# Essays about Race, Discrimination, and Inequality

by

**Jessica LaVoice**

B.S. in Economics and Mathematics, Worcester State University, 2011

M.A. in Economics, Duke University, 2014

M.A. in Economics, University of Pittsburgh, 2015

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This dissertation was presented

by

**Jessica LaVoice**

It was defended on

April 6, 2020

and approved by

Allison Shertzer, Associate Professor of Economics, University of Pittsburgh

Randall Walsh, Professor of Economics, University of Pittsburgh

Jason Cook, Assistant Professor of Economics, University of Pittsburgh

Brian Kovak, Associate Professor of Economics, Carnegie Mellon University

Dissertation Directors: Allison Shertzer, Associate Professor of Economics, University of Pittsburgh and Randall Walsh, Professor of Economics, University of Pittsburgh

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Jessica LaVoice, PhD

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This dissertation seeks to understand the role of public policies in shaping long-run neighborhood outcomes, with a specific focus on understanding the relationship between private markets, government, and race at key points in American history. In particular, this dissertation explores controversial government programs such as the federal urban renewal and slum clearance program established by the Housing Act of 1949 and the development of residential security maps (more commonly referred to as redlining maps) by the Home Owners Loan Corporation in the 1930s. In addition, this dissertation looks at modern-day disparities in debt collection judgments across white and black neighborhoods.

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## Preface

I am extremely grateful to my advisors, Randall Walsh and Allison Shertzer, for their constant guidance and encouragement. Their mentorship, and the exploratory data grant provided by the Economic History Association, made this dissertation possible. I would also like to thank Jason Cook, Brian Kovak, and Patricia Beeson, along with the rest of the faculty members and administrators in the Economics Department at the University of Pittsburgh for their advice and support. Special thanks go out to Mallory Avery, Jiyeon Kim, Rachel Landsman, and Sijie Li, along with my wonderful officemates and the rest of the graduate students in the Economics Department at the University of Pittsburgh for their friendship and encouragement. You all made coming into Posvar Hall enjoyable even on the gloomiest Pittsburgh days.

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## 1.0 The Long Run Implications of Slum Clearance: A Neighborhood Analysis

This paper analyzes the federal urban renewal and slum clearance program. This program was enacted by Title I of the Housing Act of 1949 and was one of the largest and most controversial location-based economic development policies used to rehabilitate neighborhoods in the United States. I construct a new spatial dataset documenting the locations of approximately 200 urban renewal projects across 28 U.S. cities. I use this newly constructed dataset to examine the characteristics of neighborhoods cleared for redevelopment and the effect that urban renewal projects had on neighborhoods over time. I show that conditional on experiencing urban blight, black neighborhoods were between two and three times more likely than white neighborhoods to be targeted for slum clearance. Further, the resulting redevelopment led to a persistent decline in population density, housing density, and in the share of black residents in directly treated neighborhoods. Simultaneously, median rents and median incomes increased. These results are consistent with predictions from a spatial equilibrium model of locational choice. Viewed through the lens of this model, my results imply that households in the lowest end of the income distribution were made worse off by slum clearance policies.

### 1.1 Introduction

One of the largest and most controversial location-based economic development policies used to rehabilitate neighborhoods in the United States was the federal urban renewal and slum clearance program which was enacted by Title I of the Housing Act of 1949. This program subsidized the clearance of blighted urban areas. The vacant lots were subsequently sold to private developers for redevelopment. The stated objectives of the program were to eliminate substandard and inadequate housing and to realize the goal of a decent home and suitable living environment for every American family. This program became increasingly controversial as many black neighborhoods were demolished, causing concern that the program was being used to displace black residents from urban areas.[1] Such controversies dominate the overwhelmingly negative historical narrative surrounding the program.

However, the limited economics literature exploring the urban renewal and slum clearance program finds a positive impact on cities (Collins & Shester, 2013). While understanding how this program impacted city level outcomes is an important contribution, aggregate positive outcomes can mask within city dynamics and these within city dynamics will likely have important distributional implications. To help better understand such within city dynamics and any associated distributional implications, this paper theoretically and empirically explores the federal urban renewal and slum clearance program at the neighborhood level.

To reconcile the economics literature with the broader narrative about the urban renewal and slum clearance program, this paper begins by examining the characteristics of neighborhoods targeted for urban renewal and slum clearance under the Housing Act of 1949, focusing on the role of race in determining site selection. I then use both theoretical and empirical approaches to understand the long-run effects of urban renewal and slum clearance projects on neighborhood level population density, housing density, racial composition, median incomes, and median rental rates. I am interested in identifying the impact of urban renewal projects on directly treated

[1] Among neighborhoods where the share of black residents was 50% or higher in 1950, 14% would be cleared and redeveloped over the subsequent decades.

neighborhoods as well as understanding relative changes between treated and untreated low-income neighborhoods within a city.

It was not previously possible to empirically assess how the urban renewal and slum clearance program affected neighborhoods due to the lack of any systematic data collection of project locations. Thus, in order to empirically explore neighborhood level results, I obtained a comprehensive list of all projects funded under the Housing Act of 1949 and used various primary sources to identify the exact locations and the expected land use of projects funded before 1965 in 28 large U.S. cities.[2] I combine this project level information with census tract level decennial census data from 1940 to 2000 to construct a neighborhood level dataset that identifies neighborhoods that were redeveloped under the urban renewal program. Using this newly assembled dataset, I first determine the role that race played in site selection after controlling for housing values, median income, and other observable neighborhood characteristics.

I then use a spatial equilibrium model of locational choice to help understand the impact of urban renewal and slum clearance on neighborhoods and to investigate the welfare implications of neighborhood level changes on households. In this model, households choose to live in one of two neighborhoods. Households are differentiated by income; neighborhoods are differentiated by housing supply, housing price, and neighborhood quality. An exogenous federal government can fund urban renewal projects that decrease housing supply and increase neighborhood quality in the lower quality neighborhood. The welfare implications of such projects depend on the relative magnitudes of the opposing effects caused by an increase in neighborhood quality (quality

[2] Figures A1 and A2 in the appendix show the digitized locations of urban renewal projects in Chicago and Pittsburgh respectively. Similar maps were made for all 28 cities in my sample.

effect) and a decrease in the supply of housing (supply effect) although households in the lowest end of the income distribution are made worse off in all scenarios.

Whether urban renewal projects are associated with the supply or quality effect dominating can be tested empirically by documenting the relative effects of urban renewal projects on treated and non-treated low-income neighborhoods *within* a city. To document this effect, I use a synthetic control group method to construct an artificial match for each neighborhood that received an urban renewal project. This method weights some combination of non-treated neighborhoods from the same city as a treated tract to minimize the pretreatment difference in observable characteristics of the synthetic control group and the treated tract. I then compare the post-treatment outcomes of these two groups. While informative about city-wide patterns, the synthetic control group is likely experiencing an indirect treatment effect from displaced residents and thus should not be interpreted as a valid counter-factual for the directly treated tracts. The synthetic control group should instead be thought of as an artificially constructed slum that did not receive an urban renewal project but may be experiencing an indirect treatment effect.

Due to the selection problem caused by the fact that treated tracts were more likely to be experiencing urban blight and the bias caused by within city spillover effects, it is not possible to identify the long-run impacts of urban renewal and slum clearance programs on directly treated neighborhoods by simply comparing treated and untreated tracts within the same city. To address this concern, I use a k-nearest neighbors approach to locate slums in cities with limited program participation to use as a control group for treated slums.[3] I estimate the impact of the federal urban renewal program on neighborhood level outcomes using a generalized difference-in-difference

[3] This approach exploits variation in the timing of required state legislation that limited the ability of certain cities to participate in the urban renewal program.

empirical framework and explore how these results vary over time by using an event-study framework.

I find that while the program did clear blighted urban areas, conditional on experiencing urban blight, neighborhoods with a high share of black residents were between two and three times more likely than white neighborhoods to be cleared and redeveloped. Furthermore, neighborhoods targeted for urban renewal experienced a decline in population density by 13%, a decline in housing density by 12%, and a decline in the share of black residents by 16%. These neighborhoods simultaneously experienced a 24% increase in median rents and an 18% increase in median incomes. Relative changes between median rents in treated and untreated neighborhoods within a city suggest that urban renewal drove up rental rates across all low-income neighborhoods and ultimately resulted in a decrease in the supply of affordable housing. These results are consistent with the supply effect dominating the quality effect in the spatial equilibrium model discussed above and implies that all low-income households are made worse off by slum clearance and urban renewal policies.

It is important to distinguish between the setting explored in this paper and recent work that documents the benefits of being relocated from modern day public housing. For example, there is a large literature documenting the impacts of the Moving to Opportunity (MTO) experiment which randomly allocated housing vouchers to a sample of families living in low-income public housing. This literature documents a positive impact on adult labor market outcomes for children who were young when their families moved and no detectable effect for older children (Chetty et al., 2016). Further, Chyn (2018) showed that both young and old children displaced from public housing into private housing were more likely to be employed and have higher earnings as adults when compared to their peers who remained in nearby public housing.

Such results are not at odds with the conclusions in this paper because this strand of literature focuses on the impacts of being removed from public housing, while historical accounts of many neighborhoods cleared under the urban renewal and slum clearance program document that these neighborhoods were not considered to be slums by neighborhood residents (e.g. Trotter and Day, 2010). Cleared neighborhoods, while occupied by a lower-income population and often the location of an aging infrastructure and housing stock, had a strong sense of community.

This paper contributes primarily to two different literatures, the first of which explores Title I of the Housing Act of 1949. Most of the literature is overwhelmingly negative in its assessment and highlights the controversies surrounding the program. In addition to the displacement of black residents, further criticisms included the destruction of low-cost housing, the demolition of cohesive neighborhoods, and the disregard of individual property rights (e.g. Jacobs 1961, Anderson 1964, von Hoffman 2000). Since there was no systematic collection of project locations, most of this research is qualitative or analyzes specific case studies. The one notable exception is work by Collins and Shester (2013) which identifies the causal impact of the federal urban renewal program on *city level* outcomes. They conclude that slum clearance and urban renewal had positive and economically significant effects on city level measures of income, property value, and population. They further show that these effects are not driven by changes in the demographic composition of the city. The key distinction between their work and mine is that I explore the impacts of this program on neighborhood level outcomes and Collins and Shester explore the program on city level outcomes. I contribute to this literature by documenting project locations and systematically exploring the impact of the urban renewal and slum clearance program at the neighborhood level. I show how aggregate positive outcomes can mask important negative distributional implications.

This paper also contributes to the literature documenting the determinants of neighborhood demographics and economic development. A subsample of this literature documents the role government policies, such as redlining, discriminatory zoning and highway construction, played in shaping the demographic structure within cities.[4] For example, Rothstein (2017) argues that de jure segregation promoted discriminatory patterns that continue to this day. Understanding the government's role in shaping neighborhood demographics and economic development has important legal and policy implications. The United States Supreme Court has aligned its constitutional obligation to remedy discrimination (and its negative consequences) on the distinction between state-sponsored segregation and segregation resulting from individual choices or preferences. Thus, understanding the how government policies contributed to the economic conditions of minorities in today's society remains an important research area.

The rest of this paper is structured as follows: Section 1.2 discusses background information about the Housing Act of 1949; Section 1.3 presents a spatial equilibrium model; Section 1.4 discusses the data and presents the determinants of slum clearance; Section 1.5 describes two identification strategies (one documenting the relative effect of urban renewal on treated and untreated neighborhoods and one documenting the direct of effect of urban renewal on treated neighborhoods); Sections 1.6 present long-run effects of urban renewal projects on neighborhoods; and Section 1.7 concludes.

[4] Other work highlights the impacts of individual actions and preferences. For example, Shertzer and Walsh (2016) document that white flight contributed to segregation for the pre-World War II time frame and Boustan (2010) documents the same for the post-World War II time frame.

### 1.2 Background

After the Great Depression and the end of World War II, housing policy rose to the top of the U.S. policy agenda.[5] At the time, overcrowded inner city areas with high levels of poverty and a high share of substandard housing were referred to as slums or as areas experiencing urban blight. The Housing Act of 1949 was passed with broad political support to subsidize locally planned urban renewal projects in blighted urban areas through the urban renewal and slum clearance program. This program was overseen by the Housing and Home Finance Agency (HHFA), and its overarching goal was to rebuild and recreate cities. The Act's specific objective was to eliminate substandard and other inadequate housing through the clearance of slums. Proponents of the program saw potential citywide benefits that would be driven by an increase in tax revenues. The assumption was that subsidizing the clearance of slums would help stimulate housing production and community development in order to realize the goal of a decent home in a suitable living environment for every American family.

If a city wanted to participate in the urban renewal program, it first had to form a local public agency (LPA) to initiate, plan, and execute urban renewal projects. This agency was responsible for identifying slums and obtaining plots of land for redevelopment. The agency accomplished this objective by negotiating with property owners and, if that failed, using eminent domain. Displaced residents received little help in terms of moving expenses or advice in finding new homes.[6] The land was then cleared, improved upon and sold to private developers.[7] The land

[5] During the war, resources shifted to wartime production, which lead to a housing shortage.

[6] According to a report written by the HHFA regarding a census bureau survey of families displaced from urban renewal sites during the summer of 1964, 70% of all families relocated themselves without the help of the LPA.

[7] Examples of land improvements include paving roads and adding streetlights.

was then redeveloped according to a preexisting neighborhood plan established by the local public agency. These redevelopment projects are what I refer to as urban renewal or slum clearance projects. Federal subsidies covered two-thirds of the net project cost (the difference between the cost to acquire and clear land and the revenue received from selling the land to a redevelopment firm). Most of the new buildings constructed in urban renewal areas were high-rise apartment buildings with units designed for high-income families (Anderson, pg 7).[8]

Subsequent Housing Acts modified the 1949 program slightly. Most notably, the Housing Act of 1954 expanded the program, which originally funded projects of a predominantly residential character, to include more non-residential projects such as civic centers and office buildings. The 1954 Act also added a new emphasis on rehabilitation as opposed to wholesale demolition. However, by the end of 1962, less than one percent of project costs were allocated to rehabilitation (Anderson, pg 20).

Over time, urban renewal and slum clearance became increasingly controversial. The primary criticisms of the program were motivated by the destruction of cohesive black neighborhoods. One example of a controversial urban renewal project was the Civic Arena in Pittsburgh's Lower Hill District. Trotter and Day (2010) describe the Lower Hill District in the early 1900s as a dynamic and thriving neighborhood that was on par with Harlem as one of the cultural centers of black America. The Lower Hill was home to the all-black Crawford baseball team and the Crawford Grill, which was a renowned jazz club. The population residing in the Lower Hill grew as more black residents were attracted to the area. The housing stock aged and while other areas of Pittsburgh had been modernized, the Lower Hill still had cobblestone streets.

[8] There was no incentive to build low-income housing. While public housing did exist at this time, the program was entirely separate from the urban renewal program. The two federal programs were run by two completely separate government agencies (Anderson, pg. 7).

In the 1950s, urban renewal displaced around 8000 residents and 400 businesses in the Lower Hill district for the construction of the Civic Arena.  Trotter and Day (2010) quote area residents who stated that the `most devastating thing that ever happened to the black community was to tear out the Lower Hill.'

Other examples of neighborhoods cleared to by controversial urban renewal projects include Boston's West End neighborhood where high-rise apartment buildings (with fewer than 500 apartments) for the upper middle class displaced over 2000 Jewish and Italian families and Hayti, Durham, a thriving black community that was cleared to make way for the Durham Freeway.  Similar projects occurring nationwide caused policymakers to become concerned that urban renewal was being used as a mechanism to displace unwanted populations from urban areas. This sentiment is evident in the 1959 Report of the U.S. Commission on Civil Rights which stated: “The clearance of slums occupied largely by Negro residents and their replacement with housing accommodations beyond the means of most Negroes gives rise to the question whether slum clearance is being used for `Negro clearance.’" These controversies contributed to the program's end in 1974.

## 1.3 Model

While slum clearance and urban renewal may have positive and economically significant effects on cities, the welfare implications of such a policy are unlikely to be evenly distributed across a city's population.  In this section, I construct a spatial equilibrium model of locational choice to document the impact of urban renewal projects on neighborhood outcomes and discuss the welfare implications of neighborhood level changes on households. I show that implications of such projects depend on the relative magnitudes of the opposing effects caused by an increase in neighborhood quality (quality effect) and a decrease in the supply of housing (supply effect), although, households in the lowest end of the income distribution are made worse off in all scenarios.

This model consists of a city with two neighborhood options for low-income households.[9] These neighborhoods are indexed by j where $j \in \{l, h\}$. Neighborhoods are differentiated by the level of housing supply, $S_j$, neighborhood quality, $q_j$, and housing price, $p_j$. I will refer to these two neighborhoods as the low-price, low-quality neighborhood and the high-price, high-quality neighborhood, where the high-price, high-quality neighborhood is defined in relative terms to the low-price, low-quality neighborhood. I assume that mobility between neighborhoods is costless.

There exists a continuum of low-income households that live in the city. Households are characterized by their income, $y_i$, and their race, $r \in \{b,w\}$. The distribution of income is given by f(y) and has continuous support over the interval $[y_{min}, y_{max}]$. Households choose to live in one of the two neighborhoods and, conditional on neighborhood choice, they choose their optimal level of housing. Rents are paid to exogenous absentee landlords. Household preferences are represented by the indirect utility function V(y,p,q).[10] This specification of V(.) implicitly assumes the inclusion of a numeraire whose price is normalized to one. V(.) is assumed to be continuous with bounded first derivatives that satisfy $V_y > 0$, $V_p < 0$, and $V_q > 0$.

I also assume that household preferences satisfy the "single crossing" property. This assumption requires that the slope of an indirect indifference curve in the (q,p) plane is increasing

[9] The intuition of this model can be extended to think about the city as whole.

[10] Introducing preferences over the racial composition of neighborhoods results in multiple equilibria. See Sethi and Somanathan (2004) and Banzhaf and Walsh (2013) for examples of such models.

in y.[11] In other words, higher income households are willing to pay more than a low income household for an increase in neighborhood quality. This assumption allows for a ranking of neighborhoods that increases in both p and q. Thus, neighborhood l is characterized by ($q_l$, $p_l$) and neighborhood h is characterized by ($q_h$, $p_h$) where $p_l < p_h$ and $q_l < q_h$. Further, this assumption guarantees the existence of a set of boundary households, uniquely identified by income, who are indifferent between both neighborhoods. I denote the income that characterizes these households by $\tilde{y}$.

The single-crossing property implies that household sorting will result in perfect income stratification across neighborhoods. Households with income less than $\tilde{y}$ prefer neighborhood l and households with income greater than $\tilde{y}$ prefer neighborhood h. Panel (a) of Figure A3 in the appendix shows an example of an income distribution and the vertical line indicates the income of a boundary household that is exactly indifferent between the low-quality, low-price neighborhood and the high-quality, high-price neighborhood. Panel (b) shows the two neighborhood options in the (q, p) plane, along with the indifference curves associated with three different income levels, $y_{low}$, $\tilde{y}$, and $y_{high}$ where $y_{low} < \tilde{y} < y_{high}$. Households with income less than $\tilde{y}$, like $y_{low}$, prefer neighborhood l and households with income greater than $\tilde{y}$, such as $y_{high}$, prefer neighborhood h. To the extent that minorities are more likely to be in the lower end of the income distribution, we expect there to be a higher proportion of minorities in neighborhood l.

In this model, there is an exogenous federal government that can fund slum clearance and urban renewal projects, denoted by R. Consistent with both historical accounts and the empirical evidence that follows, these projects decrease the supply of housing and increase neighborhood

[11] See Epple and Sieg (1999) and Banzhaf and Walsh (2008) for a more detailed discussion of this assumption.

quality in the low price, low quality neighborhood. There is no direct impact of urban renewal and slum clearance on neighborhood h. These effects are summarized below:

$$\frac{\partial S_l(R)}{\partial R} \leq 0, \qquad \frac{\partial q_l(R)}{\partial R} \geq 0, \qquad S_h(R) = S_h, \qquad q_h(R) = q_h \tag{1.1}$$

For simplicity, I assume that $q_l(R) < q_h$ for all R. This assumption guarantees a constant ranking of neighborhoods both before and after urban renewal.[12]

An equilibrium in this model is defined as an allocation of households across neighborhoods such that households choose their neighborhood to maximize V(y,p,q) and the housing market clears in each neighborhood. This equilibrium is characterized by the following three equations and implicitly defines unique equilibrium prices ($p_l$, $p_h$) and the boundary income $\tilde{y}$.

$$V(\tilde{y}, p_l, q_l(R)) = V(\tilde{y}, p_h, q_h) \tag{1.2}$$

$$\int_{y_{min}}^{\tilde{y}} D(p_l, y) f(y|w) dy + \int_{y_{min}}^{\tilde{y}} D(p_l, y) f(y|b) dy = S_l(R) \tag{1.3}$$

$$\int_{\tilde{y}}^{y_{max}} D(p_h, y) f(y|w) dy + \int_{\tilde{y}}^{y_{max}} D(p_h, y) f(y|b) dy = S_h \tag{1.4}$$

Equation (1.2) states that the indirect utility received by the boundary household has to be the same in neighborhood l and neighborhood h. Equations (1.3) and (1.4) state that the aggregate housing demanded by households that sort into neighborhood l and neighborhood h must equal the supply of housing in each neighborhood.

[12] I discuss the implications of relaxing this assumption in footnote 14.

