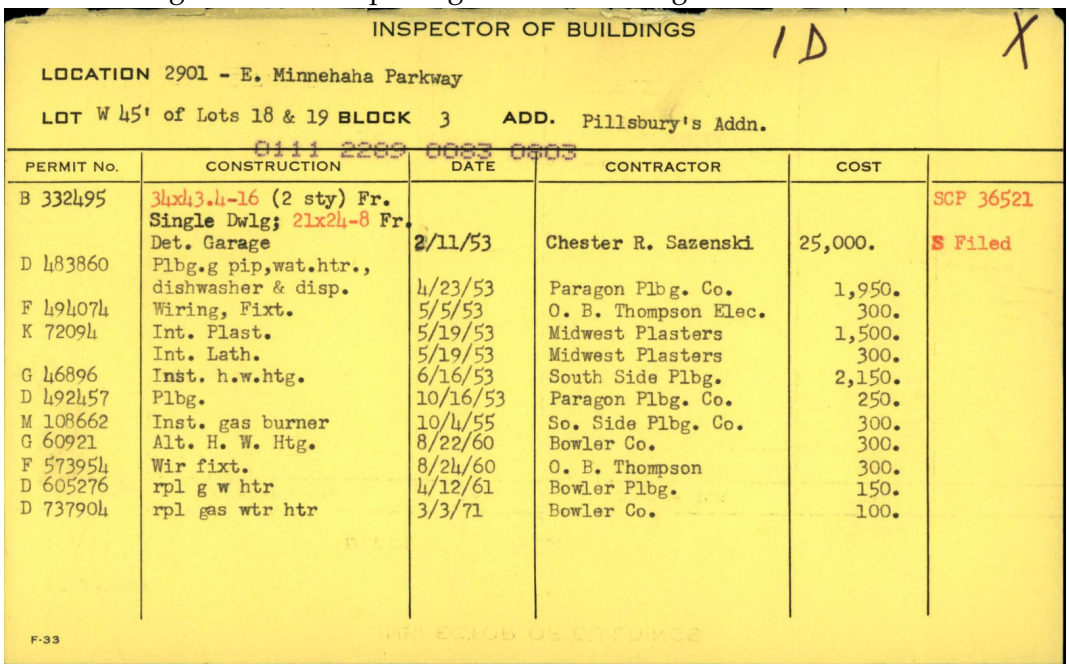
For a random sample of houses built between 1940 and 1960, we analyze data from Hennepin County's historical records to determine the time gap between the end of construction and the first sale and the duration of time residents stayed in their homes. We identify whether each house is an Abstract or Torrens property and locate the corresponding sale document. We record the first sale month and year. Moving down the register, we find the next occurrence of sale for this house, which is the year of the first resale after the house

[1]Zillow's data were freely available under an academic license in 2019 but are no longer free.

was built and first sold. Appendix Table A.1 presents the mean and median differences between the build year and first sale year and between the first sale and resale years.

## A.3 Building Permits Data and Housing Construction Start Year

Figure A.1: Example Page from a Building Permit Index Card



INSPECTOR OF BUILDINGS 1D X

LOCATION 2901 - E. Minnehaha Parkway

LOT W 45' of Lots 18 & 19 BLOCK 3 ADD. Pillsbury's Addn.

0111 2289 0083 0803

| PERMIT No. | CONSTRUCTION | DATE | CONTRACTOR | COST | |
|---|---|---|---|---|---|
| B 332495 | 34x43.4-16 (2 sty) Fr. Single Dwlg; 21x24-8 Fr. Det. Garage | 2/11/53 | Chester R. Sazenski | 25,000. | SCP 36521 S Filed |
| D 483860 | Plbg.g pip,wat.htr., dishwasher & disp. | 4/23/53 | Paragon Plbg. Co. | 1,950. | |
| F 494074 | Wiring, Fixt. | 5/5/53 | O. B. Thompson Elec. | 300. | |
| K 72094 | Int. Plast. | 5/19/53 | Midwest Plasters | 1,500. | |
| | Int. Lath. | 5/19/53 | Midwest Plasters | 300. | |
| G 46896 | Inst. h.w.htg. | 6/16/53 | South Side Plbg. | 2,150. | |
| D 492457 | Plbg. | 10/16/53 | Paragon Plbg. Co. | 250. | |
| M 108662 | Inst. gas burner | 10/4/55 | So. Side Plbg. Co. | 300. | |
| G 60921 | Alt. H. W. Htg. | 8/22/60 | Bowler Co. | 300. | |
| F 573954 | Wir fixt. | 8/24/60 | O. B. Thompson | 300. | |
| D 605276 | rpl g w htr | 4/12/61 | Bowler Plbg. | 150. | |
| D 737904 | rpl gas wtr htr | 3/3/71 | Bowler Co. | 100. | |

F-33

Note: This figure illustrates a sample page from a building permit index card, showing the date the first building permit was issued for this house in February 1953, which marks the start of this house's construction.

The data from Hennepin County's office include only the year housing construction ended. For our analysis, we also need the year of housing construction start. To obtain these data, we use historical building permit data for the city of Minneapolis, showing the month and year the first building permit was issued from the Hennepin County library. The data were extracted with Amazon's Textract from 1,899 index permit card catalogs totaling 219,664 pages, covering all permits issued between 1884 and 1973 by the city of Minneapolis. Each permit card lists all building permits until 1973 for a specific set of lots in Minneapolis. For lots with multiple permits, there may be several associated permit card pages. Figure A.1 provides an example page from an index permit card catalog. The housing construction start date is the first permit issued for a lot starting with either *A* or *B*. For example, as shown in Figure A.1, construction on this house began in February 1953, making the start year 1953.

Table A.2: Summary Statistics: Difference Between Build Year and Construction Start Year

| | $N$ | Mean | $P_2$ | $P_{25}$ | Median | $P_{75}$ | $P_{98}$ | Mode |
|---|---|---|---|---|---|---|---|---|
| $\Delta$ Build Year − Construction Year | 17,291 | 0.644 | 0 | 0 | 1 | 1 | 2 | 1 |

Note: This table shows the distribution of the difference between the build year (construction end) and housing start year for the houses for which we have raw data on both.

After processing and cleaning the output, we match the addresses on the index cards with those in our sample. This involves standardizing the addresses from both sources and restricting our sample to houses built between 1940 and 1964 to match index permit cards on the basis of street name, house number, lot, block, and addition ("ADD"). Some houses have multiple matches because they have multiple pages dedicated to a single address, and we take the earliest marked date. For those houses without a match in the first round, we focus on streets with the highest concentration of unmatched addresses and manually identify errors in the Textract text detection using the 1940 city atlas. This allows us to conduct a second round of matching with an updated list. The final match rate is 87.7% for 19,706 observations with build years between 1940 and 1960 in Minneapolis.

Table A.3: $t$-Test Difference between Housing Construction End and Start Years

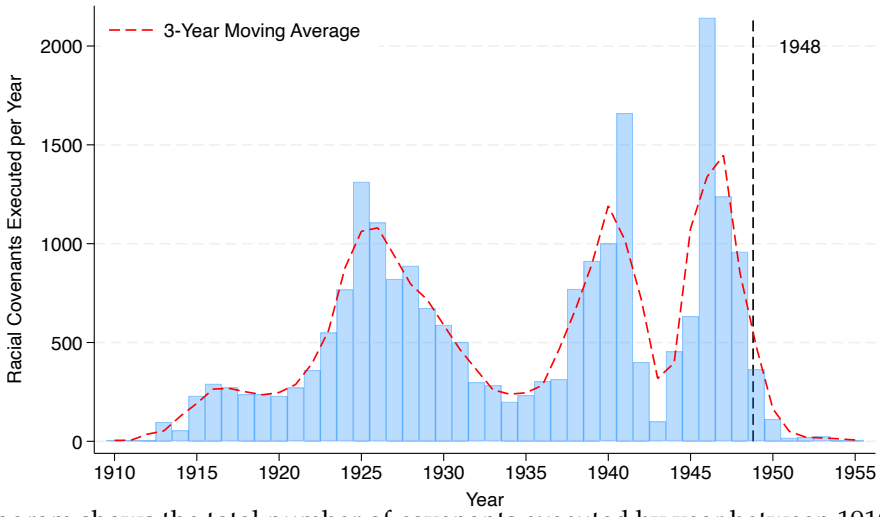
| | $N$ | Mean | Difference (Row 1 - Row 2) | $t$-Test $p$-value |
|---|---|---|---|---|
| Built 1940–48 | 6,562 | 0.636 | – | – |
| Built 1949–60 | 10,729 | 0.649 | -0.013 | 0.235 |

Note: This table provides the mean and the difference between housing construction end and start years for the sample of houses built between 1940 and 1948 and 1949 and 1960. The $p$-value from a $t$-test of the difference in means between 1940–1948 and 1949–1960 is also presented.

A total of 17,291 observations with both construction and build years are summarized in Table A.2. For housing with missing data, we impute the construction start year on the basis of the table's statistics. The median and mode of the difference between construction end and start years is 1 year, while the mean difference is 0.64 years. Table A.3 shows the mean and the difference between housing construction end and start years for the sample of houses built between 1940 and 1948 and 1949 and 1960 and the $p$-value from a $t$-test of the difference in means between 1940–1948 and 1949–1960.

## A.4 Racial Covenants Data

Figure A.2: Covenants Executed per Year




Note: This histogram shows the total number of covenants executed by year between 1910 and 1955. The dashed line at the end of 1948 represents when *Shelley v. Kramer* made covenants legally unenforceable. The 3-year moving average for the number of covenants executed is also displayed. The total number of covenants added after 1948 is 553.

The racial covenants data come from historical property sales deeds and subdivision maps from 1900 to 1960 in Hennepin County. There are 24,509 covenants in the baseline sample. We match each covenant to a parcel in three steps. First, we conduct exact matching on the basis of lot characteristics, including the addition or development number, name, and city block number, successfully matching 16,967 covenants. Second, we match remaining data if the geolocated covenant centroid falls within a parcel's polygon, which results in 7,276 additional matches. Third, we match the remaining covenants to the nearest parcel. We drop 2,536 duplicates, which leaves a final sample of 21,973 covenants. Figure A.2 shows the number of covenants executed per year, with a dashed line indicating *Shelley v. Kramer* in 1948, along with a 3-year moving average.

## A.5 Census Data

We match parcel data to census data from 1940, 1960, 1980, 1990, 2000, 2010, and 2020. Census data from 1980 onward include the share of the population by race, those under 18, and home ownership and rental by race at the census block level. We exclude 1970 data because of noise in the race statistics. The 1960 census includes population data, racial

Table A.4: County Population Over Time and Sample Neighborhoods

| Year | $N$ | Percentage White | Percentage Black | Percentage Other Minority | Percentage American Indian & Native Alaskan | Percentage Asian & Pacific Islander | Level of Analysis |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| 1940 | 568,899 | 99.1 | – | 0.9 | – | – | ED |
| 1960 | 842,854 | 98.0 | – | 2.0 | – | – | Tract |
| 1980 | 941,411 | 93.5 | 3.5 | 3.0 | 1.1 | 1.0 | Block |
| 1990 | 1,032,431 | 89.3 | 5.8 | 4.8 | 1.4 | 2.9 | Block |
| 2000 | 1,116,200 | 80.5 | 9.0 | 10.5 | 1.0 | 4.8 | Block |
| 2010 | 1,154,623 | 76.3 | 11.5 | 12.2 | 0.8 | 6.2 | Block |
| 2020 | 1,281,565 | 66.7 | 13.4 | 19.9 | 0.9 | 7.7 | Block |

Note: This table provides the Hennepin County population, level of analysis, and percentages of White residents, Black residents, and Other racial minority residents in 1940, 1960, and 1980–2020. It also presents the percentages of American Indian and Native Alaskan and Asian and Pacific Islander residents between 1980 and 2020. Note that the percentages for American Indian and Native Alaskan and Asian and Pacific Islander residents do not have to equal the Other racial minorities category. The Other minorities category in 1980 and 1990 includes American Indian and Native Alaskan, Asian and Pacific Islander, and any other race. In 2000, 2010, and 2020, this category includes American Indian and Native Alaskan, Asian and Pacific Islander, any other race, and those of two or more races. Column 7 outlines the level of analysis for each census year: enumeration district (ED), census tract, or census block.

composition, and home ownership at the tract level, while tract-level data from 1940 are unavailable, so we use data at the level of enumeration districts (EDs), roughly equivalent to modern tracts. A spatial join matches each parcel to the corresponding block, tract, or ED, with 12 parcels unmatched to a 1990 census block.

The baseline race percentage construction classifies the population into White, Black, and Other minority categories. Table A.4 provides these percentages and total populations for 1940, 1960, and 1980–2020. For robustness, we further break down the Other category into American Indian/Native Alaskan and Asian/Pacific Islander populations, as these groups are consistently reported over time. Table A.4 also shows their percentages between 1980 and 2020. Note that the White, Black, and Other minority categories include both non-Hispanic and Hispanic residents, and we do not create a separate Hispanic percentage because this category is reported inconsistently across census years.

## A.6 Data on Highways

The data on highways come from the Topologically Integrated Geographic Encoding and Referencing (TIGER) system provided by the Federal Highway Administration. We adopt the same definition of highways as Brinkman and Lin (2022), retaining all limited-access highways that connect to other roads only at interchanges, not at-grade intersections, classified as type S1100. This includes interstate highways and other limited-access highways, such as toll roads. In Hennepin County, the main highways are I-94 and I-35. We combine all highways into a multipolygon and calculate the Euclidean distance between this multipolygon and each parcel's centroid to determine the distance to the nearest highway. Additionally, we compute the travel distance in minutes between parcels and highways by downloading OpenStreetMap data for Minnesota and extracting all motorway notes. Using the latitude and longitude coordinates of housing units from the Hennepin County parcel data, we identify the closest highway entry and exit in terms of road distance. Travel time for each housing unit to the nearest highway node is then calculated with the Open Source Routing Machine (OSRM), with tags from the OSRM data helping to limit the highway nodes considered.

## A.7 Data on Zoning Regulations

*The Minnesota Star Tribune* compiled land-use regulation data on minimum lot sizes and single-family zoning for 102 Minneapolis–St. Paul metro communities in 2021. Authors Webster and Corey (2021) shared these data with us. Some municipalities in Hennepin County are not included, resulting in 7 missing observations out of 732 census blocks in the baseline 2020 sample, totaling only 0.96% missing at the census block level. Although the data on zoning regulations were collected in 2020, Kulka et al. (2026) document remarkable persistence in zoning regulations over time.

