Long Shadow of Housing Discrimination: Evidence from Racial Covenants[*]

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Abstract

Racial covenants were clauses in property deeds that prohibited the sale of property to specific racial and ethnic minorities. This paper studies the long-run causal and persistent effects of racially-restrictive covenants on racial sorting, racial homeownership differentials, and house prices. Using novel census data on racial covenants and a quasi-experimental design that exploits the 1948 U.S. Supreme Court ruling that made these covenants unenforceable, we compare newly built covenanted, and hence all-white, neighborhoods built before 1948 with covenanted neighborhoods constructed right after the verdict. We find that about 5-15% of the observed neighborhood racial residential sorting and 3-6% of racial homeownership sorting from 1980 to 2020 can be causally linked to racial covenants of the past. In addition, while we find differences in public amenities, particularly in the distance to highways and the restrictiveness of zoning regulation, between the treated and control neighborhoods, these differences do not drive the observed persistent sorting effects, limiting the scope for place-based policies.

Keywords: Housing Discrimination, Racial Covenants, Segregation

JEL: N32, N92, R12, R52

1. Introduction

Across the United States, we observe persistent neighborhood racial segregation in the 21st century. From the 1880s to the mid-20th century, neighborhood-level segregation doubled, and the form of racial segregation changed from building to the neighborhood level (Logan et al., 2015; Cutler et al., 1999; Logan and Parman, 2017). Many instruments for segregation were adopted during this period, including racially-restrictive covenants. This paper studies the persistent effects of these covenants on post-war and present-day racial sorting, racial homeownership rates, and house prices.

Racially-restrictive covenants were clauses in property deeds that prohibited the sale or rental of property to racial and ethnic minorities (see Figure 1 for examples). Throughout the first half of the 20th century, across the United States, racial covenants were added either by developers on newly plotted land or neighborhood petitions to create segregated neighborhoods. Covenants solved the neighborhood coordination problem by legally preventing neighbors from selling houses to racial and ethnic minorities at a markup (Akbar et al., 2019).

In this paper, we use the 1948 U.S. Supreme Court ruling (*Shelley v Kramer*) that made racial covenants unenforceable and compare newly built covenanted, and hence all-white, neighborhoods built before 1948 with covenanted neighborhoods constructed right after the verdict that had lost a primary legal way to segregate. In a world with moving costs, this experiment allows us to study the evolution of these two types of neighborhoods with different initial conditions created by racial covenants as well as examine the persistent effects of these initial conditions. We find that about 5-15% of the observed neighborhood racial residential sorting and 3-6% of racial homeownership sorting from 1980 to 2020 can be causally linked to racial covenants of the past.

We use a unique and newly constructed data set of all historic property deeds from 1900-1960 with information on racially-restrictive covenants for all lots in Hennepin County, Minnesota. We match this data with census and present-day tax assessor data to assess the long-term impact of these covenants. While covenants were prevalent throughout northern cities in the U.S., like New York, D.C., San Francisco, Chicago, and

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Boston, there are two distinct advantages to studying the role of racial covenants in Minneapolis and its suburbs. First, the census of the lot-level data on racial covenants exists only in Hennepin County. Second and more importantly, unlike older cities near the coast which had a long history of using racially-restrictive zoning or other instruments of housing discrimination, Minneapolis and its suburbs did not. In a newly expanding city during the first half of the 20th century, racial covenants were some of the first housing discriminatory policies to be used on a large scale. They created initial conditions around which socioeconomic spatial disparities were built.

Our primary findings are that the effects of racially-restrictive covenants are ever-present today and affect socioeconomic outcomes in a significant manner. A one standard deviation increase in initial covenants shock in neighborhoods (census blocks) results in a 0.3-1.5% fall in Black and other racial minority residents and a 0.2-0.8% fall in Black and other minority homeowners 1980-2020. Our results are also consistent with the hypothesis that covenant language was used primarily in the lower amenity neighborhoods that were more transition within the broader metro area, effectively keeping lower-middle and middle-class African-Americans and other minorities from buying houses in certain white blue-collar neighborhoods. The high prices of homes in higher natural amenity locations served as a mechanism to restrict people of color from moving in.

We then explore the specific mechanisms that result in the persistent effects of covenants. We discuss three possible mechanisms for the persistent effect of covenants–homophily bias, differences in public amenities, and differences in housing quality. First, if residents prefer to live with people of their own race (homophily bias) and there is low mobility across locations over the years (on average, subsequent resale is observed 12.2 years later), racial sorting patterns can persist even after the 1948 ruling. Second, public investment post-ruling in highways, zoning, lakes and greenways, and school quality in treated covenanted neighborhoods could differ from control covenanted neighborhoods. Better public amenities can anchor neighborhoods to white residents over time, resulting in persistent sorting effects. Lastly, house quality may differ in treated and control covenanted neighborhoods. If different racial groups make differential residen-

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tial location choices based on house prices, it may explain the persistence in sorting effects.

While we find that highways built in the 1950s, right after the 1948 ruling, were placed farther away from treated covenanted neighborhoods than control neighborhoods. We also find that a one standard deviation increase in covenant shock (34%) results in a 55.8-square-foot increase in minimum lot size. We observe no statistically significant effect for lakes and greenways or school quality. However, differences in highways and zoning regulations have little effect on racial sorting patterns, as observed in 2020. In addition, house values in the 21st century are higher by 2.8-13.8% in the treated than control covenanted neighborhoods, with price differences increasing with the increase in the initial covenant shock. These differences cannot be explained by differences in observed housing characteristics or local public amenities, indicating that homophily bias and potential differences in unobserved house quality are the likely drivers of persistent sorting effects.

The literature has studied the importance of initial conditions and their influence on city development, technology adoption, and economic growth (Redding and Sturm, 2008; Dell, 2010; Bleakley and Lin, 2012; Jedwab et al., 2017; Hornbeck and Keniston, 2017; Allen and Donaldson, 2020; Redding et al., 2011; Henderson et al., 2018; Heblich et al., 2021). Our paper studies the effect of initial conditions set by racially-restrictive covenants on long-run house prices and racial residential location choice. While the role of racial covenants has been examined in legal, historical, and sociology literature (Rose et al., 2016; Trounstine, 2018; Gordon, 2023; Brooks, 2011; Brown and Smith, 2016; Rothstein, 2017; Kucheva and Sander, 2014), this paper provides quantitative evidence of the persistent effects of racial covenants.

Several studies have housing discrimination in the 20th century Unites States. Racial covenants are part of the broader environment of housing market discrimination. See Aaronson et al. (2021) on the persistent effects of Home Owners' Loan Corporation (HOLC) maps, Fishback et al. (2020) and Fishback et al. (2022) on the discriminatory Federal Housing Authority (FHA) practices, Shertzer et al. (2016), Shertzer et al. (2022), and Troesken and Walsh (2019) on the persistent effects of zoning regulations, Baum-

Snow (2007) on highways, Bennett et al. (2022) on blockbusting, Ananat (2011) and Chyn et al. (2022) on railroad placement, and Myers Jr (1995) inequality in credit access. In contrast to these other instruments of discrimination, racial covenants were a direct approach to created segregated neighborhoods. In addition, racially-restrictive covenants studied in this paper often predate other discriminatory instruments, like HOLC, zoning regulation, and highways, and have contributed to these policies' geographic shape. Thus, some of the effects captured by the aforementioned papers are likely due to racially-restrictive covenants that shaped the city structure in the first place. Lastly, this paper also ties into the large literature studying segregation during the 20th century (Tiebout, 1956; Cutler et al., 1999; Collins and Margo, 2011; Boustan and Margo, 2013; Logan and Parman, 2017) and the effects of segregation on mobility and inequality (Chetty et al., 2018; Chyn and Katz, 2021).

The rest of the paper proceeds as follows. Section 2 describes the historical background and data construction. Section 3 provides the empirical strategy, and Section 4 discusses the persistent effects of covenants on racial sorting. Section 5 provides the theoretical and empirical framework for understanding the mechanisms behind persistent effects.

2.   Historical background and data construction

2.1   Racial covenants and housing discrimination in the 20th century

Housing discrimination has taken many forms in the United States. During the first half of the 20th century, racial covenants were among the more prevalent forms of housing discrimination in urban and suburban areas. Property covenants are a commonly used instrument that codifies restrictions on the use of a property, usually added to the sales deed. Racial covenants were such types of property covenants that included language explicitly prohibiting many racial and ethnic groups from purchasing or residing on a property. While the primary goal of racial covenants was to prevent African-Americans from moving into neighborhoods, these clauses would also commonly exclude other racial and ethnic minorities and immigrants. Examples of racial covenants in sales deeds can be seen in Figure 1. Although the earliest covenants can be traced back to

Figure 1: Racial covenants and segregation over time

$1500.00.

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.

To Have and to Hold the same, together with all hereditaments and appurtenances there
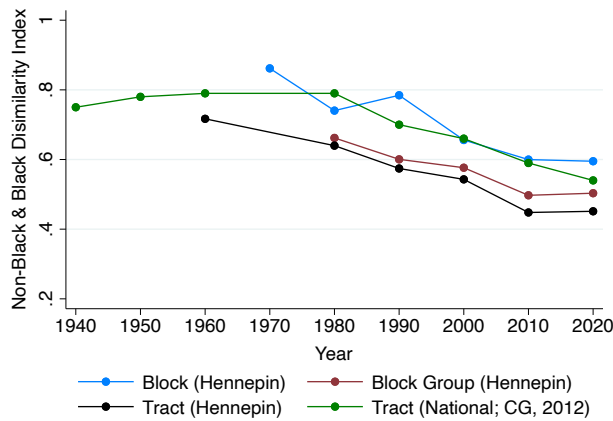
(a) Racial Covenants Example 1

be done thereon which may be or become an annoyance or nuisance to the neighborhood.
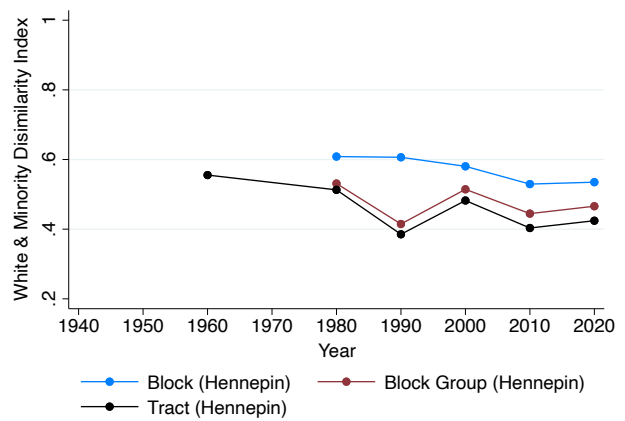
(e) No race or nationality other than the Caucasian Race shall use or occupy any building on any lot, except that this covenant shall not prevent occupancy by domestic servants of a different race or nationality employed by an owner or tenant.

