Segregation by Choice? Separating Preferences from Restrictions in Housing Markets*

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Abstract

How much of the observed segregation in neighborhoods is attributed to restrictions in the households' choice sets, such as those imposed by discrimination, versus household preferences? To answer this question, we build a two-sided housing matching model that combines admission rules with traditional discrete choice models and allows for heterogeneous choice sets across demographic groups. To estimate the model, we use a novel dataset that links households and developments to the historic street grid of the 1940 Minneapolis metro area. Using an instrumental variable approach, we first document that having *de jure* discriminatory restrictions in neighborhoods reduces the likelihood of non-White and White Eastern and Southern European immigrant households by 12.6 and 13.9 percentage points, respectively. Next, we demonstrate—both theoretically and through Monte Carlo simulations—that traditional discrete choice models of neighborhood demand with mis-specified choice sets produce biased estimates of preferences for neighborhood characteristics if there is selection in the characteristics of admissible neighborhoods. We estimate our two-sided housing matching model, which allows for the joint estimation of different choice sets and preference parameters. Our results suggest that different demographic groups faced substantially different choice sets: on average, minority households are 8 percentage points less likely to have a neighborhood in their choice set compared to non-minority households even after controlling for *de jure* forms of discrimination and rents. Our partial-equilibrium counterfactuals suggest that removing or reducing restrictions on neighborhood choice sets for minority households can result in significant, if not complete, re-sorting of minority households across developments.

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1. Introduction

Segregation across space is observed everywhere, yet the underlying causes driving it remain unclear. Understanding the causes behind segregation is central to a range of economic and policy questions, such as children's outcomes, upward mobility, labor market outcomes, and consumption inequality of private and public goods (Collins and Margo, 2000; Durlauf, 2004; Chetty et al., 2016; Chyn and Katz, 2021; Trounstine, 2018; Couture et al., 2024). Much of the literature attributes racial and ethnic segregation to preferences to live among others of their own race or ethnicity (Bayer et al., 2007, 2022) or to preferences for neighborhood design policies (Fischel, 2002). However, recent research has found that discrimination, whether explicit or implicit, as well as financial or information frictions can also play a role in the observed sorting of households across neighborhoods (Aaronson et al., 2021; Sood and Ehrman-Solberg, 2023; Christensen and Timmins, 2022, 2023; Bergman et al., 2024). Although there is a large literature on segregation, a central challenge is disentangling the effects of limits on choice sets from households' preferences, both of which can contribute to the observed segregation. In this paper, we ask: how much do restrictions in the choice set of neighborhoods versus preferences of households contribute towards segregation and shape the spatial sorting of households?

To understand the drivers behind observed segregation patterns, we construct a novel dataset that links households from the 1940 individual-count US census data and housing developments in the 1940 Minneapolis metro area to the street grid of that time. To our knowledge, this is the first dataset compiled at the development-household level in a historical context. Additionally, we can also link lot-level data on the *de jure* instrument of housing discrimination in the form of racial covenants. These covenants, added to property deeds, explicitly prohibited certain racial and ethnic minorities from buying or renting homes and served as the primary *de jure* tool of housing discrimination in urban and suburban neighborhoods in the early to mid-20th century in the US.

This setting helps us in several ways. First, because the Minneapolis metro area was rapidly expanding during this time, the decisions to admit households into a neighborhood were made at the development level rather than on an individual house-seller level,

creating a better empirical setting for us to study restrictions on the supply side of the housing market. Second, covenants served a dual purpose in the housing market: non-minority households not impacted by racial covenants might have preferred living in areas with such restrictions, while simultaneously, these covenants limited and shaped the residential options available to minority households. This dual role highlights the importance of distinguishing between preferences and restrictions when examining how choice-set restrictions arising from either discrimination or neighborhood design policies influence segregation and spatial inequality. This is relevant not only in a historical context but also in relation to current debates about zoning regulations. For example, single-family zoning can shape segregation patterns both through preferences as certain households may prefer to live in areas with such characteristics, but also by pushing up housing prices and thus making those neighborhoods unaffordable for other households.

We disentangle the role of the drivers behind segregation both theoretically and empirically. First, we present reduced-form results indicating that racial covenants had a first-order effect on the sorting of households across space. To account for the potential non-random placement of covenants across space, we instrument racial covenants with an urbanization frontier index. This index captures the availability of land for development in a radial outward expansion of the metro area in 1940. Our relevance condition primarily relies on the correlation between the radial outward expansion of the city into virgin or agricultural land given the growing popularity of covenants over time. Since racial covenants were primarily added to newly built housing, developments closer to the urbanized frontier of the metropolitan area were more likely to have racial covenants in 1940 than those closer to the city center. The exclusion restriction relies on the random availability of agricultural land that becomes available to be developed, conditional on enumeration district fixed effects, as well as other development controls such as the distance to the city center. Using this instrumental variable approach, we find that living in a fully covenanted neighborhood decreased the probability of a household being non-White by 12.6 percentage points. Additionally, we show that the impact of racial covenants extended beyond racial minorities to include ethnic minorities, including White Eastern and Southern European immigrant families. Specifically, living in a fully covenanted

neighborhood reduced the probability of being a White Eastern European immigrant household by 11.9 percentage points. These results highlight that *de jure* discrimination affected the segregation patterns of households in 1940. However, these reduced-form results cannot separate between the importance of preference for covenants by non-minority households and the restrictions created by covenants in the neighborhood choice set of minority households.

Given the dual role that restrictions like racial covenants can play in sorting, we theoretically and through Monte Carlo simulations show the significance of accounting for differences in choice sets in residential choice models. To do so, we use a simplified discrete-choice model where people choose from a set of options with non-random characteristics, such as lower prices. We show that assuming everyone considers all options—as most housing choice models do—can lead to biased estimates of preferences. Moreover, our theoretical findings indicate that the bias resulting from the mis-specified model is correlated with how the characteristics in the *latent choice set* are selected relative to the full set of neighborhoods. Specifically, if the neighborhoods in the restricted choice set tend to have lower (or higher) values of a certain characteristic than the full set of neighborhoods, then the model that ignores these restrictions will produce downward (or upward) biased estimates. Intuitively, if a researcher wrongly assumes that all households can choose from all neighborhoods, but in reality some households only consider options with lower (or higher) values for a given feature, the estimates will wrongly show that households sort more negatively (positively) on this characteristic than under their true preferences. We further quantify this bias using Monte Carlo simulations.[1] Our results indicate that the difference between true and estimated parameters in a mis-specified model widens as the share of unavailable neighborhoods for the restricted demographic group increases.

To empirically evaluate the role of choice set restrictions in segregation patterns, we propose a two-sided matching housing market model where we allow for differences in the neighborhood choice sets across different demographic groups following the literature on selective admissions and latent choice sets (Goeree, 2008; Gandhi, 2019; Abaluck and Adams-Prassl, 2021; Agarwal and Somaini, 2022). Our two-sided matching housing

[1] Crawford et al. (2021) perform a similar simulation exercise.

market model of selective admissions not only allows us to include sources of observable discrimination, such as racial covenants, but also implicit restrictions that limit the choice sets available to minority groups, which are often difficult to measure. Concretely, to incorporate differences in latent choice sets, we model real-estate developers who have preferences regarding the households they "accept" into their development according to an admission rule. This admission rule depends on the household's race or ethnicity and captures the likelihood of a neighborhood being included in different demographic groups' choice sets. These admission preferences, in turn, generate differences in the household's latent choice sets, where these choice sets are explicitly linked to race or ethnicity. On the demand side, heterogeneous households from different demographic groups choose their most preferred development from their set of development to which they've been admitted. These choices depend on household's preferences over rents, the demographic composition of the development, as well as other development characteristics.

To separately identify the parameters of the admission rule from household preferences, we follow Goeree (2008) and use an excluded shifter that affects the formation of choice sets but does not enter utility. Specifically, we assume that racial covenants influence which developments are available to households but do not affect utility directly, once we control for rents and the demographic composition of development. Intuitively, we treat racial covenants as a means to control the demographic of neighborhoods but not as a feature that directly affects the utility of households.

We estimate our model parameters following Berry et al. (1995) and Bayer et al. (2007), that allow for heterogeneous preferences across demographic groups, and extend their procedure to allow for the estimation of latent choice sets. Specifically, we classify households into *minority* and *non-minority* groups based on our reduced-form results.

Our estimation follows two steps. In the first step, we estimate the parameters governing the admission rule and the differences in preferences for development characteristics between minority and non-minority households. In the second step, we recover the non-minority group's preference parameters. Similar to Berry et al. (1995) and Bayer et al. (2007), we face several endogeneity concerns as house rents and demographic compo-

sition within a development are equilibrium outcomes and, thus, can be functions of unobservable demand shocks. To address these endogeneity concerns, we construct two instruments: one based on steep land slopes that affect construction costs and, therefore, house rents (Saiz, 2010); and another using a shift-share migration instrument to instrument for the share of minorities within a development (Card, 2001; Tabellini, 2020).

Preliminary results indicate that minority households faced substantial restrictions in choice sets and those restrictions increased with the presence of covenants. By contrast, we observe that covenanted developments favored the admission of non-minority households. Moreover, our admission rule estimates also show that non-minority households were less likely to be admitted in more expensive developments, although we do not see that rents play a role in admission for minorities. We interpret these results as capturing potential financial constraints for non-minority households. Finally, after controlling for the role of rents and covenants, minorities were 8 percentage points less likely to have the option of living in the average development, capturing other implicit forms of discrimination against minorities common to all developments.

On the demand side, we find that minority households are more sensitive to house prices compared with non-minority households. Our demand estimates also show demographic composition preferences in line with homophily bias for non-minorities. Notably, when we estimate a model that ignores differences in neighborhood choice sets across demographic groups, we find that the price coefficient for non-minority households is roughly 11.7% larger compared to our two-sided matching model, overstating the role that household preferences play in the observed segregation.

We develop two partial equilibrium counterfactual scenarios. The first scenario removes restrictions on neighborhood choice sets for both minority and non-minority households, while the second scenario reduces restrictions on neighborhood choice sets specifically for minority households. By comparing the distribution of households in these two counterfactual scenarios with the observed equilibrium from 1940, we find that eliminating or lowering restrictions on choice sets can lead to significant, if not complete, re-sorting of minority households across developments. However, this re-sorting is mitigated by the strong distaste that households, particularly non-minority households, have

towards living near minority households (homophily bias).

Literature review: Our paper contributes to the literature on neighborhood segregation and the role of policy instruments in shaping the spatial distribution of households in several ways.

First, we contribute to the residential choice literature by disentangling the role of preferences from choice set restrictions in driving segregation. Typically, the existing literature either only considers how preferences for co-patrons may shape within-city segregation (Bayer et al., 2007; Caetano and Maheshri, 2021; Almagro et al., 2024) or studies supply-side discrimination (Christensen and Timmins, 2022, 2023). A related paper is Li (2023), which uses a differentiation approach to account for restrictions in choice sets. This differentiation approach relies on the assumption of no selection in the formation of choice sets. By contrast, we model and estimate admission rules where the probability that a household is "accepted" in a neighborhood is a function of the household's demographics and neighborhood characteristics, which implicitly creates a relationship between neighborhood characteristics and household's probability of admission into a neighborhood.

Second, our paper contributes to a large literature studying how policy design contributes to segregation. Most of the literature measures localized treatment effects, often using regression discontinuity-based methods (Myers Jr, 1995; Rothstein, 2017; Trounstine, 2018; Fishback et al., 2020; Aaronson et al., 2021; Song, 2021; Sood and Ehrman-Solberg, 2023; Hynsjö and Perdoni, 2022; Shertzer et al., 2016). By contrast, our study looks at the impact on the entire metro area and allows us to measure the aggregate importance of different channels that contribute to segregation. Two exceptions are Bagagli (2023) and Weiwu (2023), who studied how segregation interacts with a specific neighborhood design policy, the construction of the US Interstate Highway System in the 1960s and 1970s, in a spatial equilibrium model. We contribute to this literature by explicitly incorporating restrictions in choice sets that differ across demographic groups using our novel data and a two-sided matching model. In doing so, we not only quantify the role of *de jure* discrimination policy instrument via racial covenants, but also the role of other forms of implicit restrictions, which are more difficult to measure, captured by differences in

admission rules across demographic groups.

Finally, to estimate differences in choice sets in the two-sided matching model, our methodology closely relates to the recent advances in the estimation of preference parameters with unobserved choice sets heterogeneity (Goeree, 2008; Conlon and Mortimer, 2013; Gaynor et al., 2016; Gandhi, 2019; Agarwal and Somaini, 2022).[2] As opposed to previous papers in the urban literature unrestricted choice sets, our paper is the first to estimate latent choice sets in the context of a housing market. Importantly, understanding how restrictions on choice sets are formed is crucial for determining the extent to which observed segregation results from these restrictions rather than from sorting preferences or housing prices, as well as for assessing their impact on spatial inequality and segregation.

2. Historical Background and Data

2.1 Historical Background

Racial covenants restricted racial and ethnic minorities from moving into covenanted neighborhoods during the first half of the twentieth century (see Appendix Figures E.1-E.3 for examples of racial covenants in property deeds). These covenants were private legal agreements between real estate developers and homeowners, making them a *de jure* form of segregation (Ellen and Steil, 2019). As an instrument of neighborhood design, racial covenants were used to restrict the presence of minority households and control neighborhood demographics. In US metro areas that were being largely urbanized during the first half of the twentieth century, including the Minneapolis metro area, almost all racial covenants were added by developers at the stage of subdivision on new developments. Developers would advertise racial covenants in their developments (see Appendix Figure E.4 for an example of such advertisements). The enforcement of racial covenants relied on "injured parties," usually neighbors, bringing lawsuits against those violating racial covenants as well as steering away of minorities by real estate agents and developers from covenanted neighborhoods.

Four key historical events established racial covenants as the primary tool for neighborhood design in urban and suburban areas of the US during the first half of the 20th

[2]See Crawford et al. (2021) for an extensive survey on this topic.

century. These events also contributed to the growing popularity of racial covenants during this period. First, in 1917, the *Buchanan v. Warley* US Supreme Court ruling prohibited municipalities from creating racially segregated neighborhoods through zoning regulations. As a result, the use of *private* racial covenants became more appealing (Lowe, 1948). Second, the prevalence of racial covenants increased when the National Association of Real Estate Boards amended its charter in 1924 to promote their use (Jones-Correa, 2000). Third, when the Supreme Court reaffirmed the legal enforceability of racial covenants in the 1926 *Corrigan v. Buckley* ruling, leading to their widespread adoption across the country (Crooks, 1948). Finally, the federal government solidified the use of racial covenants when, in its 1938 underwriting manual, the Federal Housing Administration (FHA) favored mortgages linked with "subdivision regulations and suitable restrictive covenants" (FHA, 1938). However, in 1948, the US Supreme Court declared racial covenants unenforceable in the *Shelley v. Kraemer* ruling, effectively ending the use of racial covenants to design segregated neighborhoods.

In addition to racial covenants, uncoordinated actions by developers, real estate agents, and neighbors likely contributed to steering households away from certain neighborhoods. There were also instances of collective group action, such as the threat of violence (Albright et al., 2021) and local government policies—like the placement of industrial zoning (Shertzer et al., 2016)—that influenced some developments to be welcoming to minority households, while others were unwelcoming to racial and ethnic minorities in 1940.

2.2 Data

We combine data on racial covenants, historic real estate developments, the historical street grid for Minneapolis metro area, and the 1940 US individual count decennial census. Linking all of these data sources together allow us to map racial covenants and households to the developments and geography of 1940. We focus on 1940 for two reasons. First, 1940 is when the use of racial covenants was at its peak. Second, 1940 is one of the two census years that contains data on house rents and values.[3] More detailed information about how we construct our dataset can be found in Appendix A.

[3]The 1940 census also contains information about previous residences. These data will allow us to include households' moving costs in our residential choice model, which we eventually use as a demand-only shifter in the estimation of our two-sided matching model.

Data on Racial Covenants: The data on racial covenants comes from the Mapping Prejudice Project, which provides a complete census of all covenanted deeds and development maps in Hennepin County, Minnesota, where the Minneapolis metro area is located. In addition to the location of 24,131 racial covenants, Mapping Prejudice also provides information on the date of covenant execution.[4]

Historic Boundaries and Street Grids: Linking households, developments and covenants to the present-day street day delivers less than 45% of successful matches as the historic street grid has changed in the past 80 years due to highway construction and new development over time. To overcome this problem, we digitize and geocode the 1940 street grids for Minneapolis and five neighboring suburban municipalities—St. Louis Park, Edina, Richfield, Hopkins, and Robbinsdale—using City Atlases and the Sanborn insurance maps that existed by 1940. Together with Minneapolis, these six cities form a contiguous part of the Minneapolis metro area and accounted for roughly 92% of the 1940 population of the Hennepin County. We also geocode and digitize the 1940 enumeration district boundaries, which are similar to modern census tracts, for Hennepin County.

Data on Historic Developments: At the time of our study period, Minneapolis experienced a dramatic urban expansion mostly driven by the construction of suburban residential real estate developments. Developers constructed a group of similar houses in their developments. Developments vary in size, ranging from a single housing unit to several hundred units, with an average of 47.7 households per development. An average development is 13.7% of the average enumeration district size, and there are, on average, 8.9 developments within an enumeration district (see Appendix Table A.1). In our analysis, we define neighborhoods at the development level, as we believe this most accurately reflects the unit of choice relevant to both household location demand decisions and admission decisions on the admission side of the market.

Information about historic developments that have survived is available from the Hennepin County's assessor office. However, using only the extant development-level data omits developments that were demolished or altered between 1940 and today, which

[4]Mapping Prejudice Project analyzed roughly three million pages of sales deeds and subdivision maps in Hennepin County, covering the period 1900-1960. Five people double-checked the existence and date of the covenant execution for each deed and map, limiting the scope of error in data collection.

would result in 29.7% of 1940 households in the sample unmatched to a development. To overcome this missing-development problem, we collect data on historical developments which come from the historical atlases of the municipalities of Minneapolis (1940), St. Louis Park (1939), Edina (1940), Richfield (1941), Hopkins (1954), and Robbinsdale (1941). These data provide to-scale maps of each development in these municipalities (see Figure E.5 for an example of such an atlas map). We digitize and geocode the historical atlases to construct the 1940s geo-located geography of each development and locate the lots within these developments using Hennepin County's assessor data. Importantly, our data also contain information on the year that a development was likely originally platted and on the completed construction year for most houses in our sample.