## A.8 Data on Lakes and Associated Parkways

We use two datasets to identify lakes and their associated parkways in Hennepin County: the Minnesota Land Cover Classification (MLCCS) data from the Minnesota Department of Natural Resources and the Minnesota Rivers and Lakes Shapefile from the Minnesota Metropolitan Council. The MLCCS classifies cultural features, nonnative vegetation,

and natural vegetation into a comprehensive land cover system, identifying 150 lakes in Hennepin County. Conversely, the Rivers and Lakes Shapefile recognizes many unnamed small bodies of water as lakes, totaling 511 lakes. To reconcile these datasets, we manually verify in Google Maps every water body not classified as a lake by the MLCCS. Ultimately, we identify 203 lakes in Hennepin County, most of which have associated parkways. We then combine all lakes into a multipolygon and calculate the distance from each parcel's centroid to the nearest lake.

### A.9 Data on Racial Attitudes

Residents' racial attitudes in the 1960s are inferred from their voting patterns in the 1964 and 1968 congressional elections. We geocode the 1961 electoral map for Minneapolis precincts and wards, which remained unchanged until 1971. To proxy for present-day racial attitudes, we analyze minor civil disturbances following George Floyd's death on May 25, 2020, using data from the City of Minneapolis Office of Emergency Management. Our analysis focuses on disturbances coded as 0–25%, including graffiti and broken windows, while excluding incidents in high-profile areas within a 0.2-mile radius or bandwidth of key locations such as East 38th Street/Chicago Avenue (the site of the death), Minnehaha Avenue and East Lake Street (near the third precinct police station), and the entire Lake Street and Downtown areas.

**References**

Brinkman, Jeffrey, and Jeffrey Lin. "Freeway revolts! The quality of life effects of highways." *Review of Economics and Statistics*, 2022, 1-45.

Kulka, Amrita, Aradhya Sood, and Nicholas Chiumenti, "Under the (neighbor)Hood: Understanding Interactions Among Zoning Regulations," *manuscript*, 2026.

Webster, MaryJo and Michael Corey, "How Twin Cities housing rules keep the metro segregated," *Star Tribune*, July 2021.

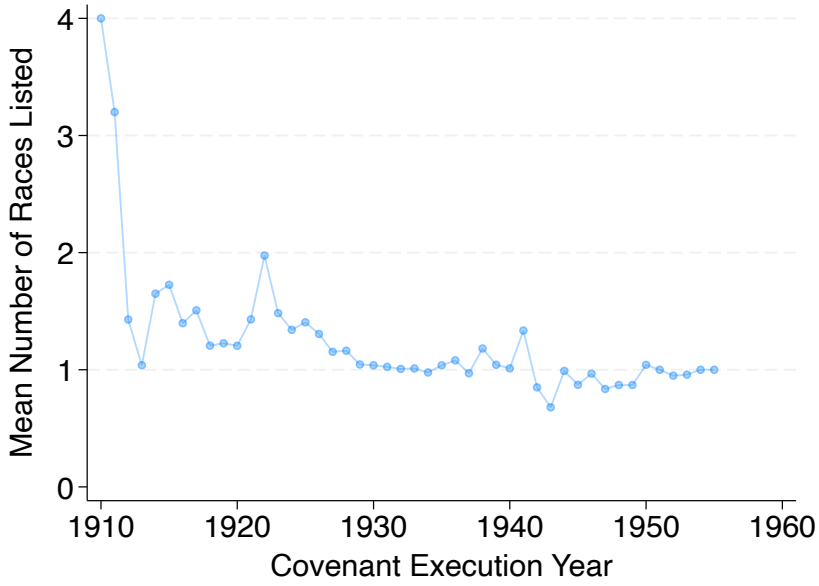
## B. Definitions and Details on Measurements

### B.1 Language Used in Racial Covenants

The language around race in the early to mid 20th-century is often ambiguous, particularly in racial covenants. While it might seem that specific phrases, such as those prohibiting

occupancy by individuals of "Chinese, Japanese, Moorish, Turkish, Negro, Mongolian, or African blood or descent," specifically target certain races, it is more accurate to say that the terminology reflects the era in which the covenant was written. As shown in Figure B.1, the number of prohibited races listed in covenants decreased after the early 1910s, indicating evolving definitions of race and efforts to strengthen these covenants. By 1920, the language primarily focused on which races were permitted rather than specifying prohibited ones.

Figure B.1: Mean Number of Races Listed in Covenants Per Year




Note: This figure plots the mean number of races specified in racial covenant language per year between 1910 and 1955.

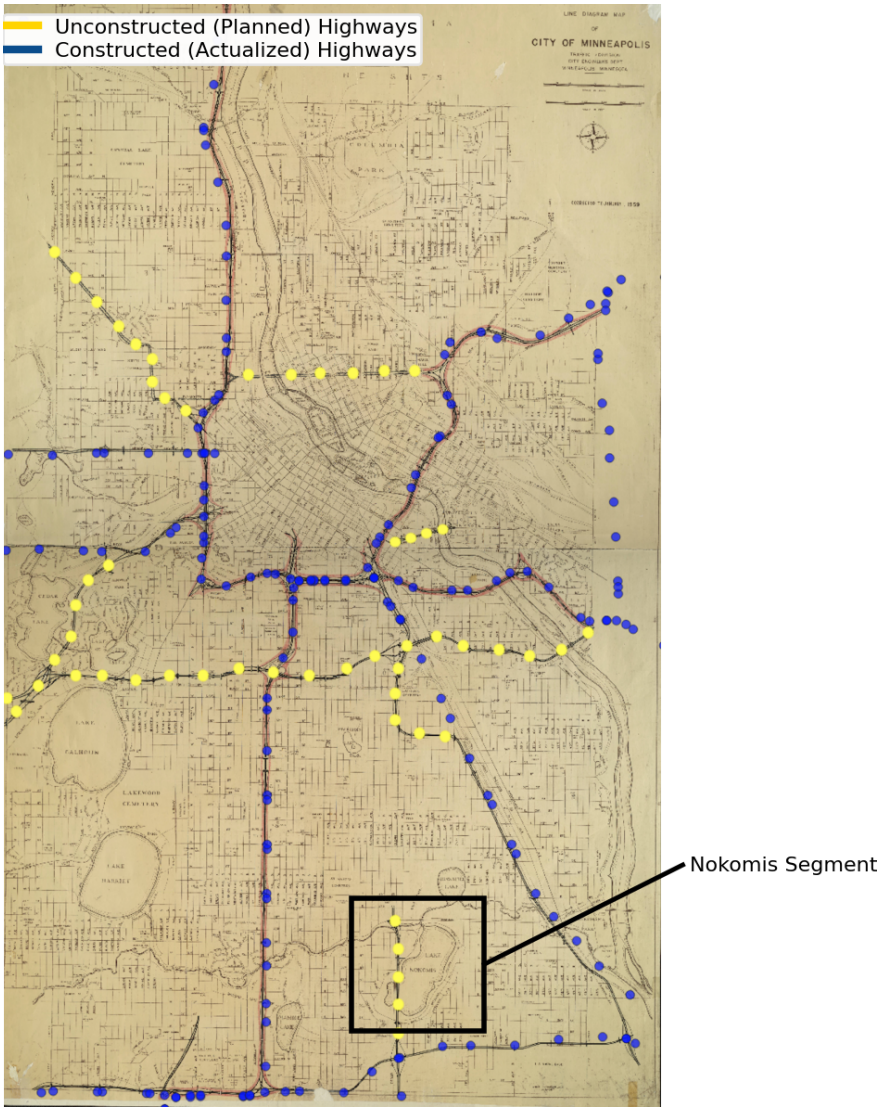
## B.2 Timeline for Planning and Construction of Interstate Highways

Our analysis adopts the definition of highways from Brinkman and Lin (2022)—those outlined in category S1100 of the Census TIGER files. These interstate highways, which include route types I and U, were designed under the Federal-Aid Highway Act of 1956.

**1940s**: Although highway development plans were initiated by the state and county as early as 1944, none of these highways were ever constructed. We manually verify that none of the 12 highway plans for which maps exist were built, likely because the federal government became involved in the highway construction project in the 1950s.

**1956**: The Federal-Aid Highway Act was enacted to promote the construction of interstate

Figure B.2: Planned vs. Constructed Highways




Note: This figure shows a map of the 1957 plan for an interstate highway. The proposed routes are shown in black and red. On top of the map, the roads in blue dots denote the final constructed highways in the area. In yellow dots, we denote planned but unconstructed highways, including the Nokomis segment.

highways nationwide, significantly influencing local projects.

**1957**: Initial drafts for the interstate highway system were released to the public (Ramer and Walker, 2025), with plans disseminated through local newspapers. Figure B.2 shows the proposed routes in red and black from the 1957 plan.

**1958**: Preparation for the construction of I-35 began with the demolition of homes and neighborhoods in 1958 (Lloyd, 2013). However, between 1959 and 1974, public feedback and protests led to some reroutes and the cancellation of certain highway segments, as

illustrated in Figure B.2 where finalized routes are in blue and unrealized routes in yellow.

**1959–1974**: The 1957 plan was not fully implemented. A notable protest involved the proposed I-94 route, which would have threatened parkland by cutting through Lake Nokomis Park and requiring the demolition of nearby neighborhoods (as shown in Figure B.2). Specifically, it would have required 25 to 30 acres of parkland while bypassing a nearby industrial area. According to the description of the 1957 plan, this project would severely limit access to the remaining parkland. In 1968, the Department of Highways attempted condemnation proceedings but faced delays (Ramer and Walker, 2025). Former Mayor P. Kenneth Peterson publicly opposed removing homes from tax rolls, stating it was "unthinkable" to displace them when open land was available. Due to mounting pressure, this project segment was nearly abandoned by 1974.

## B.3 Timeline for Proposal and Adoption of Zoning Regulations

Hennepin County's first zoning regulations were adopted in 1924 in Minneapolis, aimed at separating industrial, commercial, and residential areas. A comprehensive zoning plan was introduced in 1948, following planning efforts that started in 1946. This plan included restrictions on multifamily housing, maximum density requirements, and minimum parking standards. In 1963, amendments added minimum lot size requirements and other regulations. There were few changes to the 1963 zoning plan until 2020. Suburban municipalities such as St. Louis Park (1956), New Hope (1966), and Brookfield (1972) began adopting their own comprehensive zoning laws from the 1950s to the 1970s.[2]

## B.4 Timeline for Construction of Lakes and Associated Parkways

This section provides a timeline for the construction of lakes and associated parkways in Hennepin County. Regarding the timeline for parks and associated parkways in Minneapolis, we obtain these dates from Minneapolis Parks' official website.

**1883–1913:** The Minneapolis park board was created, with initial plans to acquire land encircling Lake Harriet. A major project involved dredging the Lake of the Isles and completing a channel between Cedar Lake and Lake of the Isles by 1913.

**1920s:** In 1922, the park board purchased land around Rice Lake (now Lake Hiawatha), as

[2]Dates based on our research of zoning ordinance maps across archives in Hennepin County.

it was the only suitable site for a golf course in South Minneapolis. From 1929 to 1931, the lake was dredged, and the golf course was constructed.

**1930s:** In 1931, the Minnehaha Creek bed was graded and redirected. In 1936, the park board acquired 72.32 acres around Pearl and Diamond Lakes, and by 1939, it owned 8.54 acres on the bed of Diamond Lake.

**1940s:** In 1947, Lake Nokomis's dimensions changed to add 0.8 acres on its east side.

**1950s and 1960s:** The park board completed its acquisition of Cedar Lake's shore in the 1950s and, in 1968, added 14 acres to Cedar Lake Park. Additionally, land north of Lake Hiawatha was filled to expand the golf course.

## B.5 Neighborhood Sample Construction

The sample of neighborhoods used in the analysis—census blocks—is selected according to two criteria. The first restriction is that at least 50% or 75% of the housing units in the block must have had covenants, regardless of the year the covenant was executed ($\underline{c}_{jt} \geq 50, 75$), as explained in the main text. The second restriction is:

$$(\%\text{built/start} \leq 1948)_{jt} = \frac{(\text{built/start}1940 - 48)_j * 100}{(\text{built/start}1940 - 60)_j} \quad \text{if} \quad \underline{b}_{jt} \geq 0.5, 0.75$$

$$\text{where} \quad \underline{b}_{jt} = \frac{(\text{built/start}1940 - 60)_j}{(\text{all houses})_{jt}}$$

where $(\text{built/start}1940 - 48)_j$ is the number of houses in block $j$ with construction start or end between 1940 and 1948, $(\text{built/start}1940 - 60)_j$ is the total houses started or built between 1940 and 1960, and $\underline{b}_{jt}$ is the share of houses started or built between 1940 and 1960 out of all houses in block $j$ in decennial year $t$ ($(\text{all houses})_{jt}$). The baseline sample requires $\underline{b}_{jt} \geq 0.5$ and $\underline{c}_{jt} \geq 50$. Additional samples have $\underline{b}_{jt} \geq 0.75$ and $\underline{c}_{jt} \geq 75$.