(b) Racial Covenants Example 2

(c) Non-Black & Black Dissimilarity Index

(d) White & Minority Dissimilarity Index

Note: This figure provides for examples of racially-restrictive covenants in sale deeds in Hennepin county. Bottom panels provide national dissimilarity index as well as census block, block group, and tract level dissimilarity indices. CG, 2012 refers to Glaeser and Vigdor (2012).

Figure 2: Expansion of buildings and racial covenants in Hennepin County, 1910-1949
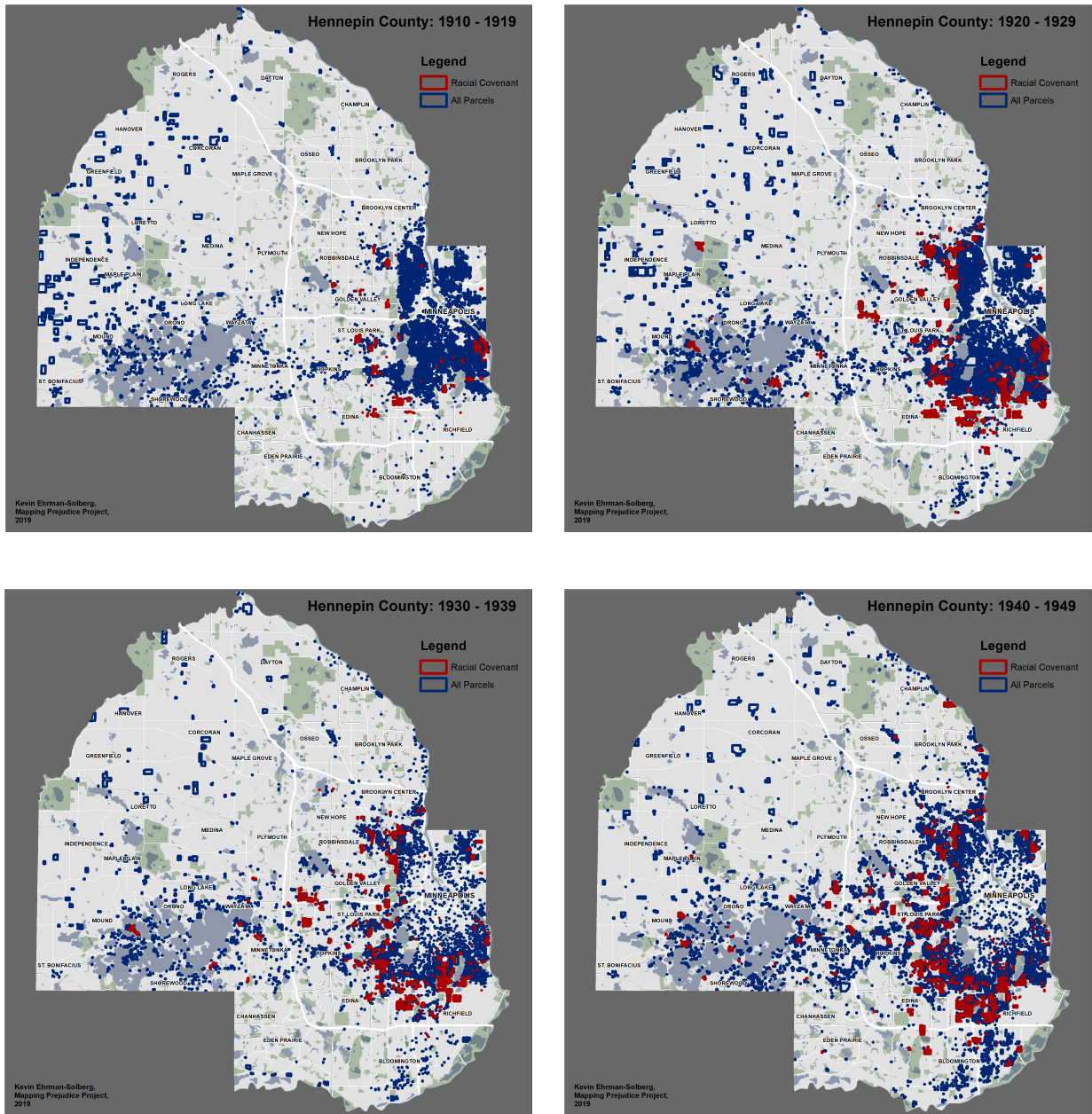
Note: New houses constructed without (in blue) and with racial covenants (in red) in Hennepin county 1910-1949.

the late 19th century (Rose et al., 2016), the first racially-restrictive deeds in Minnesota appeared in 1910.

Like all property covenants, racial covenants were attached to the land. Racial covenants were private instruments with legal protection used by individuals, developers, and neighborhood homeowners associations. The instrument of covenants also provided a form of "insurance" against future demographic changes in the neighborhood (Tiebout, 1956; Schelling, 1971; Card et al., 2008). In addition, covenants impose restrictions on what can be done on a property. Removing covenants increases the option value for the owner, and the value of covenants increases as more of the neighborhood is covenanted. Thus, in many ways, covenants are similar to zoning regulations (Turner et al., 2014; Kulka et al., 2023). The enforcement of racial covenants relied on "injured parties", usually neighbors bringing lawsuits. We document at least 31 such cases across district, state, and national courts (see Appendix Table B1).

Effects of covenants: past and present

Racial covenants have received little quantitative attention, partially due to the scarcity of data. Studying the effects of racial covenants in the past and present is important for several reasons. First, the use of racial covenants intersects with both the scope and scale of segregation in the United States. The wide use of racial covenants in the early 20th century was associated with a widening increase in racial segregation. For example, racial segregation doubled between 1890 and 1950 (Cutler et al., 1999; Logan and Parman, 2017).[1] In addition, during the first half of the 20th century, racial segregation changed from building-level sorting common before the 1910s to neighborhood-level sorting observed later throughout the 20th century (Logan et al., 2015).

Second, racial covenants were added at a crucial time period when cities were fast developing. Approximately 44.5% of the existing housing stock was built between 1910 and 1948. In addition, 94.3% of these racial covenants were added by real estate developers on newly platted land.[2] Since real estate developers platted large swaths of virgin

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[1]Factors such as increases Black migration from the South (Boustan, 2010), international immigration, and urbanization contributed to this trend (Shertzer and Walsh, 2019).

[2]Broadly speaking, there were two types of racial covenants: petition covenants and developer-imposed covenants. Petition covenants were added retroactively to already built houses by homeowners or neigh-

land and decided whether to add racially-restrictive covenants to these lots, covenants effectively determined racial residential choice. Thus, studying the effects of this period is crucial because of the breadth of impact racial covenants had in shaping the historic housing market. Lastly, while Black and non-Black segregation and white and minority segregation as measured by the dissimilarity index at census tract, block group, and block level has fallen since its peak in 1950 (Glaeser and Vigdor, 2012), starting in 1980, the segregation has leveled off at a significant level (Figure 1 Panels C and D). Thus, it is crucial to understand the role of racial covenants in creating a lasting impact on racial segregation of housing in the U.S.

Legal history of racial covenants

Three historical events resulted in the increased use of covenants during the first half of the 20th century. First, the U.S. Supreme Court decision of Buchanan v. Warley (1917) prohibited cities from enacting racial zoning policies, making racial covenants more appealing. Second, the use of racial covenants was also bolstered in 1924 by the National Association of Real Estate Boards (NAREB) when it adopted an amendment in its charter that made the use of covenants as part of its "code of ethics" (Jones-Correa, 2000). Lastly, when the U.S. Supreme Court reaffirmed the legality and enforceability of racial covenants in the 1926 *Corrigan v Buckley* ruling, the use of racial covenants became widespread across much of the United States. Figure 2 plots new construction without covenants in blue and with covenants in red in Hennepin country 1910-1950. As the figure shows, there is a continued geographical spread after 1910, particularly after 1926.

While there were repeated challenges to the *Corrigan* decision, these were all dismissed by lower court levels and reaffirmed that the Supreme Court would not interfere with the right to discriminate in private agreements. However, a tide shifted when in 1948, the Supreme Court, citing the Equal Protection Clause of the 14th Amendment, decided in *Shelley v. Kramer* that racially restrictive covenants were no longer enforceable and their language in property deeds to be void.

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borhood associations. Unlike St. Louis, where the historical data tracks the types of covenants (Gordon, 2023), we estimate the share of petition and developer covenants based on the year of house built and year of covenant execution.

2.2 Data

The novel data on racial covenants in Hennepin County, Minnesota, comes from the Mapping Prejudice Project, providing a complete census of covenanted deeds in the county. The data collection involved at least five individuals examining every sale deed from 1900 to 1960 to confirm the presence or absence of racial covenants. Along with covenants in deeds, the date of covenant execution and the language of racial restrictions were also recorded. We geocode the racial covenants data to present-day parcels in Hennepin County, resulting in 21,973 covenants (for details, see Data Appendix A). The baseline geography of present-day parcels comes from the tax assessor of Hennepin County. From the tax assessor, we also obtained additional data on assessed values for buildings (2019), sales prices (2010-2019), and house characteristics, including lot size, built area, and number of bedrooms and bathrooms. Assessed values are based on yearly home visits and marked-to-market.

Neighborhood characteristics, such as the share of residents and homeowners across race and income over time, were obtained from the U.S. Census. The IPUMS database provided 1960 tract and 1980-2020 census block-level data (Ruggles et al., 2015). We digitized and geocoded the geography for the 1940 enumeration districts and the 1970 census block, which were unavailable through IPUMS.

We also collect data on public amenities created during or after 1948, including proximity to highways, lakes, zoning regulations, and school quality. The data on highways and lakes comes from Federal Highway Administration and the Minnesota geospatial commons, respectively. Webster and Corey (2021) provide data on zoning regulations. Elementary school quality data is collected through the Minnesota Department of Education. Lastly, we also use the elementary school attendance boundaries available through School Attendance Boundary Survey conducted by National Center for Education Statistics. This allows us to match each house in our sample to their assigned elementary school to accurately study the role of school quality on racial sorting and house prices.