Individual-Count US Census Data: We use the full-count 1940 US decennial census data for the six Minneapolis metro area municipalities in our sample. The data include information about individuals like race, country of birth (to determine ethnicity), occupation, marital and ownership status, number of household members as well as residence five years ago (in 1935). Since endogamy is common among races and ethnicities during this time period, we assume that the racial and ethnic characteristics of the head of household is the representative of the household, which we also confirm using the individual count data.[5] Thus, we do our analysis at the household level. There are 164,653 households in the six Minneapolis metro area municipalities in 1940.

The census data also contains other household information like the address, enumeration district, group quarter status, housing value, and rental price. We only observe rental and housing value data for renter-occupied and owner-occupied households, respectively, and there is no price data available for households in group quarters. To enable a comparison between rents and prices, we impute the rental value of owner-occupied housing using the methodology following Katz et al. (2017).[6] The imputed rental value of owner-occupied housing, combined with rental price data, gives us the mean monthly rental value for 98% of developments and the monthly rental value for 91.5% of households in our sample.

[5] 99.7% of households in our sample marry within their race or ethnicity.

[6] see Appendix Figure E.6 for the distribution of monthly rents and housing prices relative to the imputed monthly rental values.

Other Data: We also digitize and geocode a 1899 map of Hennepin County, which highlights the locations of wetlands and other semi-submerged areas so we can measure the underlying land quality in the lake geography of Hennepin County. In addition, we obtain data on elevation and slope from the Natural Resources Conservation Service, which completed a comprehensive survey of Hennepin County in 1953. We also digitize a 1923 map highlighting the location of heavy and light industries as well as commercial zoning areas in the city of Minneapolis. Last, we also digitize the 1913 streetcar map of the Minneapolis metro area. See Appendix Table A.1 for summary statistics for the household and development characteristics.

2.3 Construction of Geo-referenced Household Level Dataset

For our analysis, we need to link households and racial covenants to geolocated developments in 1940.

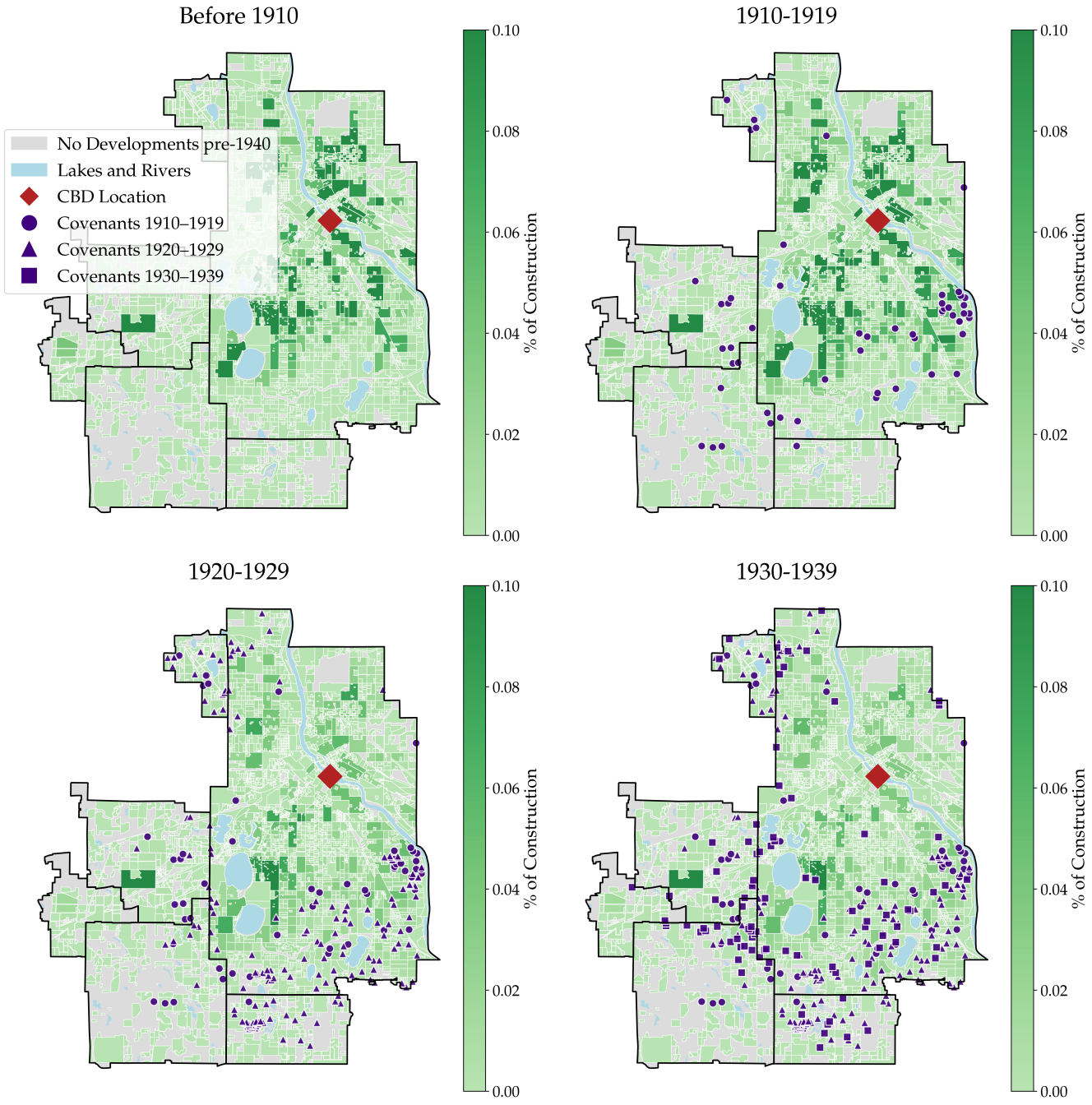
In the 1940 census, we have complete addresses for 75.1% of households. For the remaining 24.9% households with an incomplete address, we impute missing street names and house numbers following the census enumeration ordering, as enumerators collected the census data on the same side of the street in order Logan and Parman (2017). Since this order is preserved in the census, we are able to impute missing house numbers and street names for 91% of households with incomplete addresses. In total, this procedure allows us to have complete addresses for 97.7% of households.

We match households with complete addresses to the geolocated historical developments using the digitized historical street grid, achieving a high match rate of 97.7%. For the remaining 2.3% of households that could not be matched to a historic development, we randomly assign these unmatched households across developments within their respective enumeration districts.[7]

Next, we link racial covenants to the geolocated historical developments. Last, we also geolocate wetlands, soil quality, and elevation data as well as the location of lakes, the streetcar network, and industrial and commercial zone areas, which allows us to measure the distance to these characteristics from a given development. See Appendix A for further

[7] In our reduced-form evidence, we show that our findings are not determined by the random placement of these 2.3% of households. See Panel E of Appendix Table B.2 for more details.

Figure 1: Expansion of Housing Construction and Racial Covenants over Time



Note: This figure plots the percentage of a decade's overall housing construction which took place in each development in green. The purple circles, triangles, and squares indicate where a racial covenant is present in a development from the 1910s, 1920s, and 1930s respectively. The city boundary of Minneapolis, St. Louis Park, Edina, Robbinsdale, Richfield, and Hopkins is marked in black. Grey areas represent developments with no housing construction by 1940. The red diamond is the location of the central business district (CBD), which is where the St. Anthony Falls are located. Lakes and rivers are marked in blue.

details on linking across various datasets.

Figure 1 plots the distribution of new housing construction by development in four different periods–before 1910, 1910-19, 1920-29, 1930-39. Percent of new construction within a development by decade is color-coded in green. As the figure shows, the construction is expanding radially outwards over time from the central business district (CBD) indicated by the red diamond. In addition, the figure plots the racial covenants that were introduced by decade, in purple. As the figure shows, the use of racial covenants continuously spread after 1910, the year of first racial covenant, and particularly after 1930.

3. Empirical Evidence of the Effects of Racial Covenants

In this section, we provide reduced-form evidence of the effects of racial covenants on neighborhood ethnic and racial composition in the 1940 Minneapolis metro area housing market. The reduced form results help establish the following three points. First, they demonstrate that the demographic groups impacted by racial covenants included all racial minorities as well as many "White" ethnic minorities, including White immigrant households from Eastern and Southern European countries. Second, these results help us understand and classify whether a household from a specific ethnic background belongs to a minority or non-minority group. We will use this classification to estimate heterogeneity in structural parameters separately across different groups. Third, the findings motivate our structural analysis, as the reduced form results alone cannot distinguish between direct preferences of households for neighborhoods characteristics versus how those characteristics, such as covenants or prices, could have acted as restrictions in the neighborhood choice set of minority households in shaping segregation.

3.1 Reduced-Form Model

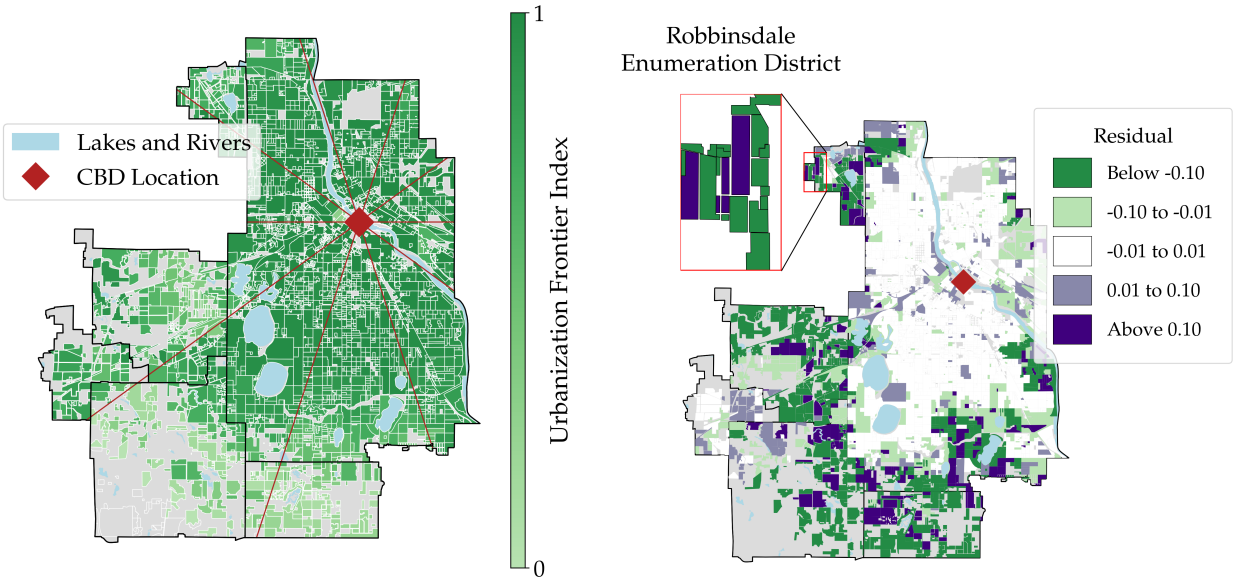
To study the effects of racial covenants on neighborhood ethnic and racial composition, we estimate the following model, where i indicates a household and j indicates a development in 1940:

$$y_{ij} = \beta_0 + \beta_1 \text{Cov-Share}_{j(i)} + \beta_2 \text{Dist-CBD}_j + \beta_3 X_i + \beta_4 X_j + \eta_k(j) + \epsilon_{ij} \tag{1}$$

In this model, y is an indicator for a household's race or immigrant ethnicity in devel-

opment j. Cov-Share$_{j(i)}$ is the share of the development j that had racial covenants. Dist-CBD$_j$ is the log of distance of the development j to the metro area's CBD. X_i represents variables for household ownership status, marital status, the number of members and working members of the household, and an indicator for occupation. X_j represents development level location quality variables including the proportion with wetlands and the log of distance to each of a heavy industry zoned area, a commercially zoned area, the nearest lake, and the streetcar. $\eta_k(j)$ indicates the enumeration district k fixed effects within which development j is located.

Figure 2: Urbanization Frontier and Residual Patterns Across Sectors



Note: The left panel displays the Urbanization Frontier Index across ten sectors of 36° radiating from the central business district. The right panel shows the residuals from regressing the Urbanization Frontier Index on the set of controls from equation 2 and enumeration district fixed effects.

Given that covenants were a decision made by the developer at the time of platting their development, it is important to account for the non-random placement of racial covenants across space. Moreover, Sood and Ehrman-Solberg (2023) show that racial covenants were more likely to be added in lower-middle and middle-income areas and in neighborhoods that were more likely to face demographic transitions in the future. We include the enumeration district fixed effects to control for this source of endogeneity and use variation across developments within the same enumeration districts k to estimate

our parameters.

However, even within enumeration districts, the location of covenants could be correlated with unobservable characteristics. To address this concern, we measure the share of covenanted development (Cov-Share$_{j(i)}$) by using the share of built-up area from the previous period in neighboring developments that are closer to the city center than development j. That is, we use the extent of earlier development in nearby areas facing the city center to capture the direction and momentum of urban expansion, assuming that covenant adoption followed this pattern outward from the city center. We denote this share as f_j, for each j, and call it *the urbanization frontier index*. Our relevance condition is based on the radial outward expansion of the city as seen in Figure 1. Thus, given that racial covenants gained popularity over time during the first half of the 20th century, as highlighted in Section 2, and were mostly added on new developments, our hypothesis suggests that developments closer to the urbanized frontier were more likely to have racial covenants than those farther away from it. In addition, since racial covenants exhibit economies of scale, their discriminatory effects should increase with the number of covenants in the vicinity. As a result, developers would have been more incentivized to introduce covenants in areas with lower previous levels of development, as these areas held greater potential for these economies of scale to take effect.

To construct the urbanization frontier index in 1940, we first divide the city from the CBD into r sectors of equal size. Within a sector r, we define $f_j \equiv \frac{\text{Built}_{r,1930}(z_j)}{\text{Lots}_{r,1930}(z_j)}$, where $\text{Built}_{r,1930}(z_j)$ is the number of houses built by 1930, and $\text{Lots}_{r,1930}(z_j)$ is the total number of buildable lots by 1930 within a given buffer zone z_j. A buffer zone for development j is within the same sector r but on the side that is closer to the city center and represents the neighboring developments for the development j. Note that the buffer zone is defined separately for each development j. Our preferred specification sets $r = 10$ and uses buffer size $z_j = 1000$ meters. Figure 2 (left panel) plots the urbanization frontier index as well as the sectors for all developments in Minneapolis metro area for 1940, where pre-period is 1930.[8]

[8]We use a ten-year interval to construct the instrument, allowing for sufficient time for housing construction to occur.

The exclusion restriction is that, conditional on enumeration district fixed effects, distance to the CBD, and location controls for the developments, which development will be built next—and the amount of that development completed by 1930—was random. Thus, the changes in the individual fortunes of developers and builders that had purchased lots from farmers and other landowners provides the underlying random variation.[9] To see the variation that our identification strategy is capturing, we plot the residuals from regressing the urbanization frontier index instrument, f_j, on distance to the CBD, location controls, and enumeration district fixed effects. Figure 2 (right panel) plots the residuals for all developments in the Minneapolis metro area, providing a closer view of the variation within a specific enumeration district in Robbinsdale. As can be seen from the figure, there is random variation across developments, even within an enumeration district.

The first stage is given by the following model:

$$\text{Cov-Share}_{j(i)} = \alpha_0 + \alpha_1 f_j + \alpha_2 \text{Dist-CBD}_j + \alpha_3 X_j + \eta_{k(j)} + \varepsilon_{j(i)}. \tag{2}$$

Observe that this instrument not only exploits temporal variation in the addition of racial covenants, it also ensures that development demographic differences within an enumeration district operate only through the addition of racial covenants on the frontier of the new development.

3.2 Reduced-Form Results

Table 1 reports the coefficients α_1 and β_1 from Equations 2 and 1, where Panels B and C present 2SLS and OLS estimates, respectively. The first-stage estimates in Panel A indicate that our instrument works as expected and the higher the urbanized built-up area in a buffer zone z_j, the higher the probability of adding racial covenants to that development j. In particular, if the urbanization frontier index goes from 0 to 1, the probability of a development being covenanted increases by 0.15 percentage points.

[9]The commercial and residential development in Minneapolis metro area until mid-1910s was focused very close to the CBD. As the large milling district started collapsing by the early 1920s, commercial and residential developments started moving away from the CBD (Danbom, 2003). The 1920s were also a challenging period for the U.S. farmers, as they faced soaring debt and a sharp decline in commodity prices after the World War I boom, leading to prolonged financial struggles, resulting in farmland sales (Irwin, 2017). Using survey data from the 1960s and 1970s from the neighboring Olmsted County, Minnesota, Pyle (1985) finds the most common reason for farm sales for housing development in the area was idiosyncratic personal shocks related to health and finances.

We find that having a fully covenanted development (Cov-Share$_{j(i)}$ goes from 0 to 1) reduced the probability of non-White households by 12.6 percentage points (column 2). Non-White households include Black, Asian, and Native American households as classified in the census, regardless of their country of birth.

In the early to mid-20th century, the groups of households facing discrimination extended beyond racial minorities. To explore this issue, we refer to real estate economist Homer Hoyt's 1933 list of immigrants, where "the most favorable come first in the list and those exerting the most detrimental effect appear last" (Hoyt, 2000). Based on Hoyt's classification, we can categorize White immigrants, classified as those born in a country other than the US, into one of four groups, ordered from "most favorable" to "least favorable": Northern Europe, including Canada, Australia and New Zealand and Western Europe as more "favorable" and Eastern and Southern Europe as well as Others (which includes immigrants from all other regions) as less "favorable".[10]

Having a fully covenanted development increased the likelihood of first-generation White immigrant households from Western Europe by 6.8 percentage points (see column 6). This suggests that White immigrant households from Western Europe were not facing discrimination; rather, they were preferred as also corroborated by ranks of one and two in Hoyt's list. We did not find any statistically significant effects for White immigrant households from Northern Europe. However, having a fully covenanted development decreased the likelihood of Eastern and Southern European immigrant households, who were placed near the bottom of Hoyt's list, by 13.9 percentage points (see column 8). Thus, racial covenants were not only used as a discriminatory tool against racial minorities, but also against certain White ethnic minorities, which during the early-to-mid 20th century included White Eastern and Southern European households.