Using this model, I assess how the equilibrium responds to urban renewal and slum clearance projects. First, I determine the impact of such projects on housing prices in neighborhood l. Using the implicit function theorem, I find the following:

$$\frac{dp_l}{dR} = \frac{\frac{1}{\int D_p(p_l,y)f(y)dy}\left[S_R^l\,(R)\left[V_y^l - V_y^h - \frac{V_p^h D(p_h,\tilde{y})f(\tilde{y})}{\int D_p(p_h,y)f(y)dy}\right] + V_q^l q_R^l(R)D(p_l,\tilde{y})f(\tilde{y})\right]}{V_y^l - V_y^h - f(\tilde{y})[\frac{D(p_l,\tilde{y})V_p^l}{\int D_p(p_l,y)f(y)dy} + \frac{D(p_h,\tilde{y})V_p^h}{\int D_p\,(p_h,y)f(y)dy}]} > 0 \qquad (1.5)$$

where $V_k^j = \frac{\partial V(\tilde{y},p_j,q_j(R))}{\partial k}$, $D_k\left(p_j,y\right) = \frac{\partial D(p_j,y)}{\partial k}$, $S_R^j(R) = \frac{\partial S_j(R)}{\partial R}$, and $q_R^j(R) = \frac{\partial q_j(R)}{\partial R}$. Thus, prices in neighborhood l must increase as the result of urban renewal projects. Second, I determine the impact of such projects on the location of the boundary household within the income distribution. Using the implicit function theorem, I find the following:

$$\frac{d\tilde{y}}{dR} = \frac{-\left(\frac{V_p^l S_R^l(R)}{\int D_p(p_l,y)f(y)dy} + V_q^l q_R^l(R)\right)}{V_y^l - V_y^h - f(\tilde{y})[\frac{D(p_l,\tilde{y})V_p^l}{\int D_p(p_l,y)f(y)dy} + \frac{D(p_h,\tilde{y})V_p^h}{\int D_p\,(p_h,y)f(y)dy}]} \qquad (1.6)$$

Since the denominator is negative,[13] the sign of equation (1.6) depends on the relative magnitudes of the two terms in the numerator. The first term, which I refer to as the supply effect, is negative and measures the decrease in utils per unit of renewal caused by the decrease in housing supply which increases rental rates. The second term, which I refer to as the quality effect, is positive and measures the increase in utils per unit of renewal associated with living in a higher quality neighborhood. Thus, the impact of renewal on equilibrium outcomes will depend on the relative magnitudes of the supply and quality effects.

[13] The single crossing property implies that $V_y^l$-$V_y^h$ is negative.

### 1.3.1 Case 1: Quality Effect Dominates

First consider the case where the quality effect is larger than the supply effect. In this case, $d\tilde{y}/dR > 0$. That is, the boundary household is characterized by a higher income level as the result of urban renewal. The increased quality of neighborhood l is large enough to incentivize some portion of the population to sort out of the relatively higher-price, higher-quality neighborhood into the lower-price, lower-quality neighborhood. Panel (a) of Figure A4 in the appendix shows the portion of the income distribution that relocates from neighborhood h to neighborhood l as a result of urban renewal and slum clearance.

Let $N_j$ be the measure of households in community j and $\bar{y}_j$ be the average income in community j where $j \in \{l, h\}$.

**Proposition 1**: If $d\tilde{y}/dR>0$, then the following must be true:

$$\frac{dN_l}{dR} > 0, \qquad \frac{dN_h}{dR} < 0, \qquad \frac{d\bar{y}_l}{dR} > 0, \qquad \frac{d\bar{y}_h}{dR} > 0, \qquad \frac{dp_l}{dR} > 0, \qquad \frac{dp_h}{dR} < 0$$

This proposition states that the low-price, low-quality neighborhood experiences an increase in population, average incomes, and rental rates as a result of urban renewal policies. The high-price, high-quality neighborhood experiences a decrease in population and rental rates, and an increase in average incomes. The first four inequalities are mechanical and can be seen visually from Panel (a) in Figure A4. The impact of R on $p_l$ is determined by the implicit function theorem and is shown in equation (1.5). Lastly, $p_h$ decreases because the loss of population from neighborhood h is associated with a decrease in housing demand and housing supply remained unchanged. The resulting neighborhood choices are shown graphically in Panel (b) of Figure A4.

To determine the welfare implications of this policy, I document the impact of renewal on the indirect utility function for households that stay in neighborhood h ($y$, $p_h$, $q_h$), households that move from h to l ($y$, $p_m$, $q_m$), and households that stay in neighborhood l ($y$, $p_l$, $q_l$). These effects are shown in equations (1.7), (1.8), and (1.9) respectively and can been seen graphically in Figure A5 in the appendix. Equation (1.7) shows that households in the high-price, high-quality neighborhood are made better off because the relocation of individuals out of their neighborhood decreased the demand for housing which led to a decrease in rental rates.

$$\begin{aligned} \frac{dV(y,p_h,q_h)}{dR} &= V_y\frac{dy}{dR} + V_{p_h}\frac{dp_h}{dR} + V_{q_h}\frac{dq_h}{dR} \\ &= V_{p_h}\frac{dp_h}{dR} > 0 \end{aligned} \tag{1.7}$$

Equation (1.8) shows the impact of urban renewal on the indirect utility of households that move from neighborhood h to neighborhood l.

$$\begin{aligned} \frac{dV(y,p_m,q_m)}{dR} &= V_y\frac{dy}{dR} + V_{p_m}\frac{dp_m}{dR} + V_{q_m}\frac{dq_m}{dR} \\ &= V_{p_m}\frac{dp_m}{dR} + V_{q_m}\frac{dq_m}{dR} \end{aligned} \tag{1.8}$$

While this sign is theoretically indeterminate, it must be the case that utility benefits from a decrease in housing price outweigh the decrease in utility caused by a decrease in neighborhood quality. This is formalized in Proposition 2. Note that (p', q') indicates the new housing price and neighborhood quality associated with neighborhoods when $R > 0$ whereas (p, q) represent the housing price and neighborhood quality before renewal. I also use $\tilde{y}$ to denote the original boundary households and $\tilde{y}_{\text{new}}$ to identify the new boundary households.

**Proposition 2**: If $d\tilde{y}/dR > 0$, then $dV(y, p_m, q_m) / dR > 0$ for all households who move from neighborhood h to neighborhood l as a result of urban renewal and slum clearance.

Proof: Movers are characterized by income $y_m$ such that $\tilde{y} < y_m < \tilde{y}_{new}$. These households originally preferred neighborhood h because $V(y_m,p_h,q_h) > V(y_m,p_l,q_l)$ but now they prefer neighborhood l because $V(y_m, p_l', q_l') > V(y_m, p_h', q_h')$. Furthermore, all households in neighborhood h could have been made better off by staying in neighborhood h because housing prices decreased and all else remained the same. This implies that $V(y, p_h, q_h) < V(y, p_h', q_h')$. By the transitive property, it must be the case that $V(y_m, p_l' ,q_l') > V(y_m, p_h, q_h)$. Thus, households that move from neighborhood h to neighborhood l as the result of urban renewal are made better off because their indirect utility from living in $l_{new}$ is higher than their indirect utility from living in neighborhood h before renewal. ■

The impact of urban renewal and slum clearance on households that remain in neighborhood l is more nuanced. Equation (1.9) shows the impact of renewal depends on the relative size of the increase in utility obtained from living in a higher quality neighborhood and the decrease in utility caused by an increase in housing price.

$$\begin{aligned} \frac{dV(y,p_l,q_l)}{dR} &= V_y\frac{dy}{dR} + V_{p_l}\frac{dp_l}{dR} + V_{q_l}\frac{dq_l}{dR} \\ &= V_{p_l}\frac{dp_l}{dR} + V_{q_l}\frac{dq_l}{dR} \end{aligned} \tag{1.9}$$

The overall effect depends on the household's location in the income distribution. This is formalized in Proposition 3.

**Proposition 3**: There exists a household y* such that

1. $dV(y, p_l, q_l)/dR < 0$ for all $y < y^*$
2. $dV(y, p_l, q_l)/dR > 0$ for all $y > y^*$

Proof: The single crossing assumption guarantees the existence of a household that is indifferent between neighborhood l, denoted by ($q_l$, $p_l$) (the low price, low quality neighborhood before renewal), and neighborhood $l_{new}$, denoted ($q_l'$, $p_l'$) (the low price, low quality neighborhood after renewal). Denote this boundary household by y*. Note that pl < pl' (see proposition 1) and $q_l < q_l'$ by assumption. For all $y \leq y^*$, the single crossing assumption implies that $V(y, p_l, q_l) \geq V(y, p_l', q_l')$. Thus $dV(y, p_l, q_l)/dR \leq 0$. Similarly, for all $y^*<y$, $V(y, p_l', q_l') > V(y, p_l, q_l)$ which implies $dV(y, p_l, q_l)/dR > 0$. ■

Thus, when the quality effect outweighs the supply effect, most low-income households are made better off by the quality improvement of neighborhood l; however, households in the lowest end of the income distribution are made worse off. They preferred the original lower quality neighborhood at the discounted price.

**1.3.2 Case 2: Supply Effect Dominates**

Now consider the case where the supply effect is larger than the quality effect. In this case, $d\tilde{y}/dR < 0$. That is, the boundary household is now characterized by a lower income level. The decrease in the supply of housing in neighborhood l is large enough to incentivize some portion of the population to sort out of the low-price, low-quality neighborhood into the high-price, high-quality neighborhood. Panel (a) of Figure A6 in the appendix shows the portion of the income distribution that relocates from neighborhood l to neighborhood h as a result of urban renewal and slum clearance.

**Proposition 4**: If $d\tilde{y}/dR<0$, then the following must be true:

$$\frac{dN_l}{dR} < 0, \qquad \frac{dN_h}{dR} > 0, \qquad \frac{d\bar{y}_l}{dR} < 0, \qquad \frac{d\bar{y}_h}{dR} < 0, \qquad \frac{dp_l}{dR} > 0, \qquad \frac{dp_h}{dR} > 0$$

This proposition states that the low-price, low-quality neighborhood experiences a decrease in population and average incomes, and an increase in housing price as a result of urban renewal policies. The high-price, high-quality neighborhood experiences an increase in population, a decrease in average incomes, and an increase in housing prices. The first four inequalities are again mechanical and can be seen visually from Figure A10. The impact of R on $p_l$ is determined by the implicit function theorem and shown in equation (1.5). In both cases, average housing cost in neighborhood l must increase since supply has declined and amenities have increased. Lastly, $p_h$ increases because the increase in population is associated with an increase in housing demand while housing supply remained unchanged. Panel (b) of Figure A6 shows the neighborhood options in the (q, p) plane as well as the indifference curves for boundary households both before and after renewal.

The impact of renewal on the indirect utility function of households that stay in neighborhood h, households that move from l to h, and households that stay in neighborhood l are shown in equations (1.10), (1.11), and (1.12) respectively and are represented graphically in Figure A7. Equation (1.10) shows that households remaining in neighborhood h are made worse off. An influx of households causes an increase the demand for housing, resulting in increased rental rates.

$$\frac{dV(y, p_h, q_h)}{dR} = V_y \frac{dy}{dR} + V_{p_h} \frac{dp_h}{dR} + V_{q_h} \frac{dq_h}{dR} \tag{1.10}$$

$$= V_{p_h} \frac{dp_h}{dR} < 0$$

Equation (1.11) shows the impact of urban renewal on the indirect utility of households that move from neighborhood l to neighborhood h.

$$\frac{dV(y,p_m,q_m)}{dR} = V_y\frac{dy}{dR} + V_{p_m}\frac{dp_m}{dR} + V_{q_m}\frac{dq_m}{dR} \qquad (1.11)$$
$$= V_{p_m}\frac{dp_m}{dR} + V_{q_m}\frac{dq_m}{dR}$$

While this sign is theoretically indeterminate, it must be the case that utility costs from an increase in housing price outweigh the increase in utility caused by an increase in housing quality. This is formalized in Proposition 5.

**Proposition 5**: If $d\tilde{y}/dR<0$, then $dV(y, p_m, q_m)/dR < 0$ for all households who move from neighborhood l to neighborhood h as a result of urban renewal and slum clearance.

Proof: Movers are characterized by income $y_m$ such that $\tilde{y}_{new} < y_m < \tilde{y}$. By definition, these households originally preferred neighborhood l because $V(y_m, p_h, q_h) < V(y_m, p_l, q_l)$ but now they prefer neighborhood h because $V(y_m, p_l', q_l') < V(y_m, p_h', q_h')$. Furthermore, all households in neighborhood h would have been made worse off by staying in neighborhood h because housing prices increased and all else remained the same. This implies that $V(y, p_h, q_h) > V(y, p_h', q_h')$. By the transitive property, it must be the case that $V(y, p_h', q_h') < V(y, p_l, q_l)$. Thus, households that move from l to h as a result of urban renewal are made worse off.

■

Equation (1.12) shows that households remaining in the low-price, low-quality neighborhood are positively impacted by an increase in neighborhood quality but negatively impacted from an increase in housing price. For households remaining in neighborhood l, it must be the case that the price effect dominates the quality effect.

$$\frac{dV(y,p_l,q_l)}{dR} = V_y \frac{dy}{dR} + V_{p_l}\frac{dp_l}{dR} + V_{q_l}\frac{dq_l}{dR} \qquad (1.12)$$
$$= V_{p_l}\frac{dp_l}{dR} + V_{q_l}\frac{dq_l}{dR}$$

This is stated and proved formally in Proposition 6.

**Proposition 6**: If $d\tilde{y}/dR < 0$, then dV(y,p_l,q_l)/dR < 0 for all households remaining in neighborhood l.

Proof: Let $d\tilde{y}/dR < 0$ and consider households with income $y < \tilde{y}$new. There exists a household y* that is indifferent between ($p_l$, $q_l$) and ($p_l$', $q_l$'). For households with income y < y*, they preferred neighborhood l over neighborhood l' so they are made worse off by renewal. Households with income y* < y prefer neighborhood l' over neighborhood l, but $V_y$(y, $p_h$, qh) > $V_y$(y, $p_l$, $q_l$) so these households were already located in neighborhood h and they stayed there after renewal took place. Thus, all households that remained in neighborhood l were made worse off. ■

Thus, when the supply effect dominates the quality effect, this model implies that all low-income households are made worse off by urban renewal. While the Housing Act was primarily residential focused, some projects funded through the act were non-residential and included stadiums, office buildings, and parking lots. In this case, the impact of renewal on neighborhood quality is limited but there is a large decrease in housing supply. These types of projects make all lower-income households in the city worse off due to increased rental rates.

### 1.3.3 Model's Implications for Empirical Work

This section briefly summarizes the model's implications for the empirical work that follows.[14] In the case where an increase in neighborhood quality is high enough to incentivize some households to leave neighborhood h and move into neighborhood l (i.e the population in neighborhood l increases *relative* to population in neighborhood h), everyone except households in the lower portion of the income distribution are made better off. In the case where a decrease in housing supply incentivizes some portion of the population in neighborhood l to move into neighborhood h despite the increase in the quality of neighborhood l (i.e. the population in neighborhood l decreases *relative* to the population of neighborhood h), all low-income households in the city are made worse off. Thus, by empirically documenting the relative impact of urban renewal and slum clearance policies between treated and untreated neighborhood within a city, we can infer the welfare implication of such policies on households.

Furthermore, if the neighborhood quality of l remains below the quality of h, we would expect the lower income population to remain in l and the relatively higher income population to remain in h. To the extent that black residents are more likely to be concentrated at the lower end of the income distribution, we would expect to see black residents remain in neighborhood l, even after redevelopment.15 If the neighborhood quality of l surpasses the quality of h, then the higher

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[14] Note that throughout this analysis I assume that $q_l(R) < q_h$ for all R which guarantees a constant ranking of neighborhoods both before and after urban renewal. These results can be easily modified to the case where $q_l' > q_h'$. In this case, urban renewal increases the quality of neighborhood l such that it surpasses the quality of neighborhood h. The results are largely consistent with three technical differences. The main difference is that the top end of the distribution sorts into l', which is now the higher quality neighborhood, and the bottom end of the income distribution sorts into h'. Second, in the case where the quality effect dominates the supply effect, low income households that are made worse off by renewal are now defined as households with income less than y**, denoting the household indifferent between l and h'. Lastly, when the supply effect dominates the quality effect, there are some households at the top of the income distribution that are made better off by the higher quality housing option. In every scenario, households at the lowest end of the income distribution are made worse off by urban renewal and slum clearance policies.

income population moves into neighborhood l and the lower income population moves into neighborhood h. To the extent that black residents are concentrated at the lowest end of the income distribution, we would expect the share of black residents residing in the cleared and redeveloped neighborhood to decline.

## 1.4 Data

Data for this analysis was collected from several different sources. The Urban Renewal Project Directory (June 1974), published by the Department of Housing and Urban Development (HUD), provides a comprehensive list of all projects funded under the Housing Act of 1949 and its subsequent amendments. This directory includes a name and ID number for every project, as well as the federal grant amount given to the local agency for the project. Through the use of various primary sources, I collected and digitized the exact locations for pre-1965 projects in 28 of the largest cities in the U.S. based on 1950 population.[15] Where available, I located projects using annual reports published by each city's primary urban renewal agency. I supplement this information with aerial photographs and project plans from the National Archives.[16] I use census tracts as a proxy for neighborhoods and use project locations to define the urban renewal treatment status for every census tract within my 28 sample cities. I define a neighborhood as treated by the

[15] My data collection and digitization progress is outlined in Table A1 of the appendix. I originally focused on the 30 largest cities based on 1950 population. Houston was dropped because of its lack of zoning laws and San Antonio was dropped because it did not have delineated census tracts by 1940. At the inception of this project, no systematic locational data was available. Much of these data has recently become available through the Digital Scholarship Lab at the University of Virginia. I have verified the accuracy of my data with the data that they collected.

[16] Figure A1 in the Appendix shows the urban renewal and slum clearance map for Chicago. Similar maps are constructed for each city in my sample.

federal urban renewal program if any part of an urban renewal project lies within that census tract's boundaries.

While the program officially ran from 1949-1974, I focus on pre-1965 projects. The HHFA, the federal agency that oversaw the urban renewal program, was restructured to become the Department of Housing and Urban Development (HUD) in 1965. While there is little evidence that this changed the structure of the program, many publications were discontinued under the HUD administration, which makes documenting the locations of projects funded under HUD more difficult. Focusing on projects funded before 1965 is likely to bias my results toward zero since I will be comparing treated census tracts to non-treated tracts plus tracts that were treated post-1965.

The Urban Renewal Project Directory also documents the month and year a project was approved for planning (during the planning phase an urban renewal plan was formulated to outline the objectives of the project, treatment to be utilized, and the controls over new land uses), execution phase (approval dates for authorization of a grant contract), and completion phase (dates for completion of a grant contract).[17] These dates correspond to transfers of grant money and not necessarily project progress. I use the planning and execution dates to define treatment timing for my outcome variables of interest since completion data are missing for many projects.[18]

These data are supplemented with information from another HUD publication, the Urban Renewal Project Characteristics (June 1966).[19] The June 1966 publication contains data on housing and demographic characteristics in the areas to be cleared for redevelopment, including

[17] A "completed" project does not imply that physical redevelopment was completed and many completion dates were left blank in the last publication of the directory in 1974.

[18] Timing variables are presented graphically in Figure A8 of the appendix. This figure shows the 204 projects included in this analysis, ranked by start date on the horizontal axis, and the year each project entered a new phase of grant payment.

[19] This is the last version of this specific publication and hence is a practical reason for focusing on pre-1965 projects.

the number of black and white families that were displaced and the number of standard and substandard houses that were demolished. Table A2 in the Appendix reports the number of standard and substandard houses that were demolished by the program, as well as the number of white and non-white families displaced. In total, 82% percent of the housing units that were demolished in these 28 cities were considered to be substandard units (this is the same percentage for the full sample of cities listed in the HUD publication) and 58% of the families that were displaced were non-white (compared to 54% of families in the full sample of cities).

This publication also documents the total land acquired for each project and the shares of acquired land to be used for each of the following purposes: residential, commercial, industrial, public, and streets. Table 1.1 summarizes these data aggregated to the city level. While the program was meant to be primarily residential, and indeed this is the largest land use of the five categories, we see that the combined total of all other uses outweighs residential uses.[20]

All outcome variables are from decennial census data and span from 1940 to 2000. This data includes census tract level measures of population, housing stock, racial composition, median income, and median rents. Income and rents are all adjusted for to be in terms of year 2000 dollars. All census data and shapefiles were acquired from IPUMS NHGIS. Census tracts had to be corrected for changes in boundaries over time. In general, as population in one tract grew, a tract was divided in half, while if the population decreased in a tract, two tracts were merged. Using arcGIS, I identify the smallest geographic unit that appeared in any census, and use weighted averages based on land area to estimate population and housing distributions in any year that the

[20] While not the focus of this paper, streets are the second largest land use, supporting the idea that inserting roads and highways through black neighborhoods is an important aspect of the urban renewal narrative.

census tract boundaries do not overlap. For median incomes and rents, the same value would be applied to both neighborhoods.

### 1.4.1 Characteristics of Targeted Tracts

The goal of the urban renewal program was to eliminate and prevent urban blight through the clearance of slums and the rehabilitation of urban areas. Thus, treated tracts should differ from non-treated tracts in predictable ways. Specifically, they should have more substandard housing units, lower rents, lower housing values, and a lower income population.