Our empirical strategy restricts analysis to neighborhoods and houses built between 1940 to 1960. Table 1 provides summary statistics for around 900 census blocks (Panel

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Table 1: Summary Statistics

Panel A: Neighborhood Statistics

			Home Ownership & Race			Home Ownership & Race		
Year		N	% White	% Black	% Others	% White	% Black	%Others
			Mean (s.d.)	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)
1940	County Pop.	570,040	99.1	0.8	0.1			
1980	County Pop.	944,500	94.0	3.6	2.4			
	Sample Blocks	905	97.7	0.8	1.5	72.2	0.2	0
			(5.5)	(4.2)	(3.4)	(39.6)	(3.3)	(0)
1990	County Pop.	1,035,100	88.6	5.8	5.6			
	Sample Blocks	916	97.9	1.1	1.0	89.2	0.9	1.0
			(5.2)	(4.3)	(2.7)	(16.1)	(3.6)	(2.5)
2000	County Pop.	1,117,800	79.0	9.2	11.8			
	Sample Blocks	889	92.4	2.7	3.4	88.7	1.5	2.1
			(10.0)	(6.1)	(5.4)	(16.5)	(4.0)	(3.7)
2010	County Pop.	1,154,300	71.8	11.8	16.4			
	Sample Blocks	895	89.6	4.1	5.0	83.4	2.0	3.5
			(11.8)	(7.4)	(6.9)	(17.9)	(4.4)	(5.2)
2020	County Pop.	1,281,300	68.0	13.7	18.3			
	Sample Blocks	863	77.4	5.1	8.7			
			(16.2)	(8.4)	(9.5)			

Panel B: Parcel Statistics

	Control Group		Treatment Group	
	Mean	(s.d.)	Mean	(s.d.)
Sale Price ($)	298,161	(262,892)	304,533	(245,439)
Assessed ($)	305,463	(210,220)	319,460	(253,312)
Lot Size (sqft)	11,277	(9,988)	9,166	(7,033)
Built Area (sqft)	1,279	(651.3)	1,323	(597.7)
Bedrooms	3.131	(0.873)	3.097	(0.851)
Bathroom	1.57	(0.702)	1.625	(0.752)
N	7,205		5,508	

Note: This table provides Hennepin county population 1940-2020. In addition, it provides the mean and standard deviation (s.d.) for the census blocks and parcels in the the baseline sample over time.

A) and 12,000 parcels (Panel B) in our baseline sample. As can be seen from the table, between 1940 and 2020, the Hennepin County population has more than doubled. In addition, share of both Black residents and homeowners and other racial minority residents and homeowners has increased significantly between 1940 and 2020.

2.3 Covenants in transition neighborhoods

Figure 3: Covenants in transition neighborhoods
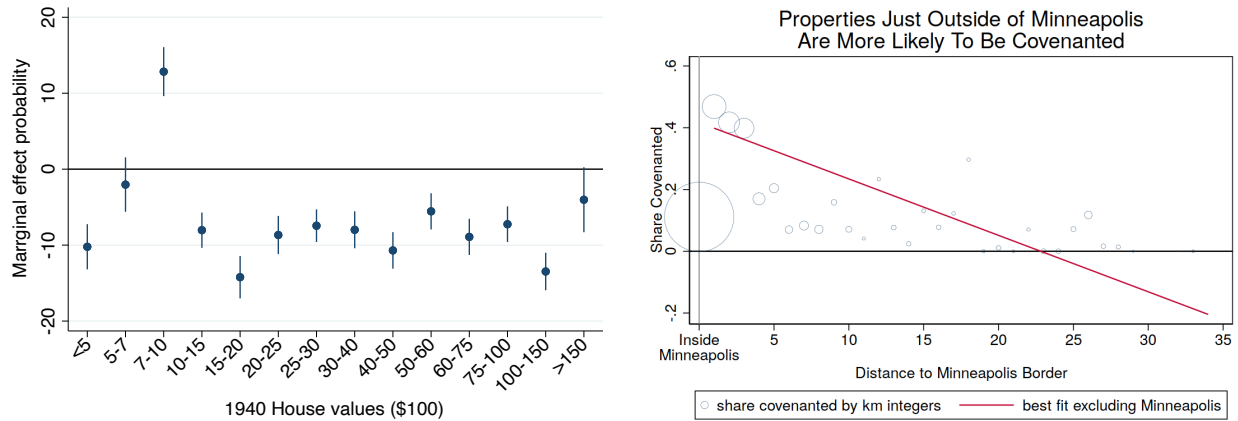
(a) Covenants & house value

(b) Covenants & Minneapolis boundary

Note: Panel A plots the marginal effects from logit specification on probability to write a covenant after 1940. Independent variable is share of houses with values between ranges in 1940 Census. Panel B plots the share of covenants outside Minneapolis against the distance to Minneapolis boundary.

Racial covenants were primarily used in lower-middle-class or middle-class neighborhoods or neighborhoods most likely to transition. Figure 3 Panel A plots the marginal effects from a logit specification on the probability of adding covenant after 1940 against the 1940 self-reported house values. The figure shows that new covenants were added in areas with historically lower-middle home values. Figure 3 Panel B plots the share of racial covenants (1910-1955) against the distance to the Minneapolis city boundary. The figure shows that there is a large mass of covenants added in the suburban cities within 5 kilometers of the city boundary. Thus, the instrument of covenants was used in the middle of the housing segment and in neighborhoods most likely to transition. Preliminary evidence from covenants in St. Louis suggests the use of covenants as an

instrument to prevent neighborhood tipping (Gordon, 2023).

In fact, there is anecdotal evidence that covenants were added in areas with low natural amenities (Kaul (2019)). The price mechanism was enough to keep people of color out at high amenity locations, such as houses near lakes. Transition neighborhoods in less coveted and cheaper locations needed covenants to keep people of color out. Developers widely advertised covenants[3] used them as a method to increase their developments' desirability and the value of the houses. Thus, the location of covenants and the developer's actions imply that our empirical strategy must address the inherent endogeneity within the act of adding racial covenants.

3. Empirical strategy

Figure 4: Time to build and baseline sample

(a) Year built & covenant execution year   (b) Houses built 1940-1960
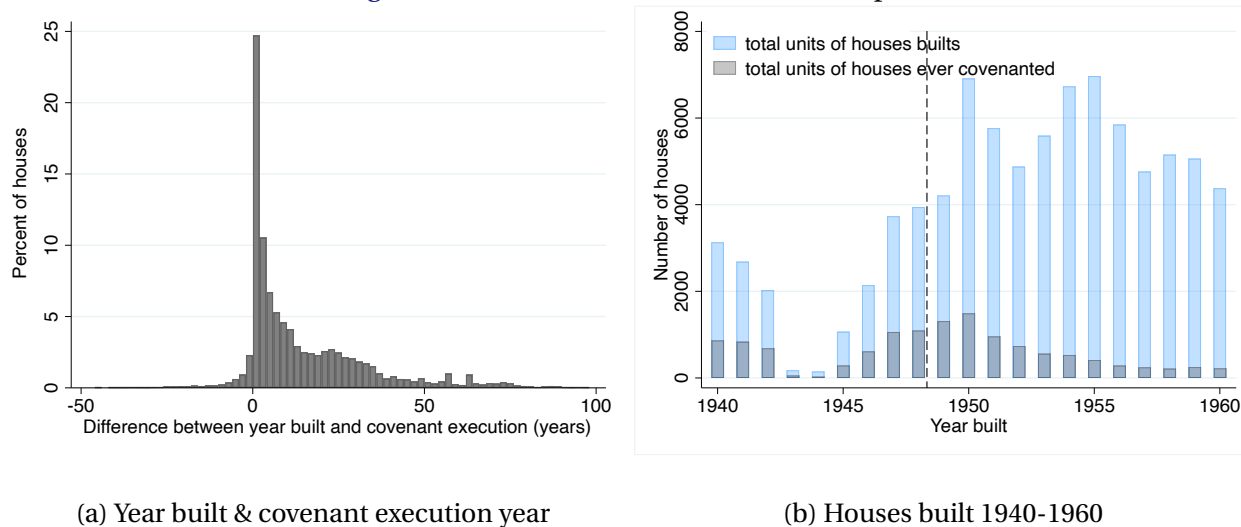
Note: Panel A plots the difference between year built of the house and the year of covenant execution. Panel B plots the total housing units built in blue and total total housing units built with a covenant in grey from 1940 to 1960. The dotted line in 1948 is the year of U.S. Supreme Court ruling that made covenants legally unenforceable.

To understand the persistent effects of housing discrimination, our goal is to identify the causal effects of the historic racially-restrictive covenants on several socioeconomic outcomes over time. Our analysis and outcomes of interest are divided into two levels. First, at the Census block or neighborhood level, we look at the racial composition of

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[3]See Appendix Figure B.2 for an example of such advertisement.

residents and racial homeownership rate. At the individual lot level, we look at house sale prices, assessed values, and house characteristics. The key variable of interest is the amount of covenant shock a neighborhood or parcel received in the past.

To causally identify the effects of racially-restrictive covenants on socioeconomic outcomes over time, we need to address the endogeneity concerns. As discussed in the previous section, neighborhoods or lots with worse unobserved quality were likely more likely to be covenanted in the past. In addition, the choice of covenanting may be correlated with the unobserved developer's racial or profit-driven intent.

3.1 Quasi-experimental design

We exploit the 1948 U.S. Supreme Court ruling that made racially restrictive covenants unenforceable to address endogeneity concerns and compare houses neighborhoods built before and after 1948. The largest bandwidth we consider is from 1940 to 1960. In particular, we restrict our analysis to lots and neighborhoods that had racially-restrictive covenants written in their deeds. This design allows us to construct a treatment and control group that both chose to use the instrument of racial covenants and avoid any selection into the choice of covenants.

Construction of the treatment and control group relies on the time to build and other delays in building houses, such that the covenant execution year is not always the same as the year built of the house. Panel A of Figure 4 plots the difference between the year built of the house and the year of covenant execution for houses built between 1940 and 1960. While most houses are built within five years of covenant execution, many are built much later.[4] If the house was built in 1948 or before, then covenanted houses and neighborhoods have enforceable covenants and zero probability of racially and ethically diverse residents. This is our treatment group. If the year built is after 1948, then due to the Supreme Court ruling, there is no enforceable covenant despite racial covenants in sales deeds positive probability of racially diverse residents. This is our control group.

This quasi-experimental design assumes that residents move in soon after purchasing a house. In addition, we assume there is no delay between the build year of the

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[4]Usually, the covenant execution year is also the year the developer platted the lot. Also, note that there is a small number of petition covenants such that the covenant is executed after the house is built, indicated by negative values in Figure 4 Panel A.

house and the sale year. To test this assumption, we look at historical county records for a random sample of 2,839 houses built 1940-1960. We find that the mean difference between the year of building and the sale year is 0.11 years. Panel B of Figure 4 plots the total number of houses built between 1940 and 1960 in blue. In grey, it plots our baseline sample–the number of houses built during this time that had ever had covenants. If there are moving costs, then houses and neighborhoods are locked into their demographics for a certain period of time (Heblich et al., 2021). In particular, the random sample of 2,839 houses built 1940-1960 indicates that the mean resale is after 12.2 years. Thus, both the treatment and control neighborhoods are locked in their demographic makeup for about a decade.