The effects of covenants on racial and ethnic sorting are not driven by the head of household's occupation, their ownership or marital status, or the number of members and working members in the household. In addition, these results are also not driven by location characteristics (columns 3, 7, 9). Furthermore, note that the OLS coefficient for

[10]See Appendix A.1, where we show which countries are in each of these categories and Figure E.7 for Hoyt's list..

all demographic groups is significantly lower than the IV estimates (Panel C), indicating a downward bias in the OLS estimation. The downward bias in the estimation aligns with the hypothesis that racial covenants were located in transition, low-middle-income, and middle-income regions that were likely of lower to middle-quality level within the larger metropolitan area.

The baseline results presented above are based on sectors $r = 10$, $z_j = 1000$ meters, and the number of houses built by 1930 as a measure to construct the urbanization index in 1940. However, the results remain robust when we consider alternative time periods, such as the number of houses built by 1935 or 1910 (see Appendix Table B.1, Panels A and B), as well as a buffer zone that decrease as the distance to the CBD increases $z_j = 500 - 2000$ meters (Appendix Table B.1, Panel C). Furthermore, the results are robust even when we control for the mean build year for each development (see Appendix Table B.2, Panel A) and when we make alternative assumptions about matching the small number of households to alternative developments (see Appendix Table B.2, Panel B). Additionally, we conduct a placebo test to see whether our instrumental variable is in reality capturing new construction. To do so, we construct an alternative urbanization frontier index which instead uses a neighbor buffer zone that *faces away from the CBD*, rather than towards it, as in the baseline analysis. The idea is that such a reversed frontier index should still capture new construction and amenities associated with it, but does not rely on the radial urban expansion of Minneapolis. As can be seen in Appendix Table B.2 Panel C, we do not find, for the most part, statistically significant effects under this placebo test.

Moving forward, based on the results from Table 1, we define minority households as those that are non-White, regardless of their birthplace, as well as White households born in Eastern and Southern Europe as well as "Other" countries. All other households are classified as non-minority households.[11] Under this classification, minority households account for 7.3% of households and 8.2% of the population in the Minneapolis metro area in 1940.

[11] Although we do not find statistically significant negative effects of racial covenants on White households born in "Other" countries of the world, we believe this is due to the small number of such households. In our baseline structural estimation, these households are included in the minority category.

Table 1: Racial Covenants and Sorting in the Housing Market

	First Stage	Non-White		White Northern European Imm.		White Western European Imm.		White Eastern & Southern European Imm.		White Other Imm.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: First Stage											
Urbanization Index	0.149***										
	(0.018)										
Panel B: 2SLS											
Share Covenanted		−0.126***	−0.112**	0.007	0.005	0.068*	0.066*	−0.139*	−0.145*	0.060	0.060
		(0.044)	(0.044)	(0.163)	(0.165)	(0.038)	(0.039)	(0.084)	(0.085)	(0.047)	(0.047)
Panel C: OLS											
Share Covenanted		−0.001**	−0.000	−0.012	−0.012	−0.005**	−0.005**	−0.009**	−0.010**	−0.000	−0.000
		(0.001)	(0.001)	(0.010)	(0.010)	(0.002)	(0.002)	(0.005)	(0.005)	(0.002)	(0.002)
Household controls		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Log CBD Distance	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Location controls	✓		✓		✓		✓		✓		✓
District FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	163,538	163,535	163,535	163,535	163,535	163,535	163,535	163,535	163,535	163,535	163,535
Mean Dep. Var.	0.017	0.010	0.010	0.150	0.150	0.008	0.008	0.054	0.054	0.009	0.009
F statistic	65.8	66.9	65.8	66.9	65.8	66.9	65.8	66.9	65.8	66.9	65.8

Note: This table plot parameter estimates from Equations 2 and 1. Non-White is an indicator for Black, Asian, or Native American households, regardless of their country of birth. White Northern European, Western European, Eastern and Southern European, and Other Immigrants (Imm.) denotes race as White and birthplace in these regions. Refer to Appendix A.1 to see which countries are in each of these categories. Share Covenanted is the fraction of lots in a development with racial covenants. Robust standard errors in parentheses. Panel A reports the first-stage coefficient of the Urbanization Frontier Index (instrument) on Share Covenanted. Panel B and C show the 2SLS and OLS estimates, respectively. Household controls include ownership tenure, occupation, marital status, and number of household members and working members. Location controls include log slope, wetlands share, and log distances to heavy-industry zoning, commercial zoning, lakes, and streetcars. District FEs are enumeration-district fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4. Model

4.1 Ignoring Restricted Choice Sets

We first show how ignoring restrictions on the household's choice sets can bias household preference estimates. To do so, we rely on a simplified economy where minority or non-minority households face restrictions in their choice sets and can only choose to live in a restricted set of neighborhoods.

We start by proving a general result that holds under mild assumptions and helps us quantify the sign of the bias of a mis-specified residential choice model. Concretely, consider the following model:

$$V_j^{k,i} = \beta^k X_j + \epsilon_j^i$$

where $V_j^{k,i}$, is the utility of household i of type $k \in \{m, nm\}$, which stands for minority and non-minority, respectively, living in neighborhood j. Utility depends on neighborhood j's characteristics X_j such as average rent, share of covenants, or house size, and an idiosyncratic component that is drawn from Type-I extreme value distribution ϵ_j^i. Each household chooses the neighborhood that gives her the highest positive utility. We denote $\mathcal{J}$ as the set of all possible neighborhoods. We denote the choice set of type-k households (the latent choice set) as $\mathcal{J}_0^k$. $\mathcal{J}_1^k$ is the set of restricted neighborhoods that are not available to type k groups, so $\mathcal{J} = \mathcal{J}_0^k \cup \mathcal{J}_1^k \; \forall k$.

We use maximum likelihood (MLE) to estimate β^k. If the choice set of the type k group is correctly specified, given our distributional assumption on idiosyncratic shocks, the probability of choosing neighborhood j is:

$$\mathbb{P}_j^k(\beta^k) = \frac{\exp\left(\beta^k X_j\right)}{\sum_{l \in \mathcal{J}_0^k} \exp\left(\beta^k X_l\right)}.$$

The log-likelihood of the type k group is given by:

$$L^k(\beta^k) = \sum_{j \in \mathcal{J}_0^k} N_j^k \log \mathbb{P}_j^k(\beta^k),$$

where N_j^k is the number of type k households living in the j neighborhood in the data. Denote $\hat{\beta}^k \equiv \arg\max L^k(\beta)$ as the solution of the correctly specified choice set.

On the other hand, if the econometrician wrongly assumes that the choice set for type k households is given by $\mathcal{J}$ instead of $\mathcal{J}_0^k$, the choice probabilities are given by:

$$\tilde{\mathbb{P}}_j^k(\beta^k) = \frac{\exp(\beta^k X_j)}{\sum_{l \in \mathcal{J}} \exp(\beta^k X_l)}.$$

In the case where the choice set is wrongly given by $\mathcal{J}$, the log-likelihood is given by:

$$\tilde{L}^k(\beta^k) = \sum_{j \in \mathcal{J}} N_j^k \log \tilde{\mathbb{P}}_j(\beta^k) = \sum_{j \in \mathcal{J}_0^k} N_j^k \log \tilde{\mathbb{P}}_j(\beta^k),$$

where the second equality follows as the empirical $N_j^k = 0$ for all $j \in \mathcal{J}_1^k$. Denote $\tilde{\beta}^k \equiv \arg\max \tilde{L}^k(\beta)$ as the solution of the problem with the mis-specified choice set.

Next, we assume differences in the characteristics of neighborhoods X_j across choice sets. We assume that neighborhoods that are not in the choice set of type k, $\mathcal{J}_1^k$, have larger X_j, on average. This could be higher rents, larger share of covenants, or larger house size.

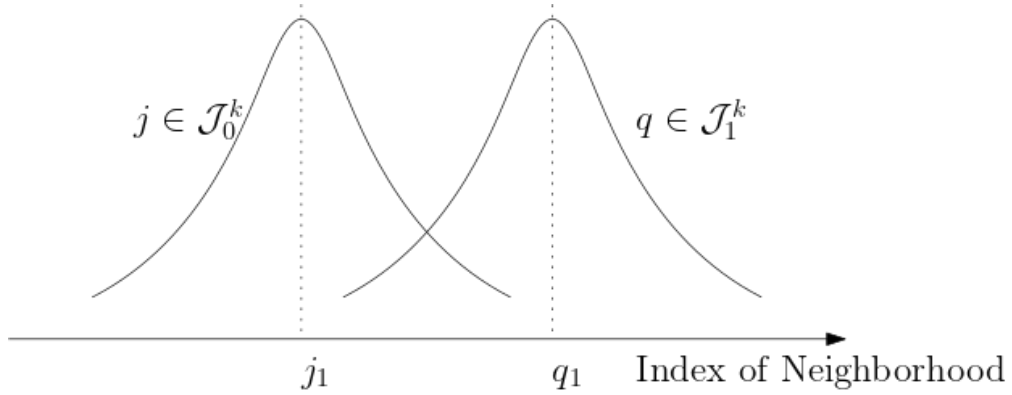
Assumption 1. *Neighborhoods in the choice set for type-k household have smaller X_j, on average:*

$$\frac{1}{N_0^k} \sum_{j \in \mathcal{J}_0^k} X_j \leq \frac{1}{N_1^k} \sum_{j \in \mathcal{J}_1^k} X_j,$$

where N_0^k and N_1^k are the number of type k households in $\mathcal{J}_0^k$ and $\mathcal{J}_1^k$, respectively.

We allow for the range of the conditional X_j distributions to have a non-empty intersection, namely that they do not have a disjoint support (see Figure 3), but this overlap cannot be too large. Concretely, let's denote $[j_1, q_1]$ the interval where the range of the X_j distributions for $\mathcal{J}_0^k$ and $\mathcal{J}_1^k$ intersect. Namely, $X_j \leq X_{q_1}, \forall j \in \mathcal{J}_0^k$ and $X_{j'} \geq X_{j_1}, \forall j' \in \mathcal{J}_1^k$. We assume that the mass of households in the restricted choice set $\mathcal{J}_1^k$ inside $[j_1, q_1]$ is smaller than the mass of households in the set $\mathcal{J}_0^k$ inside $[j_1, q_1]$:

Figure 3: Overlapping X_j Distributions



Assumption 2. *Let $N_0^{k,s}$ and $N_1^{k,s}$ denote the number of observations of the distribution of X_j inside the range $[j_1, q_1]$, conditional on choice sets $\mathcal{J}_0^k$ and $\mathcal{J}_1^k$, respectively. We assume that $\frac{N_0^{k,s}}{N_0^k} \geq \frac{N_1^{k,s}}{N_1^k}$.*

Denote the minimum and maximum values of X_j in the constrained choice set $\mathcal{J}_1^k$ as X_q^{min} and X_q^{max}, respectively. Denote the minimum and maximum values of X_j in the latent choice set $\mathcal{J}_0^k$ as X_j^{min} and X_j^{max}, respectively. Define $\underline{\beta}^k \equiv \frac{\log \frac{N_0^{k,s}}{N_0^k} \frac{N_1^k}{N_1^{k,s}}}{\log \frac{X_{q_1}}{X_q^{min}} \frac{X_j^{max}}{X_j^{min}}}$, $\overline{\beta}^k \equiv \frac{\log \frac{N_0^{k,s}}{N_0^k} \frac{N_1^k}{N_1^{k,s}}}{\log \frac{X_q^{min}}{X_q^{max}} \frac{X_j^{min}}{X_{j_1}}}$. It is easy to see that under assumption 2, $\underline{\beta}^k < 0$ and $\overline{\beta}^k > 0$.

Theorem 1. *For $\beta \in (\underline{\beta}^k, \overline{\beta}^k)$, under assumptions 1 and 2, $\tilde{\beta}^k < \hat{\beta}^k$. In other words, with mis-specified choice sets, the MLE estimate of demand sensitivity to the neighborhood characteristics for the type k group is downward biased.*

Proof. See proof in Appendix C.1. □

The intuition for the result above is straightforward. If the econometrician wrongly assumes that type-k households can choose from all neighborhoods but only observes choices from a restricted choice set whose conditional mean of the observable characteristic is lower, the MLE estimator will wrongly infer that type k households dislike neighborhoods with higher X_j more than they actually do, hence, producing an estimate of β that is downward biased.

Finally, we obtain the opposite result if we instead assume that neighborhoods in the choice set of type-k household have *higher* X_j:

Theorem 2. *Assume that houses in the choice set for type k have higher X_j, on average:*

$$\frac{1}{N_0^k}\sum_{j\in\mathcal{J}_0^k} X_j \geq \frac{1}{N_1^k}\sum_{j\in\mathcal{J}_1^k} X_j,$$

and that the mass of the overlap between distributions is not too large:

$$\frac{N_0^{k,s}}{N_0^k} \leq \frac{N_1^{k,s}}{N_1^k}.$$

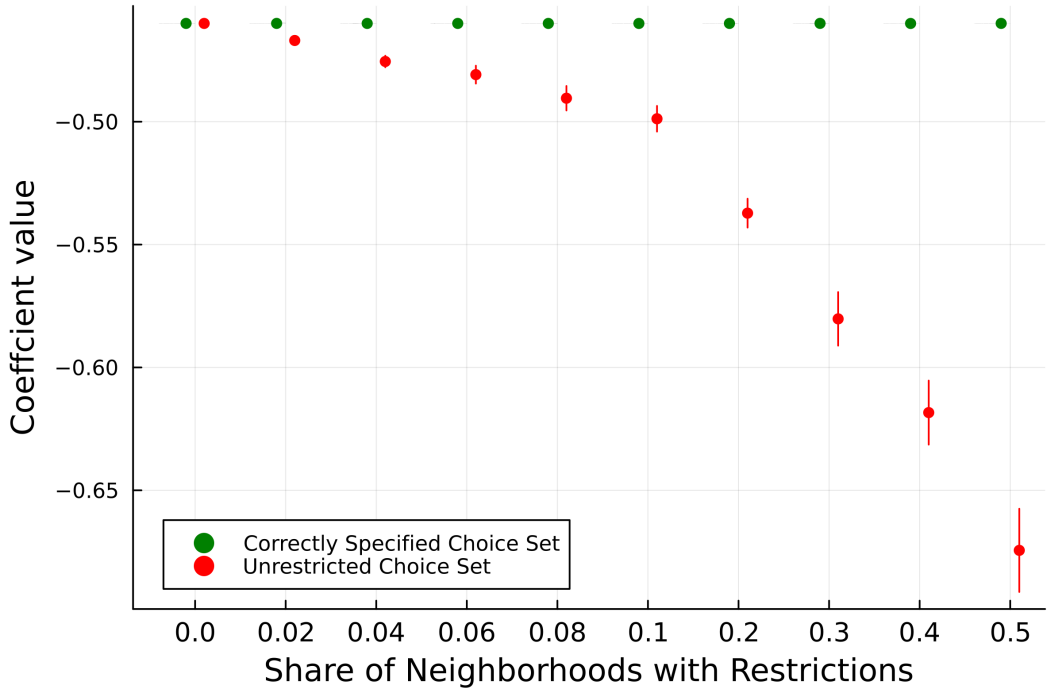
Then, for $\beta \in (\underline{\beta}^k, \overline{\beta}^k)$, it follows that $\tilde{\beta}^k > \hat{\beta}^k$. In other words, under mis-specified choice sets, the MLE estimate of the demand sensitivity to the neighborhood characteristics for the type k group is upward bias.

4.1.1 Monte Carlo Simulations

In this section, we quantify how the size of the bias in the mis-specified model increases as the number of neighborhoods that are restricted to the type k households increases. To do so, we rely on Monte Carlo simulations designed as follows. First, we simulate a residential choice model with type k group that has preferences for housing characteristics. Recall that type k households face restrictions in their choice sets and can only choose to live in a restricted set of neighborhoods. In the neighborhoods that are accessible to type k households, we simulate that the housing characteristic X_j is 50% lower, on average. For each data draw, we estimate coefficients for type k households for a mis-specified model with an unrestricted choice set and a correctly specified model with the correct choice set.

Figure 4 plots the distribution of estimates for type-k households. When the true value of β^k is -0.46, the unrestricted model estimates (in red) are lower than the true value, i.e., the mis-specified unrestricted model delivers estimates with a downward bias, which will in turn imply that type-k households have a lower willingness-to-pay for characteristic X_j than they actually do. Also note that the bias disappears under the correctly specified model (in green) where the restrictions in the choice set for type k households are known. Additionally, note that the difference between the true and estimated β^k increases as we increase the number of restricted neighborhoods for type-k households.

Figure 4: True and Estimated Value of Preferences for Covenants



Note: This figure plot the distribution of estimates for type k households. Mean and 95% confidence intervals are depicted in blue. The true value of $\beta^k = -0.46$. The vertical axis plots the coefficient values while the horizontal axis plots the share of developments that are restricted to the type k households.

4.2 A Two-Sided Matching Model of the Housing Market

Having established the importance of choice-set mis-specification, we now proceed to present a two-sided matching model with selective admissions of households into neighborhoods. Households in our model belong to different types, namely, minorities and non-minorities, with heterogeneous preferences for neighborhood characteristics across types. To model admission rules, we follow a reduced form equation based on the demographics of the household as well as development characteristics that determined who is admitted households into the development. Importantly, we assume that this admission rule is a function of household's race or ethnicity. This model of selective admissions introduces variation, and thus, potential restriction in the neighborhoods that can be accessed by different demographic groups.