**References**

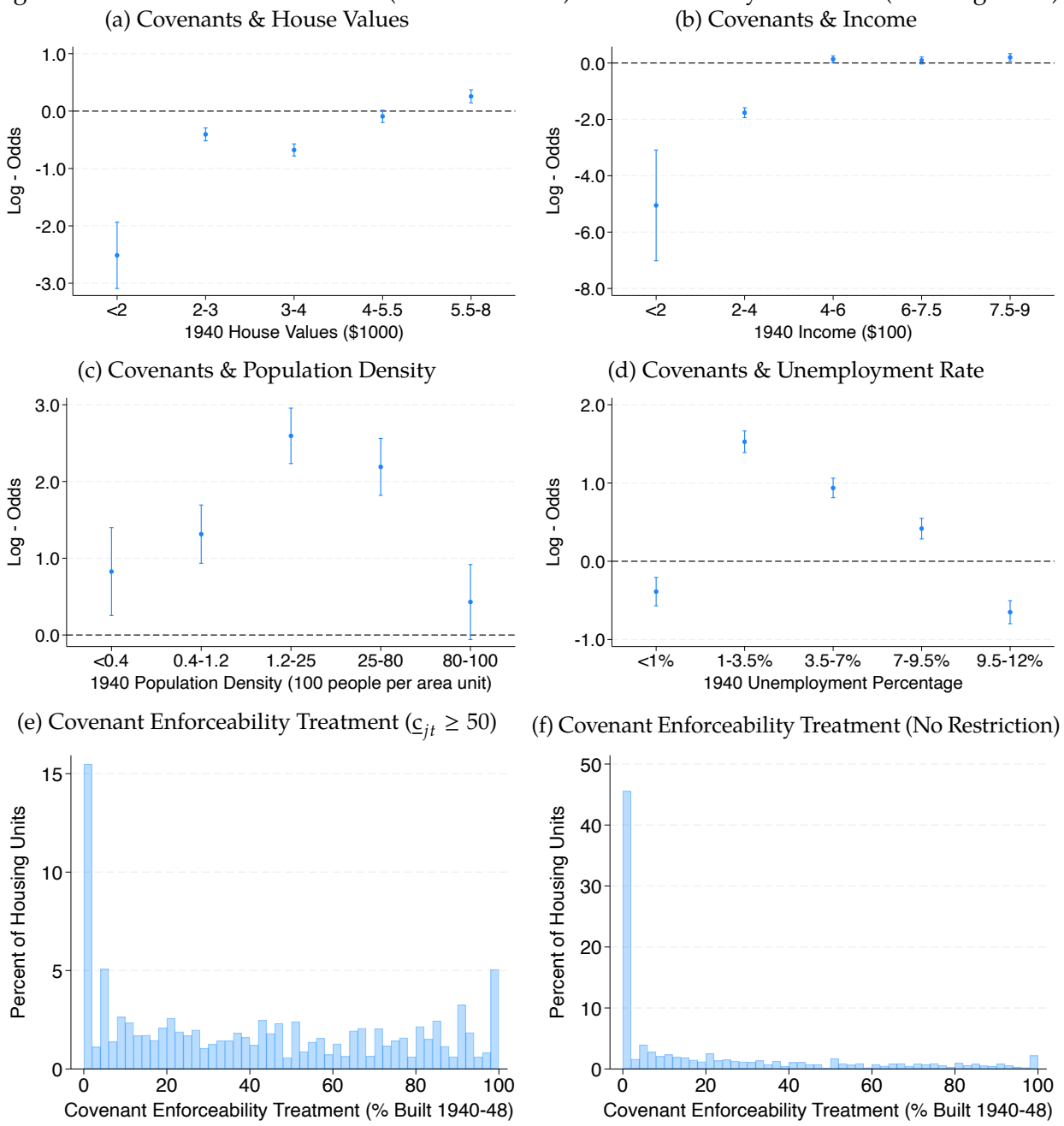
Brinkman, Jeffrey, and Jeffrey Lin. "Freeway revolts! The quality of life effects of highways." *Review of Economics and Statistics*, 2022, 1-45.

Lloyd, Ernest Lee. "How Routing an Interstate Highway through South Minneapolis Disrupted an African-American Neighborhood." Ph.D. thesis (2013).

Ramer, Hannah and Rebecca Walker, *Freeways and the Park System in Minneapolis*, University of Minnesota Press, 2025.

# C. Additional Results

Figure C.1: Covenants in Transition (Additional Var.) & Enforceability Treatment (Housing Units)




Note: This is a companion figure to Figure 3. Panels (a), (b), (c), and (d) plot the estimated log-odds (relative to the omitted base category, which is always the final bin) of addition of a covenant after 1940 from a logit specification whose independent variables are the average house value, average income, population density, and unemployment rate, all measured at the enumeration district level in 1940. Panel (e) shows the distribution of the covenant enforceability treatment, $(\%\text{built} \leq 1948)_{jt}$ from Equation 2, where the sample is restricted to neighborhoods in which at least 50% of the housing units had covenants ($\%\text{cov houses}_{jt} \geq \underline{c}_{jt} = 50$) and Panel (f) the distribution of the treatment with no restriction on the share of units with covenants ($\%\text{cov houses}_{jt} \geq \underline{c}_{jt} = 0$) for houses built between 1940 and 1960 and the 2020 census blocks sample.

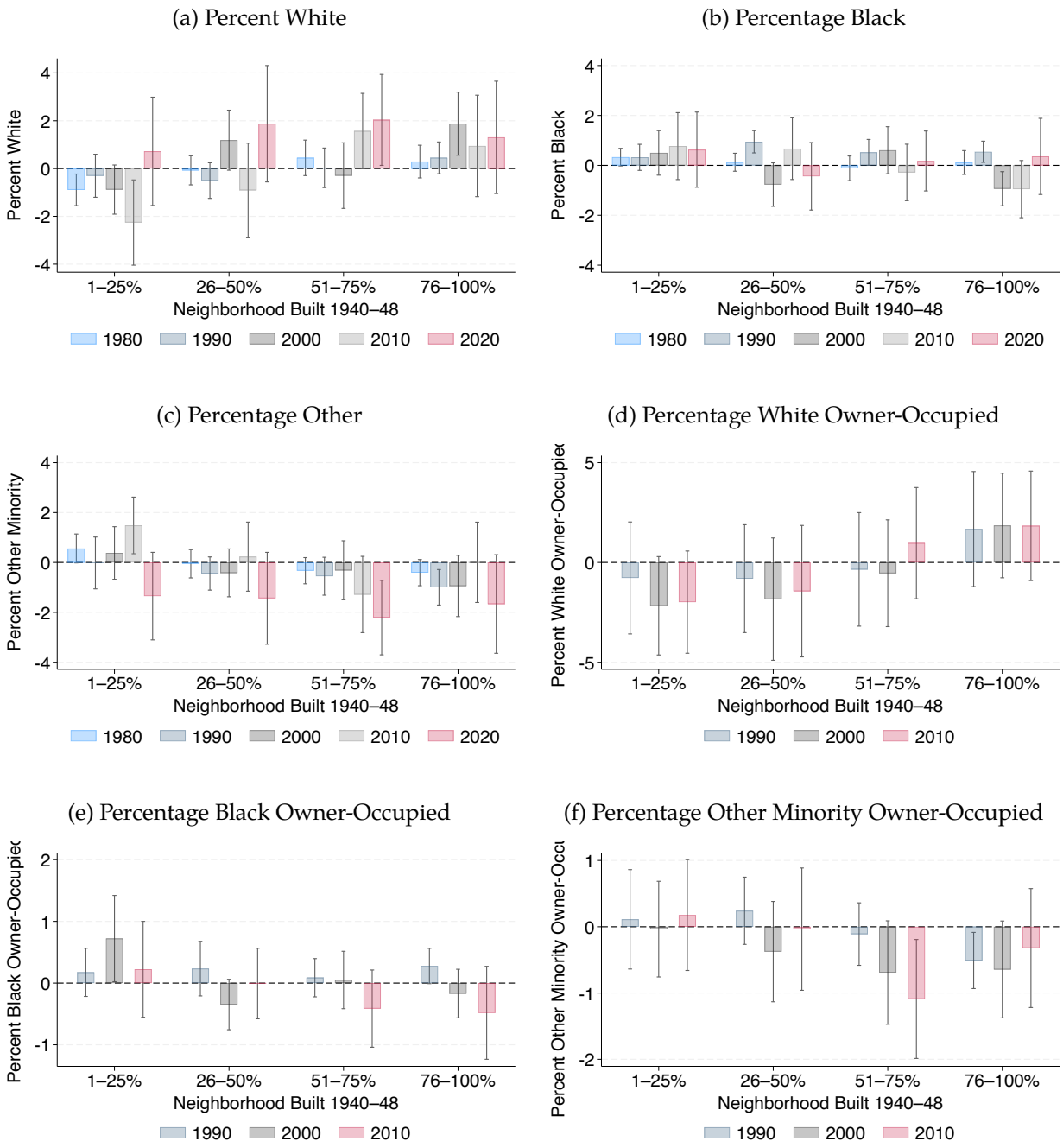
Table C.1: Effects on Housing Characteristics: Robustness 1

| Dependent Variable | $\alpha_1$ | s.e. (cluster) | s.e. (robust) | $N$ | $R^2$ | Effect of 1 SD increase | Mean of Dep. Variable |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel A: 1940–1960 w/ Buildings Built 1940–60, $\underline{c}_{jt} \geq 50$, no remodeling restriction* | | | | | | | |
| Log Built Area | 0.0008*** | (0.0003) | [0.0002] | 10,027 | 0.502 | 2.46 | – |
| No. Bedrooms | 0.0006 | (0.0004) | [0.0003] | 10,261 | 0.113 | – | 3.11 |
| No. Bathrooms | 0.0006* | (0.0003) | [0.0002] | 10,259 | 0.333 | 0.02 | 1.56 |
| No. Floors | 0.001*** | (0.0002) | [0.0001] | 10,048 | 0.158 | 0.04 | 1.22 |
| *Panel B: 1940–1960 w/ Buildings Built 1944–1953, $\underline{c}_{jt} \geq 50$* | | | | | | | |
| Log Built Area | 0.001** | (0.0004) | [0.0003] | 3,912 | 0.518 | 3.43 | – |
| No. Bedrooms | 0.001** | (0.0006) | [0.0005] | 4,104 | 0.140 | 0.04 | 3.02 |
| No. Bathrooms | 0.001*** | (0.0005) | [0.0004] | 4,104 | 0.347 | 0.04 | 1.53 |
| No. Floors | 0.001*** | (0.0003) | [0.0002] | 4,066 | 0.182 | 0.04 | 1.25 |
| *Falsification:* Log Lot Size | -0.0002 | (0.0002) | [0.00010] | 7,486 | 0.737 | – | – |
| *Panel C: 1944–1953 w/ Buildings Built 1944–1953, $\underline{c}_{jt} \geq 50$* | | | | | | | |
| Log Built Area | 0.0006 | (0.0004) | [0.0003] | 3,368 | 0.504 | – | – |
| No. Bedrooms | 0.0009 | (0.0006) | [0.0005] | 3,547 | 0.134 | – | 3.02 |
| No. Bathrooms | 0.0009* | (0.0005) | [0.0004] | 3,547 | 0.333 | 0.03 | 1.50 |
| No. Floors | 0.001*** | (0.0003) | [0.0002] | 3,513 | 0.178 | 0.03 | 1.24 |
| *Falsification:* Log Lot Size | -0.0003 | (0.0002) | [0.0001] | 6,569 | 0.745 | – | – |

| Dependent Variable | $\alpha_3$ | s.e. (cluster) | s.e. (robust) | $N$ | $R^2$ | Effect of 1 SD increase | Mean of Dep. Variable |
|---|---|---|---|---|---|---|---|
| *Panel D: $\alpha_3$ from Baseline (Reported in Table 2 Panel A)* | | | | | | | |
| Log Built Area | -0.006 | (0.014) | [0.010] | 5,642 | 0.513 | – | – |
| No. Bedrooms | -0.027* | (0.014) | [0.012] | 5,876 | 0.144 | -0.03 | 3.08 |
| No. Bathrooms | -0.015 | (0.013) | [0.009] | 5,876 | 0.352 | – | 1.60 |
| No. Floors | -0.011 | (0.007) | [0.004] | 5,769 | 0.200 | – | 1.25 |

Note: Panels A–C present the parameter estimate $\alpha_1$ (column 1) from Equation 2 with dependent variables representing housing characteristics. Standard errors clustered at the census block level appear in parentheses (column 2) and robust standard errors in brackets (column 3). Panel A includes all houses in the sample, whether remodeled or not. Panels B and C restrict the sample to houses built between 1944 and 1953 ($\underline{c}_{jt} = 0$). Panel B (C) includes census blocks at least 50% built between 1940 and 1960 (1944 and 1953), with $\underline{c}_{jt} \geq 50$. Panel D presents the parameter estimate $\alpha_3$ (column 1) from Equation 2 for the baseline effects reported in Table 2 Panel A. The effect of a 1-standard-deviation (SD) increase in covenant enforceability intensity for statistically significant variables is given in column 6. The means of the non-log dependent variables are given in column 7. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.2: Nonlinear Sorting Effects




Note: This figure plots the parameter estimates $\beta_m$ for $m$ = 1–25%, 26–50%, 51–75%, 76–100%, where the omitted category is $m$ = 0%, similar to Equation 3. The dependent variables are resident percentages by race (Panels a–c) and percentages of owner-occupied housing units by race (Panels d–f) over time. Standard errors are clustered at census tract level, and 90% confidence intervals are reported. The sample of census blocks is restricted to those where at least 50% of the housing was built between 1940 and 1960 and at least 50% was covenanted ($\underline{c}_{jt} \geq 50$).