Neighborhood level model

We define a neighborhood as census blocks i over time $t = 1980 - 2020$. The primary dependent variables we are interested in are the percentage of residents and homeowners by race ($\%Y_{it}$). The neighborhood-level model is given by:

$$\%Y_{it} = \alpha_0 + \alpha_1(\%\text{built} \leq 1948)_{it} + \alpha_2 X_{it} + \theta\eta_{kt} + \epsilon_{it} \tag{1}$$
$$if \ \ \%\text{cov houses}_{it} \geq \underline{c}_{it}$$

The main independent variable or the covenant shock is the percent of the block built (($\%\text{built} \leq 1948)_{it}$) between 1940 and 1948 (or a smaller bandwidth).To account for the quasi-experimental design, we make two restrictions on the neighborhoods we consider in our sample. First, we restrict to census blocks where at least 25% houses were built between 1940 and 1960. We do this so that we only consider neighborhoods that were majority built around the 1948 ruling. For robustness, we also restrict to neighborhoods where at least 50 or 75% of houses were built 1940-60. Second, we restrict our analysis to blocks that are at least 50, 75, or 90 % covenanted ($\%\text{cov houses}_{it} \geq 50, 75, 90$). This restriction allows us to control for the selection into the choice of covenanting neighborhoods.

We study the effect of the block-level covenant shock on racial resident and homeowner sorting within census tracts k. The tract-year level fixed effect ($\theta\eta_{kt}$) allows for

a comparison of neighborhoods in close proximity to each other, controlling for differences in unobserved neighborhood quality. In addition, in the baseline model, we control for census block population X_{it}. In additional models, we also control for neighborhood income measured at the block group or tract level.

House level model

At the individual house j-level, we use the following model for time $t = 2010 - 2019$:

$$\log Y_{ijt} = \alpha_0 + \alpha_1 \mathbb{1}\{\text{built48}_j\} + \sum_m \alpha_m \mathbb{1}(\%\text{built} \leq 1948)_{it} + \alpha_2 X_{i,2020} \tag{2}$$

$$+ \alpha_3 X_j + \theta \eta_{k,2020} + \delta_t + \epsilon_{ijt} \quad if \quad \%\text{cov houses}_{it} \geq \underline{c}_{it}$$

The main dependent variable is the log of assessed values (2019) or sale price [2010-2019] ($\log Y_{ijt}$). Both of these measures of home values have benefits and limitations. While sales prices reflect the market value, not all houses are for sale from 2010-2019. In contrast, while assessed values are available for all houses in the sample, these values can be biased (Berry, 2021; Avenancio-León and Howard, 2022). We present the results for both these measures of house value.

$\mathbb{1}\{\text{built48}_j\}$ is the dummy variable if the house built was built during or before 1948. It measures the effect of receiving an own covenant shock on own house value. We also construct dummy variables for the neighborhood (census block) level covenant shock m, where the covenant shock ranges from 0 and 1-20 to 81-100 percent. Again, we restrict our analysis to blocks that were majority covenanted such that $\underline{c}_{it} = 50, 75, 90$ percent. As in neighborhood-level models, we compare the house values within census tracts. $\theta_{k,2020}$ are the 2020 census tract fixed effects and δ_t are sale-year fixed effects. For robustness, we also control for a linear up to a fourth-degree polynomial distance to the 1948 boundary trend.

In the baseline model, we control for the 2020 block population and share of residents under 19 ($X_{i,2020}$). In an additional model, we also control for housing unit characteristics (X_j), including the log of lot size, total built area, and the number of bedrooms and bathrooms. In a third model, we control for public amenities affected by racial covenants and the housing unit characteristics to estimate the residual difference in house values arising from the quality of housing and homophily bias.

3.2 Conditions for validity of quasi-experimental design

Table 2: Amenities across treatment and control parcels and neighborhoods

	Panel A: Parcel Level			Panel B: Block Level		
		N	R^2		N	R^2
Distance to CBD	38.773	12683	0.997	-0.542	863	0.892
	(32.653)			(2.317)		
Development plat year	0.096	12683	0.601	0.021	863	0.581
	(0.644)			(0.025)		
Altitude	0.969	12678	0.915	0.017	863	0.932
	(0.601)			(0.024)		
Slope	-0.097	12683	0.461	0.001	863	0.602
	(0.098)			(0.002)		
Flood frequency	0.003	12683	0.092	0.00005	863	0.321
	(0.002)			(0.0001)		
Soil drainage	0.138***	12683	0.646	0.002	863	0.733
	(0.032)			(0.001)		
1899 wetlands	-1.258	12683	0.174	-0.021	863	0.234
	(0.826)			(0.025)		

Note: This table plots coefficients from Equations 1 and 2 across treatment and control parcels and census blocks where dependent variables are various location amenities. CBD stands for central business district. Clustered standard errors at census tract levels in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In our quasi-experimental approach, the identification comes from the change in the enforceability of covenants for the treatment and control group such that there is no corresponding difference in the observed and unobservable quality of parcels or neighborhoods built immediately before and after 1948. We begin by testing this assumption, i.e., whether there are observed and unobserved differences in parcels and neighborhoods in our treatment and control group.

Table 2 plots the coefficients from Equations 2 and 1, where the dependent variables are location quality measures for parcels (Panel A) and neighborhoods (Panel B).

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Figure 5: Percent minority residents (1960)
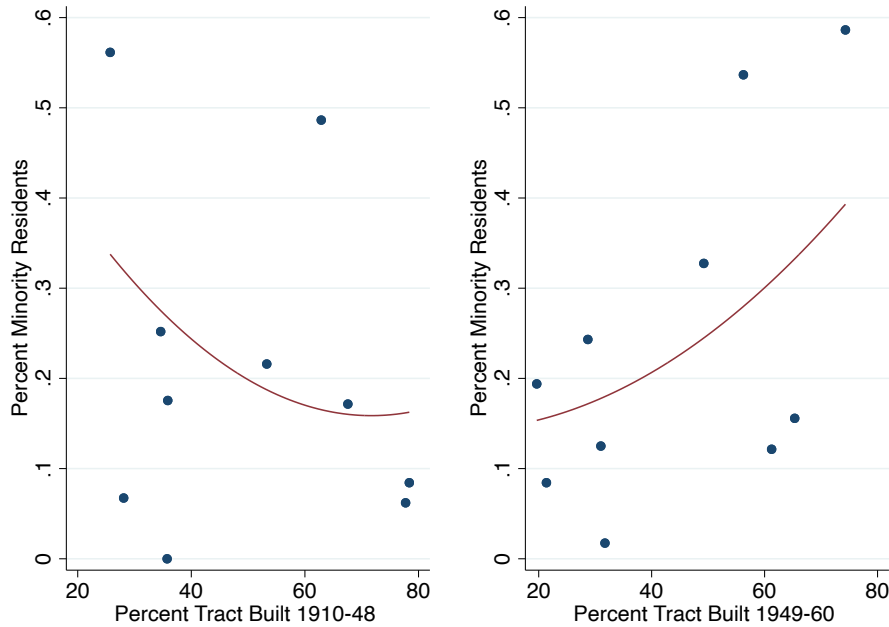
Note: This figure plots the percentage of racial minority residents against the percentage of tract built on or before 1948 or 1948 to 1960 for census tracts with at least 20% of the tract covenanted.

As seen from the table, within a census tract, parcels and neighborhoods in the treatment and control group have similar distances to the central business district (CBD). In addition, the development platting year, i.e., the year the piece of land was first subdivided into lots as well as altitude, slope, and flood frequency, are statistically similar for the treatment and control groups. However, we find that soil drainage for parcels in the treatment group is better than in the control group. This effect is only present at the parcel level and not at the neighborhood (census block) level. While one might be concerned regarding the differences in soil drainage quality, our concerns are lessened when we find that there are no statistical differences in the presence of wetlands in the treatment and control group as measured submerged areas and bogs in a map surveyed in 1899, well before any development took place in the treated and control group.

In addition, there might also be a concern that the U.S. Supreme Court ruling was anticipated, and hence houses and neighborhoods built before and after 1948 are systematically different. First, note that there had been at least 26 district, state, and national

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level court cases challenging the legality of racial covenants through the early to mid-20th century, none of which overturned covenants until Shelley v Kramer (1948) ruling (see Appendix Table B1). Contemporary observers expected the 1948 ruling to move in a similar direction as the 1926 Corrigan v Buckley ruling, which upheld the legality of racially restrictive covenants (Jones-Correa, 2000; Rothstein, 2017). Thus, because the 1948 ruling that made covenants unenforceable was not anticipated, the cutoff point is as good as randomly assigned.

Another threat to identification is whether the 1948 ruling is correlated with the probability of having an enforceable covenant and whether there are racially and ethically mixed neighborhoods post-1948. We test this by comparing the percentage of racial minority residents in 1960 against the percentage of tracts built on or before 1948 or from 1949 to 1960. For this analysis, we restrict our attention to census tracts with at least 20% of the tract covenanted.[5] As seen in Figure 5, the higher the percentage of tracts built between 1910 and 1948, the lower the tract-level share of minority residents. In contrast, the higher the percentage of tracts built between 1949 and 1960 (post *Shelly*), the higher the tract-level share of minority residents. While it would have been ideal to study the demographic makeup of neighborhoods at a finer level in 1960, the census did not collect the 1960 block-level statistics data, and the 1960 individual count data is not yet available. Nevertheless, we do see evidence of racial mixing in neighborhoods built post *Shelly*.
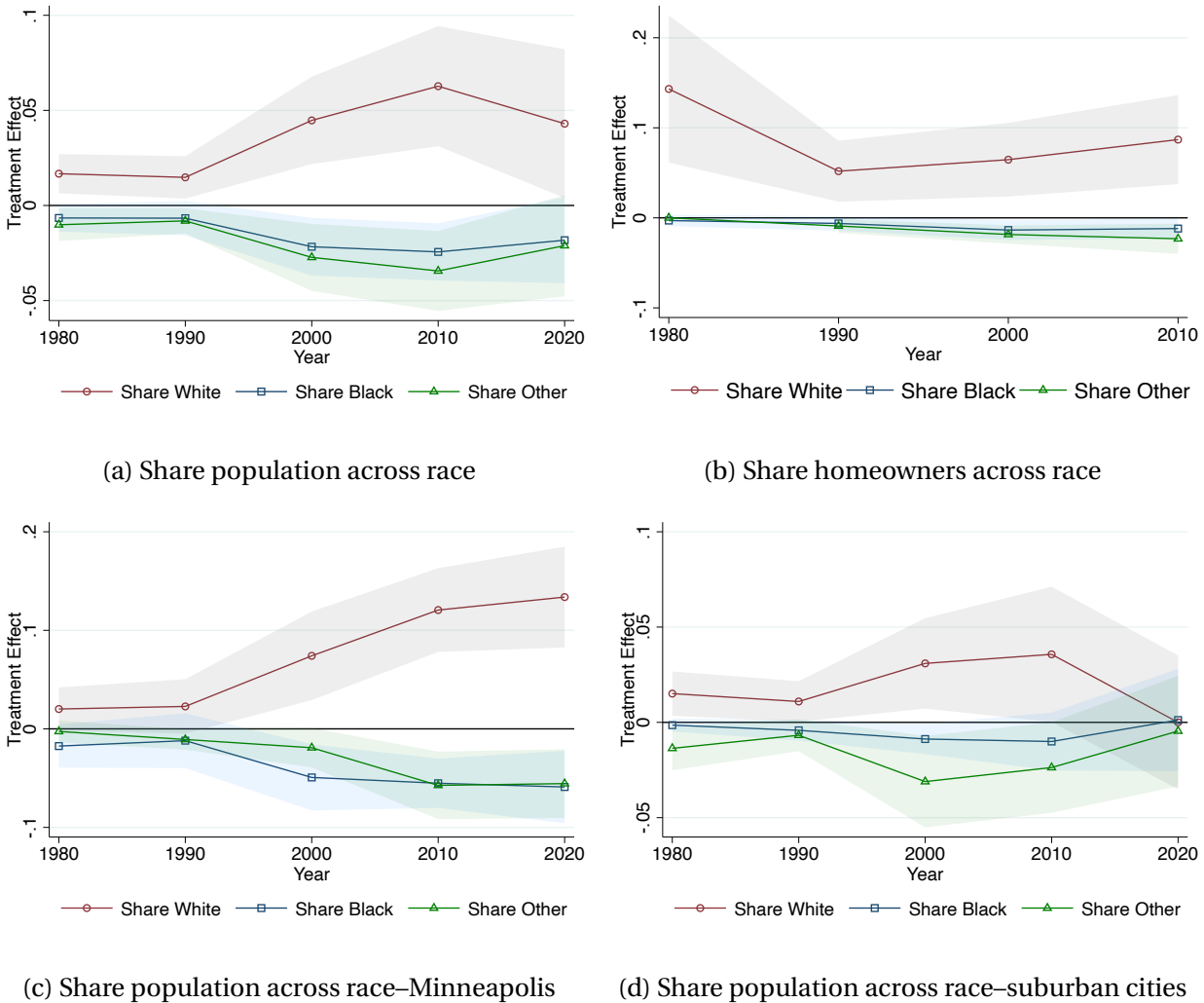
Lastly, there might be a concern that racial covenants were more of a symbolic act rather than an active way to design and restrict neighborhoods, especially since there were only about 1% of non-white residents in Hennepin County in 1940 (Table 1). This would create an identification issue for our quasi-experimental design because then, by design, the 1940 ruling would have had little effect. However, Almagro and Sood (2023) find that the discrimination associated with covenants extended well beyond racial minorities in Hennepin County. They find that all first and second-generation immigrant groups except those from north west Europe, which comprised 17% of the population