4.2.1 Demand Model

Households, indexed by i, belong to two types k: minorities m and non-minorities nm. In what follows, we fix the non-minority group as our baseline group and denote with indicator $m(i)$ if i belongs to the minority group. The indirect utility for household i from residing in development j is given by:[12]

$$U_{ij} = (\beta_x + \beta^m \mathbb{1}\{k(i) = m\})X_j^d + \xi_j + \epsilon_{ij} \tag{3}$$

$$= \delta_j + \beta^{m(i)} X_j^d + \epsilon_{ij}, \tag{4}$$

where X_j^d are observable characteristics of development j that enter the demand function, ξ_j are unobservable demand shocks, $\beta^{m(i)} = \beta^m \mathbb{1}\{k(i) = m\}$, and $\delta_j = \beta X_j^d + \xi_j$. In other words, δ_j represents the average utility of the non-minority group and β^m are just the deviations in preferences of the minority group relative to the non-minority group. The characteristics of development j that enter demand, X_j^d, include development housing price p_j and the share of minority households m_j, among other development characteristics. Thus, households have preferences on the racial and ethnic makeup of their neighbors. Finally, ϵ_{ij} is an idiosyncratic shock associated with household i's choice for development j. If ϵ_{ij} is an i.i.d. type-I extreme value error, the choice probability for development $j \in O$ conditional on choice set O for type k is given by:

$$\mathbb{P}_j^k(\boldsymbol{\delta}, \boldsymbol{\beta} | O) = \frac{\exp(v_j^k(\boldsymbol{\delta}, \boldsymbol{\beta}))}{1 + \sum_{j' \in O} \exp(v_{j'}^k(\boldsymbol{\delta}, \boldsymbol{\beta}))},$$

where (δ, β) is the vector of demand parameters and $v_{ij}(\boldsymbol{\delta}, \boldsymbol{\beta}) \equiv \delta_j + \beta^{m(i)} X_j^d$. Importantly, $\mathbb{P}_j(\boldsymbol{\delta}, \boldsymbol{\beta} | O) = 0$ if $j \notin O$. We also assume that the outside option is always in the choice set of every household, regardless of demographic type.

[12] This model requires the definition of a baseline group. If not, observe that the combination;

$$\tilde{\delta}_j = \delta_j + cX_j, \qquad \tilde{\beta}^{nm} = \beta^{nm} + c, \qquad \tilde{\beta}^m = \beta^m + c,$$

gives observationally equivalent outcomes.

4.2.2 Development Admission Rule

We introduced heterogeneity in choice sets as development admission rules, following Abaluck and Adams-Prassl (2021), Agarwal and Somaini (2022) and Gandhi (2019). Household i is admitted to development j according to the following rule:

$$\eta_i \geq \alpha X_j^s + \alpha^{m(i)} X_j^s, \tag{5}$$

where X_j^s are the development characteristics that enter in the admission rule and η_i is household i's unobservable idiosyncratic component related to her admissions. Note that this rule allows for differences in admission across demographic groups for all developments by setting $X_j = 1$ for all j, but also through interactions with development characteristics as mediated by $\alpha_1^{m(i)} X_j^s$.

From equation (5), we can see that if i is admitted into development j and $\alpha X_j^s + \alpha^{m(i)} X_j^s \geq \alpha X_{j'}^s + \alpha^{m(i)} X_{j'}^s$, then household i must also be admitted into development j'. This transitivity result implies we only have to consider $J+1$ potential choice sets for each demographic group k that can be ordered as follows:[13]

$$\{0\}, \{0, 1^{k(i)}\}, \ldots, \{0, 1^{k(i)}, ..., J^{k(i)}\}$$

where we order developments according to:

$$\alpha X_{J^{k(i)}}^s + \alpha^{m(i)} X_{J^{k(i)}}^s \geq \alpha X_{J^{k(i)}-1}^s + \alpha^{m(i)} X_{J^{k(i)}-1}^s \geq ... \geq \alpha X_{1^{k(i)}}^s + \alpha^{m(i)} X_{1^{k(i)}}^s.$$

Intuitively, the larger j is, the larger $\alpha X_{j^{k(i)}}^s + \alpha^{k(i)} X_{j^{k(i)}}^s$ and the stricter the admission rule. Therefore, for household i the probability that the set $[j^{k(i)}] \equiv \{0, 1^{k(i)}, ..., j^{k(i)}\}$ is her choice

[13] Observe that this transitivity result relies on the assumption that the unobservable component that enters into admission rules, η_i, is common across developments. This assumption may fail if households of a group are favored differently by different developments in a way that is unobservable to the econometrician. One example of this could be family ties between households and developers. While we can relax this assumption, it is helpful for computational reasons as otherwise, we would have to consider the full power set of $\{0, 1, ..., J\}$, which has 2^J elements. However, we believe that this unobservable match-specific component does not play a big role given that our model already allows developments to have different preferences over households based on observable components, such as ethnicity and the interaction of ethnicity with characteristics of the development.

set, O^i, is given by:

$$\begin{aligned}\mathbb{P}(O^i = [j^{k(i)}]|\boldsymbol{\alpha}) &= \mathbb{P}(\alpha X^s_{j+1} + \alpha^{m(i)} X^s_{j+1} > \eta_i \geq \alpha X^s_j + \alpha^{m(i)} X^s_j) \\ &= F_\eta\Big(\alpha X^s_{j+1} + \alpha^{m(i)} X^s_{j+1}\Big) - F_\eta\Big(\alpha X^s_j + \alpha^{m(i)} X^s_j\Big),\end{aligned}$$

where F_η is the cumulative distribution function of the idiosyncratic payoff error term η_i.

4.2.3 Match probabilities

Putting everything together, the probability of an observed match s_{ij} for individual i is given by:

$$\begin{aligned}\mathbb{P}(s_{ij} = 1|\boldsymbol{\delta}, \boldsymbol{\beta}, \boldsymbol{\alpha}) &= \sum_{O \in 2^{|J|}} \mathbb{P}^{k(i)}(O|\boldsymbol{\alpha}) \mathbb{P}^{k(i)}_j(\boldsymbol{\delta}, \boldsymbol{\beta}|O) \\ &= \sum_{n=1}^{J} \mathbb{P}([n^{k(i)}]|\boldsymbol{\alpha}) \mathbb{P}^{k(i)}_j(\boldsymbol{\delta}, \boldsymbol{\beta}|[n]) \\ &= \sum_{n \geq j^{k(i)}}^{J^{k(i)}} \mathbb{P}([n^{k(i)}]|\boldsymbol{\alpha}) \mathbb{P}^{k(i)}_j(\boldsymbol{\delta}, \boldsymbol{\beta}|[n]) \\ &= \sum_{n \geq j^{k(i)}}^{J^{k(i)}} \left(F_\eta\Big(\alpha X^s_{n+1} + \alpha^{m(i)} X^s_{n+1}\Big) - F_\eta\Big(\alpha X^s_n + \alpha^{m(i)} X^s_n\Big) \right) \frac{\exp(v^{k(i)}_j(\boldsymbol{\delta}, \boldsymbol{\beta}))}{\sum_{j'=1}^{n} \exp(v^{k(i)}_{j'}(\boldsymbol{\delta}, \boldsymbol{\beta}) + 1},\end{aligned} \tag{6}$$

where the third equality follows from the fact that $j^{k(i)} \notin [n^{k(i)}]$ if $j^{k(i)} > n^{k(i)}$.

5. Estimation and Results of the Two-sided Matching Model

5.1 Estimation Procedure

The estimation relies on an inner loop (first-step) and an outer-loop (second step) following Berry et al. (1995) and Bayer et al. (2007). We extend their procedure to allow for the estimation of latent choice sets. The estimation algorithm proceeds as follows:

Inner Loop For given (α, β), we construct the Maximum Likelihood function following these steps:

1. Order developments for each $k \in \{m, nm\}$ according to admission rules :

$$\Pi_{1^k}(\alpha) \leq ... \leq \Pi_{J^k}(\alpha),$$

where $\Pi_j(\alpha; k) \equiv \alpha X_j^s + \alpha^k X_j^s$.

2. Given the previous ordering, for each choice set $[n]$, construct choice set probabilities:

$$\mathbb{P}(O = [n^k]|\alpha; k) = F_\eta\Big(\Pi_{n+1}(\alpha; k)\Big) - F_\eta\Big(\Pi_n(\alpha; k)\Big).$$

3. Compute the probability of a match following a modified version of the contraction mapping in Berry et al. (1995) and Bayer et al. (2007) to recover δ_j:[14]

   (a) Start with a guess of δ_j, say δ_j^0.

   (b) For every step, s, compute match probabilities for all minority and non-minority households given α, β, and δ_j^s:

$$\mathbb{S}_j(\alpha, \beta, \delta^s; k) = \sum_{n \geq j^k}^{J^k} \mathbb{P}(O = [n^k]|\alpha; k) \cdot \frac{\exp(v_j^k(\beta, \delta_j^s))}{\sum_{j'=1}^{n} \exp(v_{j'}^k(\beta, \delta_j^s)) + 1}.$$

   (c) Compute the model-predicted match probability for development j by summing across groups:

$$\mathbb{S}_j(\alpha, \beta, \delta^s) = \sum_k \mathbb{S}_j(\alpha, \beta, \delta^s; k) \cdot \frac{N^k}{N}.$$

   (d) Update δ_j^s using:

$$\delta_j^{s+1} = \delta_j^s + \log s_j - \log \mathbb{S}_j(\alpha, \beta, \delta^s).$$

   (e) Repeat until convergence.

[14] Berry et al. (1995) show that for given observed shares $s_j \equiv \frac{1}{N}\sum_i s_{ij}$ and $\beta^{m(i)}$, there is a unique vector of δ_js that makes choice probabilities equal to s_j and that these δ_j's can be recovered using a contraction mapping.

4. Given α, β, and the resulting $\delta_j(\alpha, \beta)$, compute the log-likelihood:

$$\mathcal{L}(\alpha, \beta) = \sum_i \sum_j s_{ij} \log \mathbb{S}_j(\alpha, \beta; k(i)).$$

Outer Loop For every evaluation of the outer loop, we proceed as follows:

1. Maximize the log-likelihood function to estimate α, β and the resulting δ_j.

2. We estimate the remaining θ^{nm} by GMM using the following moments:

$$\mathbb{E}[G(\alpha, \beta)] = \mathbb{E}[\xi_j \cdot z_j] = 0,$$

where z_j are either instruments for the endogenous variables or the variables that are assumed to be orthogonal to ξ_j. Concretely, we maximize the following objective function:

$$\max_{\alpha,\beta} G(\alpha, \beta)' W G(\alpha, \beta),$$

where $G(\alpha, \beta)$ is the vector of empirical moments and W is a weighting matrix.

5.2 Identification

In this section, we explain the sources of variation that identify our parameters. We do so step by step. Recall that in the first step (inner loop) we estimate the parameters governing the admission rule α, the differences in preference parameters for minority households β as well as the utility levels δ_j of the non-minority group. In the second step (outer loop) , after recovering δ_j in the first step, we identify the parameters of the non-minority households using an instrumental variable approach.

5.2.1 Identification of the First Step: Admission Rules versus Preferences

We now discuss the variation in the data that allows us to separately identify admission-rule parameters $\boldsymbol{\alpha}$ from demand parameters $\boldsymbol{\beta}$ in our model. To make progress, we first need to make several normalizations. First, as pointed out by Train (2009), the level of utility is not identified. Thus, we normalize the value of the outside option equal to zero ($\delta_j = 0$) for all demographic groups. Because the scale of a logit model is not identified, we also normalize the variance of idiosyncratic shocks ϵ_{ij} to be equal to $\frac{\pi^2}{3}$. On the side of

the admission rule, we also need to normalize the variance of η and fix $\sigma_\eta^2 = 1$.

Next, to separate parameters that enter into the admission rule from preferences, we rely on an exclusion shifter argument (Goeree, 2008). That is, we assume the existence of a variable that shifts the formation of choice sets but not utility, conditional on the rest of the covariates. Concretely, we assume that racial covenants do not enter utility after conditioning for rents and the share of minority households.[15] Finally, we show that the estimated effect of racial covenants on utility is not statistically significant, providing suggestive support for our excluded shifter assumption. While it is possible to identify the model using its structural and parametric features alone, as we show in Appendix D, we prefer that our identification argument relies on exclusion restriction arguments, as it is a more transparent, robust, and credible strategy.

5.2.2 Identification of the Second Step: Baseline Group Preferences

To recover the preferences for the non-minority group β^{nm}, we project δ_j on X_j^d. Note that the unobservable component of utility ξ_j enters additively in δ_j. Therefore, the identification assumptions required by the OLS would give biased estimates if there are observable characteristics (X_j^{nm}) that are potentially correlated with ξ_j. In our case, there are two potentially endogenous variables that are part of household's utility. These characteristics are housing prices and the minority share, which are equilibrium objects.To overcome this endogeneity challenge, we construct moments based on instruments orthogonal to ξ_j.

First, we instrument the log of house prices using the log of slope within development j, following Saiz (2010), who documents significant increase in construction costs as slope increases over 10 or 15 degrees. The identifying assumption is that the steeper slope instrument shifts the housing supply by affecting construction costs, and thus prices, but does not affect the demand for housing, especially in the relatively flat lake geography of the Minneapolis metro area.

Second, we instrument development level minority share m_j by constructing a shift-share IV à la Card (2001); Tabellini (2020). We construct the city c level shift share

[15]We also assume that geographic characteristics of a development–such as distance to the city center or access to transportation–enter utility but not the admission rule. The presence of both types of excluded shifters enables the identification of more general and less parametric two-sided matching models with latent choice sets, as in Agarwal and Somaini (2022). However, given the structural and parametric assumptions we impose, identification in our setting does not require the presence of both shifters.

instrument $Z_{c,1940}$ for the six municipalities in our sample as:

$$Z_{c,1940} = \frac{1}{\hat{P}_c, 1940} \sum_r w_{cr}(1930) O^Q_{r,1930-40} \tag{7}$$

where w_{cr} is the share of households from foreign country r living in c in the baseline year of 1930. $O^Q_{r,1930-40}$ refers to the total yearly quota assigned to country r between 1930 and 1940, as specified by the U.S. Immigration Act of 1924. This act provides an exogenous national-level variation in the influx of households from various countries and ethnicities. Following the methodology outlined by Tabellini (2020), we weight the shift-share instrument using $\hat{P}_{c,1940}$, which is the predicted population of city c in 1940. Given that our analysis employs ED-level fixed effects to control for underlying location quality, to get variation in the municipality-level shift share instrument within an ED, we weight $Z_{c,1940}$ by the household size in the metro area.[16]

Table 2: First-Stage for Second Step of Estimation

	(A) House Prices		(B) Minority Share	
	(1)	(2)	(3)	(4)
Main instrument				
Log slope	0.132***	0.129***	—	—
	(0.015)	(0.016)		
Shift–Share	—	—	100.100***	22.500***
			(10.973)	(2.443)
Other Instruments as Controls		✓		✓
Observations	150,686	149,970	164,637	163,524
F-statistic	33.1	105.7	13.9	19.2
District fixed effects	✓	✓	✓	✓
Location controls	✓	✓	✓	✓

Notes. This table plots the first stage regression estimates for the instruments used in the second step of the two-sided matching model. Robust standard errors in parentheses. All specifications include enumeration district fixed effects and baseline controls for logged distances to the CBD, lake, commercial zone, industrial zone, and streetcar, as well as the wetland percentage. Significance levels: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

The identifying assumptions are that the lagged shares for each municipality in 1930

[16]Refer to Appendix D.2 for more details on the construction of this instrument. Since at least 87% of the households in the minority group are foreign-born, we focus on a shifter that exogenously affects the foreign-born population rather than the U.S.-born population.

are not correlated with the municipality's past unobservable factors, especially since all six municipalities are located in the same metropolitan area, where similar local macroeconomic trends are likely to influence them, and that the national shifter, represented by the 1924 quotas, is random.

We can test whether our instruments are relevant and correlated with our endogenous variables as expected. Results for the first-stage regression are shown in Table 2. We can see that the log slope is positively correlated with the log of prices, suggesting that larger slopes translate into higher construction costs, and a higher value of the shift share instrument results in a higher minority share. This relationship holds true even after controlling for the remainder instrument, as shown in columns 2 and 4. We can also see that the first stage models have an F-stat of at least 13.9.

5.3 Results

In this section, we present estimation results of the two-sided housing market model with selective admissions. Table 3 presents the parameter estimates for both the admissions rule (α) from Equation 5 and demand preferences (β) from Equation 3. Column 1 presents a model that only estimates demand preferences without accounting for selective admission, while Column 2 provides estimates that also account for selective admissions and restrictions on choice sets for each demographic group. It is important to note that since the parameter estimates for the admission rule in Equation 5 are less than or equal to η_i, a positive parameter estimate indicates a lower probability of admission. Standard errors are provided in parentheses.

We find that the admission penalty for minority households, denoted as α^m, is 0.21. This indicates that minority households are less likely to be admitted to the developments relative to non-minority households. Specifically, we can calculate the difference in admission probabilities for minority versus non-minority households across developments. On average, a minority household is 8 percentage points less likely to be admitted compared to non-minority households, even after accounting for *de jure* discrimination from racial covenants and financial restrictions arising from higher rents.[17]

[17]Note that $\Phi(0.21) \approx 0.58$ and the baseline average probability of admission for non-minority households is 0.5.