Table C.2: Racial Segregation 1980–2020: Additional Racial Groups

| | $\alpha_1$ | s.e. (cluster) | s.e. [robust] | $N$ | $R^2$ | Effect of 1 SD increase | Mean of Dep. Variable |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel A Dependent Variable: Percentage of American Indian & Native Alaskan Residents* | | | | | | | |
| 1980 | -0.001 | (0.001) | [0.001] | 830 | 0.152 | – | 0.25 |
| 1990 | 0.001 | (0.001) | [0.001] | 830 | 0.160 | – | 0.35 |
| 2000 | -0.002 | (0.002) | [0.002] | 795 | 0.162 | – | 0.39 |
| 2010 | -0.002 | (0.002) | [0.002] | 791 | 0.118 | – | 0.43 |
| 2020 | 0.001 | (0.003) | [0.003] | 731 | 0.231 | – | 0.83 |
| *Panel B Dependent Variable: Percentage of Asian and Pacific Islander Residents* | | | | | | | |
| 1980 | -0.006 | (0.004) | [0.004] | 830 | 0.110 | – | 0.89 |
| 1990 | -0.016*** | (0.005) | [0.006] | 830 | 0.117 | -0.53 | 1.54 |
| 2000 | -0.019** | (0.009) | [0.008] | 795 | 0.140 | -0.64 | 2.37 |
| 2010 | -0.025*** | (0.008) | [0.008] | 791 | 0.177 | -0.83 | 3.12 |
| 2020 | -0.008 | (0.007) | [0.008] | 731 | 0.165 | – | 4.08 |
| *Panel C Dependent Variable: Percentage of American Indian Owner-Occupied Housing Units* | | | | | | | |
| 1990 | 0.001 | (0.001) | [0.001] | 830 | 0.117 | – | 0.21 |
| 2000 | 0.001 | (0.001) | [0.001] | 795 | 0.243 | – | 0.18 |
| 2010 | 0.001 | (0.001) | [0.001] | 791 | 0.125 | – | 0.21 |
| *Panel D Dependent Variable: Percentage of Asian and Pacific Islander Owner-Occupied Housing Units* | | | | | | | |
| 1990 | -0.011*** | (0.003) | [0.003] | 830 | 0.129 | -0.37 | 0.66 |
| 2000 | -0.016*** | (0.005) | [0.005] | 795 | 0.141 | -0.52 | 1.25 |
| 2010 | -0.018*** | (0.005) | [0.006] | 791 | 0.139 | -0.60 | 1.83 |

Notes: This table displays the parameter estimate $\alpha_1$ (column 1) from Equation 1. The dependent variables are resident percentages by race–American Indian/Native Alaskan and Asian/Pacific Islander (Panels A and B)—and percentages of owner-occupied housing units by race–American Indian/Native Alaskan and Asian/Pacific Islander (Panels C and D)—over time. Standard errors clustered at census tract level are in parentheses (column 2) and robust standard errors in brackets (column 3). The sample of census blocks is limited to those where at least 50% of the census block was built between 1940 and 1960 and at least 50% had racial covenants ($\underline{c}_{jt} \geq 50$). Column 6 is the effect of a 1-standard-deviation (SD) increase in intensity of the covenant enforceability treatment for statistically significant variables. Column 7 shows means of the dependent (Dep.) variables. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Long-Run House Value Effects: Robustness

| Dependent Variable | $\alpha_1$ | s.e. (cluster) | $N$ | $R^2$ | Effect of 1 SD increase |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: Baseline + House Characteristics* | | | | | |
| Log Assessed Value (2019) | 0.0007*** | (0.0001) | 10,960 | 0.843 | 2.53 |
| Log Sales Price (2010–2019) | 0.0011*** | (0.0003) | 4,346 | 0.441 | 3.57 |
| *Panel B: Baseline + House Characteristics + Local Amenities* | | | | | |
| Log Assessed Value (2019) | 0.0007*** | (0.0001) | 10,932 | 0.844 | 2.28 |
| Log Sales Price (2010–2019) | 0.0010*** | (0.0003) | 4,331 | 0.440 | 3.36 |
| *Panel C: Baseline + House Characteristics + Local Amenities + SES Controls* | | | | | |
| Log Assessed Value (2019) | 0.0005*** | (0.0001) | 10,607 | 0.841 | 1.64 |
| Log Sales Price (2010–2019) | 0.0008*** | (0.0003) | 4,190 | 0.449 | 2.69 |
| *Panel D: Baseline + House Characteristics + Local Amenities + Time to Housing Start* | | | | | |
| Log Assessed Value (2019) | 0.0007*** | (0.0001) | 10,932 | 0.844 | 2.28 |
| Log Sales Price (2010–2019) | 0.0010*** | (0.0003) | 4,331 | 0.440 | 3.36 |
| *Panel E: 1947–1948 Missing* | | | | | |
| Log Assessed Value (2019) | 0.0016*** | (0.0003) | 646 | 0.779 | 5.70 |
| Log Sales Price (2010–2019) | 0.0049** | (0.0025) | 246 | 0.400 | 19.33 |
| *Panel F: 1948 Missing* | | | | | |
| Log Assessed Value (2019) | 0.0018*** | (0.0003) | 629 | 0.785 | 6.51 |
| Log Sales Price (2010–2019) | 0.0049** | (0.0020) | 241 | 0.409 | 19.46 |
| *Panel G: Baseline + Covenant Language* | | | | | |
| Log Assessed Value (2019) | 0.0009*** | (0.0002) | 11,438 | 0.733 | 3.09 |
| Log Sales Price (2010–2019) | 0.0013*** | (0.0003) | 4,610 | 0.414 | 4.37 |
| *Panel H: 1944–1953 w/ Buildings Built 1940–1960* | | | | | |
| Log Assessed Value (2019) | 0.0005** | (0.0002) | 8,443 | 0.740 | 1.40 |
| Log Sales Price (2010–2019) | 0.0009*** | (0.0003) | 3,398 | 0.433 | 2.75 |
| *Panel I: 1940–1960 w/ Buildings Built 1940–1960, $\underline{c}_{jt} \leq 10$* | | | | | |
| Log Assessed Value (2019) | -0.0003* | (0.0002) | 57,394 | 0.603 | -0.71 |
| Log Sales Price (2010–2019) | 0.0003 | (0.0003) | 23,083 | 0.406 | – |
| *Panel J: 1940–1960 w/ Buildings Built 1940–1960, $\underline{c}_{jt} \leq 25$* | | | | | |
| Log Assessed Value (2019) | -0.0002 | (0.0002) | 60,793 | 0.605 | – |
| Log Sales Price (2010–2019) | 0.0003 | (0.0002) | 24,436 | 0.407 | – |
| *Panel K: 1940–1960 w/ Buildings Built 1940–1960, $\underline{c}_{jt} \leq 35$* | | | | | |
| Log Assessed Value (2019) | -0.0003* | (0.0001) | 62,468 | 0.606 | -0.65 |
| Log Sales Price (2010–2019) | 0.0003 | (0.0002) | 25,119 | 0.405 | – |

Note: This table displays parameter estimate $\alpha_1$ (column 1) from Equation 2. The dependent variables are the log of assessed value (2019) and the log of the sales price (2010–2019). Standard errors are clustered at the census block level (column 2). Panel A includes housing characteristics controls—log lot size and built area, number of bedrooms, bathrooms and floors, and indicators for house remodeling and property type. Panel B additionally includes local public amenities—Euclidean distance to highways, minimum lot size, and share of neighborhood zoned for single-family. Panel C additionally controls for share over age 19, block population, median income, and any college education. Panel D additionally controls for *time to housing start*. Panels E and F remove the years 1947–1948 and only 1948, respectively, from the baseline analysis. Panel G controls for the total number of racial groups listed in the covenant language. Panel H restricts sample to housing built in 1944–1953, where $(\%\text{built} \leq 1948)_{jt}$ in Equation 2 is at least 50% built between 1940 and 1960. Panels I–K use samples with at most 10%, 25%, and 35% covenanted housing ($\underline{c}_{jt} \leq 10, 25, 35$) in the neighborhood. The effect of a 1-standard-deviation (SD) increase in covenant enforceability intensity for statistically significant variables is given in column 5. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4: Effects on Housing Characteristics: Robustness 2

| Dependent Variable | $\alpha_1$ | s.e. (cluster) | s.e. (robust) | $N$ | $R^2$ | Effect of 1 SD increase | Mean of Dep. Variable |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel A: 1940–1960 w/ Buildings Built 1940–1960, $\underline{c}_{jt} = 0$* | | | | | | | |
| Log Built Area | -0.0003* | (0.0002) | [0.0002] | 20,332 | 0.603 | -0.91 | – |
| No. Bedrooms | -0.002*** | (0.0003) | [0.0003] | 20,800 | 0.271 | -0.05 | 3.05 |
| No. Bathrooms | -0.0004 | (0.0003) | [0.0002] | 20,794 | 0.334 | – | 1.55 |
| No. Floors | 0.0009*** | (0.0001) | [0.00009] | 19,520 | 0.251 | 0.03 | 1.17 |
| *Falsification:* Log Lot Size | -0.0006*** | (0.0002) | [0.00010] | 43,778 | 0.647 | -1.35 | – |
| *Panel B: 1940–1960 w/ Buildings Built 1940–1960, $\underline{c}_{jt} \leq 10$* | | | | | | | |
| Log Built Area | -0.0003 | (0.0002) | [0.0001] | 22,132 | 0.601 | – | – |
| No. Bedrooms | -0.002*** | (0.0003) | [0.0002] | 22,625 | 0.264 | -0.05 | 3.05 |
| No. Bathrooms | -0.0004 | (0.0003) | [0.0002] | 22,619 | 0.329 | – | 1.57 |
| No. Floors | 0.001*** | (0.0001) | [0.00008] | 21,268 | 0.244 | 0.03 | 1.17 |
| *Falsification:* Log Lot Size | -0.0007*** | (0.0002) | [0.00009] | 46,648 | 0.644 | -1.77 | – |
| *Panel C: 1940–1960 w/ Buildings Built 1944–1953, $\underline{c}_{jt} = 0$* | | | | | | | |
| Log Built Area | -0.0004 | (0.0002) | [0.0002] | 10,020 | 0.612 | – | – |
| No. Bedrooms | -0.001*** | (0.0004) | [0.0003] | 10,268 | 0.191 | -0.04 | 2.93 |
| No. Bathrooms | -0.0001 | (0.0003) | [0.0002] | 10,265 | 0.326 | – | 1.53 |
| No. Floors | 0.0004** | (0.0002) | [0.0001] | 10,153 | 0.272 | 0.01 | 1.20 |
| *Falsification:* Log Lot Size | -0.0007*** | (0.0003) | [0.0001] | 19,585 | 0.656 | -1.96 | – |
| *Panel D: 1940–1960 w/ Buildings Built 1944–1953, $\underline{c}_{jt} \leq 10$* | | | | | | | |
| Log Built Area | -0.0002 | (0.0002) | [0.0002] | 11,063 | 0.609 | – | – |
| No. Bedrooms | -0.0009** | (0.0004) | [0.0003] | 11,323 | 0.187 | -0.03 | 2.94 |
| No. Bathrooms | 0.0002 | (0.0003) | [0.0002] | 11,320 | 0.317 | – | 1.54 |
| No. Floors | 0.0005*** | (0.0002) | [0.0001] | 11,190 | 0.266 | 0.01 | 1.21 |
| *Falsification:* Log Lot Size | -0.0009*** | (0.0002) | [0.0001] | 21,209 | 0.655 | -2.45 | – |

Note: This table presents the parameter estimate $\alpha_1$ (column 1) from Equation 2 with dependent variables representing housing characteristics. Standard errors clustered at the census block level appear in parentheses (column 2) and robust standard errors in brackets (column 3). In Panel A, the sample of census blocks is restricted to those at least 50% built between 1940 and 1960, with no restrictions on racial covenants ($\underline{c}_{jt} = 0$). Additional restrictions include houses built between 1940 and 1960. Panel B modifies the restriction on racial covenants ($\underline{c}_{jt} \leq 10$). Panels C and D also modify the restriction on racial covenants for the sample of houses built between 1944 and 1953 (at least 50% built between 1940 and 1960) to $\underline{c}_{jt} = 0$ and $\underline{c}_{jt} \leq 10$, respectively. The effect of a 1-standard-deviation (SD) increase in covenant enforceability intensity for statistically significant variables is given in column 6. The means of the non-log dependent variables are given in column 7. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Racial Segregation in Owner-Occupied Units 1980–2010: Robustness

| Samples/ Models | (I) Alt. Ownership I (Including Vacant Units in Denominator) | | | | | (II) Alt. Ownership II (Owner-Occupied Units as Denominator) | | | | | (III) Zipcode FE | | | | (IV) ≥75% Built 1940–1960 & $\underline{c}_{jt} \geq 75$ | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | $\alpha_1$ | s.e. | $N$ | $R^2$ | $E(Y)$ | $\alpha_1$ | s.e. | $N$ | $R^2$ | $E(Y)$ | $\alpha_1$ | s.e. | $N$ | $R^2$ | $\alpha_1$ | s.e. | $N$ | $R^2$ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) | (17) | (18) |
| *Panel A: Percentage of White Owner-Occupied* | | | | | | | | | | | | | | | | | | |
| 1980 | 0.023 | (0.032) | 692 | 0.182 | 85.87 | -0.00003 | (0.00005) | 649 | 0.503 | 99.79 | 0.012 | (0.022) | 657 | 0.157 | 0.024 | (0.021) | 442 | 0.245 |
| 1990 | 0.050*** | (0.017) | 830 | 0.261 | 87.75 | 0.012** | (0.005) | 829 | 0.168 | 97.99 | 0.047*** | (0.016) | 837 | 0.174 | 0.054** | (0.021) | 558 | 0.303 |
| 2000 | 0.068*** | (0.021) | 795 | 0.270 | 87.44 | 0.030*** | (0.009) | 794 | 0.274 | 95.91 | 0.043** | (0.017) | 799 | 0.186 | 0.070*** | (0.026) | 513 | 0.302 |
| 2010 | 0.065** | (0.025) | 791 | 0.324 | 80.31 | 0.028** | (0.013) | 788 | 0.294 | 93.68 | 0.041** | (0.018) | 798 | 0.218 | 0.081** | (0.035) | 483 | 0.358 |
| *Panel B: Percentage of Black Owner-Occupied* | | | | | | | | | | | | | | | | | | |
| 1980 | 0.000001 | (0.000002) | 692 | 0.503 | 0.16 | 0.00003 | (0.00005) | 649 | 0.503 | 0.21 | -0.0003 | (0.001) | 657 | 0.335 | – | (–) | 442 | . |
| 1990 | -0.002 | (0.002) | 830 | 0.180 | 0.77 | -0.003 | (0.002) | 829 | 0.172 | 0.90 | -0.004** | (0.002) | 837 | 0.237 | -0.004 | (0.003) | 558 | 0.238 |
| 2000 | -0.010** | (0.005) | 795 | 0.275 | 1.43 | -0.014*** | (0.005) | 794 | 0.300 | 1.65 | -0.011* | (0.006) | 799 | 0.224 | -0.009 | (0.006) | 513 | 0.303 |
| 2010 | -0.007 | (0.006) | 791 | 0.195 | 1.77 | -0.012 | (0.008) | 788 | 0.250 | 2.16 | -0.007 | (0.006) | 798 | 0.158 | -0.006 | (0.007) | 483 | 0.215 |
| *Panel C: Percentage of Other Minority Owner-Occupied* | | | | | | | | | | | | | | | | | | |
| 1990 | -0.009** | (0.003) | 830 | 0.110 | 0.99 | -0.010*** | (0.003) | 829 | 0.112 | 1.11 | -0.008*** | (0.001) | 837 | 0.068 | -0.006* | (0.004) | 558 | 0.144 |
| 2000 | -0.013** | (0.006) | 795 | 0.149 | 2.13 | -0.015** | (0.006) | 794 | 0.149 | 2.44 | -0.012** | (0.004) | 799 | 0.111 | -0.011 | (0.006) | 513 | 0.151 |
| 2010 | -0.013* | (0.007) | 791 | 0.190 | 3.47 | -0.016 | (0.010) | 788 | 0.219 | 4.16 | -0.008 | (0.006) | 798 | 0.107 | -0.012 | (0.011) | 483 | 0.196 |