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[5]We are more liberal using a lower cutoff of 20% because tracts are significantly bigger than covenants, which were instruments used at lot or development level.

in 1940, were also discriminated against. While it is difficult to study ethnic discrimination after 1950 as the census stopped collecting such data, this fact disputes claims that racial covenants were a toothless instrument in a primarily homogenous metro area.

4. Covenants and Racial Sorting

Figure 6: Racial sorting and homeownership over time

(a) Share population across race

(b) Share homeowners across race

(c) Share population across race–Minneapolis

(d) Share population across race–suburban cities

Note: This figure plots the treatment effect from Equation 1. Dependent variables are percent population across race, percent homeowners across race, percent population across race in Minneapolis, and percent population across race in suburban cities. Standard errors are clustered at census tract level at 95%.

This section presents results on the time-persistent effect of the covenants on the racial resident and homeowner sorting in and around Minneapolis. Figure 7 Panels A and B plot the coefficients from Equation 1 for the percent white, Black, and other racial

minority residents and homeowners from 1980 to 2020. We find that a one standard deviation increase (34%) in the covenant shock in the past results in an increase of white residents by 0.5-2.1% in census blocks, a fall in Black residents by 0.2-0.8%, and 0.3-1.2% fall in other racial minority residents. This explains around 5-15% of the observed standard deviation in percent Black and other minority residents across census blocks from 1980 to 2020. In addition, a one standard deviation increase in historic covenant shock results in a 1.8-4.7% increase in white homeowners within census blocks, a 0.2-0.5% fall in Black homeowners, and a 0.3-0.8% fall in other racial minority homeowners. This explains around 3-6% of the observed variation in racial sorting in homeownership across neighborhoods from 1980 to 2020.

Thus, covenants from the past can be causally linked to racial sorting in residents and homeowners over five decades in an economically meaningful way. In addition, the results presented from Equation 1 include census tract fixed effects to control for unobserved location quality and the fact that covenants were added in transition neighborhoods within the larger metro area. Thus, these results imply that white residents and homeowners sort into historically covenanted smaller neighborhoods (census blocks) within broader neighborhoods (census tracts).

These baseline results are restricted to census blocks that have at least 50% covenanted lots and have at least 25% of houses built between 1940 and 1960. However, the key results are robust to alternative restrictions on census blocks, including blocks where at least 50% of lots were built between 1940 and 1960 and 75-90% were covenanted. In addition, these racial sorting results are not driven by income effects. Controlling for income either at the census tract level (1980-2000) or block group level (2010-2020) has no substantial effect on the racial sorting effects (see Appendix Figure B.3).

As seen from Figure 7 Panels A and B, the racial sorting treatment effects increase over time. However, these results are primarily driven by the city of Minneapolis and less so than the suburban municipalities, especially in the 21st century (Figure 7 Panels C and D). Thus, while the historic covenants shape the racial sorting in the late 20th and early 21st century across all types of municipalities, the persistent effects are increasing in the city center. These results are in line with other city-center gentrification studies

that find similar effects either due to the location of new housing stock and metro-wide demand shocks (Brueckner and Rosenthal, 2009; Guerrieri et al., 2013) or due to sorting by college-educated white residents Baum-Snow and Hartley (2020); Couture and Handbury (2020).

It is worth considering who is affected by historic racial covenants' persistent effects on racial sorting. First, it is the descendants of the discriminated racial minorities or immigrant groups residing in Hennepin County in the first half of the 20th century. However, as can be seen from Table 1, as the minority county population grew from 1% in 1940 to 32% in 2020, new Black and other racial minority migrants into the metro area post-1948 also sorted into non-covenanted control neighborhoods. Thus, historic covenants continue to affect a wide array of minority residents into the 21st century. Considering that segregation in the 21st century limits upward mobility (Chetty and Hendren, 2018a,b; Chyn and Katz, 2021), hinders academic achievement (Chyn et al., 2022), and increases commuting time (Fu et al., 2023), it is important to understand the mechanisms behind persistent sorting effects of historic racial covenants.

5. Mechanisms for Persistent Effects

In this section, we study the mechanisms behind the persistent sorting effects of racial covenants from the past. There are three potential channels through which racial covenants can have persistent effects on observed segregation. First, the observed racial sorting effects today can be driven by homophily bias (Waldfogel, 1999; Bayer et al., 2022).[6] Racial covenants created initial conditions that kept residents of similar racial demographics together. Given the mean resale of houses in the treatment and control group was after 12.2 years, and if there is homophily bias, then there will be sorting along racial lines among residents, even after covenants became unenforceable in 1948.

Second, the persistent sorting effects could be driven through the channel of public amenities like school quality or distance to the nearest highway added during or after covenants were active. If the public amenities were differentially added to the treatment and control groups, they could anchor neighborhoods to white residents and

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[6]Cutler et al. (1999) refer to homophily bias as ports of entry or decentralized racism.

Table 3: Covenants and public amenities

Dependent var.	Dist. to Highway (kilometers)	Min. Lot Size (1000 sqft)	Dist. to Lakes (kilometers)	Math Proficiency
% Built ≤ 1948	118.3*	169.5*	336.0	000.9
	(58.7)	(76.6)	(601.7)	(0.005)
Location FE, SE	Tract'20	Tract'20	Tract'20	Tract'20
Controls	X	X	X	X
R^2	0.96	0.83	0.84	0.98
Observations	1,140	1,133	1,140	1,140

Note: This table the treatment effect from Equation 1. Dependent variables are distance to highways, minimum lot size, distance to lakes, and assigned elementary school quality (student proficiency in math). Minimum lot size is measured in 1000 square feet. Standard errors are clustered at census tract level at 95%. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

homeowners. This is like natural location amenities that anchor neighborhoods to high-income residents over time (Lee and Lin, 2018). Third, if there is a difference in housing quality and homeowner investment over time in the treatment and control groups, then these differences can also drive the persistent racial sorting effects. Studying the role of the three mechanisms helps with policy implications. If the persistent racial sorting effects are driven by differences in public amenities and housing quality, then place-based policies can mitigate the differential effects, albeit potential gentrification. However, if the persistent racial sorting effects are driven by homophily bias, place-based policies will be less effective.

5.1 Public amenities and sorting

A mechanism that may contribute to the observed racial sorting over time is differences in public amenities. Once neighborhoods experience different covenant shocks, disparities in infrastructure, recreational facilities, and public works investments could emerge between treated and control areas. It is possible that treated covenanted residents, which by definition were majority white, were also occupied by more politically powerful residents influencing local public policy in their favor. In this analysis, we first

Table 4: Racial sorting and public amenities

Dependent var.	% White'20	% Black'20	% Other'20
% Built $\leq$ 1948	0.044*	-0.020*	-0.026*
	(0.019)	(0.010)	(0.038)
Dist. Highway (km)	-2.84	0.402	0.510
	(3.593)	(0.009)	(1.185)
Min. Lot Size (1000 sqft)	-0.018	-0.059	-0.057
	(0.175)	(0.0036)	(0.062)
Controls	X	X	X
Location FE, SE	Tract'20	Tract'20	Tract'20
R^2	0.43	0.34	0.33
Observations	1132	1132	1132

Note: This table presents results from Equation 1, where the dependent variable is percent white, Black, and other racial minorities in 2020. Distance to highways is measured in kilometers and minimum lot size is measured in 1000 square feet. Standard errors are clustered at census tract level at 95%. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

test if there is any evidence of disparities across public amenities. Then, we examine the role of public amenities in shaping persistent sorting patterns.

First, we examine the role of highways. We focus on the interstate highways built during the 1950s, immediately after covenants were outlawed (Baum-Snow, 2007). Literature in history and sociology have provided anecdotal evidence of highways being built through or near minority neighborhoods (Ware, 2021; Connerly, 2002). We test if highways built in Hennepin County were placed farther away from treated neighborhoods that received a covenant shock. Second, we test whether zoning regulation, particularly minimum lot size, varies across the treatment and control neighborhoods. We study this using the minimum lot size observed in 2021 across Hennepin County Webster and Corey (2021). While we're using present-day zoning data, 1953 and 1968 zoning maps from the city of Minneapolis suggest that present-day zoning maps are highly correlated with historic zoning maps created soon after the 1948 ruling.

Third, we explore the role of the amenity of lakes and associated greenways. Walker et al. (2022) document that the shapes of lakes were changed by developers concurrently as they were adding covenants. In addition to changing the shapes of lakes, greenways were created near these lakes. Fourth, we investigate the role of elementary school quality in driving persistent sorting effects. For this part of the analysis, we assume that the 2015 school quality and elementary school attendance zone boundaries are correlated with historical school quality and boundaries.