Table 3: Estimation Results Comparison - With and Without Choice Set Restrictions

	Location Demand Only Model		Two-Sided Matching Model	
	(1)		(2)	
Admission Rule Parameters:				
α^{m}			0.210***	(0.012)
α^{m}_{rent}			-0.002	(0.047)
$\alpha^{nm}_{\text{rent}}$			0.135***	(0.004)
$\alpha^{m}_{\text{covenant}}$			0.041*	(0.021)
$\alpha^{nm}_{\text{covenant}}$			-0.021***	(0.003)
Demand Side Parameters:				
β^{m}_{rent}	-1.515***	(0.069)	-1.521***	(0.104)
β^{nm}_{rent}	-1.342***	(0.067)	-1.201***	(0.066)
$\beta^{m}_{\text{minority}}$	-1.386***	(0.087)	-1.298***	(0.087)
$\beta^{nm}_{\text{minority}}$	-2.493***	(0.087)	-2.469***	(0.086)
$\beta^{m}_{\text{distance to CBD}}$	-0.762***	(0.044)	-0.809***	(0.044)
$\beta^{nm}_{\text{distance to CBD}}$	-0.733***	(0.042)	-0.708***	(0.042)
$\beta^{m}_{\text{distance to streetcar}}$	-0.025**	(0.011)	-0.157***	(0.012)
$\beta^{nm}_{\text{distance to streetcar}}$	-0.176***	(0.006)	-0.163***	(0.006)
Observations	163,865		163,865	
Log Likelihood	1481741.5		1481268.6	

Notes. This table presents results from location choice demand model (column 1) and two-sided matching model (column 2). Admission rule parameters are from Equation 5 and demand side parameters are from Equation 3. Standard errors are in parenthesis. m refers to minority households; nm refers to non-minority households. CBD refers to central distance district. Significance levels: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

We also find that the estimate of $\alpha^{nm}_{covenant}$ is negative and statistically significant, in contrast to the positive and statistically significant estimate of $\alpha^{m}_{covenant}$. These estimates suggest that developments with a higher proportion of covenants are more likely to admit non-minority households, while they are less likely to admit minority households. This result reaffirms our earlier reduced-form findings that highly covenanted developments give more priority to non-minority households in their resident selection process. Although the admission restrictions related to racial covenants are significant for minority households, they account for only about 20% of the general admission restrictions faced by these households.

The parameter estimate of α^{m}_{rent} is not significantly different from zero, indicating that minority households do not experience additional high rents or financial constraints when seeking admission after controlling for general admission penalties and those associated with racial covenants. In contrast, the estimate for α^{nm}_{rent} is both positive and statistically significant. This suggests that higher rents and financial constraints serve as barriers to admission for non-minority households. In fact, the impact of rents and financial restrictions on non-minority households is substantial, nearly three times greater than the effect of covenant restrictions on minority households.

Moving on to the demand parameter estimates, we find that all households have a negative coefficient on rents. For all the specifications, we obtain a more negative β^{m}_{rent}, meaning that minority households are more price-sensitive than non-minority households. This result is in line with what previous studies have found (Bayer et al., 2007; Almagro et al., 2024).

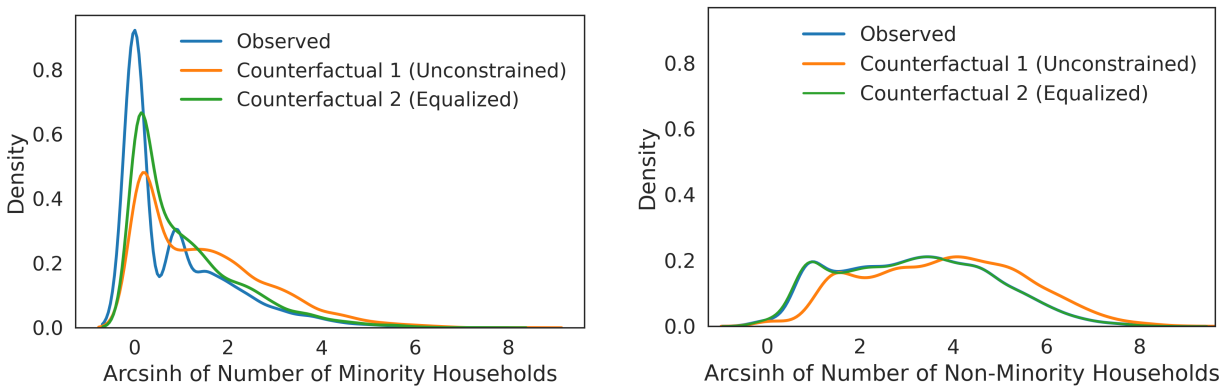
Since development rents are a critical factor influencing the admission of non-minority households into housing developments, the location demand only model overestimates the price sensitivity for non-minority households (as seen in column 1). In fact, the rent coefficient for non-minority households is actually 11.7% larger in magnitude in the location demand-only model. In contrast, the rent coefficient remains relatively stable in both models for minority households, which we have already demonstrated, is not affected by rents after controlling for the general penalty and the racial covenants associated penalty. Thus, as indicated by Theorems 1 and 2, ignoring choice set restrictions affects

the non-minority households' demand preference parameters and creates a downward bias.

Last, we find that both minority and non-minority households do not like living in developments with a higher minority share, but relative to minorities, non-minority households dislike living in these developments twice as much. In fact, for non-minority households, the share of minority households in the development is the most important factor in determining their location choice.

6. Counterfactual Analysis

Figure 5: Density of Number and Share Minority and Non-Minority Households

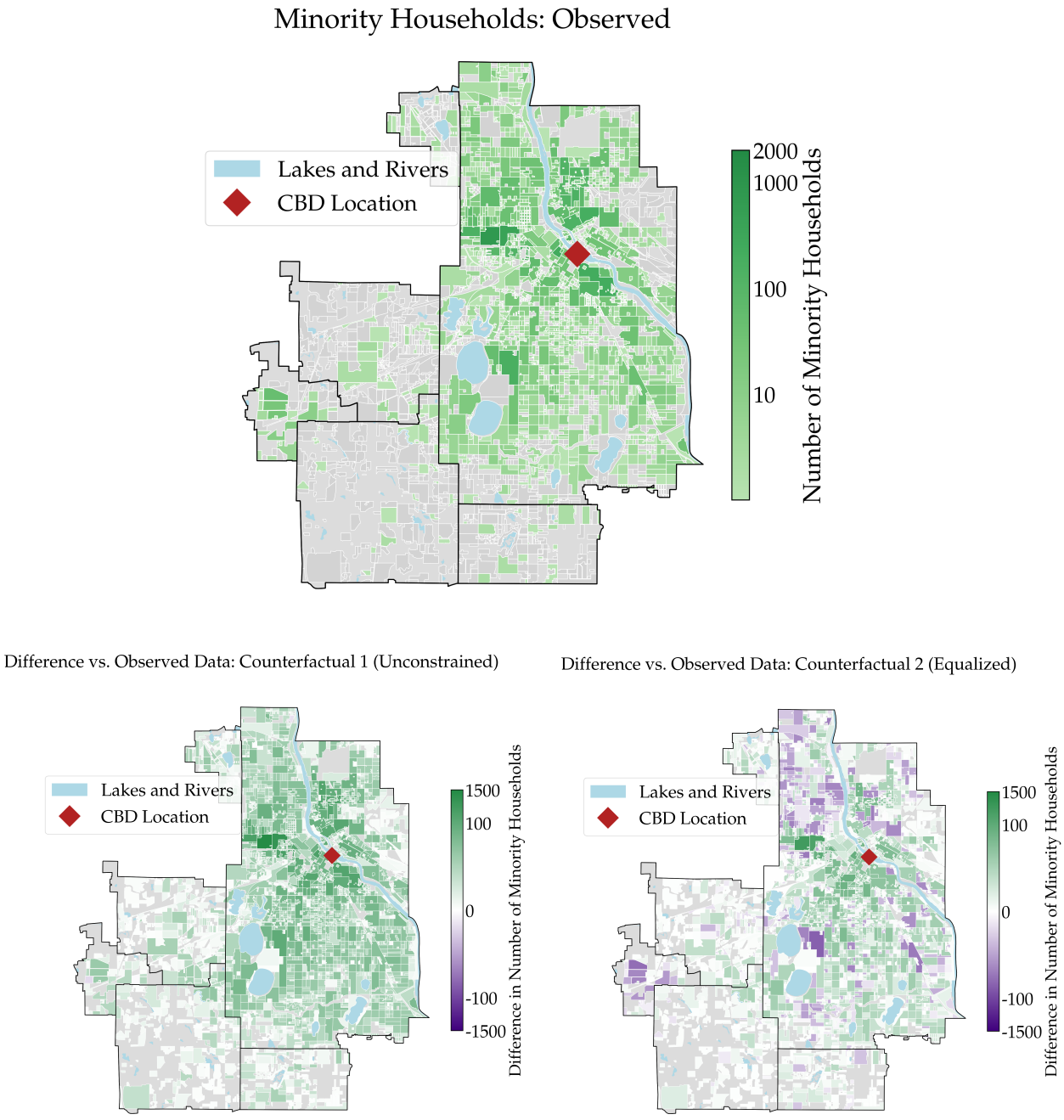


Note: This figure shows the distribution of minority and non-minority households across developments under observed equilibrium and two counterfactual scenarios.

In this section, we provide details on the counterfactual analysis aimed at understanding the impact of selective admissions on observed segregation. In the first counterfactual, we allow for open admission, meaning that both minority and non-minority households have a probability of one of being admitted to a development,i.e., there are no restrictions on the choice sets. In the second counterfactual, we equalize the admission rule parameters for minority households with those of non-minority households. In both of these counterfactual scenarios, we leave the demand parameter estimates for the two demographic groups unchanged.

It's important to note that both of these counterfactuals operate within a partial equilibrium framework. Therefore, as we do not allow housing rents to adjust in these counterfactuals, the population in the Minneapolis metro area—compared to outside options—and

Figure 6: Number of Minority Households and Differences under Counterfactuals



Note: This figure maps the distribution of minority households across developments under the 1940 observed equilibrium and the two counterfactual scenarios. CBD stands for central business district.

the re-sorting of the population within the metro do not consider potential adjustments in rental prices.

Figure 5 plots the arcsinh distribution of minority and non-minority households across neighborhoods observed in 1940, as well as under the simulations from the two counterfactual scenarios. The figure shows that the density of minority households spreads out across developments under both counterfactual scenarios compared to the observed equilibrium. This effect is more pronounced in counterfactual one, which involves no selective admissions (depicted in orange), than in counterfactual two, where selective admissions are equalized (represented in green). For non-minority households, the impact of the counterfactual one is less significant; however, there is still a slight shift to the right in the distribution compared to the observed distribution. It is important to note that, by design, there is no difference between the observed distribution and counterfactual two for non-minority households.

Figure 6 maps the number of minority households in the observed 1940 equilibrium (top panel) and the differences between the actual minority household count and the two counterfactual scenarios (bottom panel) across the developments in the Minneapolis metro area. In the first counterfactual scenario (bottom left), which assumes no selective admissions, the number of minority households increases across various developments in the metropolitan area, particularly near the city center, where a larger population of minority households already exists. In the second counterfactual scenario (bottom right), where minority households have the same selective admission probabilities as non-minority households, there is an overall increase in the number of minority households throughout the metro area. However, some developments closer to the city center experience a decline in the number of minority households.

The difference in the level of re-sorting (both loss and gain in certain developments) under the two counterfactual scenarios arises from the varying levels of increase in household types within the metro area under the two counterfactuals. In the first counterfactual, there is a significant increase in both non-minority and minority households—specifically, a 92.5% increase for non-minority households and a 137.9% increase for minority households. In contrast, the second counterfactual shows a more modest increase, with a 21.8%

rise in minority households, leading to more re-sorting compared to the first scenario. This increase in the metro-wide population is partially attributable to the partial equilibrium nature of our counterfactual analysis. Nevertheless, it's evident that there is some, if not complete, re-sorting under both counterfactuals that remove or reduce restrictions on neighborhood choice sets for the households, since the preference of households to avoid living with minority households also significantly contributed to the observed segregation in 1940.

7. Conclusion

In this paper, we investigate the extent to which observed segregation can be attributed to restrictions on neighborhood choice sets versus households' preferences. To do this, we have created a novel dataset that links historical census data with household choices and household characteristics with racial covenants, which were the primary *de jure* form of discrimination in the first half of the 20th century in the US. We provide evidence that racial covenants had a causal effect on racial and ethnic sorting in the Minneapolis metro area in 1940. Motivated by our evidence that restrictions created by covenants had their intended effect, we use a simplified model to demonstrate how ignoring choice set restrictions can bias preference parameters for neighborhood characteristics. Next, we propose a two-sided version of the traditional discrete choice models of residential choice, where we model restrictions arising from either implicit or explicit *de jure* discrimination, such as selective admissions on the supply side. We use our newly collected data to estimate our model, finding that minority households had lower probabilities of being admitted into developments, even after controlling for the share of covenants and rents. We also find that ignoring restrictions on choices would have resulted in biased estimates of the rent elasticities for non-minority households that face restrictions on their choice sets due to rent or financial constraints, a result that aligns with our theoretical predictions. Our counterfactuals suggest that removing or reducing restrictions on neighborhood choice sets for the households can result in significant, if not complete, re-sorting of minority households across developments.

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Segregation by Choice? Separating Preferences from Restrictions in Housing Markets

by Milena Almagro and Aradhya Sood

ONLINE APPENDIX

A. Data Appendix

A.1 Individual Count US 1940 Census Data

The individual-level census dataset is from Hennepin County, Minnesota, in the year 1940. The raw data files were obtained from IPUMS USA. The census data we obtain from IPUMS is full-count, i.e. there exists an observation for every individual surveyed by the Census Bureau in Hennepin County in 1940. Since we are mainly concerned with the housing market and location decisions, all measures that we use are household-level, determined by the variable for the head of the household. First, the raw data files were merged with enumeration district (ED) boundary files. This resulted in a dataset of 561,528 individual records in 1940. Then, data processing included the cleaning and standardizing coding of key demographic variables related to race, birthplace, occupation, home ownership, and house values. Additional variables relating to aggregate household composition were then generated, such as the number of family members We also incorporate census data on individuals' municipalities and location of residence 5 years ago to measure migration.

In order to classify the White immigrant households across four categories, we group these households into four categories based on their birthplace.

Northern Europe: Denmark; Finland; Iceland; Norway; Sweden; Scotland; Wales; United Kingdom; Ireland; Canada, Australia, New Zealand

Western Europe: Belgium; France; Liechtenstein; Luxembourg; Switzerland; Italy

Eastern and Southern Europe: Albania; Greece; Portugal; Spain; Bulgaria; Austria; Hungary; Poland; Romania; Yugoslavia; Central Europe; Estonia; Latvia; Other USSR/Russia

Other Regions: Turkey; Syria; Israel/Palestine; Lebanon; China; Japan; Korea; Indonesia; Philippines; India; Asia Minor; Asia (unspecified); Africa (unspecified); Puerto Rico; Mexico; Central America; Cuba; West Indies; South America; Unspecified

A.2 Assessor Office Data

We use the assessor data for all parcels in Minneapolis metro area in 2019. Extensive cleaning was done to standardize land use types, street names, and address formats. We utilize the parcel data both for collecting property-level characteristics (such as year built, development name etc.) and for matching with historic development maps. Each parcels was also spatially joined to the ED boundary files.

A.3 Historic Development Maps

We obtained development-level data on 6,800 separate developments located in the greater Minneapolis area of Hennepin County. Historical household addresses are assigned to a historical development built in 1940. The data is is first aggregated based on the 2019 Hennepin County Assesor dataset with relevant development information. We then use historical information including historical street grid to map historic developments. From the current day parcel dataset, we group parcels with the same development. This generate 5,730 non-contiguous current-day developments in the six municipalities for which we have streetgrid data. Based on the historical streetgrid information, we eliminate all the gaps between parcels within the same development and combine the nearby parcels into single polygons. Since many developments are still multipolygons (separated by other developments) even after reducing the gap between streets, we separate them into contiguous polygons, so that a development may have multiple subdevelopments (e.g. developments 1A, 1B, 1C). All developments that have at least one parcel built before 1940 are included in the dataset. Other current day developments are also retained because houses are torn down and rebuilt and current day assessor data would only record the latest building year. Based on current day information, we are able to recover about 6000 continguous developments. In addition, there can also be cases where there used be parcels but buildings were torn down but not rebuilt (e.g. houses torn down to built a highway). These geometries are manually created based on historical street grids, plat maps, or covenant geometry.

When imputing these historical graphs to fill in the remaining gaps, some historical

developments were added with no overlapping of the current data. Many, however, were overwritten in this process. We prioritized historical development additions over those originally generated from the 2019 data, and thus, many of these development geometries were altered or replaced entirely to fit a more accurate historical mapping. This leaves us with a final total of 5,257 contiguous developments, where 4,986 can be traced back to a modern-day development (although they were historically retrofitted in the data, possibly including their geometry) and 271 completely new historical additions. Including the industrial and infrastructural data discussed in previous sections, we finalize the dataset at this level by connecting it with wetland percentage, elevation, bedrock, and soil-related data.

A.4 Enumeration of District Boundaries

To analyze the data at fine geographic levels, we utilize census enumeration district (ED) boundary shapefiles for Hennepin County. These boundary files allow us to map the census microdata to spatial units for analysis. ED matching enables us to include ED fixed effects to conduct our main analysis at the subdivison level. Extensive data cleaning was performed to standardize the ED numbering systems across years and align the boundaries with other geographic datasets.

A.5 Amenity Measures

To measure proximity to industrial areas, we incorporate a shapefile of industrial-zoned land parcels. This dataset provides a 1940 land-use zoning map for the following areas only: St. Louis Park, Minneapolis, Edina, Richfield, Hopkins, and Robbinsdale. We are interested in distance to jobs from each development, so we calculate the distance to heavy and light industrial zones and commercial zones for each development. Distance from each residential parcel to the nearest industrial parcel was calculated.