Table 1.2 confirms these expectations. This table reports the means and standard deviations of treated and non-treated tracts in the pretreatment period, as well as the p-value of a two-sided t-test. Panel A presents the results for population characteristic variables. When compared to non-treated census tracts, treated tracts had a higher population density, share of black residents, and unemployment rate. We also see lower income levels in treated tracts. The difference in the mean of treated versus non-treated tracts is statistically different at the 1% significance level for every variable.

Panel B presents housing characteristics. Treated tracts had a higher housing density with a higher percentage of houses needing major repairs and a higher share of vacant units. Treated tracts also had an older housing stock. Moreover, treated tracts had a lower share of housing without running water.

Panel C presents home ownership characteristics.  Treated tracts had lower levels of homeownership, and those who did own homes had homes of a lower value.  Treated tracts also had a higher percentage of renters who typically enjoyed lower rents.[21]

I also estimate the likelihood of receiving an urban renewal project based on a neighborhood's observable characteristics.  Table 1.3 presents the results of a probit regression with city and year fixed effects.  Columns (1)-(3) present the results from different empirical specifications using various pretreatment years based on variable availability.  These results suggest that the unemployment rate, homeownership rates, median house values, housing age, and the percentage of vacant units were all important characteristics for identifying neighborhoods to be cleared.  Interestingly, the share of houses needing major repairs is not a statistically significant predictor of which neighborhood would eventually be cleared and redeveloped.

### 1.4.2 Racial Bias in Treatment

The federal urban renewal program became increasingly controversial as criticism mounted that minority neighborhoods were being disproportionately targeted for clearance. However, the role of race in determining site selection is not obvious because racial composition is correlated with other neighborhood level observables.  Table 1.3 shows that, controlling for housing quality measures and socioeconomic status, neighborhoods with a larger share of black residents are more likely to be cleared for redevelopment under the Housing Act of 1949.  To further illustrate this fact, I identify tracts that should have been treated based on observable

[21] I also look at the difference between 1940 and 1950 values broken down by treatment status and see similar results, suggesting that both levels and trends are different for treatment and non-treatment tracts for almost every variable included in my analysis.

housing and economic characteristics in a race-blind experiment and compare predicted treatment status to true treatment status by racial composition of neighborhoods. To calculate the relevant predicted probabilities, I run a probit regression of treatment on housing and economic characteristics, not controlling for race, and use this model to calculate predicted treatment for every census tract in my 28 sample cities.

The percent of tracts that were treated broken down by neighborhoods with high and low percentages of black residents and by predicted treatment quartile are shown in Panel (a) of Figure 1.1.[22] If race was not a factor in determining the locations of urban renewal projects, we would expect similar treatment rates between high and low percentage black census tracts. This analysis shows that neighborhoods with a high percentage of black residents were more than two times as likely to be treated conditional on being in the top quartile of predicted treatment.

However, it could be the case that conditional on being within the top quartile of the predicted treatment distribution, black neighborhoods were more likely to be treated due to their other observable characteristics. Panel (b) of Figure 1.1 replicates the previous analysis for only the top 10% of the predicted treatment distribution. This figure provides further evidence that black neighborhoods were disproportionately targeted for slum clearance. Differences in observable neighborhood characteristics are not driving the differences in treatment status across white and black neighborhoods.[23]

[22] A census tract is considered to have a high percentage of black population if the black percentage of the population was above the sample mean.

[23] Panel (a) and (b) of Figures A9 in the appendix show this same analysis using the two alternative specifications shown in Table 1.3 to define predicted treatment (without using race). The general patterns seen in these figures confirm the robustness of this result.

## 1.5 Identification Strategy

Using the intuition from the model presented in Section 1.3, I explore two different empirical questions. First, I ask how urban renewal and slum clearance programs impacted neighborhoods within cities. The theoretical framework highlights the importance of spillover effects on other low-income neighborhoods within a city. Thus, by comparing the *relative* effect of directly treated tracts and tracts receiving displaced residents, we can infer the welfare implications of urban renewal policies on low income residents. While I don't know exactly where displaced residents moved, I construct a within-city synthetic control group to match the pre-trend characteristics of treated tracts to artificially create an untreated slum that is as similar as possible to our treated slums. While this approach is informative about city-wide patterns and the associated theoretical welfare implications, the synthetic control should not be thought of as a valid counterfactual for directly treated tracts. This empirical exercise provides information about post-renewal differences between treated and non-treated tracts within a city that looked similar before treatment.

My second empirical exercise seeks to understand the impact of urban renewal on *directly* treated tracts independent of spillover effects. As shown in Section 1.4, the allocation of urban renewal projects cannot be viewed as a random assignment or a natural experiment. Particular neighborhoods were targeted based on pre-existing neighborhood characteristics. Therefore, any direct comparisons between treated and non-treated census tracts is likely to suffer from selection bias and spillover effects. To solve this problem, I use uses variation in program participation across cities and identify slums from cities with limited program participation to use as a control group for treated slums.

### 1.5.1 Relative Effects of Urban Renewal (Within City)

My first empirical exercise evaluates how the urban renewal and slum clearance program differentially impacted neighborhoods *within* cities. I use the synthetic control group method developed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) to construct a synthetic match for each neighborhood that received an urban renewal project. This method constructs a weighted combination of non-treated neighborhoods from the same city as the treated neighborhood to minimize the difference between treated tracts and non-treated tracts pre-treatment characteristics. Most previous papers employ synthetic matching for the case of one treatment group and one intervention; however, I follow the algorithm used in Acemoglu et al. (2016), which extends the standard method to the case of many treated units with different intervention periods. I match on pretreatment levels of population density, housing density, median rents, share black, and median income.

The synthetic control groups likely experience an indirect treatment effect from displaced residents. For example, a resident who was displaced from her home due to urban renewal could have moved into a tract that has a non-zero weight in the respective synthetic control group. In this case, the synthetic control group provides information about the *relative* post-renewal differences between treated and non-treated tracts that looked similar before treatment. The synthetic control group should *not* be interpreted as a counter-factual for the directly treated tracts. The treated tract can intuitively be thought of as neighborhood l in the model presented in Section 1.3 and the synthetic control group can be thought of as neighborhood h (which was artificially constructed to be as similar as possible to neighborhood l before urban renewal occurs).

Statistical inference is deduced by randomly assigning treatment status to neighborhoods, constructing a synthetic cohort for each treated tract, and calculating the predicted effects under random assignment. I repeat this placebo analysis 100 times and compare the distribution of predicted effects to the effect estimated from the original sample.

### 1.5.2 Direct Effects of Urban Renewal (Across City)

To identify the effect of urban renewal on *directly* treated neighborhoods I exploit differences in program participation rates at the city level. I identify slums within the control cities (cities with limited program participation) to use as control groups for treated slums. I use a k-nearest neighbors approach to identify neighborhoods experiencing urban blight within control cities. My identifying assumption is that treatment and control tracts trend similarly in the pretreatment period, an assumption I test empirically. I began with a fixed-effect empirical framework to estimate the impact of the federal urban renewal program on neighborhood level outcomes. My sample consists of treated tracts and predicted slums within control cities. My estimation equation is given below:

$$y_{ict} = \alpha + \theta_i + \gamma_t + \beta treated_{it} + \lambda_c * t + \epsilon_{it} \tag{1.13}$$

where $y_{ict}$ is an outcome for neighborhood i in city c for year t, $treated_{it}$ is a binary variable indicating whether the tract received an urban renewal project by year t, $\theta_i$ are neighborhood fixed effects, $\gamma_t$ are year fixed effects, and $\lambda_c * t$ is a city specific linear time trend. The coefficient of interest is $\beta$ which is interpreted as the average treatment effect of receiving an urban renewal project.[24] Since my control tracts are from cities with limited program participation, this strategy

[24] See Goodman-Bacon (2018) for a discussion about the interpretation of generalized Difference-in-Difference coefficients.

presents little concern about spillover effects. To explore how these results vary over time, I also use a flexible event study framework. The empirical specification is given by equation (1.14).

$$y_{ict} = \alpha + \theta_i + \gamma_t + \sum_{k=-2}^{5} \tau_k \, treated_i 1(t - t^* = k) + \lambda_c * t + \epsilon_{it} \tag{1.14}$$

where $y_{ict}$ is an outcome for neighborhood i in city c for year t, $\theta_i$ are neighborhood fixed effects, $\gamma_t$ are year fixed effects, and $\lambda_c * t$ is a city specific linear time trend. Furthermore, $1(t - t^* = k)$ are event year dummies, which are equal to 1 when the year of observation is -2, ..., 5 decades from the decade immediately preceding treatment, $t^*$, and $treated_i$ is a binary variable equal to 1 if tract i ever received and urban renewal project.[25] The coefficients of interest are $\tau_k$. The point estimates for $\tau_{-2}$ and $\tau_{-1}$ describe the evolution of the outcome variable in eventually treated neighborhoods before urban renewal began net of changes in control neighborhoods after adjusting for model covariates. The point estimates for $\tau_1$ to $\tau_5$ describes the divergence in outcomes k years after the urban renewal project net of changes in control neighborhoods after adjusting for model covariates. These estimates are interpreted as the average treatment effect of urban renewal on outcomes relative to the census year before treatment.

I repeat this analysis using only residential or nonresidential projects to explore how results differ based on primary land use. Residential projects refer to treated census tracts where more than half of the treated land was used for residential purposes and non-residential projects refer to treated tracts where more than half of the treated land was used for non-residential purposes.

[25] k=0 is omitted, and the other coefficients are interpreted relative to the census year immediately preceding treatment.

## 1.6 The Long-Run Effects of Urban Renewal

Section 1.6.1 presents the results from the within city exercise that explores the relative effect of urban renewal on treated and untreated tracts and Section 1.6.2 presents the results from the across city exercise which explores the direct effect of urban renewal on directly treated neighborhoods.

### 1.6.1 Relative Effects of Urban Renewal

To understand the impact that urban renewal and slum clearance had *within* cities I construct a synthetic control group to match the pre-trends of treated tracts. Due to spillover effects between treated and untreated neighborhoods, synthetic control groups should be interpreted as the evolution of artificially constructed untreated slums within treatment cities. This empirical exercise helps understand the within city dynamics (like those discussed in the spatial equilibrium model in Section 1.3) that result from urban renewal policies.

Figures 1.2 and 1.3 present the main set of results. Figure 1.2 shows the average outcome variables for treated tracts and the synthetic control groups separately. The matching algorithm overestimates some variables and underestimates others. While the levels are not exact, these graphs show support for the parallel trends assumption. Figure 1.3 shows the average effect (the average difference between the treated tracts and the synthetic control tracts) for each variable of interest. The shaded area shows the range of effects estimated from 100 placebo iterations in which treatment was assigned to random neighborhoods.

My first outcome of interest is population density. Panel (a) of Figure 1.2 shows the population density of treated tracts and the synthetic control group over my sample period.

Compared to artificially created slums, treated tracts experienced a decline in population density over the subsequent half century. We see from Panel (a) of Figure 1.3 that population density declined by about 2 people per every 1000 square meters and the difference between treated tracts and the synthetic control group mitigated only slightly over time. Figure 1.4 shows the trends for residential and non-residential projects separately. While the effects are larger for non-residential projects, residential projects also experienced a decline in population density when compared to the synthetic control group. Panel (b) of Figure 1.2 shows the evolution of housing density for both treated tracts and the synthetic control group. From Panel (b) of Figure 1.3, we see a decline in housing density by about 0.8 units per every 1000 sq. meters.[26]

Given that on average population density and housing density in treated tracts are declining, these results are consistent with the supply effect dominating the quality effect in the model presented in Section 1.3. The increased quality of the neighborhood was not enough to incentivize an increase in population. This causes a decrease in average income and an increase in rental rates across both neighborhoods. Hence, the theoretical predictions for the *relative changes* in rents and income across the two neighborhoods are ambiguous. Panel (c) and (d) of Figure 1.2 shows very similar trends in these variables in both the treated tracts and the synthetic control units. Thus, rents increased in treated tracts due to an increase in neighborhood quality and rents increased in other untreated slums due to an increase in the demand for housing which was driven by displaced rents.

Panel (e) of Figure 1.2 shows the average percentage of black residents for both treated tracts and synthetic control groups. We see differential trends in the share of black residents across the treatment and control groups in the pretreatment period. This is consistent with the results

[26] The results are similar for both residential and non-residential projects. See Figure A15 in the Appendix.

presented in Section 1.4 that black neighborhoods were disproportionately targeted for redevelopment. Post-redevelopment, we see the share of black residents' level off in the treatment group, and steady growth of the share of black residents in the control group. While the model presented in Section 1.3 does not make direct predictions about racial composition of neighborhoods, this result is consistent with the idea that displaced black residents relocated into similar neighborhoods, but now faced higher rents associated with both a decrease in the supply of housing and increased demand for housing among low income neighborhoods.

Effects on the share of black residents differ drastically by the land use of projects. Figure 1.5 shows that when a project was primarily used for residential purposes, the share of black residents trended similarly between the treatment and control group in the post-treatment period. However, for projects with a primarily non-residential use, the treated neighborhoods experienced a sharp decline in the share of black residents compared to the artificially created slums.

These findings, taken together with the model's insights, suggests that low income households in both treated and untreated neighborhoods were made worse off by an increase in the cost of housing. Thus, while slum clearance and urban renewal has been shown to have positive and economically significant effects on city level measures of income, property value, and population, the welfare implications of such a policy are unevenly distributed across cities' population.

### 1.6.2 Direct Effects of Urban Renewal

This section discusses the effect of urban renewal and slum clearance on directly impacted tracts (tracts that had an urban renewal project within their boundaries). Figure 1.6 graphs the aggregate trends of the treated and control slums used in this analysis. Tables 1.4-1.8 presents the regression results from equation (1.13). Column (1) of each table presents the full sample results, Column (2) limits the treated sample to residential treated tracts, and Column (3) includes only nonresidential treated tracts. Figure 1.7 presents the coefficients and 95% confidence interval for $\tau_k$ in equation (1.14). The point estimates for $\tau_{-2}$ and $\tau_{-1}$ confirm that, with the exception of share black, there is no statistical difference in the evolution of the outcome variable in eventually treated neighborhoods before urban renewal began net of changes in control neighborhoods after adjusting for model covariates. The point estimates for $\tau_1$ through $\tau_5$ describe the divergence in outcomes k decades after the urban renewal project was initiated net of changes in control neighborhoods after adjusting for model covariates.

Table 1.4 presents the results from equation (1.13) with population density as the outcome variable. Column (1) of Table 1.4 shows that on average over the post-treatment period, population density declined by 1.8 people per every 1000 square meters as the result of an urban renewal and slum clearance project. This is a 13% decline from the pretreatment average of 14 people per every 1000 square meters. Columns (2) and (3) of Table 1.4 show that this result is driven by census tracts that were primarily used for non-residential purposes. However, even tracts with a project that was used for a primarily residential purpose saw a decrease in population density. Panel (a) Figure 1.7 shows no differential trends in population density pre-treatment and a sharp decline in population density post-treatment that mitigates slightly overtime.

This decrease in population density is likely driven by a reduction in the supply of housing. Table 1.5 confirms this hypothesis. Over the post-treatment period, housing density declined by 0.54 houses per every 1000 square meters. This is a 12% decline from the pretreatment average of 4.4 houses per every 1000 square meters. In neighborhoods with primarily residential projects, there is no decrease in housing units, as seen by the statistically insignificant coefficient in column

(2) of Table 1.5; however, non-residential projects caused a decrease of .85 houses per every 1000 square meters, a 21% decline from the pretreatment average. Lastly, Panel (b) Figure 1.7 shows the results for housing density with the flexible event study framework. In the decade following treatment, there is a sharp and persistent decline in housing density that mitigates only slightly over time.

The reduction in the supply of housing, combined with an increase in neighborhood quality is likely to create upward pressure on the rental market in treated neighborhoods. Table 1.6 shows that urban renewal projects did cause an increase in median rents in directly treated neighborhood when compared to the control group. Both residential and non-residential projects experienced this increase in rental rates, although rents increased by 18% in residential projects and by 36% in non-residential projects. Panel (c) of Figure 1.7 shows the flexible event study results for median rent. In the census year immediately after the first loan execution, there was a increase in median rent by about $50 (a 19% increase from the $267 pre-treatment median) which is associated with an initial decrease in the amount of rental units available. By the following decade, median rents had increased by an additional $50 and then began to mitigate slowly over time. This secondary increase is consistent with the completion of projects occurring approximately a decade after the first grant payment was executed and such projects being developed for higher-income households.

Column (1) of Table 1.7 shows that over the post-treatment period, median incomes were on average $2,878 (measured in year 2000 dollars) higher in treated tracts compared to non-treated slums. This is a 16% increase from the pre-treatment average of $17,579. These results are similar across residential and non-residential projects, suggesting that, on average, residential and non-residential projects attracted households with similar incomes relative to baseline populations.

Panel (d) of Figure 1.7 shows the flexible event study results for median income.[27] In the census year immediately following the first grant payment, there was no change in median income. This is likely because a random selection of households within a neighborhood was displaced by the project. As seen in Figure A8, the project was unlikely to be completed until the following decade. As such, in the following census year, we see a sharp rise in income, consistent with higher income households moving to the newly improved high-quality neighborhood. However, these effects are mitigated over time and become statistical insignificant in subsequent decades.

We know from Section 1.4 that minority neighborhoods were more likely to be cleared and redeveloped. Thus, we should expect the share of black residents to decrease as a result of urban renewal. Column (1) of Table 1.8 shows that on average over the post-treatment period, the share of black residents in directly impacted neighborhoods decreased by 5 percentage points, which is a 16% decrease from treated tracts' pre-treatment average of 31%. Columns (2) and (3) of Table 1.8 show that this result is mostly driven by non-residential projects. Thus, while the previous literature highlights residential projects being out of the price range of minority residents, there is little evidence that minorities were unable to afford to live in treated neighborhood post-renewal. It seems more likely that minorities were displaced from urban areas that were being transitioned to non-residential purposes. Panel (e) of Figure 1.7 shows the event study results for the share of black residents in a treated tract. This is the only dimension that the sample of non-treated slums within control cities does not trend similarly to treated tracts within treated cities; treated cities were experiencing sharper increases in the percentage of black residents. This is not surprising given the role that race played in site selection. In the first census year after treatment, there was

[27] Median income is not available in 1940 so for this one outcome variable, only two pretreatment periods are available.

a modest decline in the share of black residents in a treated neighborhood. This gap grows even further in the following decade once projects reach completion and the new high-quality neighborhoods attracted more white residents to the area.

Overall, these findings suggest that the Housing Act of 1949 decreased the density of housing in treated neighborhoods, displacing lower income black residents and increasing rents. This demographic switch is associated with increased median incomes. In other words, these neighborhoods gentrified and remained more expensive over the subsequent 50 years.

#### 1.6.2.1 Robustness Checks

Figures A10 and A11 in the appendix show the results from the event study specification on the residential and non-residential subsamples. With the exception of the share of black residents in tracts receiving a residential project, there are no differential trends between treated and control neighborhoods in the pre-treatment period.

Tables A3-A7 replicate the results from Tables 1.4-1.8, varying the parameter used in the k-nearest neighbors matching algorithm that identifies untreated slums within control cities. Also included are the results of a k-nearest neighbors approach that matches only on 1950 values as opposed to the full pre-treatment period. Each coefficient in each table represents the results from a different regression. Figures A12-A14 in the Appendix replicate the flexible event study results presented in Figure 1.7 along these different dimensions. Signs, statistical significance, and the general magnitude of the coefficients remain consistent across all specifications.

## 1.7 Conclusion

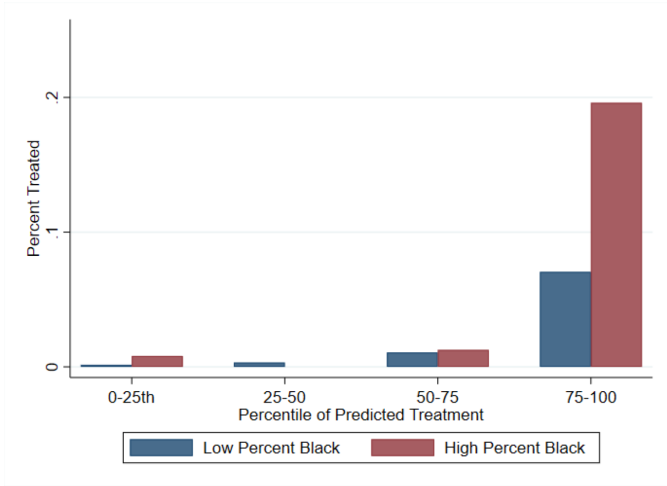
In this paper, I theoretically and empirically explored the federal urban renewal and slum clearance program. This program was one of the largest and most controversial location-based economic development policies used to rehabilitate blighted urban neighborhoods in the United States. The basic premise of this program is that urban renewal eliminates slums and substandard housing, prevents the spread of blight, and revitalizes cities by subsidizing the clearance of blighted urban areas.

This program became increasingly controversial as many black neighborhoods were demolished, causing concern that the program was being used to displace black residents from urban areas. Such controversies dominate the overwhelmingly negative historical narrative surrounding the program. However, previous research has shown that cities with higher levels of program participation saw subsequent increases in city level measures of income, property value, and population. By documenting project locations within cities, I show how aggregate positive outcomes can mask important negative distributional implications.