To quantify the effects of these public amenities, Table 3 presents the treatment effects from Equation 1. The dependent variables include Euclidean distance to highways, minimum lot size, Euclidean distance to lakes, and assigned elementary school quality (student proficiency in math). Our findings indicate that a one standard deviation increase in covenant shock (34%) results in highways being 44.3 meters further away and a 55.8-square-foot increase in the minimum lot size. We observe no statistically significant effect for lakes or school quality, even when using alternative measures such as proficiency in reading, growth in proficiency, student-teacher ratio, and the share of students on free and reduced lunch.

After establishing differences in distance to highways and zoning across the treatment and control groups, we estimate Equation 1 while controlling for relevant public amenities. Table 4 shows that controlling for distance to highways and minimum lot size has little effect on the racial sorting patterns observed in 2020. Note that this approach assumes that highways and zoning are influenced through the channel of racial covenants and not by some other endogenous process. If there are alternative channels, the coefficients on highways and zoning may be biased.

5.2 Homophily bias, housing quality, and sorting

The two other mechanisms that may contribute to persistent sorting effects are homophily bias and differences in housing quality. Houses in the treatment and control groups may have housing quality differences due to changes in building quality implemented after the 1948 ruling. For instance, developers may have used lower-quality windows or insulation in treated houses to compensate for the loss of the "all-white" amenity, which reduced their sale prices. Additionally, it is possible that treated houses

Figure 7: Covenants and house prices
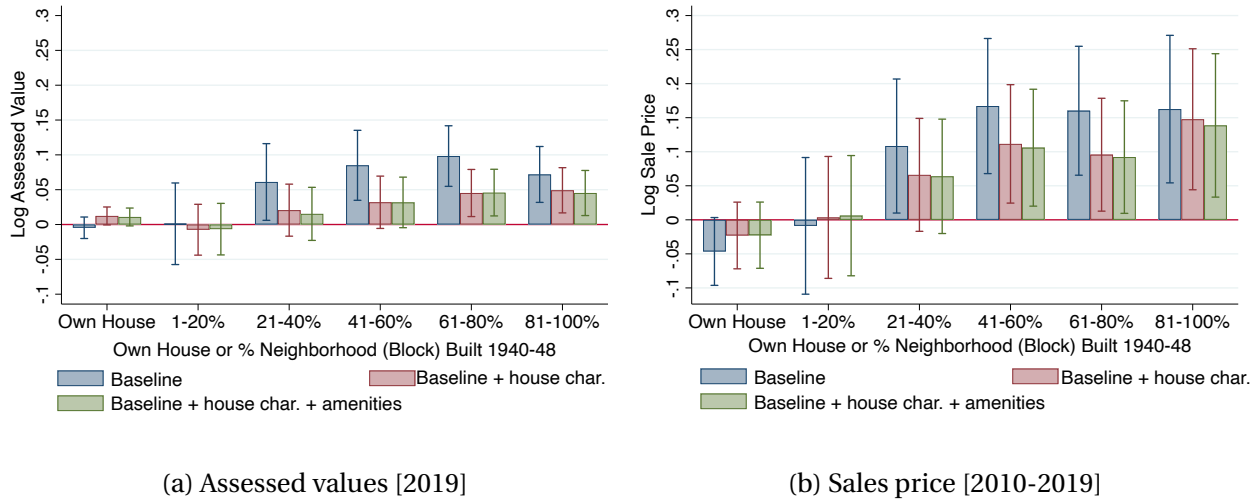
(a) Assessed values [2019] (b) Sales price [2010-2019]

Note: This figure presents the treatment effect from Equation 2 where dependent variables are log of assessed value and log of sales price. Standard errors are clustered at census block level at 95%.

received different levels of home investment from their owners compared to the control group.

To understand the role of the homophily bias and housing quality in driving persistent effects, we study a model of hedonic house prices. After controlling for housing unit characteristics and relevant public amenities, the residual house price differential would arise from either the homophily bias or differences in unobserved housing quality. If different racial groups make differential residential location choices based on house prices, it may explain the persistence in house prices and sorting effects. Figure 7 displays the treatment effects from Equation 2. The figure illustrates the treatment effects of the own covenant shock effect (α_1) and the external lot effect α_s measured by the categorical parameter which represents the share of the neighborhood (census block) that received the covenant shock (ranging from 0% to 100%). Panel A presents the treatment effects for the log of assessed house valuations in 2019, while Panel B shows the treatment effects for the log of sale prices from 2010 to 2019.

The baseline model (in blue) indicates that there is no own lot effect of receiving a covenant shock on assessed or sales value. This is not surprising since the value of covenants only exists if neighboring houses also have racial covenants. There is also lit-

tle effect of the covenant shock on home values when only 1-20% of the neighborhood received the covenant shock. However, baseline house values are higher in neighborhoods where 21-100% of the neighborhood received the covenant shock. The baseline assessed value difference between treated and control houses ranges from 6% to 10%, while the baseline sale price difference ranges from 10.5% to 16%. It is not surprising that the assessed value differences are lower than the sale price differences, as previous literature finds that the assessed value of Black-owned houses, more likely to be in the control group, is higher than that of white-owned houses, more likely to be in the treatment group (Avenancio-León and Howard, 2022). Thus, the assessed value treatment effects can be considered a conservative estimate of the 21st-century house price differential for neighborhoods that experienced a higher covenant shock in the past.

An additional model controls for the observed housing unit characteristics and reveals that the assessed value differential drops by more than half while the sales price differential drops by about a third. This indicates significant differences in housing characteristics between the treated and control houses. The final model (in green) controls for the relevant public amenities, such as Euclidean distance to the nearest highway, parcel minimum lot size, and elementary school fixed effects for the school assigned to each parcel. Although school quality was not statistically different between treated and control neighborhoods, we include school fixed effects in this model because school quality has been shown to impact house prices significantly (Black, 1999). However, controlling for relevant public amenities results in little additional reduction in the difference in house values between the treated and control groups. Similar to the previous section, these results suggest that differences in public amenities may not be the driving factor behind the house price differential or the persistent sorting effects. Therefore, the differential in house prices can be mostly attributed to the homophily bias or differences in unobserved housing quality. The quasi-experimental design cannot disentangle the relative importance of these two effects.

The models presented here provide results for the largest bandwidth in the analysis (1940-1960) and restrict to neighborhoods where at least 25% of houses were built during this period, of which 50% are covenanted. Appendix Figure B.4 demonstrates

the robustness of these results to smaller bandwidths, different neighborhood restrictions, and controlling for distance to the 1948 boundary using a polynomial from linear to fourth degree. Additionally, it is important to note that the results compare houses within census tracts. Without census tract fixed effects, we find opposite effects (see Appendix Figure B.5). This further aligns with the finding that covenants are located in transition areas and low-amenity regions within the metropolitan area.

Lastly, in addition to studying the causal effects of covenants on present-day house values to understand further the role of homophily bias and housing quality in driving persistent effects, another reason to measure causal differences in house prices is that we have established the covenants affect racial residential location choices from 1980 to 2020. If there are current house price differences across treated covenanted and control neighborhoods, it implies difficulties in closing the racial housing wealth gap moving forward (home equity constitutes 28% of wealth for 99% of households).

6. Conclusion

This paper documents the long-run causal and persistent effects of racially-restrictive covenants on racial sorting, racial homeownership differentials, and house prices. Our quasi-experimental design compares differences between treated covenanted neighborhoods "stuck" with all white residents for a longer time due to initial covenant shock and the control covenanted neighborhoods that did not benefit from the covenant shock. We find that about 5-15% of the observed neighborhood racial residential sorting and 3-6% of racial homeownership sorting from 1980 to 2020 can be causally linked to racial covenants of the past. In addition, racial sorting effects are increasing over time and affect not just the descendants of historically discriminated groups but also the new minority migrants into the metro area.

While we find differences in public amenities, particularly in the distance to highways and the restrictiveness of zoning regulation, between the treated and control neighborhoods, these differences do not drive the observed persistent sorting effects. This limits the scope of place-based policies. Instead, homophily bias and differences in housing quality create a 2.8-13.8% difference in present-day house values between the

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treated and control neighborhoods, likely driving the observed persistent racial sorting effects. Lastly, a limitation of this analysis is that by restricting our quasi-experimental design to houses and neighborhoods built from 1940 to 1960, we estimate locally average treatment effects. These results may not extend to neighborhoods built from 1910 to 1939.

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Long Shadow of Housing Discrimination: Evidence from Racial Covenants

by Aradhya Sood and Kevin Ehrman-Solberg

ONLINE APPENDIX

A. Data Appendix

Baseline Data

Our baseline data is 2019 Hennepin county GIS parcel data. There are 435,765 parcels in total, out of which 418,953 are residential parcels. Each parcel is located by a geographical polygon. 315,414 out of 418,953 residential parcels have elaborate data on building characteristics, last sale date and sale price, and assessed market value for tax purpose.

Covenants Data

Another crucial dataset is the newly constructed dataset of historic property deeds from 1910-1955 with information on racially-restrictive covenants for all lots in Hennepin County, Minnesota. There are 24,509 covenants in total. Each covenants is located by a geographical centroid. We match each covenant to a parcel in the baseline data in three steps. Firstly, conduct an exact matching from covenants data to baseline data based on attributes including addition, lot number, and block. 16,967 covenants are matched successfully in this step. Secondly, match the rest covenants data to parcel data if the centroid of a covenants is inside the polygon of a parcel. 7276 covenants are matched successfully in this step. Thirdly, match the rest of 2,040 covenants to the nearest parcel.

Lakes in Hennepin County

We use two datasets to identify lakes in Hennepin county: Minnesota Land Cover Classification(MLCCS) data by Minnesota Department of Natural Resources and Minnesota Rivers and Lakes Shapefile created by Minnesota Metropolitan Council. MLCCS integrates classification of cultural features, non-native vegetation, natural and semi-natural vegetation into a comprehensive land cover classification system. Therefore,

some commonly accepted lakes are classified into other types based on vegetation features or else. There are 150 lakes in Hennepin county based on definition of MLCCS. Minnesota Rivers and Lakes Shapefile has vague standard on lakes and it recognizes many unnamed small open waters as lakes,resulting in 511 lakes in Hennepin county. To find a balance between these two datasets, we manually verified every water bodies with area above average that are not recognized as lake by MLCCS but are recognized as lake in another dataset using information from websites. Finally, we identify 203 lakes in Hennepin county. Then we combine all the lakes as a multi-polygon and calculate the distance between the multi-polygon and the centroid of each parcel to get the distance of each parcel to the nearest lake.

Highway in Hennepin County

We adopt the same definition of highway as BrinkmanLin(2022). We keep all the limited-access highways that connect to other roads only at interchanges and not at at-grade intersections from Topologically Integrated Geographic Encoding and Referencing system (TIGER). This category includes Interstate highways, as well as all other highways with limited access (some of which are toll roads). The main highways in Hennepin County are I-94 and I-35. Therefore, we also define a smaller set of highways in Hennepin county that just include these two roads.