A.6 Street and Transportation Networks

Historical street network data is used to geocode addresses and analyze spatial connectivity: We utilize data generously provided by Brown University's Map USA Database (Logan, 2017) and supplement the data with manually collected streetgrid data for Minneapolis in 1940, and the available suburbs at that time. We note that streetgrid data is only

Table A.1: Summary Statistics, 1940

Variables	P_{25}	P_{50}	P_{75}	Mean	SD	N
Panel A: Households						
No. of Household Members	2.00	3.00	4.00	3.17	2.00	164,653
No. of Working Household Members	2.00	2.00	2.00	1.79	0.41	164,653
Home Ownership Dummy	0.00	0.00	1.00	0.40	0.49	164,653
Married Dummy	0.00	1.00	1.00	0.71	0.45	164,653
House Rental Values (USD Monthly)	20.00	35.00	108.55	145.62	354.94	150,688
Panel B: Developments						
Number of Households	3.00	12.00	43.00	47.74	123.19	3,449
Size (km^2)	0.02	0.10	0.34	0.84	8.07	3,449
Minority Share	0.00	0.00	0.06	0.07	0.16	3,449
Mean Build Year	1908	1917	1925	1917	11	2,223
Commute Distance (m)	1	101	430	586	1203	3,449
Distance to CBD (m)	3326	5321	8402	6004	3371	3,449
Distance to Heavy Industry (m)	173	596	1557	1146	1460	3,449
Distance to Lake (m)	462	1014	1700	1212	959	3,449
Distance to Streetcar (m)	7	202	742	629	1009	3,449
% Wetlands	0.00	0.00	0.00	4.01	15.61	3,449
House Rental Values (USD Monthly)	48.42	114.70	221.76	192.11	351.64	3,381
Urbanization Index	0.80	0.96	0.98	0.84	0.23	3,382
Panel C: Enumeration Districts						
Number of Developments	4.00	7.00	11.00	8.85	8.10	470
Size (km^2)	1.70	2.39	4.53	6.15	21.97	470

Note: This table provides summary statistics at household level, development level, and enumernation district level.

available in six cities in Hennepin County: St. Louis Park, Minneapolis, Edina, Richfield, Hopkins, and Robbinsdale and proceed in adding those six cities to our Minneapolis data.

The street grid files were collected, digitized, and cleaned to extract street names, types (e.g. street, avenue, boulevard), and directional prefixes/suffixes. They also include interpolated address number ranges for geocoding.

A.7 Racial Covenants

Our key variable of interest is the prevalence of racially-restrictive covenants. We utilize a historical development-level dataset of all racial covenants executed in Hennepin County

from 1910-1955, linked to 2019 parcel boundaries. We obtain parcel information from the 2019 Hennepin County assessor data. We match the covenant information to parcels within each developmemt and aim to recover share of covenants within each development. Parcels are tagged with a binary variable indicating the presence of a racial covenant, as well as the date of covenant execution.

There are 25818 covenant observations in the entire Hennepin County, of which we identify 25165 unique covenant observations based on the following attributes: language used on covenants, addition, city, block, lot, grantor, and date of execution. Of those, there are 19,668 unique observations in the six cities we focus in this paper mentioned previously. Covenants are matched to parcels based on common attributes (Addition, City, Block, and Lot), and geographical location (spatial match). Around 90% of the covenants were matched with a relevant parcels. The remaining were matched manually (by GIS experts). Since the parcels and covenant can cover different geographic geometries, we allow for one parcel to be matched to multiple covenants if the parcel size is larger than the historical covenants, and we allow for multiple parcels to be matched to a single covenant if historically the covenant is larger than current parcel geography. After identifying which parcels have been covered under covenants and at which dates, we calculate the share of covenants in the developments at different time periods.

The covenants are then connected to our developments data to obtain parcel- and development-level information for each covenant in the six cities of focus within Hennepin County. Roughly 85% were matched via parcel-related attribute matches (also spatially, if there were multiple contiguous developments matched to a single covenant using the attributes). The remaining matches were completed with the assistance of a combination of location, historical development names, and parcel-level attributes.

A.8 Address Geocoding

To enable household-level analysis, we cleaned, geocoded, and matched 1940 households to historical (1940) developments. This process began with the raw input file of 1940 census addresses. It contained 180,424 household-level address records in Hennepin County with variables for the raw address string household ID. There were 164,578 raw addresses in six cities for which streetgrid datafiles were available. Extensive cleaning of the raw address

strings was conducted in order to extract house numbers, directional prefixes, street names, and street types. This involved a combination of string parsing, pattern matching, and fuzzy text matching using the cleaned street name data as a reference. After this, missing address components (especially house numbers) were imputed based on adjacent records. The raw file contained 75.1% coverage of addresses with full addresses before cleaning (street addresses and house number), which improved to 96.1% after cleaning. 95.6% of the data entries contained street address information before imputation, which improved to 96.9% after imputation. Meanwhile, 78.1% of entries contained house number information before imputation, which was improved to 98.3% after imputation.

Table A.2: Quality of Imputation

Address Information	Raw	%	After Imputation	%
Street Name	157,547	95.64	159,633	96.91
House Number	128,654	78.10	162,010	98.35
Full Addresses	123,768	75.13	158,355	96.13
Total	1164,578	100.00	164,578	100.00

Note: Full Addresses means this address contains both street name and house number information.

The cleaned addresses were then geocoded by matching with the parcel and street grid files. Matches were attempted based on ED, house number, street name, street type, and direction. Both exact and fuzzy matching was used. 40.6% addresses were matched using this process. The rest were given to GIS experts to manually geocode using historical streetgrid mapping data. The output of the geocoding process is a dataset containing the original household id, the cleaned and geocoded address fields, and the matched parcel identifier. Of the 164,578 addresses in our final cleaned dataset, we were able to geocode 72,882 (44%) ourselves prior to utilizing GIS experts.

The final step in the data construction process involved extensive spatial data processing to link all of the datasets together. Spatial joins between the parcel, ED boundary, street grid, industrial zoning, wetland geography, and covenant datasets were used to merge datasets. For each parcel, we calculated the percent of area overlapping each ED (for map-

ping between 1940 and 2019 geographies) and the distance to the nearest industrial-zoned parcel. Finally, data was aggregated from parcel-level data (land use, covenant status, etc.) to the development level based on the spatial crosswalks. The output is an development-level dataset for 1940 Hennepin County that contains, for each ED, the geometry defining the geographic boundary; the aggregate population and housing characteristics from the 1940 census microdata; parcel-level distributions of land use, property characteristics, and covenant prevalence; and spatial metrics like distance to industrial areas.

B. Additional Empirical Evidence

Table B.1: Robustness Checks – IV Estimates (Panels A–C)

Panel A: Using Shorter Time Period 1935-1940 for Instrument and Covenants			
	Non-White	WE Imm.	SE & EE Imm.
Share Covenanted	-0.041	0.066*	-0.104
	(0.029)	(0.040)	(0.085)
Observations	163,535	163,535	163,535
First-Stage F-Stat	48.1	48.1	48.1
Mean Dep. Variable	0.010	0.008	0.054
Panel B: Cumulative Covenants from 1910-1940 with Instrument from 1930-1940			
	Non-White	WE Imm.	SE & EE Imm.
Share Covenanted	-0.094***	0.056*	-0.121*
	(0.036)	(0.033)	(0.071)
Observations	163,535	163,535	163,535
First-Stage F-Stat	70.6	70.6	70.6
Mean Dep. Variable	0.010	0.008	0.054
Panel C: Varying the Buffer Zone for Constructing the Instrument			
	Non-White	WE Imm.	SE & EE Imm.
Share Covenanted	-0.149**	0.084*	-0.206*
	(0.060)	(0.051)	(0.113)
Observations	163,560	163,560	163,560
First-Stage F-Stat	39.3	39.3	39.3
Mean Dep. Variable	0.010	0.008	0.054

Note: This table plot parameter estimates from Equations 2 and 1. Non-White is an indicator for Black, Asian, or Native American households, regardless of their country of birth. White Western European and Eastern and Southern European Immigrants (Imm.) denotes race as White and birthplace in these regions. Refer to Appendix A.1 to see which countries are in each of these categories. Share Covenanted is the fraction of lots in a development with racial covenants. Robust standard errors in parentheses. Household controls include ownership tenure, occupation, marital status, and number of household members and working members. Location controls include log slope, wetlands share, and log distances to heavy-industry zoning, commercial zoning, lakes, and streetcars as well as distance to the central business district. There are also enumeration-district fixed effects. Panel A and B refer to a shorter (1935-1940) and longer (1910-1940) pre-period for urban frontier instrument, respectively. Panel C varies the buffer zone 500 – 2000 meters. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Robustness Checks – IV Estimates (Panels D–F)

Panel D: Build Year Control			
	Non-White	WE Imm.	SE & EE Imm.
Share Covenanted	-0.057	0.071	-0.198**
	(0.036)	(0.047)	(0.100)
Observations	147,309	147,309	147,309
First-Stage F-Stat	52.1	52.1	52.1
Mean Dep. Variable	0.010	0.008	0.054
Panel E: Alternative Matching of Households to Developments			
	Non-White	WE Imm.	SE & EE Imm.
Share Covenanted	-0.110**	0.066*	-0.145*
	(0.043)	(0.039)	(0.084)
Observations	163,535	163,535	163,535
First-Stage F-Stat	66.9	66.9	66.9
Mean Dep. Variable	0.010	0.008	0.054
Panel F: Placebo: Inverted Instrument			
	Non-White	WE Imm.	SE & EE Imm.
Share Covenanted	0.007	0.006	-0.139**
	(0.014)	(0.025)	(0.060)
Observations	163,908	163,908	163,908
First-Stage F-Stat	167.2	167.2	167.2
Mean Dep. Variable	0.010	0.008	0.054

Note: This table plot parameter estimates from Equations 2 and 1. Non-White is an indicator for Black, Asian, or Native American households, regardless of their country of birth. White Western European and Eastern and Southern European Immigrants (Imm.) denotes race as White and birthplace in these regions. Refer to Appendix A.1 to see which countries are in each of these categories. Share Covenanted is the fraction of lots in a development with racial covenants. Robust standard errors in parentheses. Household controls include ownership tenure, occupation, marital status, and number of household members and working members. Location controls include log slope, wetlands share, and log distances to heavy-industry zoning, commercial zoning, lakes, and streetcars as well as distance to the central business district. There are also enumeration-district fixed effects. Panel D takes the baseline model and add mean development build year as a control variable. Panel E runs the model with alternative matching between a small number of households and developments. Panel F is a placebo test that inverts the urban frontier index to build it facing away from the city center. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

C. Model Appendix

C.1 Proof of Theorem 1

Proof. By definition of $\tilde{\beta}$ and $\hat{\beta}$, we have the following inequalities:

$$\tilde{L}^m(\hat{\beta}^m) < \tilde{L}^m(\tilde{\beta}^m), \quad L^m(\tilde{\beta}^m) < L^m(\hat{\beta}^m)$$

Combining both inequalities, we get the following equivalence relations:

$$\begin{aligned}
&\tilde{L}^m(\hat{\beta}^m) - L^m(\hat{\beta}^m) < \tilde{L}^m(\tilde{\beta}^m) - L^m(\tilde{\beta}^m) \iff \\
&\sum_{j\in\mathcal{J}0} N_j \log\Big(\frac{\tilde{\mathbb{P}}j(\hat{\beta}^m)}{\mathbb{P}j(\hat{\beta}^m)}\Big) < \sum_{j\in\mathcal{J}0} N_j \log\Big(\frac{\tilde{\mathbb{P}}j(\tilde{\beta}^m)}{\mathbb{P}j(\tilde{\beta}^m)}\Big) \iff \\
&\sum_{j\in\mathcal{J}0} N_j \log\Big(\frac{\sum_{k\in\mathcal{J}0}\exp(\hat{\beta}^m \log p_k)}{\sum_{k\in\mathcal{J}}\exp(\hat{\beta}^m \log p_k)}\Big) < \sum_{j\in\mathcal{J}0} N_j \log\Big(\frac{\sum_{k\in\mathcal{J}0}\exp(\tilde{\beta}^m \log p_k)}{\sum k\in\mathcal{J}\exp(\tilde{\beta}^m \log p_k)}\Big) \iff \\
&\log\Big(\frac{\sum_{k\in\mathcal{J}0}\exp(\hat{\beta}^m \log p_j)}{\sum_{k\in\mathcal{J}}\exp(\hat{\beta}^m \log p_k)}\Big) < \log\Big(\frac{\sum_{k\in\mathcal{J}0}\exp(\tilde{\beta}^m \log p_k)}{\sum_{k\in\mathcal{J}}\exp(\tilde{\beta}^m \log p_k)}\Big) \iff \\
&\frac{\sum_{j\in\mathcal{J}_0}\exp(\hat{\beta}^m \log p_j)}{\sum_{j\in\mathcal{J}}\exp(\hat{\beta}^m \log p_j)} < \frac{\sum_{j\in\mathcal{J}_0}\exp(\tilde{\beta}^m \log p_j)}{\sum_{j\in\mathcal{J}}\exp(\tilde{\beta}^m \log p_j)}
\end{aligned}$$

Define the following function:

$$F(\beta) = \sum_{j\in\mathcal{J}_0} \tilde{\mathbb{P}}_j(\beta) = \frac{\sum_{j\in\mathcal{J}_0}\exp\left(\beta \log p_j\right)}{\sum_{j\in\mathcal{J}}\exp(\beta \log p_j)}.$$

Under this definition, the above inequalities can be written as:

$$F(\hat{\beta}) < F(\tilde{\beta}).$$

Taking derivatives of $F(\beta)$, we get:

$$\begin{aligned}\frac{\partial F(\beta)}{\partial \beta} &= \sum_{j\in\mathcal{J}_0} \tilde{\mathbb{P}}_j(\beta)\Big\{\log p_j - \sum_{j\in\mathcal{J}} \tilde{\mathbb{P}}_j(\beta)\log p_j\Big\} \\ &= \sum_{j\in\mathcal{J}_0} \tilde{\mathbb{P}}_j(\beta)\log p_j - F(\beta)\sum_{j\in\mathcal{J}} \tilde{\mathbb{P}}_j(\beta)\log p_j.\end{aligned}$$

From the last expression, it is easy to see that[1]

$$\frac{\partial F(\beta)}{\partial \beta} < 0 \iff \sum_{j\in\mathcal{J}_0} \mathbb{P}_j(\beta)\log p_j - \sum_{j\in\mathcal{J}} \tilde{\mathbb{P}}_j(\beta)\log p_j < 0$$

Observe that

$$\begin{aligned}&\sum_{j\in\mathcal{J}_0} \mathbb{P}_j(\beta)\log p_j - \sum_{j\in\mathcal{J}} \tilde{\mathbb{P}}_j(\beta)\log p_j \\ =& \sum_{j\le j_1, j\in\mathcal{J}_0} [\mathbb{P}_j(\beta) - \tilde{\mathbb{P}}_j(\beta)]\log p_j + \sum_{j_1<j<q_1, j\in\mathcal{J}_0} [\mathbb{P}_j(\beta) - \tilde{\mathbb{P}}_j(\beta)]\log p_j \\ &- \sum_{j_1\le q\le q_1, q\in\mathcal{J}_1} \tilde{\mathbb{P}}_q(\beta)\log p_q - \sum_{q>q_1, q\in\mathcal{J}_1} \tilde{\mathbb{P}}_q(\beta)\log p_q \\ <& \sum_{j\le j_1, j\in\mathcal{J}_0} [\mathbb{P}_j(\beta) - \tilde{\mathbb{P}}_j(\beta)]\log p_j + \sum_{j_1<j<q_1, j\in\mathcal{J}_0} [\mathbb{P}_j(\beta) - \tilde{\mathbb{P}}_j(\beta)]\log p_j \\ &- \sum_{j_1\le q\le q_1, q\in\mathcal{J}_1} \tilde{\mathbb{P}}_q(\beta)\log p_{j_1} - \sum_{q>q_1, q\in\mathcal{J}_1} \tilde{\mathbb{P}}_q(\beta)\max_{j\in\mathcal{J}_0}\log p_j\end{aligned}$$

The inequality follows from assumption 1.

The probability should satisfy

$$\sum_{j\le j_1, j\in\mathcal{J}_0} [\mathbb{P}_j(\beta) - \tilde{\mathbb{P}}_j(\beta)] + \sum_{j_1<j<q_1, j\in\mathcal{J}_0} [\mathbb{P}_j(\beta) - \tilde{\mathbb{P}}_j(\beta)] = \sum_{j_1\le q\le q_1, q\in\mathcal{J}_1} \tilde{\mathbb{P}}_q(\beta) + \sum_{q>q_1, q\in\mathcal{J}_1} \tilde{\mathbb{P}}_q(\beta)$$

[1]Notice that $\frac{\tilde{\mathbb{P}}_j(\beta)}{F(\beta)} = \frac{\tilde{\mathbb{P}}_j(\beta)}{\sum_{k\in\mathcal{J}_0}\tilde{\mathbb{P}}_k(\beta)} = \mathbb{P}_j(\beta)$.

Replace $\sum_{q>q_1,q\in\mathcal{J}_1}\tilde{\mathbb{P}}_q(\beta)$ in the above inequality, we get

$$
\begin{aligned}
&\sum_{j\in\mathcal{J}_0}\mathbb{P}_j(\beta)\log p_j-\sum_{j\in\mathcal{J}}\tilde{\mathbb{P}}_j(\beta)\log p_j\\
<&\sum_{j\le j_1,j\in\mathcal{J}_0}[\mathbb{P}_j(\beta)-\tilde{\mathbb{P}}_j(\beta)][\log p_j-\max_{j\in\mathcal{J}_0}\log p_j]+\sum_{j_1<j<q_1,j\in\mathcal{J}_0}[\mathbb{P}_j(\beta)-\tilde{\mathbb{P}}_j(\beta)][\log p_j-\max_{j\in\mathcal{J}_0}\log p_j]\\
&-\sum_{j_1\le q\le q_1,q\in\mathcal{J}_1}\tilde{\mathbb{P}}_q(\beta)[\log p_{j_1}-\max_{j\in\mathcal{J}_0}\log p_j]\\
<&\sum_{j_1<j<q_1,j\in\mathcal{J}_0}[\mathbb{P}_j(\beta)-\tilde{\mathbb{P}}_j(\beta)][\log p_j-\max_{j\in\mathcal{J}_0}\log p_j]\\
&+\Big\{\sum_{j\le j_1,j\in\mathcal{J}_0}[\mathbb{P}_j(\beta)-\tilde{\mathbb{P}}_j(\beta)]-\sum_{j_1\le q\le q_1,q\in\mathcal{J}_1}\tilde{\mathbb{P}}_q(\beta)\Big\}[\log p_{j_1}-\max_{j\in\mathcal{J}_0}\log p_j]
\end{aligned}
$$

The first term is negative[2]. If we show the terms in the brace is negative, then the above formula is negative.