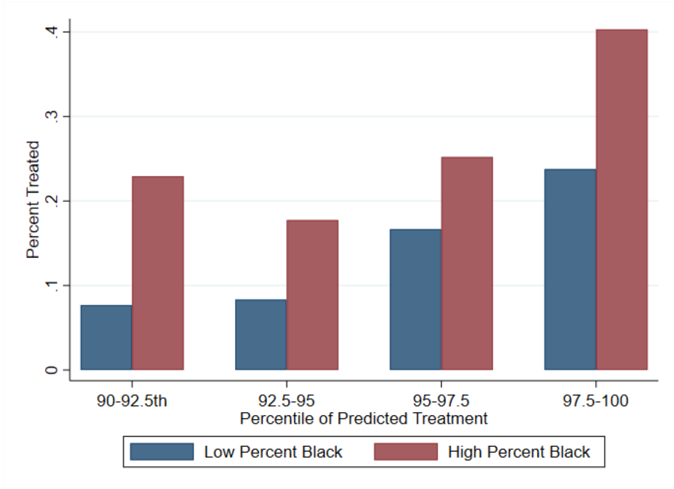
Consistent with historical concerns, I find that while the program did clear blighted urban areas, conditional on experience urban blight, neighborhoods with a high share of black residents were more than twice as likely to be cleared and redeveloped. Furthermore, this program had persistent impacts on the demographic and economic structure of cities; neighborhoods targeted for urban renewal experienced a persistent decline in population density, housing density, and in the share of black residents in directly treated neighborhoods while simultaneously experiencing increases in median rents and median incomes. Relative changes between median rents in treated and untreated neighborhoods within a city suggest that urban renewal drove up rental rates across all low-income neighborhoods, and ultimately resulted in a decreased supply of affordable housing. A spatial equilibrium model of locational choice suggests that urban renewal policies had negative welfare implications for household at the lowest end of the income distribution.

The trade-off between urban growth and within city equity is not specific to U.S. cities in the 1950s, but is a struggle many cities across the world still face. Documenting the city-wide benefits and understanding the welfare and distributional implications of such programs can help inform policies related to these issues.

## 1.8 Figures and Tables



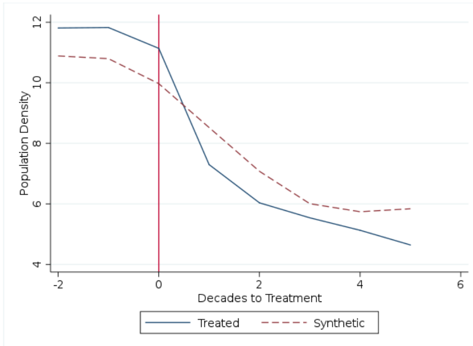

(a) Full Predicted Treatment Distribution



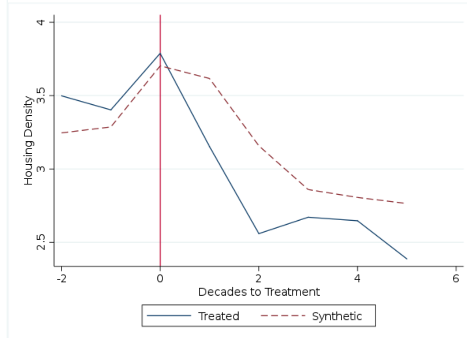

(b) Top 10% of Predicted Treatment Distribution

**Figure 1.1: Racial Bias in Slum Clearance Site Selection**

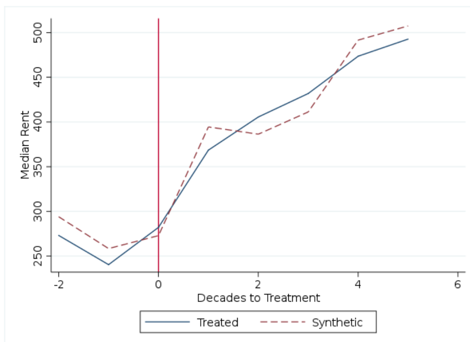
Notes: This figure graphs the share of tracts that received an urban renewal project by quartile of predicted treatment and the share of black residents in a neighborhood. High and low percent black are defined as being above and below the average share of black residents in the sample. Predicted treatment was calculated using a probit regression of treatment on all observable characteristics of neighborhood except racial composition.



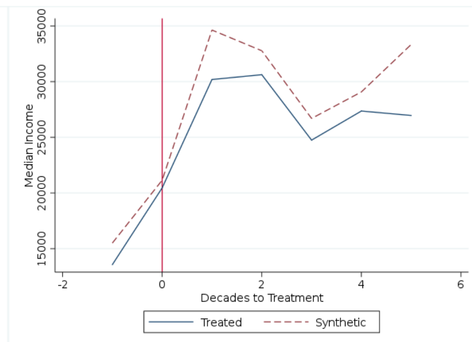

(a) Population Density



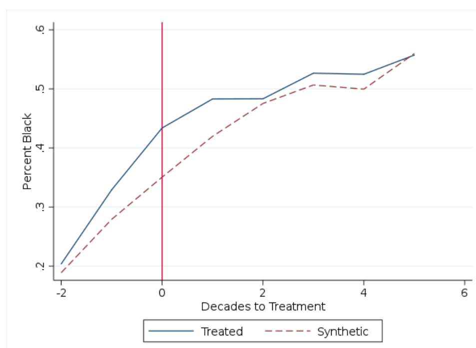

(b) Housing Density




(c) Median Rent



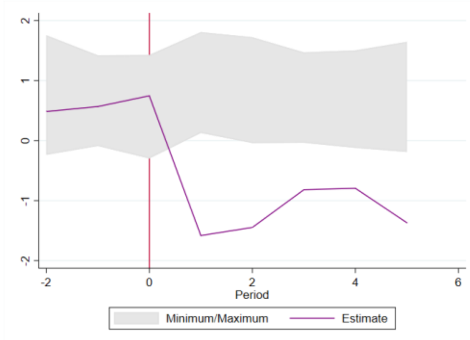

(d) Median Income



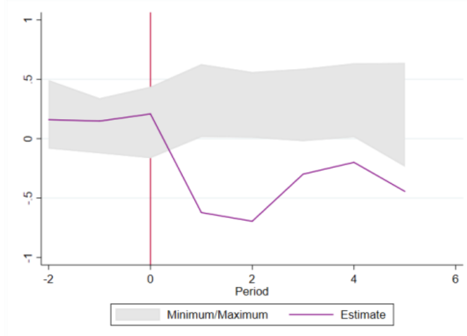

(e) Percent Black

**Figure 1.2: Relative Impacts Within Cities (Summary Statistics)**

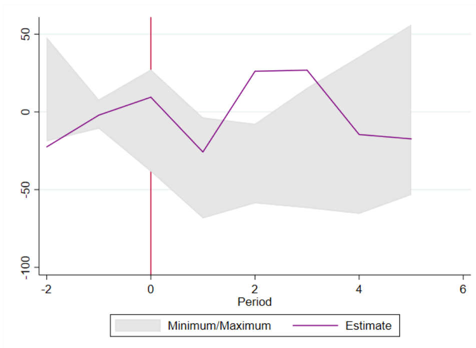
Notes: This figure shows the averaged data for treated neighborhoods and the synthetic control groups. A different synthetic control group was constructed for each treatment neighborhood in my sample. The synthetic control group was constructed to minimize the pretreatment differences in observable characteristics between the treatment and control groups.



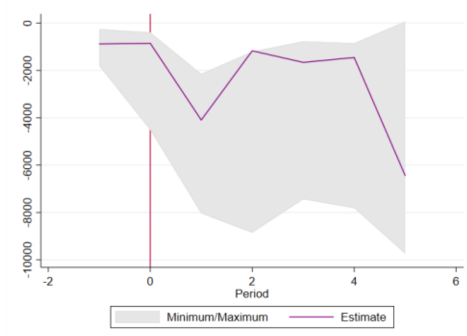

(a) Population Density



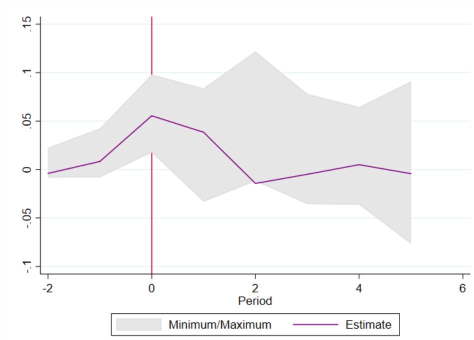

(b) Housing Density




(c) Median Rent



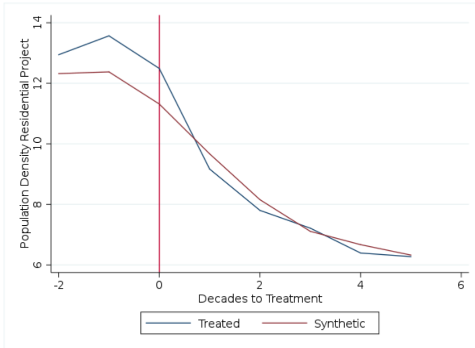

(d) Median Income

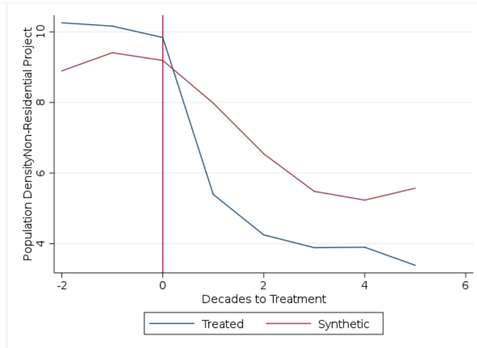



(e) Percent Black

**Figure 1.3: Relative Effects of Urban Renewal (Synthetic Control Framework)**

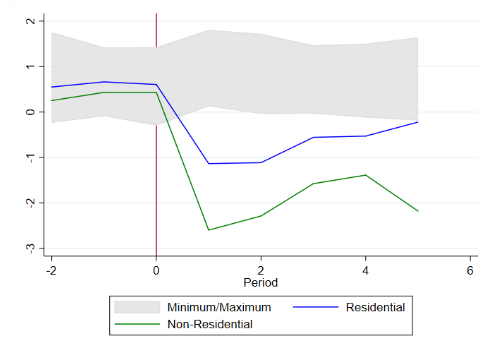
Notes: This figure shows the average differences between treated neighborhoods and the synthetic control groups. The shaded area shows the range of placebo effects estimated when treatment is randomly assigned to neighborhoods.




(a) Residential Projects

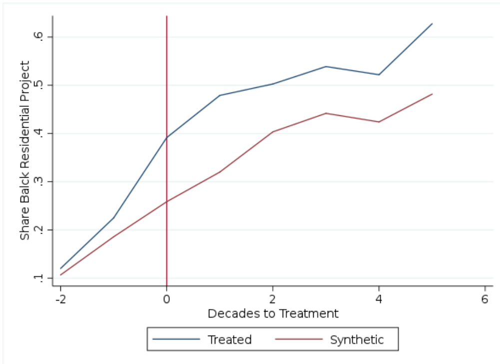
(b) Non-Residential Projects



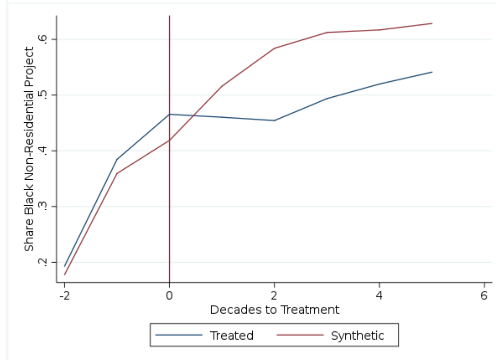

(c) Treatment Effects

**Figure 1.4: Relative Effects on Population by Project Type**

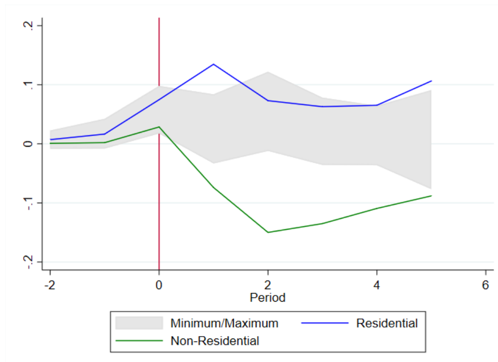
Notes: The outcome variable of interest in this figure is population per 1000 sq meters. Panel (a) and (b) of this figure shows the averaged data for treated neighborhoods and the synthetic control groups across residential and non-residential projects separately. A different synthetic control group was constructed for each treatment neighborhood in my sample. The synthetic control group was constructed to minimize the pretreatment differences in observable characteristics between the treatment and control groups. Panel (c) of this figure shows the average differences between treated neighborhoods and the synthetic control groups. The shaded area shows the range of placebo effects estimated when treatment is randomly assigned to neighborhoods.




(a) Residential Projects



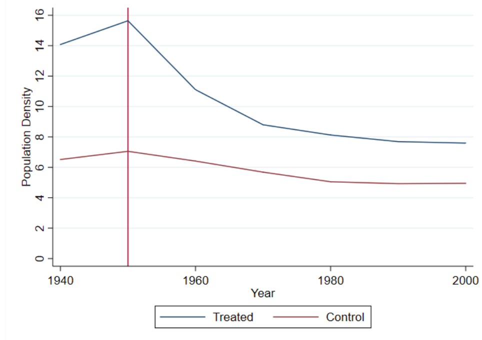

(b) Non-Residential Projects



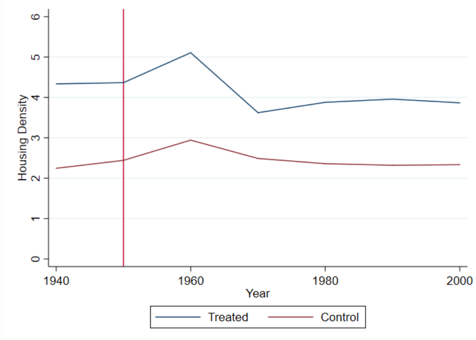

(c) Treatment Effects

**Figure 1.5: Relative Effects on Share Black by Project Type**

Notes: The outcome variable of interest in this figure is the share of black residents in a neighborhood. Panel (a) and (b) of this figure shows the averaged data for treated neighborhoods and the synthetic control groups. A different synthetic control group was constructed for each treatment neighborhood in my sample. The synthetic control group was constructed to minimize the pretreatment differences in observable characteristics between the treatment and control groups. Panel (c) of this figure shows the average differences between treated neighborhoods and the synthetic control groups. The shaded area shows the range of placebo effects estimated when treatment is randomly assigned to neighborhoods.



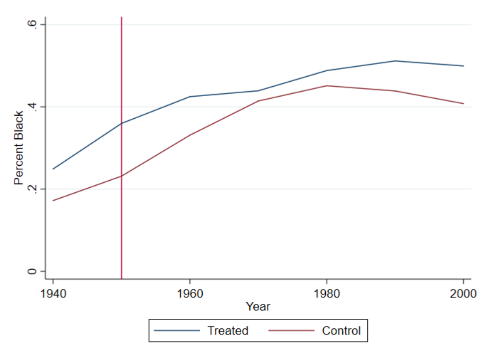

(a) Population Density




(b) Housing Density


(c) Median Rent

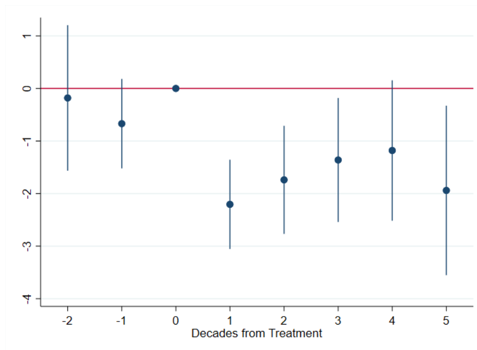

(d) Median Income

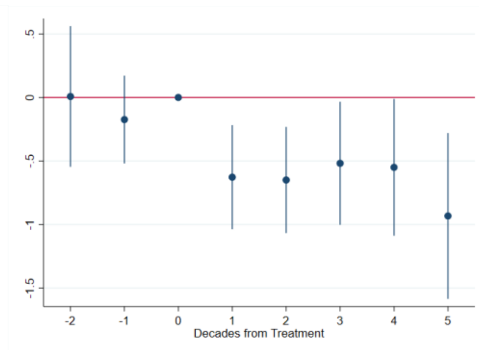



(e) Percent Black

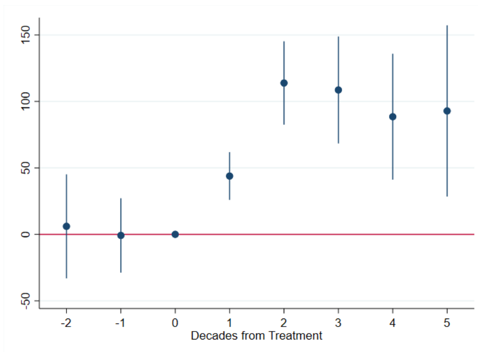
**Figure 1.6: Direct Impact of Urban Renewal (Summary Statistics)**

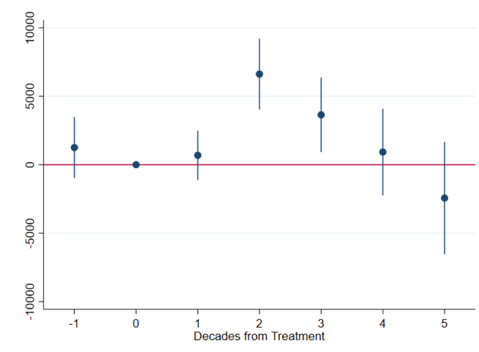
Notes: This figure shows the raw data for treated neighborhoods and predicted slums from control cities. A control city is defined as a city with four or fewer projects during my study period (1949-1965). Slums in control cities are determined through the use of a k-nearest-neighbor algorithm. Neighbor matches are restricted to neighborhoods in cities within the same region of the country to help control for regional effects. In this specification, k=5 was used in the k-nearest neighbors algorithm.



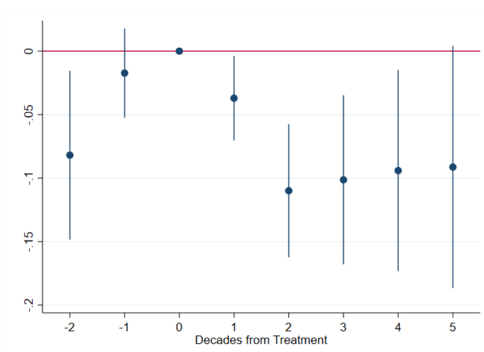




(a) Population Density (b) Housing Density




(c) Median Rent (d) Median Income




(e) Percent Black

**Figure 1.7: Direct Effects of Urban Renewal (Flexible Event Study Framework)**

Notes: This figure shows the regression results on the $\tau_k$ coefficients from equation (1.14). In this specification, k=5 was used in the k-nearest neighbors technique to identify urban blight in control cities. Robust standard errors are clustered at the neighborhood level.

**Table 1.1: Project Characteristics - Land Use**

| City | Projects | Acres | Resid. | Comm. | Indus. | Public. | Streets |
|---|---|---|---|---|---|---|---|
| Baltimore | 15 | 433 | 151 | 107 | 8 | 47 | 120 |
| Boston | 9 | 760 | 369 | 82 | 77 | 78 | 154 |
| Buffalo | 2 | 454 | 227 | 52 | 0 | 30 | 144 |
| Chicago | 31 | 1202 | 464 | 138 | 106 | 201 | 294 |
| Cincinnati | 5 | 432 | 39 | 47 | 171 | 33 | 143 |
| Cleveland | 7 | 597 | 255 | 50 | 122 | 48 | 123 |
| Columbus | 6 | 365 | 138 | 37 | 52 | 27 | 112 |
| Dallas | 0 | - | - | - | - | - | - |
| Detroit | 16 | 984 | 355 | 118 | 120 | 135 | 256 |
| Denver | 4 | 100 | 32 | 10 | 28 | 1 | 29 |
| Washington, DC | 6 | 755 | 217 | 47 | 100 | 93 | 298 |
| Indianapolis | 0 | - | - | - | - | - | - |
| Kansas City | 2 | 13 | 0 | 5 | 0 | 0 | 8 |
| Los Angeles | 1 | 136 | 16 | 73 | 0 | 7 | 39 |
| Louisville | 6 | 791 | 261 | 77 | 101 | 77 | 275 |
| Memphis | 6 | 561 | 230 | 62 | 36 | 36 | 197 |
| Milwaukee | 5 | 175 | 65 | 15 | 15 | 17 | 64 |
| Minneapolis | 6 | 429 | 157 | 45 | 90 | 4 | 133 |
| Newark | 11 | 389 | 172 | 58 | 8 | 32 | 119 |
| New Orleans | 2 | 15 | 3 | 1 | 2 | 4 | 6 |
| New York | 25 | 656 | 336 | 64 | 0 | 49 | 207 |
| Oakland | 2 | 192 | 31 | 3 | 66 | 3 | 89 |
| Philadelphia | 21 | 260 | 93 | 18 | 19 | 50 | 80 |
| Pittsburgh | 6 | 590 | 148 | 63 | 59 | 129 | 161 |
| Portland | 2 | 92 | 8 | 34 | 10 | 8 | 31 |
| San Francisco | 4 | 757 | 459 | 66 | 0 | 6 | 229 |
| Seattle | 2 | 112 | 24 | 11 | 43 | 6 | 27 |
| St. Louis | 4 | 710 | 163 | 30 | 293 | 9 | 215 |
| Total | 206 | 11930 | 4413 | 1313 | 1526 | 1130 | 3556 |

Notes: This information was obtained from "Urban Renewal Project Characteristics" (June 30,1966) U.S. Department of Housing and Urban Development - Renewal Assistance Administration.