We combine all the highways in Hennepin county as a multi-polygon and calculate the distance between the multi-polygon and the centroid of each parcel to get the distance of each parcel to the nearest highway. Given the two different set of highways, we get two versions of distance to the nearest highway.

School attendance zoning

School attendace zoning information comes from the School Attendance Boundary Survey (SABS) conducted by the National Center for Education Statistics (NCES) with assistance from the U.S. Census Bureau to collect school attendance boundaries for the 2013-2014 and 2015-2016 school years. The SABS collection includes boundaries for more than 70,000 schools in over 12,000 school districts throughout the U.S.

We utilize school-level attendance zone shapefiles to identify all the unique elementary school attendance zones. Each elementary school is assigned an unique ID. Areas

overlapped by multiple schools are treated as a distinct school attendance zone and are assigned a separate ID. Then, match centroids of parcels to school zones spatially. Therefore, each parcel is matched with an unique school attendance zone ID.

Zoning characteristics

The zoning data containing minimum lot size and zone type is compiled by the Star Tribune from 102 metro communities in Minneapolis–Saint Paul that are within the regional sewer system. Some towns in Hennepin county are not covered in this data.

Distance to the boundary of Minneapolis

We calculate distance from each parcel in the baseline data to the boundary of Minneapolis city which is downloaded from website of Minneapolis city government. If a parcel is within the city, the distance is negative. Otherwise, the distance is positive.

Census Data

We also match parcel data to census data in 1940, 1960, 1980, 1990, 2000 ,2010 and 2020. Census data in 1980, 1990, 2000,2010 and 2020 contains population, share of population by race, share of people under 18, share of home ownership/rental by race at block level.Census data in 1980, 2000, 2010 and 2020 also have share of population living in group quarters at block level. Census data in 2000, 2010 and 2020 also has median household income at census tract level. Census data in 1980 has medium house value and medium rent value.

Census data in 1960 contains population, share of population by race, share of people under 18, share of home ownership/rental by race, share of population living in group quarters, house value and gross rent at census tract level.

Block level data at 1940 is not available. As an alternative, we download 1940 census data at enumeration district(ED) level from IPUMS. 1940 census data includes population density at ED level, share of ownership rate at ED level, unemployment rate at ED level, and share of professional workers at ED level. Even though income level data is not available in 1940, statistics on professions can be a fair alternative. We match each parcel to the corresponding block/tract by a spatial join.

House price

We have three sources of house price: assessed market value in tax data, last sales

price in parcel data, and inferred historical price from mortgage record. In addition, we also have assessed date in tax data, last sales date in parcel data and mortgage record date. Therefore, we are able to deflate all the prices with 2019 as base year using monthly historical CPI.

Historical house price

The historical house price is inferred from mortgage records. We selected 3000 parcels randomly from the baseline data. Then, we search for mortgage records from 1945-1955 of these 4,400 parcels and find 2800 of them. Mortgage records contains information on principal, down payment policy, interest rate, term year, monthly payment, and borrowers' name and age. There are three different down payment policies: conventional, FHA, and VA. FHA and VA are preferential policies featuring with lower down payment rate. Given that exact down payment rate is not available, we make three different assumptions on down payment rate. Under high level down payment rate assumption, the down payment rate for conventional, FHA and VA are 5%,5%, and 20% respectively. Under medium down payment rate assumption, the down payment rate for conventional, FHA and VA are 2.5%,2.5%, and 12.5% respectively. Under low level down payment rate assumption, the down payment rate for conventional, FHA and VA are 0%,0%, and 5% respectively. Total housing price can be calculated as $principal + \frac{principal}{downpayment-rate}$. Therefore, we can construct three historical house price with different down payment rate assumptions.

B. Additional Tables and Figures

Figure B.1: Example of Racial Covenants in Sale Deed

Note: Example of racial covenant on a sale deed page.

Table B1: Legal Cases regarding Racial Covenants in the U.S.

Case	Year	State	Level*	Description
Gandolfo v. Hartman	1892	CA	State	Covenant restricting Chinese to own properties. Resolution in line with the covenant.
Queensborough Land Co. v. Cazeaux	1915	LA	State	Covenant against property purchase by African American individuals. Neighbourhoods started the suit and resolution was in line with the covenant.
Buchanan v. Warley	1917	KY	State National	Covenant impeding African Americans to live in neighborhoods with most Caucasian inhabitants. State court upheld such regulation, US supreme court reversed it.
Koehler v. Rowand	1918	MO	County State	African American family (the Rownads) acquire a property under racial covenant. Plaintiffs demanded the property back, judge ordered the Rowands to turn it back.
Los Angeles Investment Co. v. Gary	1919	CA	County State	Defendant, African American, bought a property, which until 1930 could not be owned by non-Caucasian. Plaintiff, a real estate company, started the suit. Court resolution against defendant.
Parmalee v. Morris	1922	MI	State	The Morris, an African American couple, entered into a contract to purchase a lot. Neighbours made use of a covenant to impede operation. The circuit ruled against the Morris.
Corrigan v. Buckley	1926	DC†	District National	Corrigan broke the restrictions put in place by a covenant, selling her land to a black couple. Buckley sued and the Court of the District Ruled in his favour. Corrigan appealed and US supreme court maintained the decision.
Burke v. Kleiman	1934	IL	State	Burke leased a covenanted apartment to Hall, African American man. Kleiman sued. First sentence was favourable to Kleiman. Burke appealed, alleging that since the agreement was signed (1927) conditions have so 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 this 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 racial covenant was sold to Edward Meade, an African American, paying $150 in cash, with the balance to be paid in monthly instalments. Due to the covenant, Meade required deposit to be refunded. Court verdict was favourable 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 Fourteenth Amendment.
Moore v. Adams	1940	AR	State	Lot with covenant stating the once sold, 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, whom had purchased lots in the same subdivision, to restrain Moore's construction. Court failed in favour of Adams.

Note: Some cases start at a given level (i.e. county or state) ending at a higher level (state or national); † District of Columbia.

Table B.5: Legal Cases regarding Racial Covenants in the U.S. (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 favourable to Wallen. Lyons appealed with no success.
Fairchild v. Raines	1944	CA	State	The Winsells sold a property under racial covenant to the Raines (African Americans). Farichild, signer of the covenant back in 1921, sued Raines. In first instance, the court voided the Raines from using the property. Defendants appeal 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 appeal the sentence to be reversed with no success.
Scholtes v. McColgan	1945	MD	State	Suit by Scholtes against McColgan to restrain the defendant from selling any part of a tract of land to any African American person. Complaint was dismissed and the plaintiff appealed. In 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, defendant considers herself an octoroon (person having 1/8 African American blood). Court rules favourably to plaintiff.
Bogan v. Saunders	1947	DC†	District	Saunders (African American) bought a covenanted property. Plaintiffs look to (1) enforce the covenant and declare null and void the sale. Rulling for Bogan.
Hurd v. Hodge	1947	DC†	District	Lena and Frederic Hodge sued broker, Raphael Urciolo and the new owners of a sold property, James and Mary Hurd, 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 in *Hurd v. Hodge* 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, African American, signed a contract for a property from Meckler. A day after the contract was signed the agent presented Baughwith a new contract including a racial covenant. Baugh decided not to buy the house under these conditions, and demanded the refund of the deposit paid. Resolution favourable to Meckler.
Northwest Civic Ass'n v. Sheldon	1947	MI	County State	Subdivision of 338 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 is the only one of defendants who is not Caucasian. The court considered that Sheldon was told about the covenant when purchasing the property. Decree was modified such that Otis Sheldon could enter the property but not inhabit it.
Sipes v. McGhee	1947	MI	State National	The McGhees purchased a house in a neighbourhood whose association agreement included a racial covenant. The 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. Case went to the US Supreme Court, where ruling was reversed.

Note: Some cases start at a given level (i.e. county or state) ending at a higher level (state or national); † District of Columbia.

Table B.5: Legal Cases regarding Racial Covenants in the U.S. (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 of "pure Caucasian blood" was incapable of exact determination. However, 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 to occupy a racially restricted property. Ruling favourable to the Smiths.
Ralph v. Trawick	1948	DC[†]	District	Ralph, 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. Riling was favourable for Ralph.
Shelley v. Kraemer	1948	MO	County State National	The Shelleys moved into a neighbourhood without knowing about a covenant against African and Asian Americans. Kraemer brought suit to enforce the covenant, supported by other members of the neighbourhood. The Supreme Court consolidated that the enforcement of the racially restrictive covenants in state court violated the Equal Protection Clause of the Fourteenth 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 is a breach and violation of the covenants. Ruling favourable for Levy.
Weiss v. Leaon	1949	MO	State	Defendants Leaon sold or were about to sell their lot to defendants Street, African Americans. The lots in Santa Fe were subject to a racial covenant. Under the decision of the Supreme Court of the United States in *Shelley v. Kraemer*, the trial court dismissed the action.
Amschler v. Remijasz	1950	DC[†]	State	The Amschlers filed a complaint seeking to restrain defendants from selling to African Americans. Ruling was favourable to plaintiffs, being defendants obligated to leave the property in 1947. By the decision in previous cases, the US Supreme Court made racial restrictive covenants unenforceable. On May 24, 1948 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 plaintiffs' property. In the first instancen the case was decided against the plaintiff, who appealed and got the sentence to be reversed.
Barrows v. Jackson	1953	CA	State	Covenant restricting the use and occupancy of lands to Caucasian persons, obligating the signers to incorporate this restriction in all transfers of their 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 restrictive covenant and to recover damages for Jackson's breach of that covenant. The trial court relied on the holding in *Shelley v. Kramer* and ruled in favour of Jackson.

Note: Some cases start at a given level (i.e. county or state) ending at a higher level (state or national); [†] District of Columbia.