Recall that $\mathbb{P}_j(\beta)=\frac{\exp(\beta\log p_j)}{\sum_{j\in\mathcal{J}_0}\exp(\beta\log p_j)}$ and $\tilde{\mathbb{P}}_j(\beta)=\frac{\exp(\beta\log p_j)}{\sum_{j\in\mathcal{J}}\exp(\beta\log p_j)}$, the terms in the brace can be written as:

$$
\begin{aligned}
&\sum_{j\le j_1,j\in\mathcal{J}_0}[\mathbb{P}_j(\beta)-\tilde{\mathbb{P}}_j(\beta)]-\sum_{j_1\le q\le q_1,q\in\mathcal{J}_1}\tilde{\mathbb{P}}_q(\beta)\\
=&\sum_{j<j_1,j\in\mathcal{J}_0}\frac{\exp(\beta\log p_j)}{\sum_{j\in\mathcal{J}_0}\exp(\beta\log p_j)}-\sum_{j<j_1,j\in\mathcal{J}_0}\frac{\exp(\beta\log p_j)}{\sum_{j\in\mathcal{J}}\exp(\beta\log p_j)}-\sum_{j_1\le q\le q_1,q\in\mathcal{J}_1}\frac{\exp(\beta\log p_q)}{\sum_{j\in\mathcal{J}}\exp(\beta\log p_j)}\\
=&\frac{\sum_{j<j_1,j\in\mathcal{J}_0}\exp(\beta\log p_j)\sum_{q\in\mathcal{J}_1}\exp(\beta\log p_q)-\sum_{j_1\le q\le q_1,q\in\mathcal{J}_1}\exp(\beta\log p_q)\sum_{j\in\mathcal{J}_0}\exp(\beta\log p_j)}{\sum_{j\in\mathcal{J}_0}\exp(\beta\log p_j)\sum_{j\in\mathcal{J}}\exp(\beta\log p_j)}\\
=&\frac{\sum_{j<j_1,j\in\mathcal{J}_0}\exp(\beta\log p_j)\sum_{q>q_1,q\in\mathcal{J}_1}\exp(\beta\log p_q)}{\sum_{j\in\mathcal{J}_0}\exp(\beta\log p_j)\sum_{j\in\mathcal{J}}\exp(\beta\log p_j)}\\
&-\frac{\sum_{j_1\le q\le q_1,q\in\mathcal{J}_1}\exp(\beta\log p_q)\sum_{j_1\le j\le q_1,j\in\mathcal{J}_0}\exp(\beta\log p_j)}{\sum_{j\in\mathcal{J}_0}\exp(\beta\log p_j)\sum_{j\in\mathcal{J}}\exp(\beta\log p_j)}\\
=&\sum_{j<j_1,j\in\mathcal{J}_0}\mathbb{P}_j(\beta)\sum_{q>q_1,q\in\mathcal{J}_1}\tilde{\mathbb{P}}_q(\beta)-\sum_{j_1\le j\le q_1,j\in\mathcal{J}_0}\mathbb{P}_j(\beta)\sum_{j_1\le q\le q_1,q\in\mathcal{J}_1}\tilde{\mathbb{P}}_q(\beta)\\
=&\sum_{q>q_1,q\in\mathcal{J}_1}\tilde{\mathbb{P}}_q(\beta)-\sum_{j_1\le j\le q_1,j\in\mathcal{J}_0}\mathbb{P}_j(\beta)\sum_{q\in\mathcal{J}_1}\tilde{\mathbb{P}}_q(\beta)
\end{aligned}
$$

[2]Note that for all $j\in\mathcal{J}_0$, $\mathbb{P}_j(\beta)>\tilde{\mathbb{P}}_j(\beta)$ for all β

The last line can be written into:

$$\begin{aligned}
&\sum_{q>q_1, q\in\mathcal{J}_1} \tilde{\mathbb{P}}_k(\beta) - \sum_{j_1\le j\le q_1, j\in\mathcal{J}_0} \mathbb{P}_j(\beta) \sum_{q\in\mathcal{J}_1} \tilde{\mathbb{P}}_q(\beta) \\
&= \sum_{q\in\mathcal{J}_1} \tilde{\mathbb{P}}_q(\beta)[\frac{\sum_{q>q_1, q\in\mathcal{J}_1} \tilde{\mathbb{P}}_q(\beta)}{\sum_{q\in\mathcal{J}_1} \tilde{\mathbb{P}}_q(\beta)} - \sum_{j_1\le j\le q_1, j\in\mathcal{J}_0} \mathbb{P}_j(\beta)] \\
&= \sum_{q\in\mathcal{J}_1} \tilde{\mathbb{P}}_q(\beta)[\sum_{q>q_1, q\in\mathcal{J}_1} \overline{\mathbb{P}}_q(\beta) - \sum_{j_1\le j\le q_1, j\in\mathcal{J}_0} \mathbb{P}_j(\beta)]
\end{aligned}$$

where $\overline{\mathbb{P}}_q(\beta) = \frac{\exp(\beta \log p_q)}{\sum_{q'\in\mathcal{J}_1} \exp(\beta \log p_{q'})}$.

It is clear that:

$$\sum_{q>q_1, q\in\mathcal{J}_1} \overline{\mathbb{P}}_q(\beta) < \sum_{j_1\le j\le q_1, j\in\mathcal{J}_0} \mathbb{P}_j(\beta) \iff \sum_{j\in\mathcal{J}_0} \mathbb{P}_j(\beta) \log p_j - \sum_{j\in\mathcal{J}} \tilde{\mathbb{P}}_j(\beta) \log p_j < 0 \iff \tilde{\beta} < \hat{\beta}$$

For $\beta \in [0, \overline{\beta})$, we have:

$$\begin{aligned}
\sum_{j_1\le q\le q_1, q\in\mathcal{J}_1} \overline{\mathbb{P}}_q(\beta) &= \frac{\sum_{j_1\le q\le q_1, q\in\mathcal{J}_1} \exp(\beta \log p_q)}{\sum_{q\in\mathcal{J}_1} \exp(\beta \log p_q)} > \frac{N_1^s \exp(\beta \log p_q^{min})}{N_1 \exp(\beta \log p_q^{max})} \\
\sum_{j<j_1, j\in\mathcal{J}_0} \mathbb{P}_j(\beta) &= \frac{\sum_{j<j_1, j\in\mathcal{J}_0} \exp(\beta \log p_j)}{\sum_{j\in\mathcal{J}_0} \exp(\beta \log p_j)} < \frac{N_0^s \exp(\beta \log p_{j_1})}{N_0 \exp(\beta \log p_j^{min})} \\
\frac{N_1^s \exp(\beta \log p_q^{min})}{N_1 \exp(\beta \log p_q^{max})} \Big/ \frac{N_0^s \exp(\beta \log p_{j_1})}{N_0 \exp(\beta \log p_q^{min})} &= \frac{N_1^s N_0}{N_1 N_0^s} \exp\big(\beta \log \frac{p_q^{min}}{p_q^{max}} \frac{p_j^{min}}{p_{j_1}}\big) \\
&> \frac{N_1^s N_0}{N_1 N_0^s} \exp\big(\frac{\log \frac{N_0^s}{N_0} \frac{N_1}{N_1^s}}{\log \frac{p_q^{min}}{p_q^{max}} \frac{p_j^{min}}{p_{j_1}}} \log \frac{p_q^{min}}{p_q^{max}} \frac{p_j^{min}}{p_{j_1}}\big) \\
&> 1
\end{aligned}$$

So $\sum_{j_1\le q\le q_1, q\in\mathcal{J}_1} \overline{\mathbb{P}}_q(\beta) > \sum_{j<j_1, j\in\mathcal{J}_0} \mathbb{P}_j(\beta)$ for $\beta \in [0, \overline{\beta})$. This is equivalent to $\sum_{q>q_1, q\in\mathcal{J}_1} \overline{\mathbb{P}}_q(\beta) < \sum_{j_1\le j\le q_1, j\in\mathcal{J}_0} \mathbb{P}_j(\beta)$ for $\beta \in [0, \overline{\beta})$.

For $\beta \in (\underline{\beta}, 0)$, we have:

$$\sum_{j_1 \le q \le q_1, q \in \mathcal{J}_1} \overline{\mathbb{P}}_q(\beta) = \frac{\sum_{j_1 \le q \le q_1, q \in \mathcal{J}_1} \exp(\beta \log p_q)}{\sum_{q \in \mathcal{J}_1} \exp(\beta \log p_q)} > \frac{N_1^s \exp(\beta \log p_{q_1})}{N_1 \exp(\beta \log p_q^{min})}$$

$$\sum_{j < j_1, j \in \mathcal{J}_0} \mathbb{P}_j(\beta) = \frac{\sum_{j < j_1, j \in \mathcal{J}_0} \exp(\beta \log p_j)}{\sum_{j \in \mathcal{J}_0} \exp(\beta \log p_j)} < \frac{N_0^s \exp(\beta \log p_j^{min})}{N_0 \exp(\beta \log p_j^{max})}$$

$$\begin{aligned}
\frac{N_1^s \exp(\beta \log p_{q_1})}{N_1 \exp(\beta \log p_q^{min})} \Big/ \frac{N_0^s \exp(\beta \log p_j^{min})}{N_0 \exp(\beta \log p_j^{max})} &= \frac{N_1^s N_0}{N_1 N_0^s} \exp\left(\beta \log \frac{p_{q_1}}{p_q^{min}} \frac{p_j^{max}}{p_j^{min}}\right) \\
&> \frac{N_1^s N_0}{N_1 N_0^s} \exp\left(\frac{\log \frac{N_0^s}{N_0} \frac{N_1}{N_1^s}}{\log \frac{p_{q_1}}{p_q^{min}} \frac{p_j^{max}}{p_j^{min}}} \log \frac{p_{q_1}}{p_q^{min}} \frac{p_j^{max}}{p_j^{min}}\right) \\
&> 1
\end{aligned}$$

So $\sum_{q > q_1, q \in \mathcal{J}_1} \overline{\mathbb{P}}_q(\beta) < \sum_{j_1 \le j \le q_1, j \in \mathcal{J}_0} \mathbb{P}_j(\beta)$ also holds for $\beta \in (\underline{\beta}, 0)$.

So within the range of $(\underline{\beta}, \overline{\beta})$, the inequality $\sum_{q > q_1, q \in \mathcal{J}_1} \overline{\mathbb{P}}_q(\beta) < \sum_{j_1 \le j \le q_1, j \in \mathcal{J}_0} \mathbb{P}_j(\beta)$ holds, so we have $\tilde{\beta} < \hat{\beta}$. □

D. Identification and Estimation Details

D.1 Identification under parametric restrictions

To show that a model with parametric restrictions without shifters is identified, we need to show that there's an injective mapping from the parameters to the data. In what follows, we show that such an injective mapping exists.

First, to build on intuition, note that in a model with full consideration where errors follow a logit distribution, the following equality holds:

$$s_j = \exp(\beta X_j^d - \beta X_{j'}^d) s_{j'}, \tag{8}$$

for all j and j', which follows from the independence of irrelevant alternatives property (IIA) implied by logit errors. In other words, under logit errors, the following moments should hold:

$$\mathbb{E}[\frac{s_j}{s_{j+1}}|\mathbf{X}_1, ..., \mathbf{X}_J] = \mathbb{E}[\frac{s_j}{s_{j+1}}|X_j^d, X_{j+1}^d] \implies \mathbb{E}\left[\left(\frac{s_j}{s_{j+1}} - \mathbb{E}[\frac{s_j}{s_{j+1}} \mid X_j^d, X_{j+1}^d]\right) \cdot h(\mathbf{X}_1, ..., \mathbf{X}_J)\right] = 0,$$

for all h such that $\mathbb{E}|h(\mathbf{X}_1, \mathbf{X}_2)| < \infty$, where $\mathbf{X}_j = (X_j^s, X_j^d)$.

We now consider a model without full consideration. We start with a simplified example to build on intuition, where we only have one demographic group, with utility given by $v_{ij} = \beta X_j^d + \epsilon_{ij}$, and an admission rule based on a scalar X_j^s:

$$\eta_i \geq \alpha X_j^s.$$

Let's assume $\alpha \geq 0$.[3] Under this monotonicity condition, we know that we can order developments according to increasing X_j^s:

$$X_1^s \leq ... \leq X_J^s.$$

[3] It is straightforward to extend the argument when $\alpha \leq 0$.

Given this ordering, we know that:

$$
\begin{aligned}
s_j &= \mathbb{P}(s_{ij} = 1|\boldsymbol{\delta}, \boldsymbol{\beta}, \boldsymbol{\alpha}) \\
&= \sum_{n \geq j}^{J} \left(F_\eta\left(\alpha X^s_{n+1}\right) - F_\eta\left(\alpha X^s_n\right) \right) \frac{\exp(\beta X^d_j)}{\sum_{j'=1}^{n} \exp(\beta X^d_{j'}) + 1} \\
&= \sum_{n \geq j+1}^{J} \left(F_\eta\left(\alpha X^s_{n+1}\right) - F_\eta\left(\alpha X^s_n\right) \right) \frac{\exp(\beta X^d_j))}{\sum_{j'=1}^{n} \exp(\beta X^d_{j'}) + 1} + \left(F_\eta\left(\alpha X^s_{j+1}\right) - F_\eta\left(\alpha X^s_j\right) \right) \frac{\exp(\beta X^d_j)}{\sum_{j'=1}^{j} \exp(\beta X^d_{j'}) + 1} \\
&= s_{j+1} \exp\left(\beta(X^d_j - X^d_{j+1})\right) + \left(F_\eta\left(\alpha X^s_{j+1}\right) - F_\eta\left(\alpha X^s_j\right) \right) \frac{\exp(\beta X^d_j)}{\sum_{j'=1}^{j} \exp(\beta X^d_{j'}) + 1}. \qquad (9)
\end{aligned}
$$

Because we assume that the outside is always included the choice set, we can also obtain the following expression for all $j = 1, ..., J$:

$$
s_0 = s_j \exp(-\beta X^d_j) + \sum_{n=1}^{j-1} \left(F_\eta\left(\alpha X^s_{n+1}\right) - F_\eta\left(\alpha X^s_n\right) \right) \frac{1}{\sum_{j'=1}^{n} \exp(\beta X^d_{j'}) + 1}.
$$

Comparing the previous expressions with (8), we see that, given our assumption of logit errors, any violations to IIA in the data arise because of lack of full consideration that is captured by the second summand of expression (8). By contrast, a model without full consideration with logit errors:

$$
\mathbb{E}[\frac{s_j}{s_{j+1}} | \mathbf{X}_1, ..., \mathbf{X}_J] = \mathbb{E}[\frac{s_j}{s_{j+1}} | X^s_j, X^s_{j+1}, X^d_1, ..., X^d_{j+1}],
$$

which implies the following moments

$$
\mathbb{E}\left[\left(\frac{s_j}{s_{j+1}} - \mathbb{E}[\frac{s_j}{s_{j+1}} \mid X^s_j, X^s_{j+1}, X^d_1, ..., X^d_{j+1}] \right) \cdot h(\mathbf{X}_1, ..., \mathbf{X}_J) \right] = 0
$$

for all h such that $\mathbb{E}|h(\mathbf{X}_1, ..., \mathbf{X}_J)| < \infty$.

Next, observe that that we can extend equation (9) to all j, forming a system of $J + 1$

equations in the observed shares of matches $\{s_j\}_{j=1}^J$:

$$
\begin{aligned}
s_0 &= s_1 \exp(-\beta X_1^d) + F_\eta\left(\alpha X_1^s\right) \\
&\vdots \\
s_j &= s_{j+1} \exp\left(\beta(X_j^d - X_{j+1}^d)\right) + \left(F_\eta\left(\alpha X_{j+1}^s\right) - F_\eta\left(\alpha X_j^s\right)\right) \frac{\exp(\beta X_j^d)}{\sum_{j'=1}^j \exp(\beta X_{j'}^d) + 1} \\
&\vdots \\
s_J &= \left(1 - F_\eta\left(\alpha X_J^s\right)\right) \frac{\exp(\beta X_J^d)}{\sum_{j'=1}^J \exp(\beta X_{j'}^d) + 1}.
\end{aligned}
$$

The system above shows that, under a specific functional form assumption on F_η, we can identify the parameters α and β, as we have a system of $J+1$ equations and two unknown variables. In this simplified model with two parameters, having only one location and the outside option is enough. Generalizing this argument, we can identify up to $J+1$ parameters using the previous system of equations.

D.1.1 Adding match-specific admission rule unobserved heterogeneity.

Finally, we can write a similar system of equations when we add match-specific admission rule unobserved heterogeneity. Assume for simplicity our baseline model with only one group and one covariate entering in each side of the market. Assume that the admission rule is given by

$$\eta_{ij} \geq \alpha X_j^s.$$

In this more general model, we can write the probability of having choice set C as follows:

$$\mathbb{P}(C) = \prod_{j \in C} F_\eta(\alpha X_j^s) \prod_{j \notin C} (1 - F_\eta(\alpha X_j^s)),$$

where η_{ij} is i.i.d. distributed according to F_η. Following a similar argument as above, we have that for all j:

$$\begin{aligned} s_0 &= s_j \exp(-\beta(X_j^d)) + \sum_{\{C|j\notin C\}} \mathbb{P}(C) \frac{1}{1+\sum_{k\in C}\exp(\beta X_k^d)} \\ &= s_j \exp(-\beta(X_j^d)) + \sum_{\{C|j\notin C\}} \prod_{j'\in C} F_\eta(\alpha X_{j'}^s) \prod_{j'\notin C}(1-F_\eta(\alpha X_{j'}^s)) \frac{1}{1+\sum_{k\in C}\exp(\beta X_k^d)}. \end{aligned}$$

Observe that under parametric assumptions of F_η, we have a system of J equations in two unknowns, α and β. The previous expression implies:

$$\frac{\partial}{\partial X_j^s}\frac{s_0}{s_j} = -\prod_{j'\in C} F_\eta(\alpha X_{j'}^s) \prod_{j'\notin C, j'\neq j}(1-F_\eta(\alpha X_{j'}^s)) \frac{1}{1+\sum_{k\in C}\exp(\beta X_k^d)} f_\eta(\alpha X_j^s) X_j^s < 0$$

D.2 Shift-Share Instrument Construction

To address the potential endogeneity in minority household location decisions, we construct a modified version of the shift-share instrument following Boustan and Tabellini. The instrument combines three key components: predicted city growth rates from non-Minnesota metropolitan areas, historical migrant settlement patterns, and immigration quotas from the Immigration Act of 1924. We use the 1930 and 1940 IPUMS ACS censuses to construct our instrument. First, we calculate the predicted 1940 population for each of the six cities in our sample, c, using the average growth rate of non-Minnesota metropolitan areas V between 1930 and 1940:

$$\hat{P}_{c,1940} = P_{c,1930}(1 + g_{1930-1940}) \tag{10}$$

where $g_{1930-1940}$ represents the average population growth rate across all non-Minnesota metropolitan areas.