**Table 1.2: Neighborhood Characteristics**

| | 1940 | | | 1950 | | |
|---|---|---|---|---|---|---|
| | treated | non-treated | p-value | treated | non-treated | p-value |
| Panel A: Population Characteristics | | | | | | |
| Population Density | 13.8 | 6.3 | [0.000] | 14.4 | 6.5 | [0.000] |
| | (0.60) | (0.08) | | (0.71) | (0.08) | |
| Unemployment Rate | 0.27 | 0.15 | [0.000] | 0.16 | 0.10 | [0.000] |
| | (0.01) | (0.00) | | (0.01) | (0.00) | |
| Percent Black | 0.25 | 0.05 | [0.000] | 0.31 | 0.07 | [0.000] |
| | (0.02) | (0.00) | | (0.02) | (0.00) | |
| Median Income | | | | 1906.6 | 2781.2 | [0.000] |
| | | | | (50.11) | (18.12) | |
| Panel B: Housing Characteristics | | | | | | |
| Housing Density | 4.25 | 1.87 | [0.000] | 4.3 | 2.01 | [0.000] |
| | (0.21) | (0.03) | | (0.23) | (0.03) | |
| Percent Vacant | 0.08 | 0.05 | [0.000] | 0.04 | 0.04 | [0.992] |
| | (0.00) | (0.00) | | (0.00) | (0.00) | |
| Percent Needing Repairs | 0.15 | 0.08 | [0.000] | | | |
| | (0.01) | (0.00) | | | | |
| Percent No Water | 0.04 | 0.06 | [0.000] | | | |
| | (0.00) | (0.00) | | | | |
| Median Housing Age | | | | 38.7 | 27.6 | [0.000] |
| | | | | (0.25) | (0.10) | |
| Panel C: Home Ownership Characteristics | | | | | | |
| Percent Owner | 0.13 | 0.39 | [0.000] | 0.17 | 0.50 | [0.000] |
| | (0.01) | (0.00) | | (0.01) | (0.00) | |
| Median Value | 1839.17 | 3710.59 | [0.000] | 1931.76 | 7000.48 | [0.000] |
| | (126.27) | (21.31) | | (161.05) | (45.37) | |
| Percent Renter | 0.79 | 0.54 | [0.000] | 0.80 | 0.46 | [0.000] |
| | (0.01) | (0.00) | | (0.01) | (0.00) | |
| Median Rent | 22.80 | 26.69 | [0.000] | 31.86 | 35.82 | [0.000] |
| | (0.51) | (0.12) | | (1.34) | (0.17) | |
| Observations | 448 | 14939 | | 448 | 14939 | |

Notes: This table presents summary statistics for the 28 cites in my sample broken down by treated and non-treated census tracts. P-values are from 2-sided t-tests. The null hypothesis that the difference of treated and non-treated means is equal to zero. Median income and median house age are not available in 1940. Share needing major repairs and share without running water are only available in 1940.

**Table 1.3: Determinants of Urban Renewal**

| | (1) | (2) | (3) |
|---|---|---|---|
| Percent Black | 0.37*** | 0.37*** | 0.60*** |
| | (0.11) | (0.13) | (0.17) |
| Unemployment Rate | 1.51*** | 0.98*** | 3.99*** |
| | (0.27) | (0.50) | (0.51) |
| Median Income | | -0.000 | |
| | | (0.000) | |
| Median House Age | | 0.021*** | |
| | | (0.004) | |
| Percent Vacant | 1.38*** | 0.840** | 4.92*** |
| | (0.36) | (0.398) | (0.62) |
| Median House Value | -0.000 | -0.000*** | -0.000*** |
| | (0.000) | (0.000) | (0.000) |
| Median Rent | -0.001*** | -0.001 | -0.002*** |
| | (0.0003) | (0.001) | (0.0004) |
| Percent Owned | -3.23*** | -3.09*** | -0.51 |
| | (0.31) | (0.312) | (0.43) |
| Percent Rented | -0.51** | -0.560*** | 2.10*** |
| | (0.23) | (0.231) | (0.38) |
| Population Density | 0.001 | -0.032*** | -0.009 |
| | (0.007) | (0.010) | (0.018) |
| Housing Density | 0.025 | 0.082*** | 0.068 |
| | (0.016) | (0.020) | (0.064) |
| Share Needing Major Repairs | | | -0.29 |
| | | | (0.41) |
| Share with No Running Water | | | -2.31*** |
| | | | (0.62) |
| City Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Years Included | 1940-1960 | 1950-1960 | 1940 |
| Observations | 34204 | 21757 | 11220 |
| R-squared | 0.34 | 0.35 | 0.38 |

Notes: This table presents the results of a probit regression of treatment on observable neighborhood characteristics. Robust standard errors are in parenthesis. Standard errors are clustered at the neighborhood level when panel data is used. *p < .10, **p < .05, ***p < .01. The outcome variable equals one if a tract received an urban renewal project before 1965.

**Table 1.4: Direct Effects of Urban Renewal on Population Density**

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated | -1.79*** | -1.47*** | -3.02*** |
| | (0.41) | (0.51) | (0.60) |
| Neighborhood Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| City Specific Linear Time Trend | Yes | Yes | Yes |
| Pretreatment Mean of the Treated | 14.00 | 15.39 | 12.58 |
| Sample | Full | Residential | Non. Resid. |
| Observations | 6286 | 4746 | 4746 |
| R-squared | 0.86 | 0.88 | 0.84 |

Notes: Robust standard errors are clustered at the neighborhood level. *p < .10, **p < .05, ***p < .01. The outcome variable in all columns is population per 1000sq meters. In this specification, k=5 was used in the k-nearest neighbors technique to identify urban blight in control cities. Column (1) uses all treated tracts while column (2) uses only treated tracts where the majority of the land was used for residential purposes and column (s) uses only treated tracts were the majority of the land was used for non-residential purposes.

**Table 1.5: Direct Effects of Urban Renewal on Housing Density**

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated | -0.54*** | -0.24 | -0.85*** |
| | (0.19) | (0.27) | (0.23) |
| Neighborhood Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| City Specific Linear Time Trend | Yes | Yes | Yes |
| Pretreatment Mean of the Treated | 4.42 | 4.82 | 4.01 |
| Sample | Full | Residential | Non. Resid. |
| Observations | 6286 | 4725 | 4746 |
| R-squared | 0.88 | 0.90 | 0.87 |

Notes: Robust standard errors are clustered at the neighborhood level. *p < .10, **p < .05, ***p < .01. The outcome variable in all columns is housing units per 1000sq meters. In this specification, k=5 was used in the k-nearest-neighbors technique to identify urban blight in control cities. Column (1) uses all treated tracts while column (2) uses only treated tracts where the majority of the land was used for residential purposes and column (s) uses only treated tracts were the majority of the land was used for non-residential purposes.

**Table 1.6: Direct Effects of Urban Renewal on Median Rent**

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated | 64.68*** | 52.10*** | 87.43*** |
| | (10.62) | (12.17) | (16.08) |
| Neighborhood Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| City Specific Linear Time Trend | Yes | Yes | Yes |
| Pretreatment Mean of the Treated | 267 | 287 | 245 |
| Sample | Full | Residential | Non. Resid. |
| Observations | 6286 | 4725 | 4746 |
| R-squared | 0.67 | 0.74 | 0.67 |

Notes: Robust standard errors are clustered at the neighborhood level. *p < .10, **p < .05, ***p < .01. The outcome variable in all columns is median rents. In this specification, k=5 was used in the k-nearest neighbors technique to identify urban blight in control cities. Column (1) uses all treated tracts while column (2) uses only treated tracts where the majority of the land was used for residential purposes and column (s) uses only treated tracts were the majority of the land was used for non-residential purposes.

**Table 1.7: Direct Effects of Urban Renewal on Median Income**

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated | 2878*** | 4423*** | 3755*** |
| | (901) | (1274) | (1214) |
| Neighborhood Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| City Specific Linear Time Trend | Yes | Yes | Yes |
| Pretreatment Mean of the Treated | 15949 | 17072 | 14662 |
| Sample | Full | Residential | Non. Resid. |
| Observations | 5388 | 4050 | 4746 |
| R-squared | 0.67 | 0.74 | 0.87 |

Notes: Robust standard errors are clustered at the neighborhood level. *p < .10, **p < .05, ***p < .01. The outcome variable in all columns is median income. In this specification, k=5 was used in the k-nearest neighbors technique to identify urban blight in control cities. Column (1) uses all treated tracts while column (2) uses only treated tracts where the majority of the land was used for residential purposes and column (s) uses only treated tracts were the majority of the land was used for non-residential purposes.

**Table 1.8: Direct Effects of Urban Renewal on Share Black**

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated | -0.05*** | 0.02 | -0.15*** |
| | (0.02) | (0.03) | (0.02) |
| Neighborhood Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| City Specific Linear Time Trend | Yes | Yes | Yes |
| Pretreatment Mean of the Treated | 0.31 | 0.29 | 0.34 |
| Sample | Full | Residential | Non. Resid. |
| Observations | 6286 | 4725 | 4746 |
| R-squared | 0.79 | 0.81 | 0.79 |

Notes: Robust standard errors are clustered at the neighborhood level. *p < .10, **p < .05, ***p < .01. The outcome variable in all columns is percentage black. In this specification, k=5 was used in the k-nearest neighbors technique to identify urban blight in control cities. Column (1) uses all treated tracts while column (2) uses only treated tracts where the majority of the land was used for residential purposes and column (s) uses only treated tracts were the majority of the land was used for non-residential purposes.

## 2.0 Race, Risk, and the Emergence of Federal Redlining

(joint with Price V. Fishback, Allison Shertzer, and Randall P. Walsh)

Federal "redlining" maps showing the perceived riskiness of lending in particular neighborhoods were created for nearly all major cities by the Home Owners' Loan Corporation at the height of the Great Depression. These maps have become a visual shorthand for government-sanctioned housing market discrimination in recent decades as formerly redlined neighborhoods continue to struggle. We provide a systematic empirical analysis of the redlining process, focusing on the role of race. In sum, we find little evidence that black neighborhoods were targeted for the highest risk rating, conditional on observable characteristics such as home values and income. Our results suggest that the federal government reinforced fundamental existing disparities between black and white neighborhoods rather than having directly causing racial inequity in housing markets.

### 2.1 Introduction

The federal government intervened in the housing market to an unprecedented degree in at the height of the Great Depression, loaning individual homeowners over three billion dollars between 1933 and 1935 (in 2020 dollars). By the mid-1930s, the federally funded Home Owner's Loan Corporation (HOLC) held about a tenth of all non-farm mortgages in the United States. To track the real estate risk associated with this vast and rapidly acquired portfolio of assets, the Federal Home Loan Bank Board (FHLBB) instructed its agents to develop city-level residential security maps in 1935. Between 1936 and 1939, after all HOLC loans had been made, teams of local real estate agents and bank officials working with the government agency representatives created maps delineating neighborhoods by lending risk along with detailed surveys of neighborhood-level characteristics for over 200 cities.

The characteristic color scheme of the HOLC maps, in which neighborhoods with the highest risk level were shaded in red, have become a visual shorthand for government-sanctioned housing market discrimination in America. The discourse on "redlining," which largely highlights the fact that black neighborhoods were almost entirely shaded in red, has spread across academia, the popular press, grass-roots activism, and presidential platforms as part of the debate on the relationship between government policy, racial discrimination and disparate outcomes.[28] Within economics a growing literature uses boundaries between security zones to assess the long-run impact of redlining on a range of issues including housing values, neighborhood racial composition, and crime (Aaronson, Hartley, and Mazumder 2019; Anders 2018; Krimmel 2017; Appel and Nickerson 2016).

Missing from this literature is a systematic evaluation of who was redlined in the late 1930s and why. Given the focus on racial discrimination in the redlining discourse, a careful analysis of the role of race in the security grade determination would be particularly useful. Did the vast majority of black households see their neighborhoods rated as the highest lending risk because of prejudiced assessors? Or was it the case that existing discrimination in employment, education,

[28] One influential academic work outside of economics is Rothstein's aptly-named *The Color of Law* (2017). The influential 2014 essay "The Case for Reparations" by Ta-Nehesi Coates prominently cited redlining as rationale for compensating African Americans for the economic harms of twentieth-century discrimination. As a presidential candidate, Elizabeth Warren proposed to compensate residents of formerly redlined neighborhoods in her *American Housing and Economic Mobility Act*. Even the term "redlining" has spread to many related areas; for instance, Zillow and GreatSchools have come under fire for their color-coded school ratings on real estate listings, which critics have referred to as "educational redlining" (for instance, see https://janresseger.wordpress.com/2020/01/13/22699/).

and housing left black households with few options outside of neighborhoods marked by crowding, dilapidated structures, and depreciating prices that would have represented poor lending risks? To what extent were white households exposed to redlining, and how did the security grade determination process differ by race? In sum, to what degree did the federal government drive prewar racial disparities in mortgage lending?

To answer these questions, we construct a unique spatial dataset covering households and neighborhoods for nine of the ten largest cities in the United States in 1930 and 1940. We also digitized the HOLC maps for these cities along with the detailed survey that accompanied each map. We begin by exploring how housing and economic characteristics varied across security zones, both in levels in 1930 and 1940 and trends across the decade. Using a dataset of almost 300,000 addressed matched to both census years, we then explore how demographic and economic characteristics varied at the boundary of HOLC security grades. Finally, we undertake a series of empirical exercises to understand the role that race played in the creation of HOLC's residential security maps. In particular, we assess the relative importance of racial discrimination in the mapping process in explaining oft-cited fact that the vast majority of black families lived in neighborhoods receiving the lowest (D) rating.

We begin by examining zone-level characteristics. In our sample of the largest cities, over 95 percent of black households lived in "D" zones. At the same time, the majority of the redlined populations was white, with over 4.4 million whites living in such areas (compared to fewer than one million blacks). Rents, home values, and occupational scores all declined with the security grade, with the "D" zones having the poorest residents and cheapest housing. However, redlined white neighborhoods had *better* census economic characteristics on average compared with redlined neighborhoods with an above-average number of black residents, suggesting that race was positively correlated with lending risk conditional on income and housing price.[29] We undertake several empirical exercises to further explore this notion and find little evidence that black neighborhoods were zoned disadvantageously or that race drove the security grade determination.

The descriptive statistics thus suggest that real estate agents did not discriminate against black neighborhoods in the rating process relative to economically similar white neighborhoods. However, this finding does not rule out discrimination in the placement of security zone boundaries. Using our matched address sample, we explore the location of black households with respect to C-D borders. We find that while most yellow-lined black households lived within 200 meters of a C-D boundary, there is little evidence of heaping on the redlined side, suggesting that the maps were not drawn to disproportionately zone black households into the highest risk category. We also perform a simple counterfactual in which we move the boundary between C-D boundary to eliminate potentially discriminatory placement. Because black families were typically living deep within neighborhoods that met HOLC's criteria for the highest lending risk, even sizeable boundary shifts would have led to only a small reduction in overall black exposure to redlining. We find that reassigning blacks that were potentially zoned in a discriminatory manner into yellowlined zones would only reduce the share of black houses in D zones by less than 4%.

What determined boundary placement if not race? We conduct a formal boundary analysis of the differences in economic characteristics of houses very close to C-D borders. Housing values were around \$400 lower and rents about \$1.50 (per month) lower on the redlined side of the boundary in 1930, years before the maps were drawn. Houses on the lower-graded side of the border were also occupied by individuals with lower occupational scores. Real estate agents thus appear to have accurately delineated neighborhoods by observable characteristics that impacted

[29] We use census enumeration districts as our proxy for neighborhood; see Shertzer, Walsh and Logan (2015).

lending risk. We also investigate if the agents drawing the maps accurately captured trends in neighborhood characteristics. Our results suggest that housing values on the lower-graded side declined by $130 dollars more relative to houses on the higher-graded side between 1930 and 1940. At the same time, the occupational standing of residents on the lower-graded side was deteriorating.

Our empirical results collectively suggest a more nuanced view of the HOLC and its successor the Federal Housing Administration (FHA), which also made use of neighborhood risk maps.[30] Our findings support the notion that these federal agencies reinforced and locked into place fundamental and longstanding disparities between black and white neighborhoods rather than having directly causing racial inequity in housing markets through the creation of discriminatory security grade maps. Our contention that the maps exhibit little evidence of explicit racial bias aligns with the conclusions of Hillier's unique 2003 study of Philadelphia, which found that lenders were avoiding areas that would be redlined before the maps were made and represents one of the very few empirical analyses of the redlining process in the existing literature.

A more complete understanding of the emergence of redlining must necessarily grapple with the fundamental changes that were occurring in private lending markets in the early twentieth century. A final contribution of our paper is to provide this context, focusing on how the security grading process reflected several ongoing and interrelated changes in how mortgages were made. Most prominently, we discuss the key interaction between the proliferation of longer-term, direct reduction loans, primarily from Building and Loans (Rose and Snowden, 2013), and the shift away from the highly localized lending. Arm's length, long-term mortgages elevated the perceived

[30] Unlike the HOLC maps, for which a nearly complete copy was stored in the National Archives, most FHA maps have been lost. Two exceptions are Greensboro, NC (Snowden) and Chicago (Aaronson, Hartley, and Mazumder).

importance of neighborhood trends for determining the riskiness of a loan. Real estate professionals of the day broadly agreed that racial transition was one factor that would sharply alter the trajectory of neighborhood home values (Babcock 1932, Burgess 1928). Thus, by the onset of the Great Depression, private banking institutions were already changing their practices to in response to this risk in ways that would disadvantage black homeowners.

## 2.2 Background and Historical Context

### 2.2.1 Home Finance Before the Crash

By the 1920s academics and other real estate professionals understood that neighborhoods were dynamic and that housing values would change over time (Burgess (1928), Cressey (1938), Gibbard (1941), Schietinger (1951, 1954)). Cities were viewed as a composition of zones and property values within those zones were impacted by the succession process: invasion of new demographic group, reaction/abandonment from the original residents, and the achievement of a new equilibrium of communal stability. Most neighborhood successions were gradual, but one factor that hastened neighborhood transition was race (Babcock 1932, Burgess 1928). As Frederick M. Babcock observed in *The Value of Real Estate* in 1932, “Most of the variations and differences between people are slight and value declines are, as a result, gradual. But there is one difference in people, namely race, which can result in a very rapid decline.” The dynamics of neighborhood successions meant that long term home loans carried an increased level of risk compared to short term loans, and that some of this uncertainty was driven by the potential for racial transitions that would impact property values.

Through the 1920s, lenders in the mortgage market had mitigated the risk of neighborhood succession by focusing predominately on localized lending and/or short-term loans. At the time, a typical mortgage was a 3-5 year straight, “balloon” loan through a bank, insurance company, or other lending institution (Snowden, 2013). Interest rates ranged from six to eight percent and loan amounts typically did not exceed 50 percent of a home’s value. Longer-term loans existed but were primarily originated in very localized markets. A common misconception is that the federal government was the first to issue standard 10-year, amortized installment mortgages in the United States, however, Building and Loan (B&L) companies used this type of loan since the 1880s (Rose and Snowden, 2013).[31] While less widespread, private lenders had also began to adopt long-term, locally issued loans in the 1920s (Van Dyke, 1929).

By the start of the Great Depression, B&Ls held more mortgage debt on single-family homes than all other lending institutions combined, with approximately 40% of home buyers borrowing through a B&L for some part of their mortgage (Snowden, 2010). Loans originated by B&Ls looked like standard 10-year, amortized installment mortgages. Originally, B&Ls were small, mutually owned organizations and focused on local real-estate markets. Members of B&Ls met regularly and paid weekly or monthly dues. Membership in B&Ls was typically limited to the existing members’ social and familial networks, meaning that members could acquire information about each other at low costs (Snowden, 2003). Furthermore, it was surveyors, title specialists, attorneys, real estate and insurance agents, homebuilders, and building material suppliers who organized and operated B&Ls on a part-time or voluntary basis (Snowden, 2003). The localized nature of early B&Ls allowed them to effectively manage the investment risk

[31] Building and loan companies were issuing loans with long time horizons as early as 1830, but it wasn’t until the 1880s that direct reduction loans were used by the industry (Rose and Snowden, 2013).

associated with changing neighborhood trends and demographics while simultaneously offering long-term loans.

During the 1920s, B&L industry leaders began to push for external regulation, and this regulation fell primarily to individual state building and loan leagues. The resulting regulations differed drastically from state to state and lacked uniformity (Snowden, 2003). The ultimate result was that by the late 1920s, the building and loan industry was increasingly heterogeneous in structure. While traditional small B&Ls still existed, additional types of B&Ls emerged, one of which was referred to as a permanent association (Snowden, 2003). Permanent associations looked more like savings banks. For example, they built home offices, opened for regular hours, and employed full-time, departmentalized staffs. These large, bureaucratic, professionally managed permanent associations eventually dominated the B&L industry. As a result, the market for home loans started to shift away from localized loans even before the federal government became involved via depression-era programs. This shift away from localized lending heightened the importance of perceived neighborhood trends in determining lending risk from afar.[32] When combined with the fact that racial transition was associated with a decline in housing values, these changes meant that the mortgage market was already changing in a way that would harm potential black borrowers.