*Note:* This table reports the parameter estimate $\alpha_1$ from Equation 1. The dependent variables represent the percentage of owner-occupied housing units by race for 1980–2010. Standard errors are clustered at the census tract level unless otherwise stated. Columns Groups I and II use alternative ownership measures. The means of the dependent variables ($E(Y)$) are reported in Column Groups I and II. Column Group III adds zip code fixed effects $\eta_{k(j)}$ with zip code–clustered standard errors. Column Group IV restricts the sample to census blocks with ≥75% built 1940–1960 and $\underline{c}_{jt} \geq 75$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: Racial Segregation 1980–2020: Additional Robustness

| | (I) + SES Controls | | | | (II) + Time to Housing Start | | | | (III) Removing 1947 & 1948 | | | | (IV) Removing 1948 Only | | | | (V) + Covenant Language | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | $\alpha_1$ | s.e. | $N$ | $R^2$ | $\alpha_1$ | s.e. | $N$ | $R^2$ | $\alpha_1$ | s.e. | $N$ | $R^2$ | $\alpha_1$ | s.e. | $N$ | $R^2$ | $\alpha_1$ | s.e. | $N$ | $R^2$ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) | (17) | (18) | (19) | (20) |
| *Panel A: Percentage of White Residents* | | | | | | | | | | | | | | | | | | | | |
| 1980 | 0.011** | (0.005) | 788 | 0.170 | 0.011*** | (0.004) | 830 | 0.159 | 0.006 | (0.004) | 863 | 0.479 | 0.009** | (0.004) | 861 | 0.497 | 0.012*** | (0.004) | 830 | 0.153 |
| 1990 | 0.020** | (0.007) | 830 | 0.200 | 0.017** | (0.007) | 830 | 0.192 | 0.012* | (0.006) | 874 | 0.360 | 0.015** | (0.006) | 865 | 0.362 | 0.017** | (0.007) | 830 | 0.195 |
| 2000 | 0.033*** | (0.012) | 793 | 0.339 | 0.034*** | (0.012) | 795 | 0.330 | 0.033*** | (0.011) | 843 | 0.379 | 0.035*** | (0.011) | 833 | 0.382 | 0.035*** | (0.012) | 795 | 0.331 |
| 2010 | 0.052*** | (0.018) | 791 | 0.449 | 0.042** | (0.019) | 791 | 0.417 | 0.058*** | (0.017) | 846 | 0.416 | 0.055*** | (0.017) | 833 | 0.423 | 0.044** | (0.019) | 791 | 0.416 |
| 2020 | 0.026 | (0.019) | 707 | 0.459 | 0.035* | (0.020) | 731 | 0.430 | 0.030 | (0.019) | 798 | 0.436 | 0.042** | (0.020) | 783 | 0.425 | 0.038* | (0.020) | 731 | 0.428 |
| *Panel B: Percentage of Black Residents* | | | | | | | | | | | | | | | | | | | | |
| 1980 | -0.002 | (0.003) | 788 | 0.149 | -0.003 | (0.002) | 830 | 0.130 | -0.002 | (0.004) | 863 | 0.665 | -0.003 | (0.003) | 861 | 0.702 | -0.003 | (0.002) | 830 | 0.128 |
| 1990 | -0.003 | (0.004) | 830 | 0.227 | -0.002 | (0.003) | 830 | 0.220 | -0.005 | (0.004) | 874 | 0.528 | -0.005 | (0.003) | 865 | 0.537 | -0.002 | (0.003) | 830 | 0.225 |
| 2000 | -0.019*** | (0.006) | 793 | 0.321 | -0.018*** | (0.006) | 795 | 0.309 | -0.019*** | (0.007) | 843 | 0.410 | -0.020*** | (0.007) | 833 | 0.402 | -0.018*** | (0.007) | 795 | 0.310 |
| 2010 | -0.018** | (0.009) | 791 | 0.347 | -0.015 | (0.009) | 791 | 0.326 | -0.026*** | (0.009) | 846 | 0.329 | -0.028*** | (0.008) | 833 | 0.330 | -0.017* | (0.010) | 791 | 0.325 |
| 2020 | -0.002 | (0.014) | 707 | 0.349 | -0.003 | (0.013) | 731 | 0.329 | -0.018* | (0.011) | 798 | 0.327 | -0.019* | (0.011) | 783 | 0.324 | -0.005 | (0.013) | 731 | 0.328 |
| *Panel C: Percentage of Other Minority Residents* | | | | | | | | | | | | | | | | | | | | |
| 1980 | -0.009** | (0.004) | 788 | 0.142 | -0.008** | (0.004) | 830 | 0.137 | -0.004 | (0.003) | 863 | 0.130 | -0.006* | (0.003) | 861 | 0.134 | -0.009** | (0.004) | 830 | 0.133 |
| 1990 | -0.017*** | (0.006) | 830 | 0.134 | -0.016*** | (0.006) | 830 | 0.129 | -0.007 | (0.005) | 874 | 0.120 | -0.010** | (0.005) | 865 | 0.123 | -0.015*** | (0.006) | 830 | 0.128 |
| 2000 | -0.014 | (0.010) | 793 | 0.211 | -0.015 | (0.010) | 795 | 0.207 | -0.014* | (0.008) | 843 | 0.214 | -0.015* | (0.009) | 833 | 0.216 | -0.016 | (0.010) | 795 | 0.205 |
| 2010 | -0.034** | (0.014) | 791 | 0.356 | -0.026* | (0.014) | 791 | 0.323 | -0.032** | (0.014) | 846 | 0.320 | -0.027** | (0.013) | 833 | 0.328 | -0.027* | (0.014) | 791 | 0.324 |
| 2020 | -0.023 | (0.016) | 707 | 0.379 | -0.031* | (0.017) | 731 | 0.341 | -0.012 | (0.019) | 798 | 0.338 | -0.022 | (0.019) | 783 | 0.330 | -0.033* | (0.018) | 731 | 0.341 |
| *Panel D: Percentage of White Owner-Occupied Housing Units* | | | | | | | | | | | | | | | | | | | | |
| 1980 | 0.006 | (0.016) | 635 | 0.360 | 0.003 | (0.018) | 649 | 0.243 | 0.050 | (0.039) | 863 | 0.192 | 0.072* | (0.042) | 861 | 0.197 | 0.007 | (0.018) | 649 | 0.240 |
| 1990 | 0.033** | (0.015) | 830 | 0.410 | 0.044** | (0.017) | 830 | 0.276 | 0.049*** | (0.016) | 874 | 0.283 | 0.042** | (0.017) | 865 | 0.280 | 0.046** | (0.018) | 830 | 0.275 |
| 2000 | 0.056*** | (0.018) | 793 | 0.408 | 0.062*** | (0.019) | 795 | 0.274 | 0.066*** | (0.016) | 843 | 0.284 | 0.062*** | (0.017) | 833 | 0.280 | 0.064*** | (0.020) | 795 | 0.273 |
| 2010 | 0.071*** | (0.020) | 791 | 0.417 | 0.066*** | (0.022) | 791 | 0.355 | 0.078*** | (0.024) | 846 | 0.336 | 0.080*** | (0.025) | 833 | 0.335 | 0.074*** | (0.025) | 791 | 0.346 |
| *Panel E: Percentage of Black Owner-Occupied Housing Units* | | | | | | | | | | | | | | | | | | | | |
| 1980 | -0.000 | (0.000) | 635 | 0.552 | 0.000 | (0.000) | 649 | 0.503 | -0.003 | (0.003) | 863 | 0.863 | -0.001 | (0.001) | 861 | 0.974 | 0.000 | (0.000) | 649 | 0.503 |
| 1990 | -0.003 | (0.002) | 830 | 0.186 | -0.002 | (0.002) | 830 | 0.182 | -0.004 | (0.004) | 874 | 0.587 | -0.003 | (0.002) | 865 | 0.651 | -0.002 | (0.002) | 830 | 0.187 |
| 2000 | -0.010** | (0.004) | 793 | 0.281 | -0.010** | (0.004) | 795 | 0.277 | -0.009* | (0.005) | 843 | 0.431 | -0.010** | (0.005) | 833 | 0.426 | -0.010** | (0.005) | 795 | 0.279 |
| 2010 | -0.007 | (0.006) | 791 | 0.212 | -0.007 | (0.006) | 791 | 0.202 | -0.009 | (0.006) | 846 | 0.279 | -0.010 | (0.006) | 833 | 0.283 | -0.007 | (0.006) | 791 | 0.202 |
| *Panel F: Percentage of Other Minority Owner-Occupied Housing Units* | | | | | | | | | | | | | | | | | | | | |
| 1990 | -0.010*** | (0.004) | 830 | 0.116 | -0.009** | (0.003) | 830 | 0.113 | -0.005 | (0.004) | 874 | 0.108 | -0.007* | (0.004) | 865 | 0.108 | -0.009** | (0.003) | 830 | 0.110 |
| 2000 | -0.012** | (0.006) | 793 | 0.151 | -0.013** | (0.006) | 795 | 0.150 | -0.011** | (0.005) | 843 | 0.150 | -0.015*** | (0.005) | 833 | 0.161 | -0.013** | (0.006) | 795 | 0.149 |
| 2010 | -0.015** | (0.007) | 791 | 0.212 | -0.013* | (0.008) | 791 | 0.197 | -0.010 | (0.007) | 846 | 0.196 | -0.010 | (0.008) | 833 | 0.201 | -0.014* | (0.007) | 791 | 0.201 |

Note: This table reports $\alpha_1$ parameter estimates from Equation 1 for four robustness checks: Column Group I add SES controls (share over age 19, block population, median income, and any college education) to the baseline; Column Group II adds the *time to housing start* control to the baseline. Column Group III removes years 1947 and 1948 from the baseline analysis, while Column Group IV removes 1948 only. Column Group V adds a control for the mean number of racial groups listed in the covenant language in a block. Dependent variables are percentages of residents or owner-occupied housing units by race over time. Standard errors clustered at the census tract level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.7: Racial Segregation 1980–2020: Additional Placebo Tests