The 1924 quota gives annual quota numbers for a set of sending regions R. For each sending region $r \in R$ and city c, we calculate historical settlement patterns using 1930 data:

$$\omega_{cr} = \frac{M_{cr,1930}}{\sum_{i\in\{V\cup c\}} M_{ir,1930}} \tag{11}$$

where $M_{cr,1930}$ represents the migrant population from region r in city c in 1930.

We then predict the number of new migrants in each city by combining these settlement patterns with immigration quotas:

$$\hat{M}_{c,1940} = \frac{\sum_r \omega_{cr} \cdot Q_r \cdot 10}{h_c} \tag{12}$$

where Q_r represents the annual quota for region r under the Immigration Act of 1924, multiplied by 10 to cover the decade. h_c represents the average number of residents per household in city c in 1930, as we convert from the individual count of the quota to the unit of measure for this paper, the household.

For the addition-level instrument, we distribute the predicted city-level migrants across additions based on each addition's share of city households:

$$\hat{M}_{ac,1940} = \hat{M}_{c,1940} \cdot \frac{H_{ac,1940}}{\sum_a H_{ac,1940}} \tag{13}$$

where $H_{ac,1940}$ represents the household count in addition a of city c.

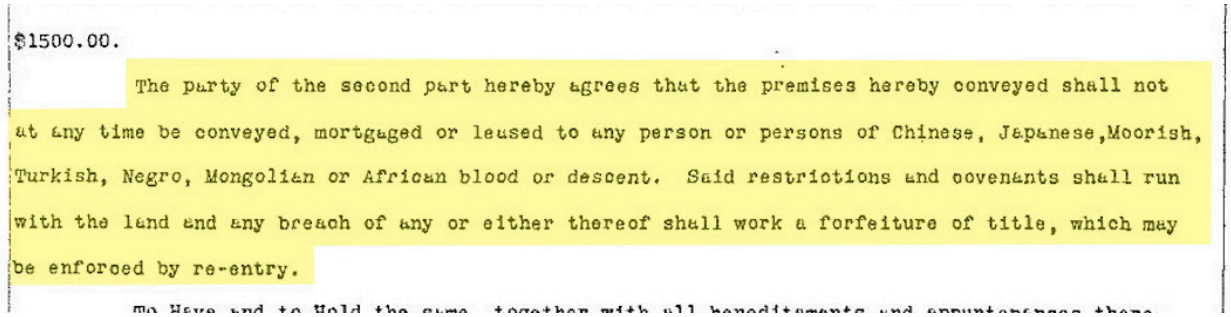
Finally, we construct our addition-level instrument as:

$$Z_{ac,1940} = \frac{M_{ac,1930} + \hat{M}_{ac,1940}}{\hat{P}_{ac,1940}} \tag{14}$$

where $M_{ac,1930}$ represents the existing migrant population in addition a of city c in 1930, and $\hat{P}_{ac,1940}$ is the predicted 1940 population of the addition, calculated by distributing the predicted city population proportionally to addition household counts. This instrument satisfies the exclusion restriction as it relies on two plausibly exogenous components: national immigration quotas that were set without consideration of local conditions, and historical settlement patterns from 1930 that are unlikely to be correlated with addition-level changes in the 1930s after controlling for city characteristics.

E. Additional Figures

Figure E.1: Example 1 of a Racial Covenant Deed

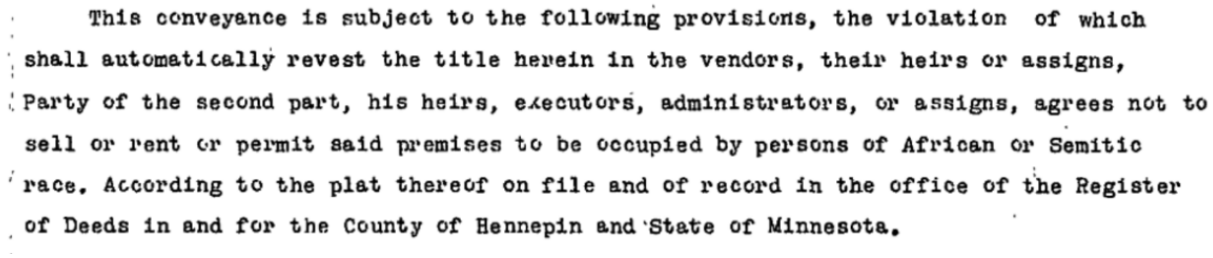


$1500.00.

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.

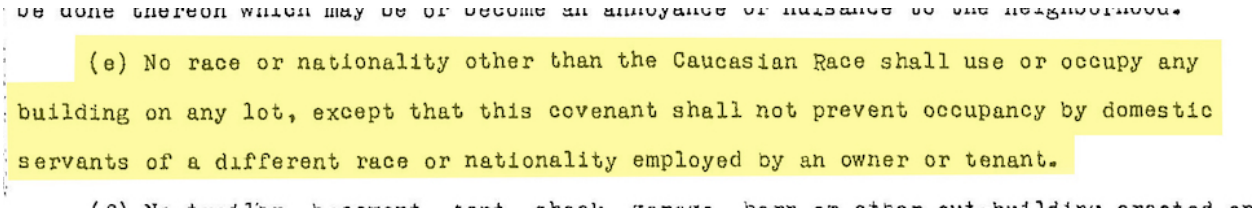
To Have and to Hold the same, together with all hereditaments and appurtenances there-

Figure E.2: Example 2 of a Racial Covenant Deed



This conveyance is subject to the following provisions, the violation of which shall automatically revest the title herein in the vendors, their heirs or assigns, Party of the second part, his heirs, executors, administrators, or assigns, agrees not to sell or rent or permit said premises to be occupied by persons of African or Semitic race. According to the plat thereof on file and of record in the office of the Register of Deeds in and for the County of Hennepin and State of Minnesota.

Figure E.3: Example 3 of a Racial Covenant Deed



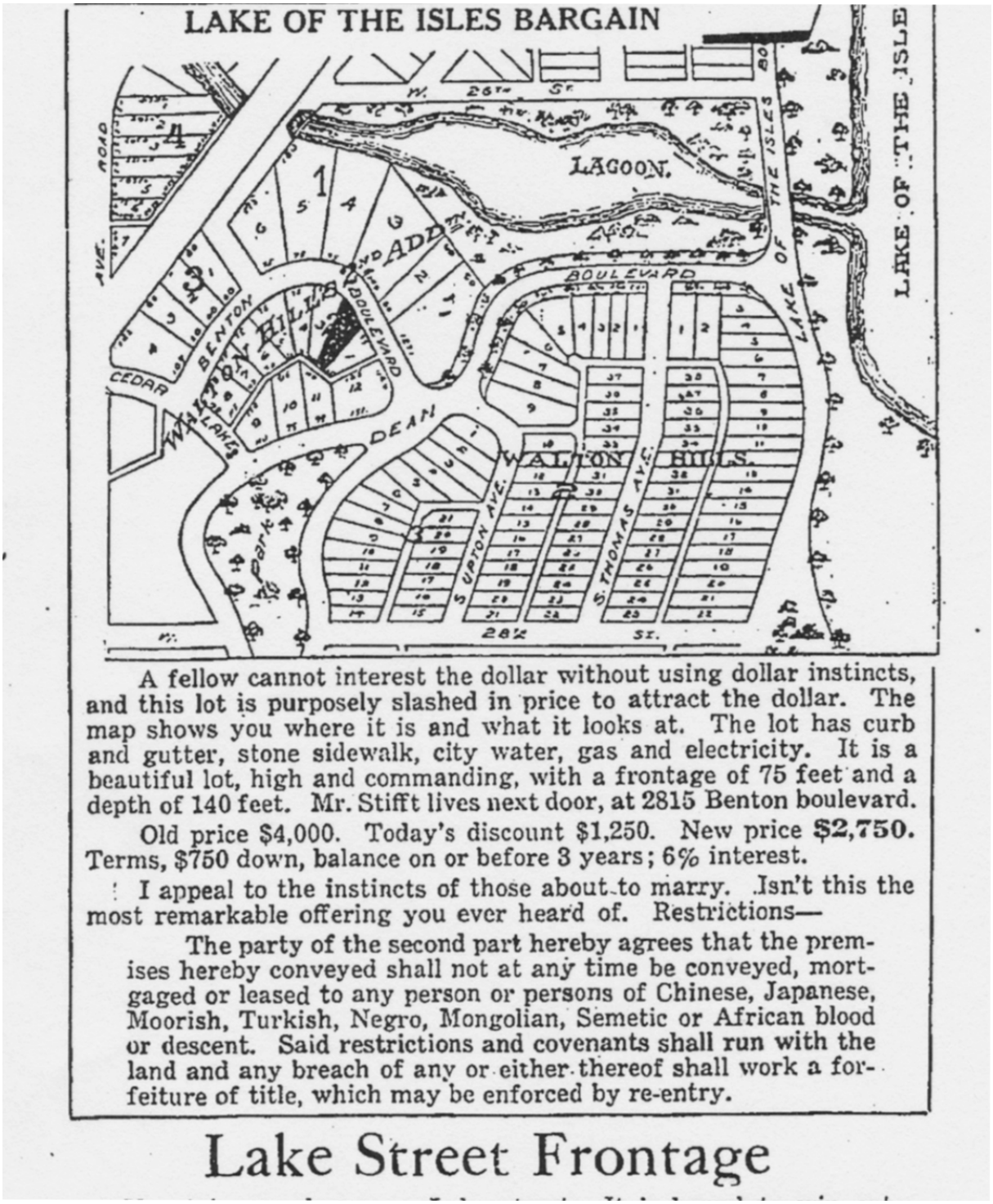
be done thereon which may be or become an annoyance or nuisance to the neighborhood.

(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.

(f) No trailer, basement, tent, shack, garage, barn or other out building erected on

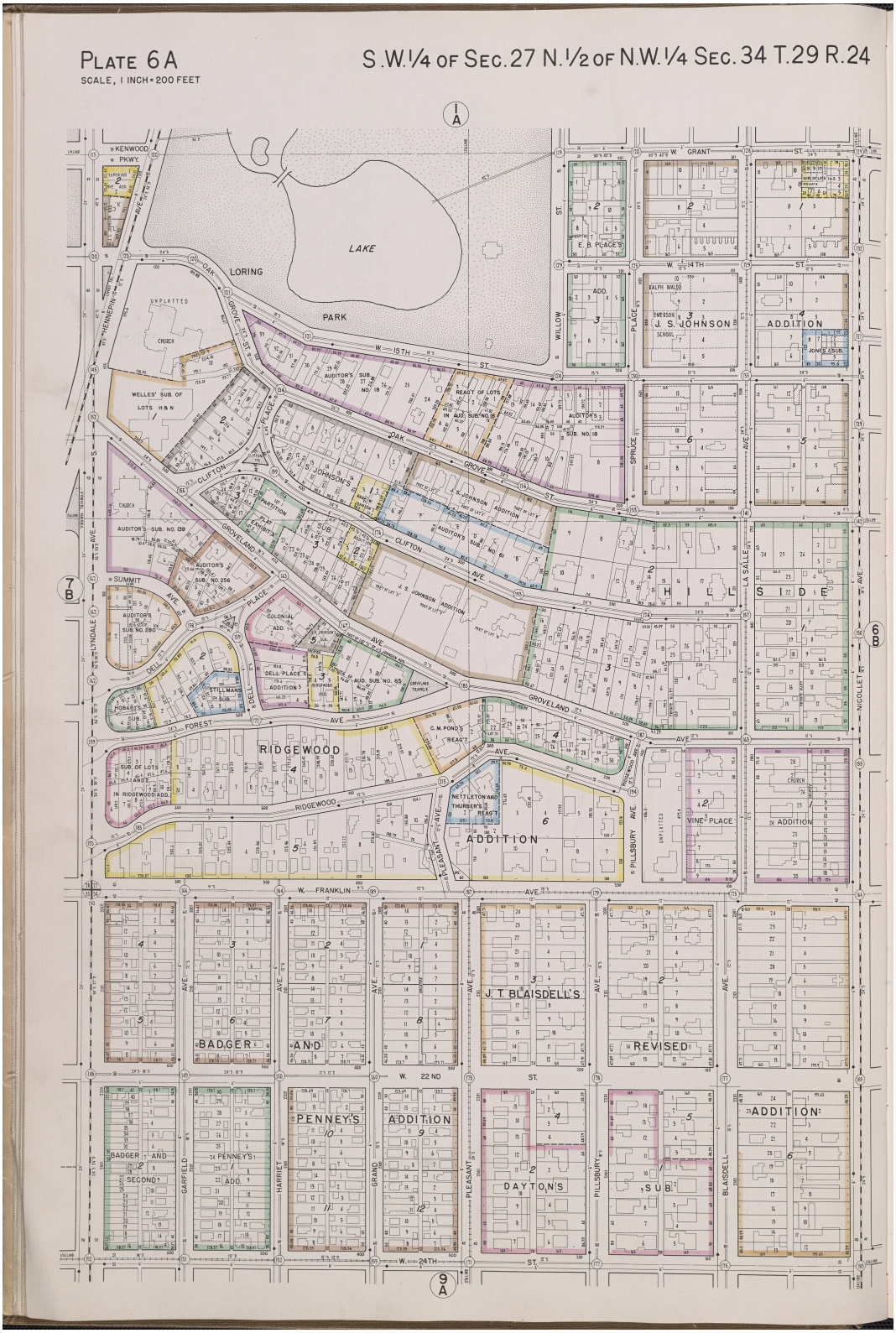
Note: Figures E.1 -E.3 provide three examples of racial covenants in three sales deeds.

Figure E.4: Newspaper advertisement of Walton Hills development



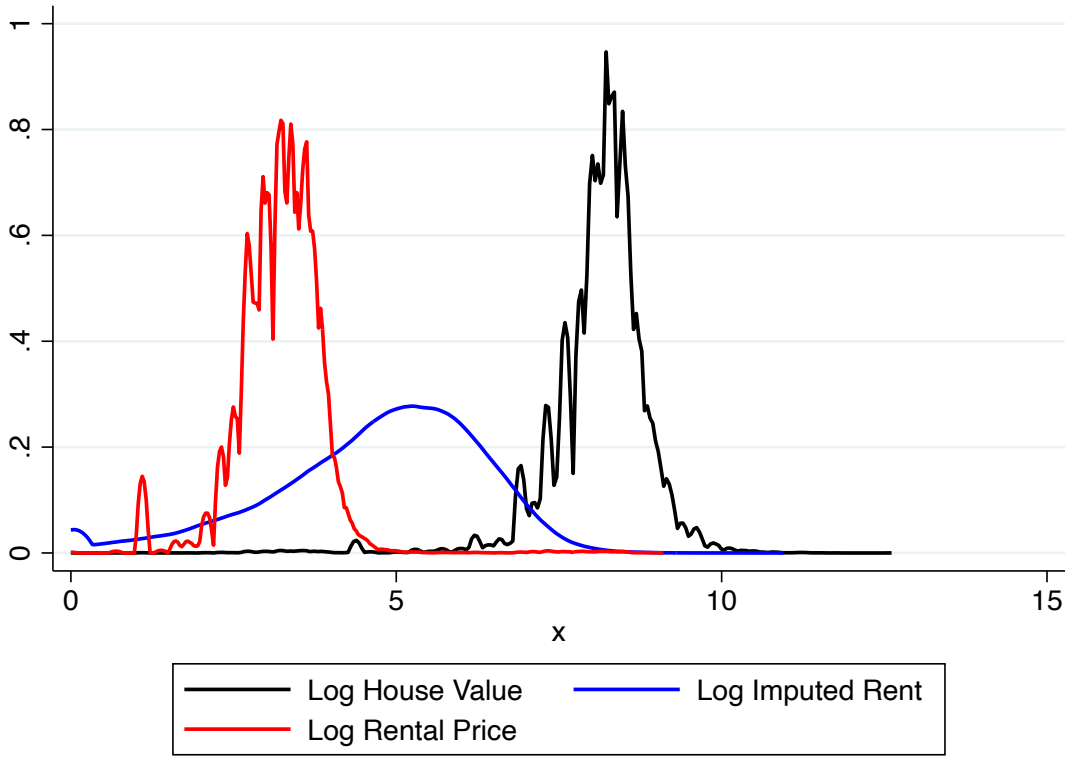
Note: This is an example of a newspaper advertisement of the "Walton Hills" development which is also advertising racial covenants.

Figure E.5: Example of Development Data from 1940 Minneapolis City Atlas



Note: This is an example page of developments from the 1940 Minneapolis City Atlas.

Figure E.6: House Value, Rental Price, and Imputed Rental Value Distributions



Note: This figure plots the log of house values for owner-occupied housing (in black) and log of monthly rents for renter-occupied housing (in red) from the 1940 Census. In blue, it plots the combined log monthly rents for renter-occupied housing and imputed monthly owner cost of housing for owner-occupied housing.

Figure E.7: Homer Hoyt's Ranking of Immigrants

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most favorable come first in the list and those exerting the most detrimental effect appear last.[32]

1. English, Germans, Scotch, Irish, Scandinavians
2. North Italians
3. Bohemians or Czechoslovakians
4. Poles
5. Lithuanians
6. Greeks
7. Russian Jews of the lower class
8. South Italians
9. Negroes
10. Mexicans

Note: Source is from "One hundred years of land values in Chicago: The relationship of the growth of Chicago to the rise of its land values, 1830-1933" by Homer Hoyt (1935).