[32] Organizers of the national associations argued that the innovation provided several benefits, one of which was greater safety due to having a geographically diverse loan portfolio (Snowden, 2003).

### 2.2.2 The Great Depression, HOLC, and the FHA

Shortly after large B&Ls began to dominate the home mortgage market, the Great Depression occurred. During the height of the Great Depression, an estimated 40 percent of mortgages were in default. Families struggled to refinance their loans and B&Ls became “frozen” (Rose and Snowden, 2013). Large B&Ls converted into Savings and Loans institutions which were required to have reserve holdings and savings accounts, meanwhile small B&L associations disappeared (Rose and Snowden, 2013).

The federal government responded to the unprecedented mortgage default rates by passing the Home Owner’s Loan Act in 1933. This act established the Home Owner’s Loan Corporation (HOLC), which was organized under the Federal Home Loan Bank Board (FHLBB). The purpose of the act was to help borrowers in danger of losing their homes by refinancing mortgages currently in default. HOLC purchased delinquent mortgage loans from lenders and refinanced the loans for the borrowers on more favorable terms. HOLC adopted features of B&L loans, for example, HOLC loans were amortized 15-year loans with five percent interest rates. Between 1933 and 1935 HOLC made over one million loans (Fishback et al., 2011). By 1936, HOLC had stopped originating loans and was focused solely on the management of the outstanding loans it made in the earlier half of the decade. Within a short period of time, HOLC discovered that despite the improved loan terms, many borrowers were still having trouble repaying their refinanced mortgages. HOLC ultimately foreclosed on 200,000 loans by the early 1940s (Fishback et al., 2011).

Due to the large number of mortgages that HOLC acquired, as well as the uncertainty surrounding the long-term mortgages it made as a non-local lender, HOLC introduced its City Survey program in the late 1930s to assess the risk of its current mortgage portfolio. Between

1935 and 1938, which was after the time period during which HOLC had made all of its loans, HOLC's City Survey Program created residential security maps for virtually every city with more than 40,000 residents (Greer, 2012). The residential security maps categorized neighborhoods, for risk assessment purposes, into the following four categories with identifying colors and letters: green (A); blue (B); yellow (C); and red (D). The green (A) rating signified the lowest level of lending risk, while a red (D) rating signified the highest. To aid in the development of the maps, the City Survey Program consulted with local real-estate professionals including local bank loan officers, city officials, and realtors in every major city to assess perceived lending risk on a neighborhood-by-neighborhood basis.

Neighborhoods were categorized based on several criteria including the age and condition of housing, access to amenities such as transportation and parks, the neighborhood's racial and ethnic composition, and the economic status of neighborhood residents. Field agents also documented the availability of mortgage funds within each of the security grade zone. In general, lenders were most willing to make a maximum loan, meaning a loan in the amount of 75-80% of an appraised home value and amortized over a 10-15 year period, in A zones. In B zones, lenders had a tendency to keep loan commitments 10-15% under the maximum loans seen in A zones. In C zones, lenders were more conservative and held loan commitments under the lending ratio for both A and B zones. Lastly, in D zones, loans were made on the most conservative terms and some lenders refused to make any loans in D zones.[33]

HOLC made about 50 copies of each map which were distributed to various government agencies, including the Federal Housing Administration (FHA) (Hillier, 2003a). The FHA was

[33] This statement has been verified using archival material and journal articles from the 1930s, all of which suggests that lenders were avoiding redlined areas before HOLC made its maps (Hillier, 2003a).

established by the National Housing Act of 1934 to help stabilize the mortgage market by setting underwriting standards and insuring mortgage loans. To determine eligibility for FHA mortgage insurance, the FHA appraised each property to determine the risk of default. To guide this appraisal process, the FHA published an Underwriting Manual, the first of which was issued in 1935. Unlike HOLC, which rated entire neighborhoods, FHA only appraised individual mortgages. However, FHA underwriters did incorporate neighborhood risk along with information about the borrower and the property in determining insurance eligibility.

While the theory that HOLC's redlining maps were originally made to facilitate discrimination is not supported by history, HOLC residential security maps are often cited as a discriminatory governmental policy which caused the urban blight afflicting many redlined areas. Although it is not the focus of our paper, the typical narrative regarding the long-run discriminatory effects of HOLC residential security maps can be outlined as follows. FHA obtained HOLC's residential security maps and used them to decline mortgage insurance applications in redlined areas.[34] The lack of FHA insurance, and/or bank use of the HOLC security maps as an underwriting factor, led to banks charging higher interest rates and offering less favorable lending contracts to borrowers located in redlined areas. And, finally, the less favorable lending contracts discouraged homeownership and increased home vacancies, leading to depreciated housing values and ultimately causing a decades long decline in neighborhood outcomes.

[34] We know that while the FHA had a copy of HOLC's security grade maps, they also created their own maps. FHA maps have largely been lost to history with few exceptions.

## 2.3 Data

In this paper, we construct a novel spatial dataset linking 1930 and 1940 census data covering single family homes and neighborhoods to HOLC residential security zones in nine cities to empirically investigate the creation of HOLC's redlining maps.[35] We began by digitizing both residential security maps and detailed survey that accompanied the maps for each of our cities, giving us a total of 927 security zones. These surveys documented housing characteristics, including housing prices, construction type (brick, frame, or other), and the general state of repair (excellent, good, fair or poor), as well as population characteristics, including the occupation, income, and racial composition of neighborhood residents. These surveys also documented the future desirability trend of each zone. Descriptive characteristics from HOLC surveys are presented by security grade in Panel A of Table 2.1.[36] Recall that neighborhoods with a green (A) rating signified the lowest level of perceived lending risk, while a red (D) rating signified the highest. Reported income, housing values, and rents are negatively correlated with perceived lending risk, while the share of black and foreign-born residents have the opposite association.

In previous work we constructed fine-grained, spatially-identified demographic data for neighborhoods in nine of the largest northern cities for 1930 and 1940 and created neighborhoods that are comparable over time across these two years.[37] Neighborhoods are measured by an

[35] Our sample includes data from the following large northern cities: Baltimore, Boston, Brooklyn, Chicago, Cleveland, Detroit, Manhattan, Philadelphia, Pittsburgh, and St. Louis.

[36] Summary statistics of additional variables are presented in Table B1 in the appendix. Additional variables include construction type (brick, frame, other) and the general state of repair. Green (A) and blue (B) zones were more likely to have brick houses in good condition, while yellow (C) and red (D) zones were more likely to consist of frame houses in fair or poor condition. The future trend desirability of different zones is also highly correlated with security grades; green (A) and blue (B) zones were more likely to be coded as trending upward while yellow (C) and red (D) zones were more likely to be coded as trending downward.

[37] See Shertzer, Walsh, and Logan (2016) and Akbar et al. (2019) for more details.

enumeration district which typically covered between one and four city blocks in an urban area. For this paper, we also calculated the share of each neighborhood that lies in each security grade. Summary statistics for our ED data aggregated to the zone level are presented in Panel B of Table 2.1.[38] This data confirms that most black households were redlined; over 95% of black households were designated into redlined zones. However, most households living in redlined zones were white, with over 4.4 million white families from our sample cities were redlined compared to less than one million black families. Furthermore, among the approximately 200,000 owner occupied homes in redlined areas, less than 8% were occupied by black families. This panel once again highlights the large differences in observable characteristics across security grades. Like the summary statistics for the HOLC survey variables, we see that housing values, rents, occupation scores, and the share of owner-occupied housing are negatively correlated with perceived lending risk, while the share of black and foreign residents have the opposite association.

Given the importance of race in the general narrative of HOLC's redlining maps, in columns (5)-(8) of Table 2.1 we present summary statistics for yellow-lined and redlined zones based on the racial composition of each zone. High share black residential security zones are defined as security zones with an above average share of black residents and low share black residential security zones are security zones with a below average share of black residents. A general pattern emerges with high share black redlined zones having worse reported survey and census outcomes than low share black redlined zones. For example, high share black zones were reported in HOLC's surveys as having an average family income of $1346 and average house

[38] We also compare the reported survey data to census data. This information was collected at different points during the decade, so we don't expect the information to be exactly the same. Even so, we can generally conclude that HOLC surveyors were able to accurately report neighborhood characteristics, which highlights the knowledge of HOLC surveyors had regarding local mortgage markets. These results are presented in Figures B3-B6 in the appendix.

values of $3799 while the respective numbers for low share black zones were $1563 and $4325. We see similar patterns in the census data with high share black zones having average occupation scores of 22.61 and average housing values of $6720 while the respective numbers for low share black zones were 24.35 and $7237. Thus, on average, high share black zones generally had characteristics that were more consistent with high levels of lending risk, suggesting that black neighborhoods were not disproportionately redlined conditional on observable characteristics, a pattern that we further explore in the empirical section of this paper.

In addition to ED and zone level data, we also use a panel dataset of geocoded addresses matched between 1930 and 1940 which first presented in Akbar et al., 2019. To create this dataset, we developed an algorithm following the individual matching literature while also leveraging the structure of the census manuscripts digitized historical street files, and neighborhood geography to improve match accuracy.[39] For this paper, we assigned each address to a security zone and to the nearest HOLC boundary. We also calculated the distance from each address to its assigned HOLC boundary.[40] This address level data is particularly useful for empirically exploring the delineation of HOLC boundaries. Household level summary statistics are reported for both 1930 and 1940 by security grade in Table B2 in the appendix.[41] The overall patterns of this data are similar to the patterns presented in Panel B of Table 2.1 and confirm that security grade classification represents economically and statistically significant differences in observable characteristics.

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[39] For more on the individual matching literature, see Long and Ferrie (2013), Feigenbaum (2016) and Bailey et al. (2017).

[40] We drop all houses that are within 30 meters from an HOLC boundary to mitigate any concern over measurement error and to prevent any comparisons of a households directly across the street from one another. We choose 30 meters since this is the average depth of a household plot.

[41] For example, housing values averaged around $9400 in 1930 for zones coded as green (A) while only averaging $5400 in zones coded as red (D). The percentage change of each variable is also reported. Average housing values decreased by 20% in green zones between 1930 and 1940 but decreased by 43% in red zones.

## 2.4 Empirical Approach

### 2.4.1 To What Extend Did HOLC Maps Accurately Reflect Observable Characteristics and Lending Risk?

The extent to which HOLC maps reflected neighborhood characteristics is evident from the summary statistics in Table 2.1. We focus the rest of this paper on C-D boundaries since this distinction had the biggest importance on mortgage availability and terms. To explore if HOLC boundaries represent meaningful discontinuities in observable neighborhood characteristics we estimate the following regression equation:

$$y_{ij1940} = \alpha + \beta lgs_{ij} + \rho dist_{ij} + \varphi dist_{ij} * lgs_{ij} + \gamma_j + \epsilon_{ij} \tag{2.1}$$

where $y_{ic1940}$ is an outcome for address i on boundary j in 1940, and $dist_{ij}$ is a measure of the distance of address i to boundary j. $lgs_{ij}$ equals 1 if address i is on the lower-grade side of boundary j, $\gamma_c$ are boundary fixed effects, and $\epsilon$ is the error term. The coefficient of interest is $\beta$ which measures the extent to which addresses on the lower-grade side of a boundary were different from addresses on the higher-grade side. We are specifically interested in housing values, rents, occupational scores, and racial demographics. We focus on 1940 as our main specification since the surveys and maps were created in the late 1930s.[42] Boundary fixed effects will control for any unobservable characteristics that vary continuously across the boundary. Our main specification reports results based on an optimal bandwidth selection proposed by Calonico, Cattaneo, and Titiunik (2014) and we document the robust of our results to other bandwidth choices.

[42] All results are replicated for 1930. All results are similar in sign, magnitude, and statistical significance.

To understand the extent to which assigned grades accurately reflected real estate lending risk, we utilized the sample of matched addresses to determine if addresses assigned to the lower grade side of a HOLC boundary trended differently between 1930 and 1940 than addresses on the higher-grade side. We estimate the following regression equation:

$$y_{ij1940} = \alpha + \beta lgs_{ij} + \delta y_{ij1930} + \rho dist_{ij} + \varphi dist_{ij} * lgs_{ij} + \gamma_j + \epsilon_{ij} \qquad (2.2)$$

where $y_{ic1930}$ is the lagged value of the outcome variable and all other variables are the same as previously defined. The coefficient of interest is $\beta$ which measures the extent to which addresses on the lower-grade side of a boundary were trending differently from addresses on the higher-grade side.

### 2.4.2 What Role Did Race Play in the Creation of HOLC Maps?

We are particularly interested in understanding how race did or did not distort the mapping process. We use various empirical exercises to determine the role race played in the mapping process. We begin by calculating the predicted probability an enumeration district is redlined, ignoring race, and comparing these distributions based on the racial composition of the enumeration district. If black enumeration districts were disproportionally redlined conditional on other observable characteristics, we would expect to see the predicted probability density of high share black redlined neighborhoods fall to the left of the predicted probability of low share black redlined neighborhoods. On the other hand, if the predicted probability of being redlined for the high share black neighborhoods falls to the right of the distribution of the low share black redlined neighborhoods, then the large share of black households being redlined is likely due to differences in observable characteristics. A similar analysis is performed on yellow lined neighborhoods.

Another potential way in which discrimination could have been codified into HOLC's redlining maps is if natural zone boundaries were shifted to encapsulate black households. To explore this potential phenomenon, we begin by graphing the share of black households by distance to a C-D boundary to show there was potential for this type of mapping distortion. We then graph kernel densities of the number of black households based on distance to the C-D boundary. If the mapping process was distorted to specifically redline black households, we would expect to see a bunching of black households directly adjacent to a C-D boundary in redlined zones.

We are also able to utilize the survey question that documented the perceived trend of neighborhood desirability in the next ten to fifteen years, independent of the assigned security grade. We use census data aggregated to the zone level as our unit of analysis and estimate the following model:

$$y_{ic1940} = \alpha + \beta desirabiliity_{ic} + y_{ic1930} + \gamma_c + \epsilon_{ic} \tag{2.3}$$

where $y_{ic1940}$ is an outcome for zone i in city c in 1940 and $y_{ic1930}$ is the lagged value of the outcome variable; $desirability_{ic}$ is a categorical variable indicating if the zone was perceived to be trending downward, slightly downward, slightly upward, upward, or remain static; $\gamma_c$ are city fixed effects which control for any unobservable characteristics that are constant across all security zones in a city. The coefficients of interest are the $\beta s$ which tells us the extent to which the predictions about the desirability of security zones are accurately reflected in the census data. Focusing on how the estimated $\beta s$ vary for different outcome variables can help inform which factors survey informants considered important when determining the desirability of a neighborhood.

## 2.5 Empirical Results and Discussion

### 2.5.1 To What Extend Did HOLC Maps Accurately Reflect Observable Characteristics and Lending Risk?

We begin our empirical analysis of the creation of redlining maps with Figure 2.1 which plots 1940 household level census data for households assigned to a C-D HOLC boundary.[43] Binned means are graphed by distance to a C-D boundary with negative distances representing yellow-lined zones and positive distances representing redlined zones. Figure 2.2 presents analogous information but adds controls for 1930 levels. Our variables of interest are house values, rents, occupation scores, and race. Consistent with Table 2.1, these figures show meaningful differences in observable characteristics between C and D zones, both in levels and trends. HOLC boundaries also appear to represent meaningful discontinuities in neighborhood characteristics.

We confirm that economically significant discontinuities exist at HOLC boundaries by estimating equations (2.1) and (2.2). The results are presented in Table 2.2. Columns (1) and (2) present the regression results from equation (2.1) using only 1930 data and Columns (3) and (4) present the same specifications using only 1940 data and not controlling for any lagged variables. Housing values were approximately $400 lower on the lower grade side of a C-D boundary, and rents were around $1.50 cheaper. Furthermore, houses on the lower grade side of a boundary were more likely to be occupied by individuals with lower occupation scores and black families.

[43] Figure B7 in the appendix replicates this figure with 1930 data. We choose to focus on 1940 since the survey data was collected relative closer to 1940 than to 1930. All results are similar in size and magnitude if we instead focus on 1930.

We are also interested in the extent to which households on either side of a HOLC boundary were trending differently between 1930 and 1940. Columns (5) and (6) of Table 2.2 show the regression results from equation (2.2) with controls for 1930 values. We find that house values on the redlined side of a C-D boundary declined by $130 dollars between 1930 and 1940 compared to house values on the yellow-lined side. Occupation scores declined by around .8 and the share of black households increased by 1.6 percentage points. Figures 2.3 and 2.4 present the coefficient and 95% confidence intervals estimated when we vary the bandwidth used in the estimation procedure.[44]

This analysis, combined with the summary statistics presented in Table 2.1, provides evidence that HOLC security zones reflect meaningful differences in both the levels and trends of neighborhood characteristics, including housing values, rents, and occupation score. When HOLC was assigned the task of assessing neighborhood risk, they were able to accurately identify neighborhood levels and trends. These results also highlight an important and oft cited relationship between race and risk assessment. We see high shares of black households in D zones and discontinuities in the share of black households at C-D boundaries. We also see that the share of black households was increasing between 1930 and 1940 in neighborhoods that were redlined. These effects could be driven by two potential forces: discrimination in the mapping process or discrimination in other private markets that resulted in black communities being synonymous with lower income, declining communities.

We know that racial transition was associated with decreased housing values in the private market, so assigning a lower security grade rating to neighborhoods that had an increasing trend

[44] When we use a similar bandwidth to Aaronson et al. 2019, we obtain similar point estimates. Results are not exactly the same given that our paper focuses on a sample of 9 northern cities and uses a set of addresses linked between 1930 and 1940.

in the share of black residents is consistent with understanding of lending risk at the time. Thus, while HOLC maps did not introduce this dynamic into the housing market, it did institutionalize the relationship between race and risk into its maps. We have also shown that the great majority of redlined houses were white and that white redlined enumeration districts tended to have higher housing values, rents, incomes, and other more desirable neighborhood characteristics compared to high share black redlined enumeration districts. This provides more suggestive evidence that the large numbers of black households in redlined areas was not driven by discrimination in the mapping process.

### 2.5.2 What Role Did Race Play in the Creation of HOLC Maps?

To help further understand the extent to which race did or did not distort the mapping process, Figure 2.5 graphs the predicted probability of being redlined for high and low share black enumeration districts separately. Panel A focuses only on redlined neighborhoods and Panel B focuses on yellow lined neighborhoods. In both panels the predicted probability distribution of high share black neighborhoods falls to the right of the predicted probability of low share black neighborhoods. This analysis suggests that black neighborhoods had other characteristics, such as lower incomes and lower housing values, that increased their likelihood of being redlined, independent of racial composition. Previous discrimination in private labor and housing markets contributed to the creation of lower income, segregated communities, many of which were redlined based on non-racial observable characteristics.

Another potential mechanism through which discrimination could enter the mapping process is if map boundaries were delineated to encapsulate black households. If black households lived close to a prospective HOLC boundary, map creators could have extended the boundaries to ensure that black families lived in redlined communities. However, we find no evidence that this sort of discrimination existed in the mapping process. Panel A of Figure 2.6 shows the share of households that are black based on the distance to a C-D boundary. This figure shows that while most black households lived in redlined neighborhoods, there were yellow-lined black households living within 200 meters of a C-D boundary (i.e. there was potential for discrimination). Panel B of Figure 2.6 shows the kernel densities of the share of black households living in both C and D zones by the distance to a C-D boundary. The great majority of black households living in yellow-lined areas resided within 200 meters of a redlined area and they could have been easily incorporated into redlined communities. We don't see any similar mass in the number of black households on the redlined side of HOLC boundaries. While the location of black households in yellow-line areas provided an opportunity for HOLC to act in a discriminatory way, we find no evidence that HOLC took this opportunity to disproportionately redline black houses. While we do see a slight jump in the probability a given household is occupied by a black family on the redlined side of the boundary, reassigning blacks that were potentially zoned in a discriminatory manner into yellowlined zones would only reduce the share of black houses in D zones by less than 4%.

Lastly, we focus on the survey of HOLC zones specifically. For every zone, enumerators were asked to document the area's future trend desirability. We estimate equation (2.3) using zone level data and graph the coefficients and 95% confidence intervals in Figure 2.7. Panel A suggests that zones that were coded as have an upward desirability trend had higher housing values by 1940, controlling for housing values in 1930, than zones that were coded as trending downward.[45] Furthermore, the magnitude of the coefficients for each category (slightly downward, static,

[45] Regression results are also presented in Table B3 in the appendix.

slightly upward, upward) are consistent with trends in the census data. Similar patterns emerge for rents and occupation scores in Panel B and Panel C respectively. This suggests that survey information collected for the creation of HOLC security grade maps accurately identified the desirability of neighborhoods based on housing prices and income of the relevant population. Panel D replicates this analysis for the share of black residents in a zone. Here we find no statistical differences between zones that were coded as decreasing in desirability and that those coded as increasing in desirability. This suggests that race was not the primary factor in determining how the real-estate professions who informed the creation of HOLC maps answered the question as to which neighborhoods were becoming more desirable. We have replicated this result for only yellow-lined and redlined zones and consistently find no relationship between the trend of future desirability and the racial composition of a neighborhood.