| | (I) Cov ≤ 10 | | | | (II) Cov ≤ 25 | | | | (III) Cov ≤ 35 | | | | (IV) Entropy Balancing ($\underline{c}_{jt} = 0$) | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | $\alpha_1$ | s.e. | $N$ | $R^2$ | $\alpha_1$ | s.e. | $N$ | $R^2$ | $\alpha_1$ | s.e. | $N$ | $R^2$ | $\alpha_1$ | s.e. | $N$ | $R^2$ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
| *Panel A: Percentage of White Residents* | | | | | | | | | | | | | | | | |
| 1980 | 0.006 | (0.004) | 5,521 | 0.757 | 0.006 | (0.004) | 5,731 | 0.755 | 0.007 | (0.004) | 5,848 | 0.753 | 0.006 | (0.005) | 5,303 | 0.703 |
| 1990 | 0.005 | (0.004) | 5,661 | 0.753 | 0.006 | (0.004) | 5,873 | 0.749 | 0.007 | (0.004) | 5,979 | 0.748 | 0.007 | (0.004) | 5,446 | 0.684 |
| 2000 | 0.008 | (0.006) | 5,547 | 0.758 | 0.010 | (0.006) | 5,751 | 0.757 | 0.010* | (0.006) | 5,851 | 0.755 | 0.008 | (0.007) | 5,334 | 0.649 |
| 2010 | 0.019** | (0.008) | 5,437 | 0.666 | 0.020** | (0.008) | 5,634 | 0.661 | 0.020*** | (0.007) | 5,730 | 0.660 | 0.019** | (0.008) | 5,226 | 0.597 |
| 2020 | 0.020** | (0.010) | 4,954 | 0.623 | 0.021** | (0.010) | 5,153 | 0.621 | 0.020** | (0.010) | 5,245 | 0.621 | 0.016 | (0.011) | 4,754 | 0.573 |
| *Panel B: Percentage of Black Residents* | | | | | | | | | | | | | | | | |
| 1980 | -0.005* | (0.003) | 5,521 | 0.822 | -0.005* | (0.003) | 5,731 | 0.820 | -0.005* | (0.003) | 5,848 | 0.818 | -0.005* | (0.003) | 5,303 | 0.784 |
| 1990 | -0.002 | (0.003) | 5,661 | 0.826 | -0.003 | (0.003) | 5,873 | 0.823 | -0.003 | (0.003) | 5,979 | 0.823 | -0.0008 | (0.003) | 5,446 | 0.783 |
| 2000 | -0.003 | (0.004) | 5,547 | 0.760 | -0.004 | (0.004) | 5,751 | 0.759 | -0.004 | (0.004) | 5,851 | 0.757 | -0.002 | (0.004) | 5,334 | 0.660 |
| 2010 | -0.004 | (0.006) | 5,437 | 0.606 | -0.003 | (0.006) | 5,634 | 0.596 | -0.003 | (0.006) | 5,730 | 0.596 | -0.005 | (0.006) | 5,226 | 0.529 |
| 2020 | -0.016*** | (0.005) | 4,954 | 0.587 | -0.017*** | (0.005) | 5,153 | 0.584 | -0.017*** | (0.005) | 5,245 | 0.582 | -0.015*** | (0.005) | 4,754 | 0.554 |
| *Panel C: Percentage of Other Minority Residents* | | | | | | | | | | | | | | | | |
| 1980 | -0.001 | (0.003) | 5,521 | 0.265 | -0.0008 | (0.003) | 5,731 | 0.259 | -0.001 | (0.003) | 5,848 | 0.256 | -0.0008 | (0.004) | 5,303 | 0.260 |
| 1990 | -0.003 | (0.004) | 5,661 | 0.296 | -0.003 | (0.003) | 5,873 | 0.293 | -0.004 | (0.003) | 5,979 | 0.289 | -0.006* | (0.004) | 5,446 | 0.241 |
| 2000 | -0.004 | (0.005) | 5,547 | 0.421 | -0.006 | (0.005) | 5,751 | 0.420 | -0.007 | (0.005) | 5,851 | 0.418 | -0.006 | (0.005) | 5,334 | 0.363 |
| 2010 | -0.015** | (0.006) | 5,437 | 0.415 | -0.017*** | (0.006) | 5,634 | 0.412 | -0.017*** | (0.006) | 5,730 | 0.411 | -0.014** | (0.006) | 5,226 | 0.380 |
| 2020 | -0.004 | (0.009) | 4,954 | 0.372 | -0.004 | (0.009) | 5,153 | 0.370 | -0.003 | (0.009) | 5,245 | 0.372 | -0.001 | (0.010) | 4,754 | 0.338 |
| *Panel D: Percentage of White Owner-Occupied Housing Units* | | | | | | | | | | | | | | | | |
| 1980 | 0.021* | (0.013) | 4,118 | 0.546 | 0.024* | (0.013) | 4,264 | 0.542 | 0.025* | (0.013) | 4,352 | 0.539 | 0.023* | (0.014) | 3,958 | 0.600 |
| 1990 | 0.018 | (0.013) | 5,661 | 0.522 | 0.019 | (0.012) | 5,873 | 0.518 | 0.021* | (0.012) | 5,979 | 0.516 | 0.009 | (0.014) | 5,446 | 0.546 |
| 2000 | 0.019 | (0.012) | 5,544 | 0.558 | 0.020* | (0.012) | 5,748 | 0.555 | 0.021* | (0.012) | 5,848 | 0.553 | 0.023* | (0.014) | 5,331 | 0.542 |
| 2010 | 0.023* | (0.012) | 5,436 | 0.512 | 0.024** | (0.012) | 5,633 | 0.508 | 0.025** | (0.012) | 5,729 | 0.506 | 0.023* | (0.013) | 5,225 | 0.498 |
| *Panel E: Percentage of Black Owner-Occupied Housing Units* | | | | | | | | | | | | | | | | |
| 1980 | 0.0002 | (0.004) | 4,118 | 0.703 | -0.0001 | (0.004) | 4,264 | 0.696 | -0.0000 | (0.004) | 4,352 | 0.690 | -0.002 | (0.004) | 3,958 | 0.678 |
| 1990 | -0.0003 | (0.002) | 5,661 | 0.714 | -0.001 | (0.002) | 5,873 | 0.713 | -0.0009 | (0.002) | 5,979 | 0.712 | -0.0006 | (0.002) | 5,446 | 0.665 |
| 2000 | 0.002 | (0.003) | 5,544 | 0.720 | 0.001 | (0.003) | 5,748 | 0.719 | 0.002 | (0.003) | 5,848 | 0.718 | -0.0003 | (0.002) | 5,331 | 0.660 |
| 2010 | -0.001 | (0.004) | 5,436 | 0.453 | -0.0001 | (0.004) | 5,633 | 0.440 | 0.0003 | (0.004) | 5,729 | 0.440 | -0.003 | (0.004) | 5,225 | 0.394 |
| *Panel F: Percentage of Other Minority Owner-Occupied Housing Units* | | | | | | | | | | | | | | | | |
| 1980 | 0.0003 | (0.0002) | 4,118 | 0.276 | 0.0003 | (0.0002) | 4,264 | 0.276 | 0.0003 | (0.0002) | 4,352 | 0.276 | 0.0003 | (0.0002) | 3,958 | 0.314 |
| 1990 | -0.003 | (0.002) | 5,661 | 0.067 | -0.003 | (0.002) | 5,873 | 0.065 | -0.003* | (0.002) | 5,979 | 0.064 | -0.002 | (0.002) | 5,446 | 0.066 |
| 2000 | 0.0002 | (0.004) | 5,544 | 0.209 | -0.0005 | (0.003) | 5,748 | 0.209 | -0.0010 | (0.003) | 5,848 | 0.207 | -0.0006 | (0.004) | 5,331 | 0.180 |
| 2010 | -0.005 | (0.004) | 5,436 | 0.217 | -0.006* | (0.004) | 5,633 | 0.218 | -0.007* | (0.004) | 5,729 | 0.217 | -0.005 | (0.004) | 5,225 | 0.206 |

Note: This table is plots parameter estimates $\alpha_1$ from Equation 1. In Panels A–C, the dependent variables are the percentage of residents by race over time. In Panels D–F, the dependent variables are the percentage of owner-occupied housing units. Standard errors are clustered at the census tract level. Column Groups I, II, and III restrict to neighborhoods where at least 50% of the census block was built between 1940 and 1960 and had different coverage of racial covenants ($\underline{c}_{jt} \leq 10, 25, 35$, respectively). Column Group IV restricts neighborhoods to $\underline{c}_{jt} = 0$ but uses entropy balancing to match this placebo sample to the baseline sample on population, share of population over 18, and share of the population with more than a high school diploma at the census block group level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: Racial Segregation 2020–2020: Controlling for House Values

| | (I) Baseline Sample | | | | (II) + Control for House Values at Block Group Level | | | | (III) + Control for Sales Prices at Block Level | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | $\alpha_1$ | s.e. | $N$ | $R^2$ | $\alpha_1$ | s.e. | $N$ | $R^2$ | $\alpha_1$ | s.e. | $N$ | $R^2$ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| *Panel A: Percentage of White Residents* | | | | | | | | | | | | |
| 2000 | 0.034*** | (0.012) | 795 | 0.330 | 0.033*** | (0.012) | 795 | 0.331 | – | – | – | – |
| 2010 | 0.043** | (0.019) | 791 | 0.416 | 0.040** | (0.019) | 791 | 0.419 | 0.041** | (0.020) | 788 | 0.415 |
| 2020 | 0.038* | (0.020) | 731 | 0.428 | 0.036* | (0.020) | 731 | 0.430 | 0.038** | (0.018) | 729 | 0.444 |
| *Panel B: Percentage of Black Residents* | | | | | | | | | | | | |
| 2000 | -0.018*** | (0.006) | 795 | 0.308 | -0.018*** | (0.007) | 795 | 0.309 | – | – | – | – |
| 2010 | -0.017* | (0.010) | 791 | 0.325 | -0.015 | (0.010) | 791 | 0.328 | -0.012 | (0.011) | 788 | 0.328 |
| 2020 | -0.005 | (0.013) | 731 | 0.327 | -0.005 | (0.013) | 731 | 0.327 | -0.003 | (0.012) | 729 | 0.331 |
| *Panel C: Percentage of Other Minority Residents* | | | | | | | | | | | | |
| 2000 | -0.016 | (0.010) | 795 | 0.205 | -0.015 | (0.010) | 795 | 0.206 | – | – | – | – |
| 2010 | -0.026* | (0.014) | 791 | 0.323 | -0.025* | (0.014) | 791 | 0.324 | -0.029** | (0.014) | 788 | 0.326 |
| 2020 | -0.033* | (0.018) | 731 | 0.341 | -0.031* | (0.018) | 731 | 0.343 | -0.035** | (0.017) | 729 | 0.349 |
| *Panel D: Percentage of White Owner-Occupied Housing Units* | | | | | | | | | | | | |
| 2000 | 0.064*** | (0.019) | 795 | 0.273 | 0.061*** | (0.019) | 795 | 0.275 | – | – | – | – |
| 2010 | 0.074*** | (0.025) | 791 | 0.346 | 0.068*** | (0.024) | 791 | 0.350 | 0.073*** | (0.025) | 788 | 0.339 |
| *Panel E: Percentage of Black Owner-Occupied Housing Units* | | | | | | | | | | | | |
| 2000 | -0.010** | (0.005) | 795 | 0.277 | -0.010** | (0.005) | 795 | 0.277 | – | – | – | – |
| 2010 | -0.007 | (0.006) | 791 | 0.202 | -0.007 | (0.006) | 791 | 0.203 | -0.007 | (0.006) | 788 | 0.206 |
| *Panel F: Percentage of Other Minority Owner-Occupied Housing Units* | | | | | | | | | | | | |
| 2000 | -0.013** | (0.006) | 795 | 0.149 | -0.014** | (0.006) | 795 | 0.149 | – | – | – | – |
| 2010 | -0.014* | (0.008) | 791 | 0.196 | -0.014* | (0.008) | 791 | 0.197 | -0.014* | (0.008) | 788 | 0.196 |

Notes: This table displays the parameter estimate $\alpha_1$ from Equation 1 for 2000–2020. Column Group I shows the baseline results from Table 3. Column Group II additionally controls for house values at census block group levels. The 1980 census did not collect this data, while block group–level house values for 1990 are not included, as 39% of the sample has missing values. Column Group III controls for sales prices at block level in the baseline model by taking the house-level sales price mean at the block level for 2010 and 2020. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.9: Racial Segregation 1980–2020 and Long-Run House Value Effects: Placebo Time Periods

| Samples/ Models | 1920–30–40 | | | 1927–35–47 | | | 1928–36–48 | | | 1945–54–64 | | | 1949–57–69 | | | 1950–58–70 | | | 1951–59–71 | | | 1952–60–72 | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | $\alpha_1$ | s.e. | $N$ | $\alpha_1$ | s.e. | $N$ | $\alpha_1$ | s.e. | $N$ | $\alpha_1$ | s.e. | $N$ | $\alpha_1$ | s.e. | $N$ | $\alpha_1$ | s.e. | $N$ | $\alpha_1$ | s.e. | $N$ | $\alpha_1$ | s.e. | $N$ |
| *Panel A: Percentage of White Residents* | | | | | | | | | | | | | | | | | | | | | | | | |
| 1980 | 0.013 | (0.010) | 451 | -0.006 | (0.008) | 588 | -0.006 | (0.007) | 655 | 0.011 | (0.016) | 711 | 0.002 | (0.020) | 561 | -0.002 | (0.018) | 534 | -0.005 | (0.006) | 530 | -0.002 | (0.008) | 479 |
| 1990 | 0.000 | (0.012) | 447 | -0.002 | (0.010) | 578 | 0.001 | (0.012) | 645 | 0.008 | (0.011) | 708 | 0.012 | (0.022) | 549 | 0.039 | (0.032) | 523 | -0.004 | (0.011) | 525 | -0.011 | (0.009) | 465 |
| 2000 | -0.018 | (0.016) | 413 | -0.016 | (0.019) | 538 | -0.010 | (0.018) | 603 | 0.051 | (0.037) | 663 | -0.093 | (0.060) | 503 | -0.003 | (0.063) | 476 | 0.010 | (0.020) | 472 | 0.000 | (0.019) | 412 |
| 2010 | 0.001 | (0.027) | 374 | -0.002 | (0.025) | 512 | 0.008 | (0.027) | 579 | 0.034 | (0.039) | 642 | -0.030 | (0.067) | 480 | -0.031 | (0.089) | 450 | 0.044 | (0.028) | 446 | 0.046 | (0.029) | 386 |
| 2020 | -0.022 | (0.037) | 318 | -0.020 | (0.026) | 435 | -0.009 | (0.033) | 495 | -0.015 | (0.060) | 564 | -0.016 | (0.057) | 406 | 0.019 | (0.082) | 371 | 0.082 | (0.056) | 368 | 0.034 | (0.052) | 306 |
| *Panel B: Percentage of Black Residents* | | | | | | | | | | | | | | | | | | | | | | | | |
| 1980 | -0.005 | (0.007) | 451 | 0.006 | (0.006) | 588 | 0.008 | (0.006) | 655 | -0.007 | (0.011) | 711 | -0.005 | (0.010) | 561 | -0.000 | (0.014) | 534 | -0.001 | (0.004) | 530 | -0.003 | (0.005) | 479 |
| 1990 | -0.001 | (0.009) | 447 | 0.000 | (0.007) | 578 | -0.006 | (0.009) | 645 | -0.007 | (0.010) | 708 | -0.006 | (0.020) | 549 | 0.010 | (0.024) | 523 | 0.003 | (0.009) | 525 | 0.008* | (0.005) | 465 |
| 2000 | -0.000 | (0.009) | 413 | 0.007 | (0.013) | 538 | 0.005 | (0.013) | 603 | -0.013 | (0.016) | 663 | 0.034 | (0.031) | 503 | 0.018 | (0.033) | 476 | -0.003 | (0.010) | 472 | 0.005 | (0.009) | 412 |
| 2010 | 0.008 | (0.014) | 374 | -0.001 | (0.012) | 512 | -0.002 | (0.015) | 579 | -0.026 | (0.029) | 642 | 0.050 | (0.052) | 480 | -0.006 | (0.036) | 450 | -0.021 | (0.016) | 446 | -0.018 | (0.019) | 386 |
| 2020 | 0.011 | (0.024) | 318 | -0.011 | (0.019) | 435 | -0.013 | (0.019) | 495 | -0.035 | (0.031) | 564 | 0.070 | (0.062) | 406 | 0.021 | (0.040) | 371 | -0.026 | (0.029) | 368 | -0.002 | (0.026) | 306 |
| *Panel C: Percentage of Other Minority Residents* | | | | | | | | | | | | | | | | | | | | | | | | |
| 1980 | -0.008 | (0.005) | 451 | 0.000 | (0.006) | 588 | -0.002 | (0.004) | 655 | -0.004 | (0.009) | 711 | 0.003 | (0.015) | 561 | 0.003 | (0.013) | 534 | 0.006 | (0.006) | 530 | 0.005 | (0.005) | 479 |
| 1990 | 0.001 | (0.005) | 447 | 0.002 | (0.006) | 578 | 0.005 | (0.007) | 645 | -0.001 | (0.008) | 708 | -0.006 | (0.016) | 549 | -0.048*** | (0.015) | 523 | 0.001 | (0.009) | 525 | 0.002 | (0.010) | 465 |
| 2000 | 0.018 | (0.013) | 413 | 0.010 | (0.013) | 538 | 0.005 | (0.014) | 603 | -0.038 | (0.027) | 663 | 0.059 | (0.038) | 503 | -0.015 | (0.056) | 476 | -0.008 | (0.015) | 472 | -0.005 | (0.015) | 412 |
| 2010 | -0.008 | (0.023) | 374 | 0.003 | (0.024) | 512 | -0.006 | (0.022) | 579 | -0.008 | (0.024) | 642 | -0.019 | (0.031) | 480 | 0.037 | (0.083) | 450 | -0.023 | (0.018) | 446 | -0.028* | (0.016) | 386 |
| 2020 | 0.011 | (0.025) | 318 | 0.031 | (0.023) | 435 | 0.022 | (0.028) | 495 | 0.049 | (0.044) | 564 | -0.053 | (0.059) | 406 | -0.040 | (0.073) | 371 | -0.057 | (0.037) | 368 | -0.032 | (0.037) | 306 |
| *Panel D: Log Assessed Value (2019) & Log Sales Price (2010-2019)* | | | | | | | | | | | | | | | | | | | | | | | | |
| Log Ass. Value | -0.003*** | (0.001) | 3876 | -0.000 | (0.001) | 5301 | 0.001 | (0.000) | 5801 | -0.001 | (0.001) | 6994 | 0.001 | (0.002) | 1336 | -0.000 | (0.001) | 1169 | 0.000 | (0.000) | 3303 | 0.000 | (0.000) | 2511 |
| Log Sales Price | -0.001 | (0.002) | 1391 | 0.001 | (0.002) | 2033 | 0.001 | (0.001) | 2228 | -0.002 | (0.001) | 2838 | 0.000 | (0.004) | 520 | -0.001 | (0.003) | 468 | 0.000 | (0.001) | 1291 | -0.001 | (0.001) | 975 |