Figure B.2: Advertisement of racial covenants

Note: This figure provides an example of advertisement of racial covenants.
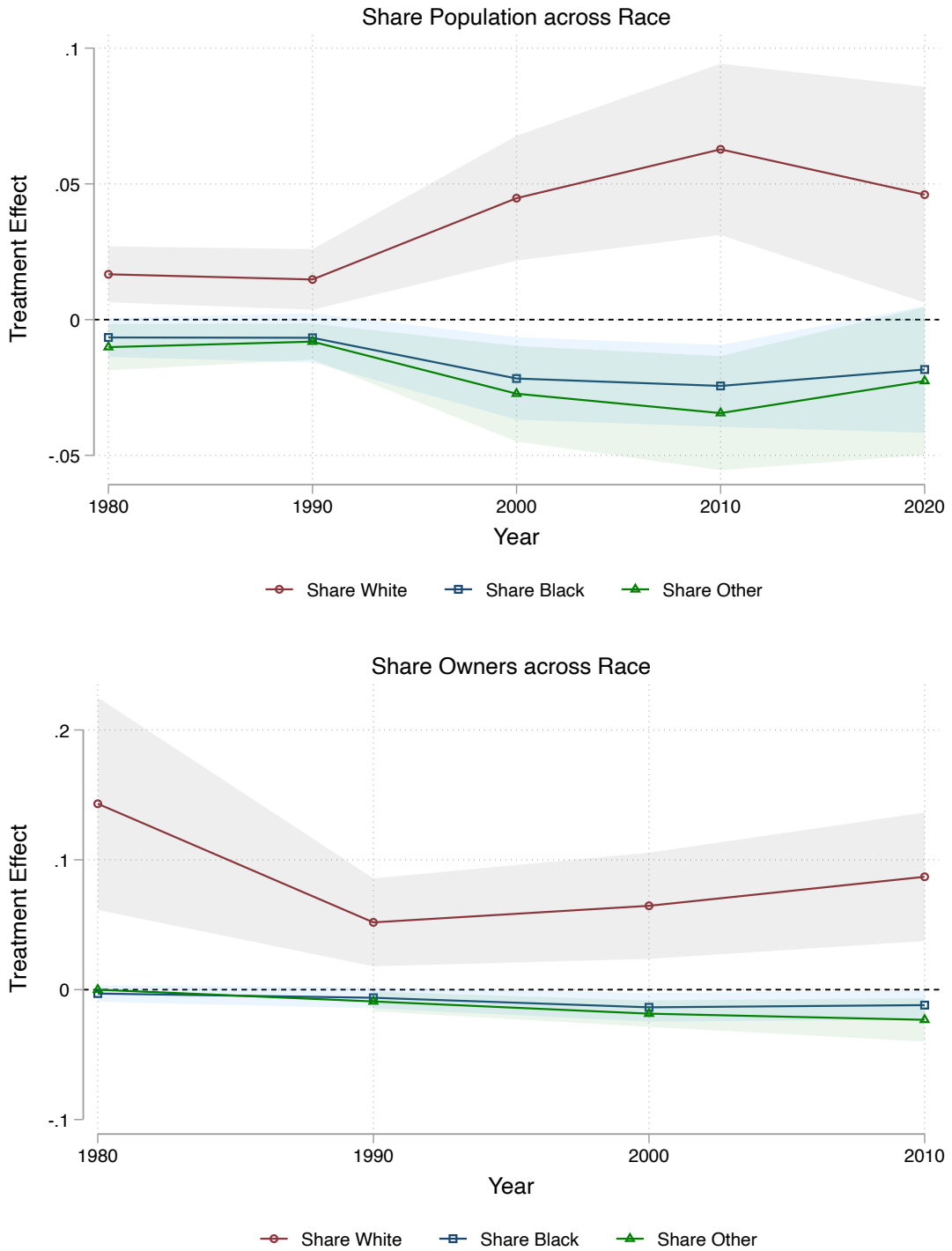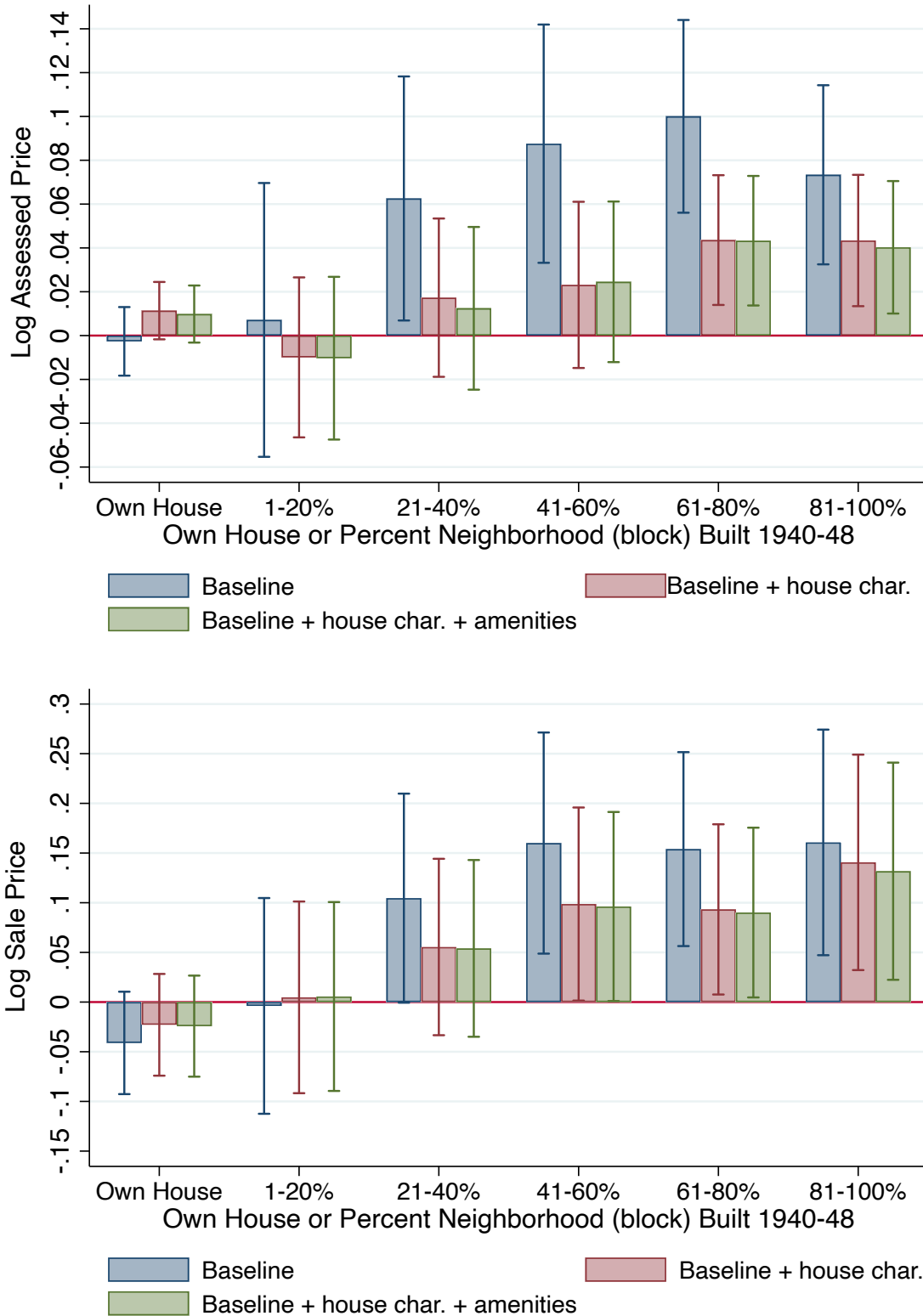
**Figure B.3**: Racial sorting (with income controls)

Note: This figure presents treatment effects 1 after controlling for neighborhood income. Standard errors are clustered at census tract level at 95%.

Figure B.4: Covenants and house prices (1940-57)

Note: This figure presents the treatment effect from Equation 2 where dependent variables are log of assessed value and log of sales price. Bandwidth is 1940-57. Standard errors are clustered at census block level at 95%.

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Figure B.5: Covenants and house prices (no tract FE)
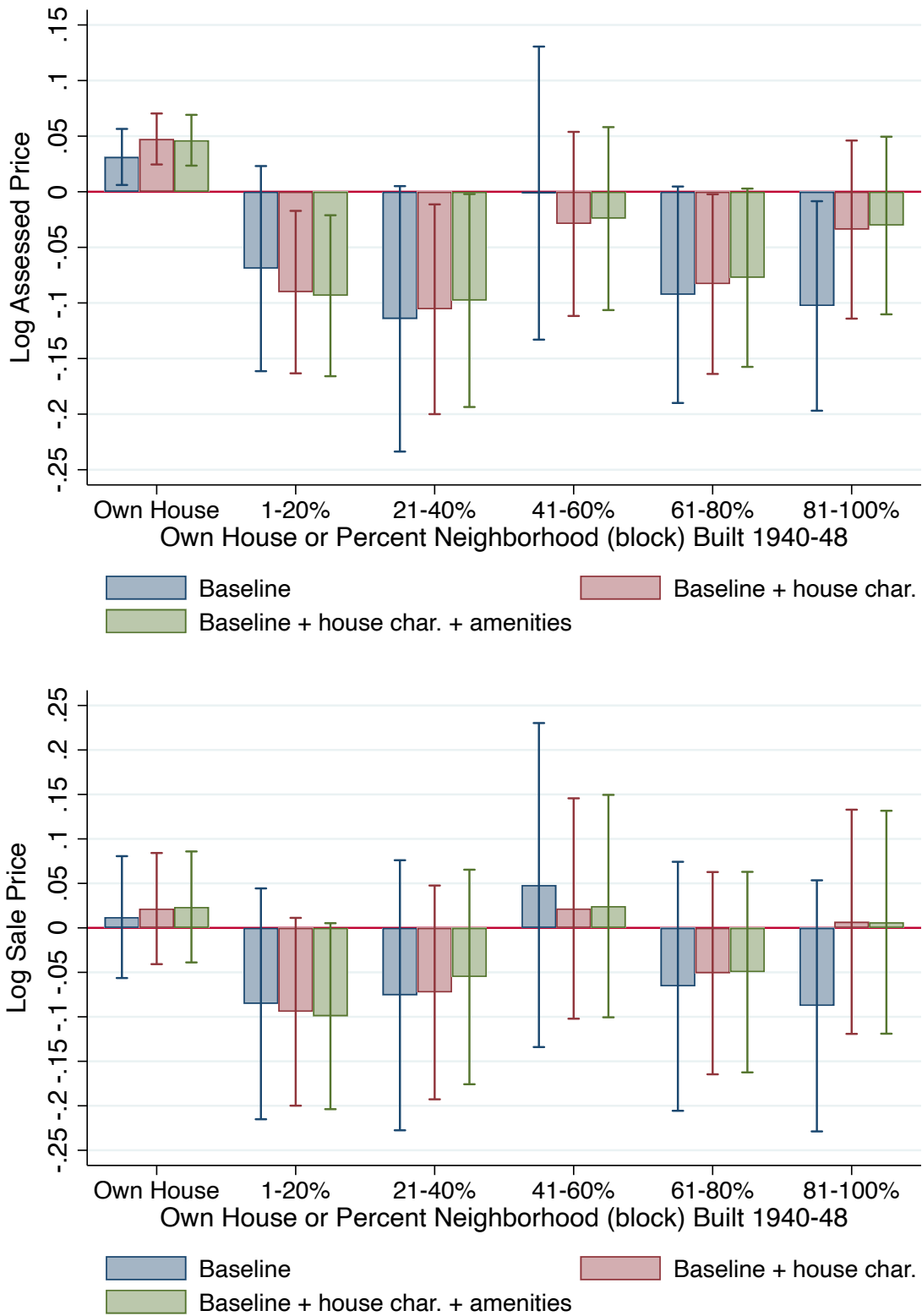
Note: This figure presents the treatment effect from Equation 2 without tract fixed effects where dependent variables are log of assessed value and log of sales price. Standard errors are clustered at census block level at 95%.

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