Taken together, these results suggest that while most black households were redlined, this wasn't the result of discrimination by HOLC specifically. High share black redlined neighborhoods had observable characteristics that were more consistent with high lending risk than low share black redlined neighborhoods, implying that black neighborhoods were not disproportionately redlined based on observable characteristics. Further, HOLC had the opportunity to encapsulate all black households into D zones but they didn't, and race does not appear to be a factor in documenting the reported future trend desirability of a given neighborhood. It is more likely that private market discrimination, both in housing and labor markets, resulted in the concentration of low-income black families living in low cost communities before the federal government intervened in the housing market.

## 2.6 Conclusion

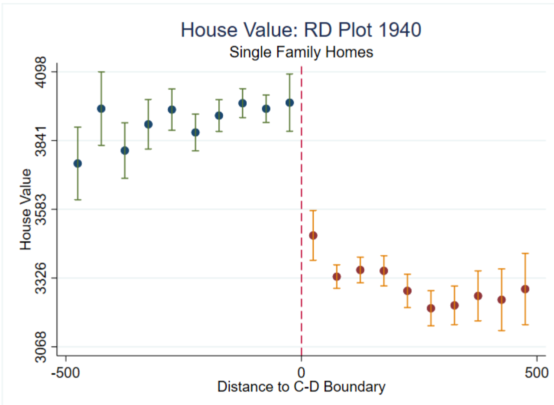
Given the attention received by HOLC's redlining maps, and their legacy as a mechanism of government sponsored discrimination, in this paper we provide an analysis of the redlining process, with a particular interest in understanding how accurately these maps represented lending risk and the role race played in the creation of the maps. We began by placing HOLC and redlining in a broader economic history context, focusing on how banking institutions, lending practices, and risk assessment were changing during the early twentieth century. The repeated narrative of government sponsored redlining maps often lacks historical context of the mortgage market pre-1930 and falsely claims that HOLC and FHA introduced the modern mortgage as it is known today. In this paper we highlight that the building and loan industry had been using direct reduction, long-term loans since the 1880s and that changes in the B&L industry caused these companies to become larger more bureaucratic institutions that began lending at arm's length. We also highlight that academics and other real estate professionals at the time understood that neighborhoods characteristics changed over time, with racial transitions hastening neighborhood dynamics (Burgess, 1928; Cressey, 1938; Gibbard, 1941; Schietinger, 1951, 1954)). This ultimately increased the risk associated with issuing long-term mortgages. In summary, banking institutions were already changing a way that was going to harm potential black borrowers even before HOLC and the FHA intervened in the housing market.

With this understanding of the relationship between lending risk and race in the early 1900s, we then provide an empirically analysis of the redlining process, focusing on the extent to which the redlining maps reflect the lending assessment knowledge of the time. We find that maps were created in a way that accurately reflected neighborhood lending risk given the historical setting. Neighborhood characteristics varied across security grades, both in levels and trends. We also show that HOLC boundaries represented meaningful discontinuities in neighborhood characteristics.

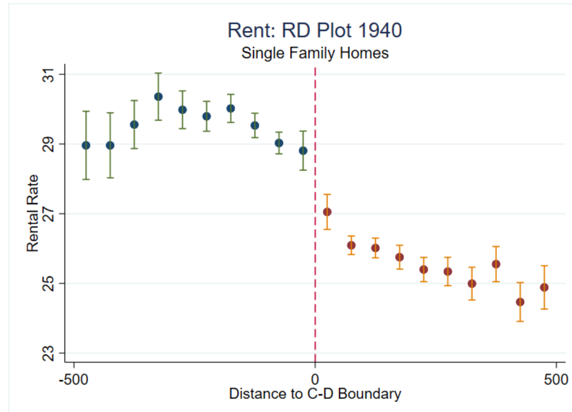
Lastly, we show that race doesn't appear to have been the determining factor in grade assignment. High share black redlined neighborhoods had a higher likelihood of being redlined based on other observable characteristics, such as housing values and incomes, compared to low share black redlined neighborhoods. Further, given the relative location of yellow-lined black households, we show that HOLC had the opportunity to delineate security zone boundaries in a way that encapsulated all black households into redlined areas; however, we don't find any evidence that HOLC altered the maps in this manner. Lastly, race does not appear to be a factor in documenting the reported future trend desirability of a given zone.

We concluded that most black households were redlined largely as the result of private market discrimination, both in housing and labor markets, which limited the housing options available to low-income black families. While HOLC's security grade maps didn't introduce new racist dynamics into the housing market, they did institutional existing, racially biased lending dynamics already prevalent in the private market. The government's actions during this period can be described as malignant neglect and represent a missed opportunity by the federal government to remedy discriminatory private market outcomes.

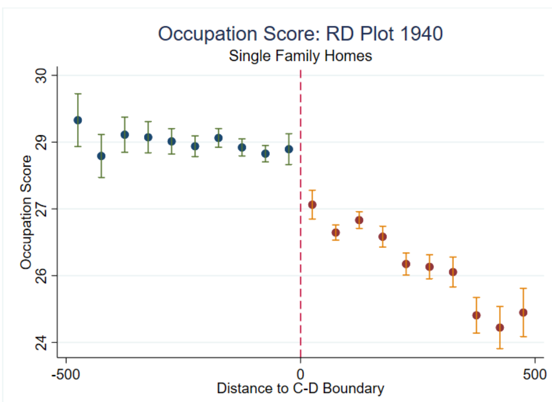
## 2.7 Figures and Tables



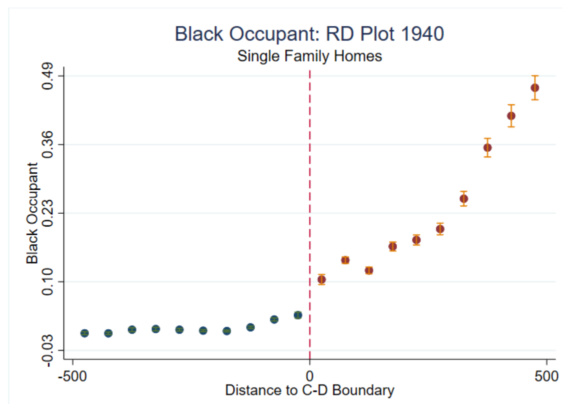

(a) House Values




(b) Rents



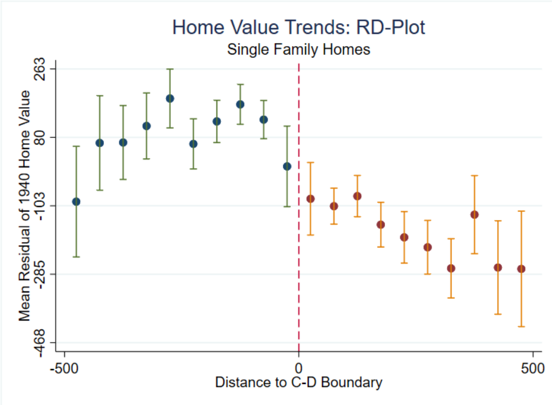

(c) Occupation Scores



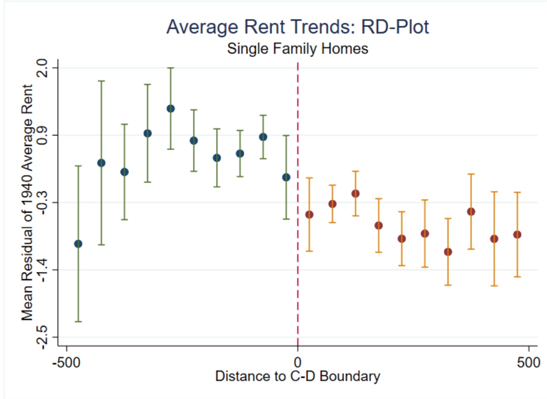

(d) Racial Composition

**Figure 2.1: 1940 Levels by Distance to HOLC Boundary**

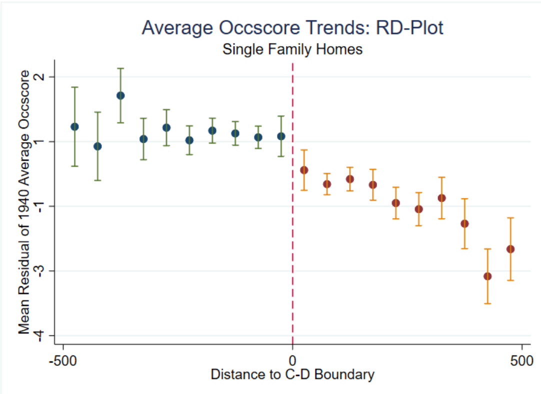
Notes: This figure shows binned averages of 1940 census data for single family households by distance to a C-D HOLC boundary. The red dotted line represents the HOLC boundary, positive distances represent households in the redlined zone, and negative distances represent houses in the yellow-lined zones. All distances are measured in meters.



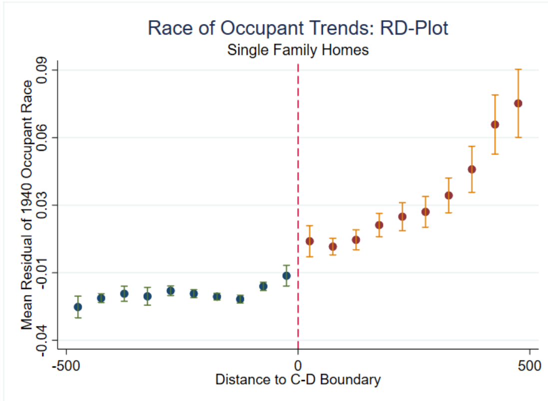

(a) House Values




(b) Rents



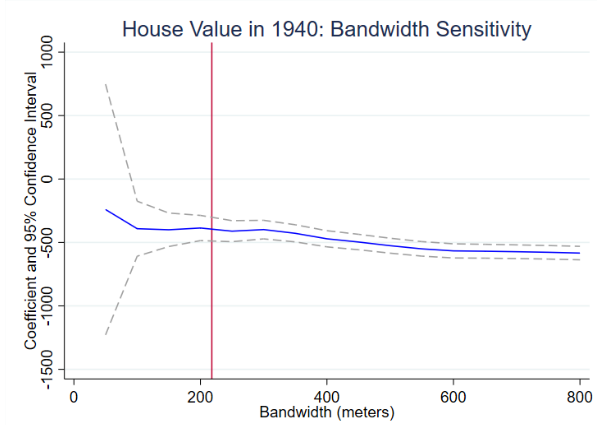

(c) Occupation Scores



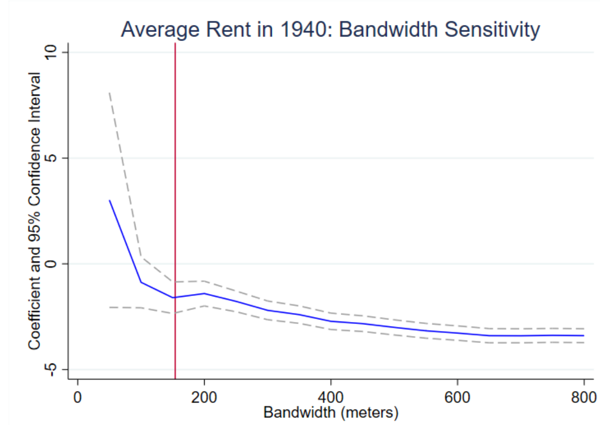

(d) Racial Composition

**Figure 2.2: Trends by Distance to HOLC Bundary**

Notes: This figure shows binned averages of 1940 census data, controlling for 1930 values, for single family households by distance to a C-D HOLC boundary. The red dotted line represents the HOLC boundary, positive distances represent households in the redlined zone, and negative distances represent houses in the yellow-lined zones. All distances are measured in meters.

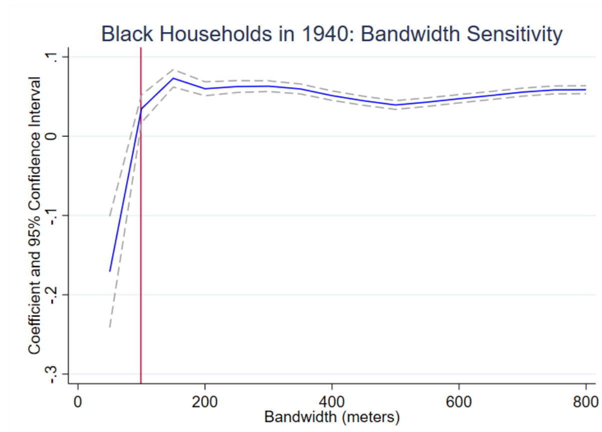



(a) House Values




(b) Rents

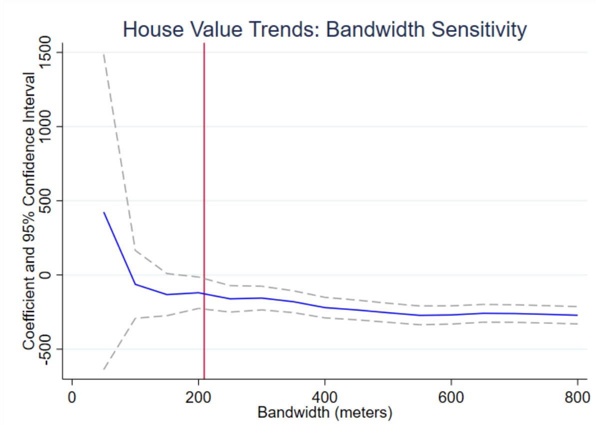

(c) Occupation Scores



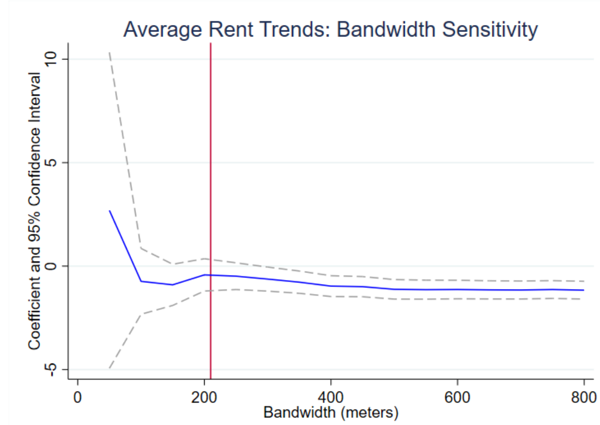

(d) Racial Composition

**Figure 2.3: Optimal Bandwidth and Bandwidth Sensitivity for 1940 Levels**

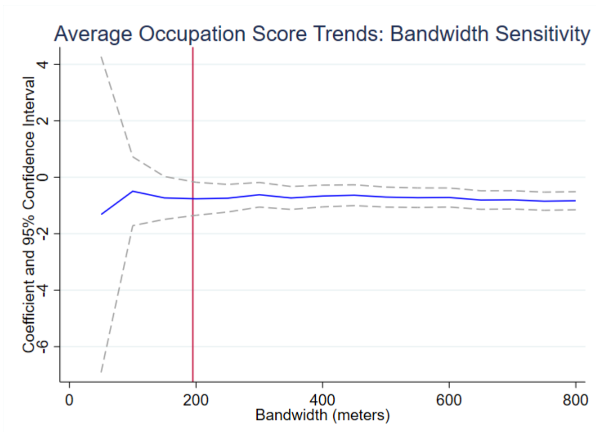
Notes: This figure graphs the estimated coefficients and 95% confidence intervals of $\beta$ from equation (2.1) as we vary the bandwidth around a HOLC boundary. Bandwidth selection ranges from 50 to 800 meters. The red line represents the optimal bandwidth selection procedure proposed by Calonico, Cattaneo, and Titiunik (2014).



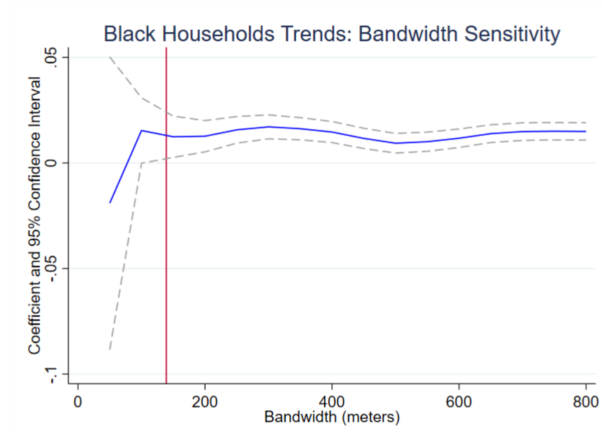

(a) House Values




(b) Rents



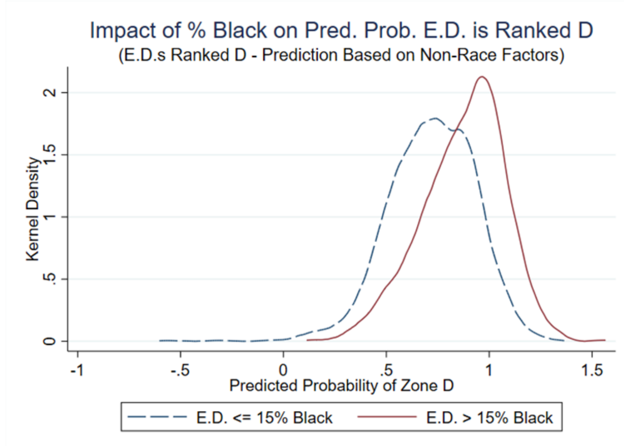

(c) Occupation Scores



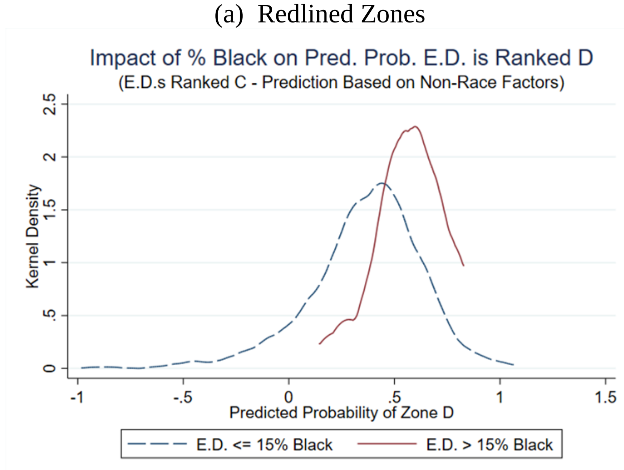

(d) Racial Composition

**Figure 2.4: Optimal Bandwidth and Bandwidth Sensitivity for Trends**

Notes: This figure graphs the estimated coefficients and 95% confidence intervals of β from equation (2.2) as we vary the bandwidth around a HOLC boundary. Bandwidth selection ranges from 50 to 800 meters. The red line represents the optimal bandwidth selection procedure proposed by Calonico, Cattaneo, and Titiunik (2014).



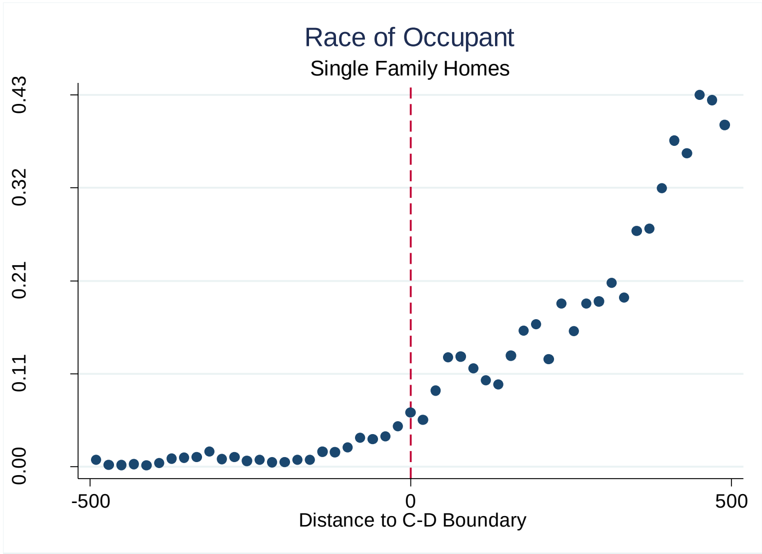

(a) Redlined Zones



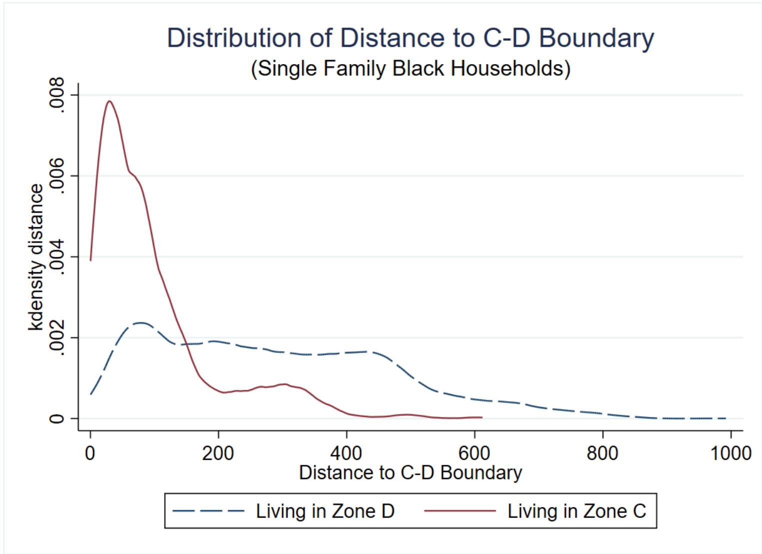

(b) Yellow-lined Zones

**Figure 2.5: Observable Characteristics and Discrimination**

Notes: This figure shows the distribution of an enumeration districts predicted probability of being redlined. Predicted probabilities were calculated using 1930 ED census data. Data is presented based on the share of black households in an enumeration district.