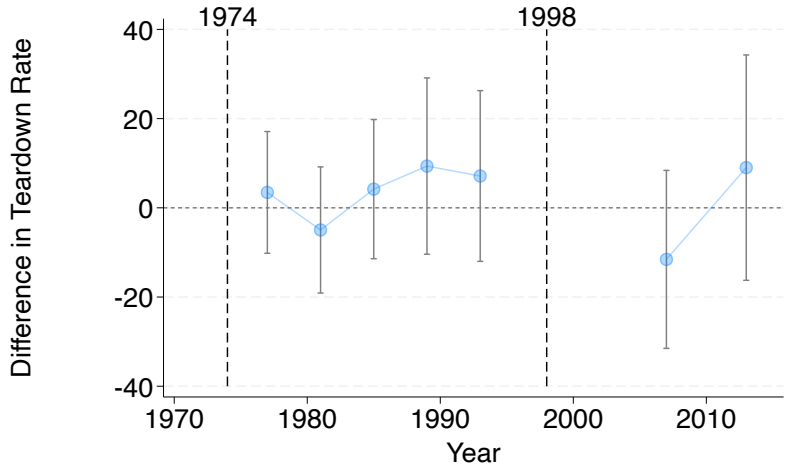
Notes: This table plots the parameter $\alpha_1$ (column 1) from Equation 1 in Panels A–C and Equation 2 in Panel D. In Panels A–C, the dependent variables are the percentage of residents by race over time, and standard errors are clustered at census tract level. In Panel D, the dependent variables are log assessed (Ass.) value (2019) and log sales price (2010–2019), and standard errors are clustered at census block level. The table presents placebo tests simulating alternative years of the *Shelley* decision. The placebo periods are 1920–1940 (with 1930 as the year of the *Shelley* decision), 1927–1947 (1935, respectively), 1928–1948 (1936), 1945–1964 (1954), 1949–1969 (1957), 1950–1970 (1958), 1951–1971 (1959), and 1952–1972 (1960). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.10: Public Official Responses: Robustness

| Dependent Variable | $\alpha_1$ | s.e. (cluster) | s.e. (robust) | $N$ | $R^2$ | Effect of 1 SD increase | Mean of Dep. Variable |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Dist. Highways: 1940–1956 (m) | 1.588*** | (0.543) | [0.415] | 687 | 0.965 | 53.75 | 1,043 |
| Share Single-Family (Time to Housing Start) | 0.023 | (0.029) | [0.017] | 732 | 0.961 | – | 71.06 |
| Min. Lot Size (sq ft) (Time to Housing Start) | 0.802 | (1.055) | [2.459] | 725 | 0.859 | – | 7,926 |
| *Falsification:* | | | | | | | |
| Dist. Lakes & Parkways (m) (Time to Housing Start) | -0.545 | (0.502) | [0.372] | 732 | 0.841 | – | 1,007 |

Note: This table plots the parameter $\alpha_1$ (column 1) from Equation 1. Row 1 shows the effect on the Euclidean distance to highways for the smaller 1940–1956 time window. Rows 2–4 use $(\%\text{start} \leq 1948)_{jt}$ and *time to housing start* as a control. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.3: Property Teardown Rate (1974–2013)




Note: This figure illustrates the difference in the teardown rates for houses built between 1940 and 1949 and between 1950 and 1959 based on two samples of houses from the American Housing Survey. The first sample was drawn in 1974 and was used in the surveys conducted in 1977, 1981, 1985, 1989, and 1993. The second sample was drawn in 1998 and was used in the surveys conducted in 2007 and 2013. The vertical bars represent 90% confidence intervals for the difference in teardown rates.

## C.1 Alternative Empirical Design

The baseline empirical design restricts attention to majority-covenanted neighborhoods (to control for the endogenous choice to covenant) and uses the 1948 ruling and 1940–1960 timeline to construct the treatment intensity. An alternative empirical design could instead compare covenanted houses and neighborhoods built by 1948 with covenanted houses and neighborhoods built after 1948 and uncovenanted houses and neighborhoods built

between 1940 and 1960. To implement this alternative design, we use an instrumental variable (IV) approach and the 1948 *Shelley* ruling.

### C.1.1 Neighborhood- and House-Level Models

$$\begin{aligned} Y_{jt} = \theta_0 + \theta_1(\%\text{enforceable cov})_{jt} + \theta_2(\text{mean time to build})_{jt} \\ + \theta_4 X_{jt} + \eta_{k(j)t} + \epsilon_{1jt} \end{aligned} \tag{C.1}$$

$$(\%\text{enforceable cov})_{jt} = \gamma_0 + \gamma_1(\%\text{built} \leq 1948)_{jt} + \eta_{k(j)t} + \epsilon_{2jt} \tag{C.2}$$

In Equation C.1, $Y_{jt}$ is the percentage of residents or owner-occupied housing units by race in block $j$. We then define a new term $(\%\text{enforceable cov})_{jt}$, the percentage of houses with an enforceable racial covenant (i.e., the covenant was placed on the house by 1948 and the house was built by 1948), out of all houses built between 1940 to 1960 in neighborhood $j$ at year $t$. We instrument $(\%\text{enforceable cov})_{jt}$ with the $(\%\text{built} \leq 1948)_{jt}$ in the first-stage Equation C.2. As with the baseline empirical design, we restrict this sample to neighborhoods where at least 50% of the houses were built between 1940 and 1960. To maintain comparability with the baseline design, we include controls for $(\text{mean time to build})_{jt}$ and housing characteristics ($X_{jt}$) and tract–year fixed effects, $\eta_{k(j)t}$.

We estimate outcomes at the house level with the same IV framework:

$$\begin{aligned} \log Y_{ijt} = \theta_0 + \theta_1(\%\text{enforceable cov})_{jt} + \theta_2(\text{mean time to build})_{jt} \\ + \eta_{k(j)t} + \delta_t + \epsilon_{1ijt} \end{aligned} \tag{C.3}$$

$$(\%\text{enforceable cov})_{jt} = \gamma_0 + \gamma_1(\%\text{built} \leq 1948)_{jt} + \eta_{k(j)t} + \epsilon_{2ijt} \tag{C.4}$$

In Equation C.3, $Y_{ijt}$ is the log of the assessed value or sale price, as in our baseline house-level models. Again, we restrict this sample to neighborhoods where at least 50% of the houses were built between 1940 and 1960, control for $(\text{mean time to build})_{jt}$, and include tract–year fixed effects, $\eta_{k(j)t}$, and sale year fixed effects, $\delta_t$, when applicable.

Under this design, $(\%\text{built} \leq 1948)_{jt}$ must satisfy both the exclusion restriction and the relevance condition. Regarding the former, for the neighborhood-level model, we assume that residents of different races do not systematically differ in their preferences for homes built before versus after 1948 (e.g., that White residents preferred homes built from 1940

Table C.11: Long-Run Racial Segregation & House Value Effects: Alternative Empirical Design

| | $\theta_1$ | s.e. (cluster) | $N$ | $R^2$ | Effect of 1 SD increase | Mean of Dep. Variable | First-Stage $F$-Stat |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel A Dependent Variable: Percentage of White Residents* | | | | | | | |
| 1980 | 0.035** | (0.017) | 6845 | -0.002 | 0.57 | 96.06 | 59.05 |
| 1990 | 0.047** | (0.020) | 6977 | 0.006 | 0.75 | 93.35 | 59.38 |
| 2000 | 0.093*** | (0.029) | 6811 | 0.009 | 1.49 | 86.63 | 62.12 |
| 2010 | 0.162*** | (0.036) | 6682 | 0.014 | 2.56 | 81.25 | 58.46 |
| 2020 | 0.140*** | (0.042) | 6126 | 0.011 | 2.26 | 70.84 | 62.36 |
| *Panel B Dependent Variable: Percentage of Black Residents* | | | | | | | |
| 1980 | -0.023* | (0.012) | 6845 | -0.004 | -0.37 | 2.02 | 59.05 |
| 1990 | -0.016 | (0.014) | 6977 | 0.005 | – | 3.46 | 59.38 |
| 2000 | -0.038** | (0.018) | 6811 | 0.009 | -0.61 | 5.54 | 62.12 |
| 2010 | -0.044* | (0.026) | 6682 | 0.019 | -0.70 | 7.55 | 58.46 |
| 2020 | -0.086*** | (0.023) | 6126 | 0.016 | -1.39 | 8.70 | 62.36 |
| *Panel C Dependent Variable: Percentage of Other Minority Residents* | | | | | | | |
| 1980 | -0.012 | (0.012) | 6845 | 0.002 | – | 1.92 | 59.05 |
| 1990 | -0.031* | (0.016) | 6977 | 0.002 | -0.50 | 3.19 | 59.38 |
| 2000 | -0.055** | (0.023) | 6811 | 0.002 | -0.87 | 7.82 | 62.12 |
| 2010 | -0.118*** | (0.027) | 6682 | -0.002 | -1.86 | 11.20 | 58.46 |
| 2020 | -0.054 | (0.038) | 6126 | 0.012 | – | 20.46 | 62.36 |
| *Panel D Dependent Variable: Percentage of White Owner-Occupied Housing Units* | | | | | | | |
| 1980 | 0.128** | (0.055) | 5136 | 0.111 | 2.15 | 84.63 | 61.44 |
| 1990 | 0.152** | (0.059) | 6977 | 0.102 | 2.42 | 82.08 | 59.38 |
| 2000 | 0.168*** | (0.054) | 6808 | 0.083 | 2.67 | 80.64 | 62.28 |
| 2010 | 0.194*** | (0.058) | 6681 | 0.069 | 3.06 | 75.81 | 58.47 |
| *Panel E Dependent Variable: Percentage of Black Owner-Occupied Housing Units* | | | | | | | |
| 1980 | 0.006 | (0.015) | 5136 | 0.005 | – | 0.79 | 61.44 |
| 1990 | -0.005 | (0.008) | 6977 | 0.005 | – | 1.49 | 59.38 |
| 2000 | -0.003 | (0.014) | 6808 | 0.006 | – | 2.73 | 62.28 |
| 2010 | -0.013 | (0.017) | 6681 | 0.002 | – | 2.80 | 58.47 |
| *Panel F Dependent Variable: Percentage of Other Minority Owner-Occupied Housing Units* | | | | | | | |
| 1990 | -0.022*** | (0.008) | 6977 | 0.001 | -0.35 | 1.29 | 59.38 |
| 2000 | -0.023 | (0.016) | 6808 | 0.005 | – | 3.29 | 62.28 |
| 2010 | -0.051*** | (0.017) | 6681 | 0.002 | -0.80 | 4.59 | 58.47 |
| *Panel G Dependent Variables: Log Assessed Value (2019) & Log Sales Price (2010–2019)* | | | | | | | |
| Log Assessed Value (2019) | 0.001** | (0.0003) | 64784 | 0.002 | 1.54 | – | 422.38 |
| Log Sales Price [2010-2019] | 0.002*** | (0.001) | 26311 | -0.001 | 3.93 | – | 371.91 |

*Notes:* This table displays the parameter $\theta_1$ (column 1) from Equations C.1 and C.3. The dependent variables are resident percentages by race (Panels A–C), percentages of owner-occupied housing units by race (Panels D–F), and log assessed value (2019) and log sales price (2010–2019) in Panel G. Standard errors clustered at census tract level are in parentheses (column 2). Column 5 is the effect of a 1-standard-deviation (SD) increase in treatment in Equations C.1 and C.3 for statistically significant variables. Column 6 shows means of the dependent variables. Column 7 shows the first-stage $F$-stat. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

to 1948 but minority residents preferred homes from 1949 to 1960). We find it reasonable to believe that race does not correlate with preferences for homes within the 1940–1960 construction window. For the house-level model, the price of a specific house must not depend on the proportion of neighborhood construction completed before 1948, which is a harder assumption to defend. For the relevance condition to hold for both models, there must be a correlation between the percentage of houses in a neighborhood with enforceable covenants and those built before 1948. This is, of course, mechanically true, as—without considering *time to build*—if every house were built after 1948, then very few covenants would ever have been placed on the newly constructed houses, and none would have ever been enforceable. The time between the covenant being placed on a property and the end of construction, however, adds some variability. Regardless, as seen in Figure 2, if the home was built before 1948, it was likelier to be subject to a covenant, which, given the timing of *Shelley*, would have been enforceable. Moreover, the first-stage $F$-stat for both models is well above 50 (Table C.11, column 7).

The results from Equation C.1, presented in Table C.11, suggest that a 1 SD increase in the percentage of houses with an enforceable covenant in a neighborhood is associated with a 0.6–2.6 pp increase in the percentage of White residents. For Black or other minority residents, we see a negative relationship between enforceable covenants and the percentage of residents. These results align with our baseline estimates. Panels D, E, and F on ownership reflect similar findings. The results of the household-level model are presented in Table C.11 Panel G. The effects on assessed values and sales prices remain consistent with those of the baseline model; however, the impact of enforceability on assessed values is slightly smaller than the baseline.

In summary, we believe this empirical design does not adequately control for unobserved differences between neighborhoods with and without enforceable covenants. Nevertheless, we find effects mostly similar to baseline with this design, which we offer as additional suggestive evidence for the effect of the loss of racial covenant enforceability.

## C.2 Relative Role of Early Responses in Persistent Effects

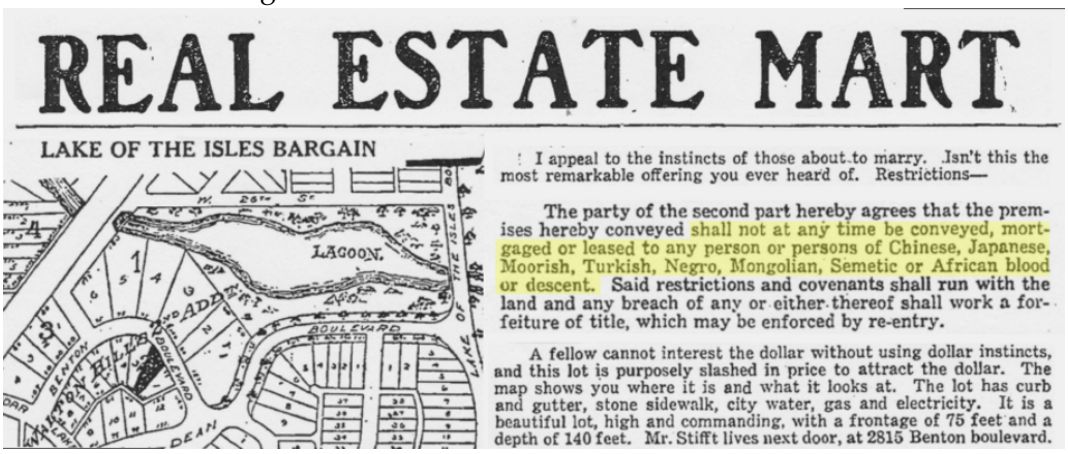
To evaluate the relative contribution of early responses to the baseline persistent effects, we use a hedonic house price model. Because all the early-stage responses of developers, initial

residents, and public officials are capitalized into house values, the hedonic model helps disentangle the relative roles of each. Each of these early-stage responses is an intermediate mechanism, so these results are suggestive rather than causal.

When we control for housing unit characteristics (log lot size and building size, number of bedrooms, bathrooms and floors, and indicators for house remodeling and property type), $\alpha_1$ decreases by 18.1% and 22.9% for assessed value and sales prices, respectively, from the baseline total effect parameter (Appendix Table C.3 Panel A). When we additionally control for local public amenities (Euclidean distance to highways, minimum lot size, and share of neighborhood zoned for single-family), the estimate falls by 26.2% and 23.1% for assessed value and sales prices, respectively, from the baseline total effect parameter (Appendix Table C.3 Panel B). Thus, the historical differences in the characteristics of the constructed housing and local public amenities resulting from different levels of covenant enforceability potentially account for 23–26% of the persistent effects observed today. The remaining 74–77% of these effects may be attributed to two factors. First, unobservable differences in housing characteristics and local public amenities impacted by the *Shelley* ruling across treatment could drive some of these remaining effects. Second, factors such as racial segregation and racial attitude differences across space might play a role in driving the remainder of the persistent effects.

# D. Additional Figures and Tables

Figure D.1: Advertisement of Racial Covenants


REAL ESTATE MART

LAKE OF THE ISLES BARGAIN

I appeal to the instincts of those about to marry. Isn't this the most remarkable offering you ever heard of. Restrictions—

The party of the second part hereby agrees that the premises hereby conveyed shall not at any time be conveyed, mortgaged or leased to any person or persons of Chinese, Japanese, Moorish, Turkish, Negro, Mongolian, Semetic or African blood or descent. Said restrictions and covenants shall run with the land and any breach of any or either thereof shall work a forfeiture of title, which may be enforced by re-entry.

A fellow cannot interest the dollar without using dollar instincts, and this lot is purposely slashed in price to attract the dollar. The map shows you where it is and what it looks at. The lot has curb and gutter, stone sidewalk, city water, gas and electricity. It is a beautiful lot, high and commanding, with a frontage of 75 feet and a depth of 140 feet. Mr. Stifft lives next door, at 2815 Benton boulevard.

Note: This figure provides an example of advertisement of racial covenants in a development.

Table D.1: Selected Legal Cases Regarding Racial Covenants in the US

| Case | Year | State | Level* | Description |
|---|---|---|---|---|
| *Gandolfo v. Hartman* | 1892 | CA | District National | Covenant restricting Chinese from owning properties. The court invalidated the use of racial covenants, but this decision was largely ignored, because it did not align with existing interpretations of the Fourteenth Amendment. |
| *Queensborough Land Co. v. Cazeaux* | 1915 | LA | State | Covenant against property purchase by African American individuals. Neighborhoods started the suit, and resolution was in line with the covenant. |
| *Koehler v. Rowand* | 1918 | MO | County State | African American family (the Rowands) acquired a property under racial covenant. Plaintiffs demanded the property back; judge ordered the Rowands to return it. |
| *Los Angeles Investment Co. v. Gary* | 1919 | CA | County State | Defendant, African American, bought a property, which until 1930 could not be owned by a non-Caucasian. Plaintiff, a real estate company, sued. The resolution was against the defendant. |
| *Parmalee v. Morris* | 1922 | MI | State | The Morrises, an African American couple, entered into a contract to purchase a lot. Neighbors made use of a covenant to impede operation. The circuit ruled against the Morrises. |
| *Janss Investment Co. v. Walden* | 1925 | CA | County State | Walden broke a covenant from Janss Investment Company, conveying his property to a black woman, Betty Willing. The company sued Walden. The Superior Court of Los Angeles County rendered in favor of the plaintiff. The defendant Walden appealed to the Supreme Court of California. The court maintained the original decision. |
| *Corrigan v. Buckley* | 1926 | DC† | District National | Corrigan broke a covenant, selling her land to a Black couple. Buckley sued, and the Court of the District ruled in his favor. Corrigan appealed; the US Supreme Court maintained the decision. |
| *Burke v. Kleiman* | 1934 | IL | State | Burke leased a covenanted apartment to Hall, a African American man. Kleiman sued. First sentence was favorable to Kleiman. Burke appealed, alleging that since the agreement was signed (1927) conditions had changed in the area. The court affirmed the first sentence. |
| *Grady v. Garland* | 1937 | DC† | District | Group of properties in DC had a racial covenant. Plaintiff Grady, owner of a lot, brought suit on behalf of himself and other owners and parties interested in other lots to eliminate the deed. Judge ruled against the plaintiff. |
| *Meade v. Dennistone* | 1938 | MD | State | A property subject to a racial covenant was sold to Edward Meade, an African American, for $150 in cash, with the balance to be paid in monthly installments. Due to the covenant, Meade required the deposit to be refunded. Court verdict was favorable to Dennistone. |
| *Hanberry v. Lee* | 1940 | IL | State National | Homeowner who had signed a restrictive covenant sold his home to Hansberry, an African American. Lee sought to enforce the covenant and void the sale. Illinois courts held that the seller could not sell to Hansberry. The US Supreme Court reversed, holding that the state courts' application of *res judicata* violated the due process clause of the 14th amendment. |
| *Moore v. Adams* | 1940 | AR | State | A covenant stated that once the lot was sold, the constructed property should not have a value lower than $3,400 and could not be conveyed to people of "negro blood". Lot was conveyed to Moore, who proposed to erect a tourist camp of a value greater than $4,000. Suit was filed by Adams, who had purchased lots in the same subdivision, to restrain Moore's construction. Court found in favor of Adams. |

Note: *Some cases started at a given level (county or state) and ended at a higher level (state or national); † District of Columbia.

Table D.1: Selected Legal Cases Regarding Racial Covenants in the US (Continued)

| Case | Year | State | Level* | Description |
|---|---|---|---|---|
| *Lyons v. Wallen* | 1942 | OK | State | Property under racial covenant was intended to be sold to Lyons, an African American. Wallen sued to impede the transaction. Decision was favorable to Wallen. Lyons appealed with no success. |
| *Doherty v. Rice* | 1942 | WI | State | Courts upheld restrictions excluding certain races from use or occupancy as valid and not fettering alienation. |
| *Fairchild v. Raines* | 1944 | CA | State | The Winsells sold a property under racial covenant to the Raines (African Americans). Fairchild, signer of the covenant back in 1921, sued Raines. In the first instance, the court prohibited the Raines from using the property. The defendants appealed, and the previous sentence was reversed. |
| *Mays v. Burgess* | 1945 | DC† | District | Mays, an African American, bought a racially restricted property from Jane Cook. In trial, the District Court stated that the covenant remained valid. Mays appealed the sentence with no success. |
| *Scholtes v. McColgan* | 1945 | MD | State | Suit by Scholtes to restrain McColgan from selling any part of a tract of land to any African American person. The complaint was dismissed, and the plaintiff appealed. On appeal, the racial covenant was affirmed. |
| *Phillips v. Wearn* | 1946 | NY | County | Philips and Wearn become owners of two covenanted parcels of land in Westchester County. However, the defendant considered herself an octoroon (person having 1/8 African American blood). The court ruled favorably to the plaintiff. |
| *Vernon v. R. J. Reynolds Realty Co.* | 1946 | NC | State | The court refused to cancel a racial restriction despite the surrounding area being occupied by Black and White residents unwilling to buy the property, holding the restriction protected the restricted area itself. |
| *Bogan v. Saunders* | 1947 | DC† | District | Saunders (African American) bought a covenanted property. The plaintiffs sought to enforce the covenant and void the sale. The ruling was for Bogan. |
| *Hurd v. Hodge* | 1947 | DC† | District | The Hodges sued broker Raphael Urciolo and the new owners of a sold property, the Hurds, for violating a racial covenant. DC courts upheld the covenant, but the US Supreme Court agreed to hear the case as a companion to *Shelley vs Kramerer*. DC is not a state and is not subject to the 14th amendment; hence, the ruling was based on the Civil Rights Act of 1866, which requires the federal government to treat citizens equally. |
| *Meckler v. Baugh* | 1947 | DC† | District | Baugh, an African American, signed a contract for a property from Meckler. A day after the contract was signed, the agent presented Baugh with a new contract including a racial covenant. Baugh decided not to buy the house under these conditions and demanded a refund of the deposit paid. The resolution was favorable to Meckler. |
| *Northwest Civic Ass'n v. Sheldon* | 1947 | MI | County State | Subdivision of 338 lots, of which 310 were subject to a racial covenant. The Sheldons purchased one of the lots outside the covenant. As disclosed by the testimony, Otis Sheldon was the only defendant who was not Caucasian. The court considered that Sheldon was told about the covenant when purchasing the property. Decree was modified such that Sheldon could enter the property but not inhabit it. |
| *Sipes v. McGhee* | 1947 | MI | State National | McGhees purchased a house in a covenanted neighborhood. The neighborhood association went to court to have them removed, and the court, upholding the covenant, ordered the McGhees to leave the property. The Michigan Supreme Court affirmed that decision. The case went to the US Supreme Court, where the ruling was reversed. |

Note: *Some cases started at a given level (county or state) and ended at a higher level (state or national); † District of Columbia.

Table D.1: Selected Legal Cases Regarding Racial Covenants in the US (Continued)

| Case | Year | State | Level* | Description |
|---|---|---|---|---|
| *Claremont Improvement Club v. Buckingham* | 1948 | CA | State | Plaintiffs sued to enforce a racial covenant to forfeit the sale of a lot occupied by defendants and to restrain their continued residence. Defendants alleged that the expression excluding those not of "pure Caucasian blood" was incapable of exact determination. Ruling went for plaintiffs. |
| *Goetz v. Smith* | 1948 | MD | State | Wanda Goetz and Charles Bell sued Hiram Smith and Lulu Smith, aiming to impede them from occupying a racially restricted property. The ruling was favorable to the Smiths. |
| *Ralph v. Trawick* | 1948 | DC† | District | Ralph, an African American, entered into a contract for the purchase of real estate. Before the contract was consummated, he learned the property was subject to a covenant. He then refused to complete the purchase and sued the broker for the deposit paid. Ruling was favorable to Ralph. |
| *Shelley v. Kraemer* | 1948 | MO | County State National | The Shelleys moved into a neighborhood without knowing about a covenant against African and Asian Americans. Kraemer brought suit to enforce the covenant, supported by other members of the neighborhood. The Supreme Court consolidated that the enforcement of the racial covenants in state court violated the equal protection clause of the 14th amendment. |
| *Tovey v. Levy* | 1948 | IL | State | In 1944, a racially covenanted parcel was conveyed to Hayman Levy, who signed a deed to convey it to Cadillac Hotel Corporation, Inc, which leased the property to Joseph Allen, an African American. The amended complaint alleged that occupancy by Allen was breached the covenants. Ruling favorable for Levy. |
| *Weiss v. Leaon* | 1949 | MO | State | The Leaons sold or were about to sell their lot to the Streets, African Americans. The lots in Santa Fe were subject to a racial covenant. Under the decision of the US Supreme Court in *Shelley v. Kraemer*, the trial court dismissed the action. |
| *Amschler v. Remijasz* | 1950 | DC† | State | The Amschlers filed a complaint seeking to restrain the defendants from selling to African Americans. Ruling was favorable to plaintiffs, with the defendants being obligated to leave the property in 1947. By decision in previous cases, the US Supreme Court made racial covenants unenforceable. On May 24, 1948, the defendants moved for dissolution of the injunction and presented their suggestion of damages. |
| *Correll v. Earley* | 1951 | OK | State | Correll sued to enforce covenants in an agreement by property owners restricting against alienation to African Americas and for damages as a result of a conspiracy to injure the value of the plaintiffs' property. In the first instance, the case was decided against the plaintiff, who appealed; the sentence was reversed. |
| *Barrows v. Jackson* | 1953 | CA | State | A covenant restricted the use and occupancy of lands to only Caucasian persons, obligating the signers to incorporate this restriction in all transfers of the land. Jackson conveyed a parcel of her land to a non-Caucasian. Barrows and other landowners filed a lawsuit in California state court seeking to enforce the covenant and recover damages for Jackson's breach of the covenant. The trial court relied on the holding in *Shelley v. Kramer* and ruled in favor of Jackson. |

Note: *Some cases started at a given level (county or state) and ended at a higher level (state or national); † District of Columbia.