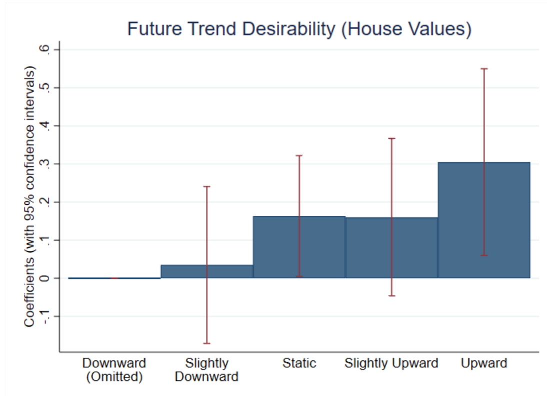

(a) Share Black by Distance to CD Boundaries



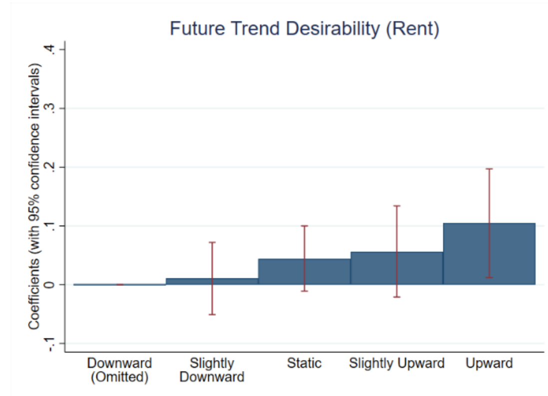

(b) Distribution of Black Households by Distance to CD Boundaries

**Figure 2.6: Black Families relative Location to CD Boundaries**

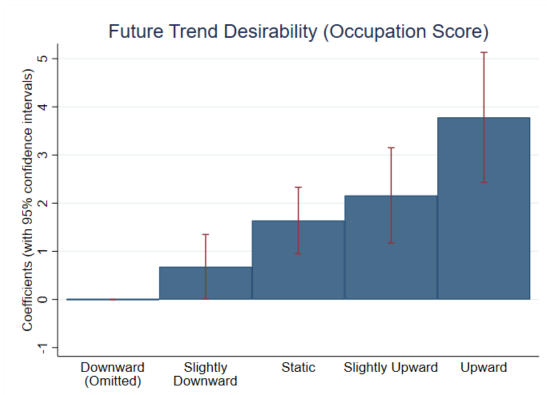
Notes: This figure shows the relationship between race and distance to a C-D boundary. Panel A shows the share of black residents based on distance to a HOLC boundary, with negative distances representing yellowlined areas and positive distances representing redlined areas. Panel B shows the kernel densities of black households and their distance to a C-D boundary.



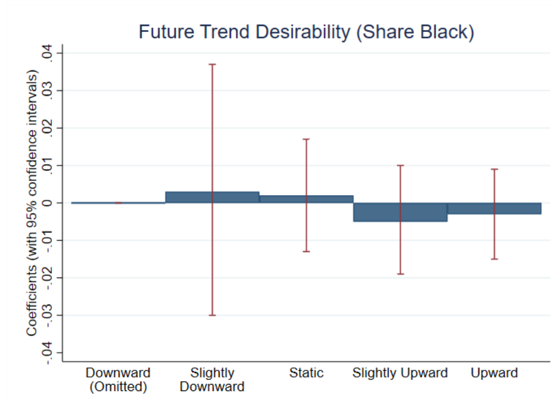

(a) House Values




(b) Rents




(c) Occupation Scores




(d) Racial Composition

**Figure 2.7: Determinates of Future Trend Desirability Code**

Notes: This figure shows the coefficient and 95% confidence interval for the indicator variables related to the future trend desirability of a neighborhood with downward being the omitted category. Each panel presents the results from a separate regression. Each regression controls for the 1930 value of the outcome variable and includes security grade fixed effects.

**Table 2.1: Census and Survey Data Summary Statistics**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Zone A All | Zone B All | Zone C All | Zone D All | Zone C Black Only | Zone C White Only | Zone D Black Only | Zone D White Only |
| Panel A: Survey Data | | | | | | | | |
| Family Income | 21716 | 6836 | 3241 | 1467 | 3315 | 3236 | 1346 | 1563 |
| | (22467) | (8453) | (6242) | (827) | (2404) | (6457) | (594) | (969) |
| House Value | 16683 | 10697 | 7283 | 4175 | - | 6631 | 3799 | 4325 |
| | (12441) | (5001) | (9856) | (2534) | - | (3249) | (2805) | (2416) |
| Rent | 83.10 | 62.19 | 40.92 | 24.88 | 88.17 | 40.42 | 22.71 | 25.72 |
| | (56.16) | (22.90) | (14.64) | (10.99) | (3.30) | (13.88) | (13.73) | (9.70) |
| Share Black | 0.00 | 0.00 | 0.00 | 0.19 | 0.04 | 0.00 | 0.47 | 0.02 |
| | 0.00 | (0.00) | (0.02) | (0.30) | (0.05) | (0.02) | (0.33) | (0.05) |
| Share Foreign | 0.02 | 0.09 | 0.27 | 0.45 | 0.11 | 0.27 | 0.33 | 0.51 |
| | (0.06) | (0.16) | (0.27) | (0.27) | (0.16) | (0.27) | (0.24) | (0.27) |
| Share Occupied | 98.17 | 96.87 | 97.32 | 94.88 | 96.30 | 97.35 | 94.49 | 95.10 |
| | (2.32) | (8.50) | (2.88) | (4.94) | (4.62) | (2.80) | (5.59) | (4.54) |
| Share Owner Occupied | 49.11 | 52.48 | 58.12 | 40.46 | 15.50 | 59.73 | 24.56 | 49.50 |
| | (48.17) | (41.22) | (30.79) | (32.35) | (34.36) | (29.54) | (26.89) | (31.79) |
| Panel B: Census Data | | | | | | | | |
| Occupation Score | 36.87 | 31.00 | 27.41 | 23.73 | 27.21 | 27.42 | 22.61 | 24.35 |
| | (5.37) | (4.47) | (3.09) | (1.90) | (4.93) | (3.02) | (1.63) | (1.76) |
| House Value | 10411 | 9734 | 8358 | 7051 | 8526 | 8352 | 6720 | 7237 |
| | (3157) | (3189) | (2611) | (2977) | (3056) | (2600) | (2967) | (2979) |
| Rent | 58.545 | 55.93 | 46.865 | 32.33 | 43.95 | 46.97 | 32.49 | 32.24 |
| | (16.85) | (15.42) | (13.77) | (10.47) | (6.01) | (13.96) | (8.78) | (11.33) |
| Share Black | 0.017 | 0.004 | 0.006 | 0.121 | 0.092 | 0.003 | 0.326 | 0.006 |
| | (0.02) | (0.01) | (0.02) | (0.22) | (0.04) | (0.01) | (0.27) | (0.01) |
| Share Foreign | 0.204 | 0.179 | 0.239 | 0.283 | 0.193 | 0.241 | 0.225 | 0.316 |
| | (0.12) | (0.09) | (0.09) | (0.12) | (0.10) | (0.09) | (0.14) | (0.10) |
| Share Labor Force | 0.495 | 0.441 | 0.453 | 0.453 | 0.499 | 0.452 | 0.476 | 0.441 |
| | (0.11) | (0.09) | (0.08) | (0.08) | (0.09) | (0.07) | (0.08) | (0.08) |
| Share Owner Occ | 0.4349 | 0.43 | 0.3998 | 0.2855 | 0.336 | 0.408 | 0.276 | 0.302 |
| | (0.32) | (0.26) | (0.20) | (0.19) | (0.18) | (0.20) | (0.19) | (0.19) |
| Number of Zones | 26 | 112 | 286 | 204 | 10 | 276 | 73 | 131 |
| Number of EDs | 68 | 725 | 2332 | 3791 | 92 | 2240 | 1952 | 1839 |
| Number of People | 79,551 | 1,046,617 | 3,484,700 | 5,410,078 | 122,639 | 3,362,061 | 2,759,526 | 2,650,552 |
| Whites | 78,272 | 1,042,099 | 3,462,193 | 4,459,905 | 112,564 | 3,349,629 | 1,835,016 | 2,624,889 |
| Blacks | 1,279 | 4,518 | 22,507 | 950,173 | 10,075 | 12,432 | 924,510 | 25,663 |
| Number of Owned Homes | 5,230 | 104,329 | 322,512 | 295,834 | 8,090 | 314,422 | 142,339 | 153,495 |
| Houses Owned by Whites | 5,222 | 104,184 | 321,267 | 273,629 | 7,554 | 313,713 | 121,098 | 152,531 |
| Houses Owned by Blacks | 7 | 113 | 1,151 | 21,987 | 532 | 618 | 21,119 | 868 |

Notes: Data in this table comes from both aggregated census enumeration districts as well as HOLC survey data. We limit our sample to only enumeration districts which are at least 90% contained in a given zone. Black zones represent zones with an above average share of black residents and white zones represent zones with a below average share of black residents.

**Table 2.2: Levels and Trends at CD Boundaries**

| | (1)<br>1930 | (2)<br>1930 | (3)<br>1940 | (4)<br>1940 | (5)<br>Trends | (6)<br>Trends |
|---|---|---|---|---|---|---|
| Panel A: House Values | | | | | | |
| Redlined Side | -576.24*** | -496.76*** | -528.73*** | -378.71*** | -135.42** | -129.31** |
| | (76.92) | (66.08) | (52.31) | (46.69) | (54.82) | (51.26) |
| Panel B: Rents | | | | | | |
| Redlined Side | -0.64 | -0..80* | -1.67*** | -1.55*** | -0.70* | -0.33 |
| | (0.48) | (0.42) | (0.41) | (0.36) | (0.38) | (0.38) |
| Panel C: Occupation Score | | | | | | |
| Redlined Side | -1.44*** | -1.28*** | -1.12*** | -1.09 *** | -0.86*** | -0.80** |
| | (0.18) | (0.18) | (0.19) | (0.19) | (0.29) | (0.30) |
| Panel D: Share Black | | | | | | |
| Redlined Side | 0.06*** | 0.05*** | 0.04*** | 0.04*** | 0.01** | 0.02*** |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Optimal Bandwidth | Yes | Yes | Yes | Yes | Yes | Yes |
| Matched Sample | Yes | Yes | Yes | Yes | Yes | Yes |
| Boundary FE | No | Yes | No | Yes | No | Yes |

Notes: Each coefficient is estimated from a separate regression. *p < .10, **p < .05, ***p < .01. We used the optimal bandwidth selection procedure proposed by Calonico, Cattaneo, and Titiunik (2014).

## 3.0 Racial Disparities in Debt Collection

(joint with Domonkos F. Vamossy)

A distinct set of disadvantages experienced by black Americans increases their likelihood of experiencing negative financial shocks, decreases their ability to mitigate the impact of such shocks, and ultimately results in debt collection cases being far more common in black neighborhoods than in non-black neighborhoods. In this paper, we create a novel dataset that links debt collection court cases with information from credit reports to document the disparity in debt collection judgments across black and non-black neighborhoods and to explore potential mechanisms that could be driving this judgment gap. We find that majority black neighborhoods experience approximately 40% more judgments than non-black neighborhoods, even after controlling for differences in median incomes, median credit scores, and default rates. The racial disparity in judgments cannot be explained by differences in debt characteristics across black and non-black neighborhoods, nor can it be explained by differences in attorney representation, the share of contested judgments, or differences in neighborhood lending institutions.

### 3.1 Introduction

The distinct set of disadvantages experienced by black Americans increases their likelihood of experiencing negative financial shocks and limits their ability to mitigate the impact of such shocks. This ultimately makes them more likely to enter into default and have unpaid balances sent to collections. If a collection is brought to court and a guilty verdict, referred to as a judgment, is received, the defendant's wages can be garnished, or their bank account can be seized. This fact, combined with compounding interest and various legal fees, could ultimately hinder the debtor's ability to accumulate wealth and overcome any initial economic disadvantage.[46]

In this paper, we document a disparity in debt collection judgments across black and non-black neighborhoods and explore potential mechanisms through which this racial disparity could be entering the debt collection system.[47] Such mechanisms include neighborhood level differences in income and credit score distributions, differences in default rates, differences in lending institutions, and the differences in debtors' likelihood to contest the debt in court. In order to document this disparity, we construct a novel zip code level panel dataset from 2004 to 2013 that links the number of debt collection judgments in each zip code in Missouri to data from Experian credit reports and the American Community Survey. Our main threat to identification is omitted variable bias. To mitigate this concern, we use a rich set of control variables from the Experian credit report data to track all aspects of a neighborhoods' financial liabilities, including the types of debt incurred with the number of accounts and balances, as well as the neighborhoods' median credit score and detailed delinquency information. Not only does this help mitigate omitted variable bias, but it provides insight into the specific mechanisms that may or may not be driving the racial disparity in debt collection judgments. We address concerns about differences in unobservable characteristics across black and non-black neighborhood by limiting our sample to

[46] ADP, the nation's largest payroll services provider, documented that more than one in 10 employees between the ages of 35 to 44 had their wages garnished in 2013.

[47] This racial disparity was highlighted by Paul Keil and Anne Waldman in a ProPublica article. They documented a disproportionate number of judgments in predominantly black communities when compared to white ones, with the risk of judgment being twice as high in majority black neighborhoods than in majority white neighborhoods with similar income levels.

only neighborhoods with common support over observables and controlling for county and year fixed effects.

We find that the judgment rate is 85% higher in majority black neighborhoods than in majority non-black neighborhoods. Over half of this baseline judgment gap can be explained by differences in incomes, credit scores, default rates, and housing values across black and non-black neighborhoods; however, even after controlling for these differences, majority black neighborhoods have approximately 40% more judgments than non-black neighborhoods. Differences in total debt levels, debt composition, payment amounts, utilization ratios, and delinquency rates do not further mitigate the judgment gap suggesting that credit scores are accurately capturing the relevant information from consumers' credit reports.

We also hypothesize that defendants from black neighborhoods could be less likely to hire an attorney or to contest the debt in court, making it less costly for debt collectors to obtain judgments in black neighborhoods. However, we show that there is no statistical difference in the share of contested judgments across black and non-black communities and controlling for attorney representation has limited effect on the judgment gap. Another potential theory regarding the racial gap in debt collection judgments is that differences in lending institutions across black and non-black neighborhoods cause the positive correlation between neighborhood racial composition and the judgment rate. However, the racial gap in judgments remains after controlling for the number of banks and payday lenders in a given area. Lastly, we explore the extent to which certain plaintiff types (e.g. major bank, debt collector, high cost lender) are driving our results. Judgment rates are higher in black communities for every plaintiff type, though, certain types of plaintiffs consistently show more racial imbalance in their lawsuits than others. Our results are robust to an alternative measure of credit score, alternative samples, and to using a gradient boosted trees machine learning estimation strategy. Furthermore, these results can be replicated by using the estimated racial composition of defendants as opposed to the racial composition of neighborhoods.

There are two potential explanations that we cannot explore using our current data: differences in wealth that are not driven by housing values and discrimination. Laws prohibit using race to make decisions regarding access to credit, and thus many creditors do not collect information on race. Furthermore, juries are not typically used in debt collection cases and, if a case is heard in front of a judge, such cases are usually fairly algorithmic with a limited amount of subjectivity involved. It is more likely that the unexplained racial gap in debt collection judgments are the result of the broader disadvantages experienced by minority communities that have persisted into our modern-day society. For example, according to estimates provided by the United States Census Bureau in 2016, the typical black household has a net worth of $12,920, while that of a typical white household is $114,700 - this is a $101,780 difference in wealth that could have important implications for a household's ability to mitigate negative financial shocks. About $35,000 of this wealth gap is not driven by home equity. By translating this wealth gap into differences in annual income and using our estimates of the relationship between income and judgements, we calculate that a wealth gap of this size would explain almost all of the remaining judgment gap across black and non-black communities.[48]

There is a large literature that documents the important role that race plays in the labor market and the housing/mortgage markets (e.g. Bartlett et al. 2019, Ritter and Taylor 2011,

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[48] Our most conservative estimate of the judgment gap is 0.34 more judgments per 100 people in majority black neighborhoods compared to majority non-black neighborhoods and is derived from Oster (2016). We computed the difference in annual savings needed over a 40-year horizon to generate a wealth gap of $35,000. We found that an annual difference of $2,910 is sufficient to generate the wealth gap in net present value. For interest rate, we applied the historical return of the stock market, which between 1957 through 2018 is roughly 8%. Consistent with estimates from the U.S. Bureau of Economic Analysis, we assume an 8% personal savings rate. This translates into an annual income difference of $36,375. Increasing the median income of majority black neighborhoods by this amount would decrease the judgment rate by 0.25 judgments per 100 people

Bertrand and Mullainathan 2004, and Turner et al. 2002). Blacks also face discrimination in the legal and criminal justice system; for example, they are more likely to be searched for contraband (Antonovics and Knight 2009), to have biased bail hearings (Arnold et al. 2018), and to be charged with a serious offense (Rehavi and Starr 2014). Furthermore, racial differences in wealth are large and a growing literature explores how the racial wealth gap was generated and persisted over time (e.g. McKernan et al. 2014 and Akbar et al. 2019). While much attention has been given to racial disparities in general, this is the first economic analysis to empirically document racial disparities in debt collection judgments.

Aside from the literature documenting racial disparities across many different dimensions, this paper also contributes to a growing literature about the debt collection industry.[49] We know that consumers who are sued by creditors and debt collectors are drawn predominantly from lower-income areas (Hynes, 2008). Other more recent studies have investigated the role of information technology in the collection of consumer debts (Drozd and Serrano-Padial, 2017), documented the link between debt collection regulations and the supply of consumer credit (Fedaseyeu, 2015), and determined if consumers are made better or worse off by settling their debt outside of court (Cheng et al., 2019). Interpreted broadly, the impacts of the debt collection process on consumer outcomes has been well documented; however, racial disparities in debt collection have not been empirically explored.

The rest of our paper is outlined as follows: Section 3.22 discusses the typical debt collection litigation process in the United States, as well as the laws regulating access to credit and debt collection procedures, Section 3.3 describes our data, Section 3.4 outlines our empirical

[49] See Hunt (2007) for an overview of the debt collection industry and details about its institutional structure and regulatory environment.

strategy, Section 3.5 documents the racial gap in debt collection judgments and discussions potential mechanisms driving the disparity, Section 3.6 presents various robustness checks, and Section 3.7 concludes.

## 3.2 Background Information

The debt collection industry in the U.S. is large and the amount of debt being placed into collection continues to grow. According to a 2018 annual report by the Consumer Financial Protection Bureau, debt collection is a $10.9 billion dollar industry that employs nearly 120,000 people across approximately 8,000 collection agencies in the United States. In 2010 alone, U.S. businesses placed $150 billion in debt with collection agencies. When the debt is unsecured, the owner of the debt (i.e. the original creditor or the debt buyer) can either write off the debt, negotiate with the debtor to bring their debt to current, or file a debt collection lawsuit. In this section, we will summarize the key institutional details surrounding debt collection lawsuits and the laws regulating the debt collection industry.

### 3.2.1 Debt Collection Litigation Process

Debt collection litigation typically begins when a creditor files a "Summons and Complaint" in a state civil court.[50] This document names the parties involved and states the amount owed (including interest and, in some cases, attorney fees and court costs). The summons is served

[50] These courts have many different names including municipal court, superior court, justice court, county court, etc.

to the defendant to notify them that they are being sued. It provides additional information including the deadline for which the debtor must file a formal response, referred to as the “answer”, to the court. If this deadline is not met, the creditor will usually ask the court to enter a default judgment. Default judgments occur when the defendant has failed to perform a court-ordered action, and results in the court settling the legal dispute in favor of the plaintiff. The defendant is obligated to abide by the court's ruling and is subject to the punishments requested by the court.

For most routine debt collection lawsuits, if the debtor files a formal response to the lawsuit a trial date will be requested and set by the court. In some courts, there will be a settlement conference before the trial date to try to settle the case before trial. Once a judgment is obtained by the creditor, the creditor might request a “debtor's examination,” which would require the debtor to appear in court and answer questions about their finances. This process informs the creditor how it can collect the judgment. The most common methods for enforcing the judgments are to garnish wages or bank accounts.[51] If a dispute is settled before trial, the creditor gives up the ability to collect on the debt by garnishing the debtor's bank accounts or wages, and therefore often requires a onetime lump sum payment to drop the suit.

### 3.2.2 Laws Regulating Debt Collection

Debtors are granted some protections throughout the debt collection process. The Fair Debt Collection Practices Act (FDCPA), which was enacted in 1977, is the primary federal law governing debt collection practices. The statute's stated purposes are as follows: to eliminate the abusive practices used to collect consumer debts such as calling the debtor at all hours of the night

[51] Courts can also seize and sell the debtor's personal property, though this is relatively uncommon.

and showing up to their place of employment; to promote fair debt collection; and to provide consumers with an avenue for disputing and obtaining validation of debt information in order to ensure the information's accuracy.

Furthermore, the Consumer Credit Protection Act (CCPA) of 1968 restricts the amount of earnings that creditors can garnish from defendants' weekly disposable income to 25% or the amount by which disposable earnings are greater than 30 times the minimum wage. The share of wages protected from debt collection garnishments can be increased by state law. For example, a creditor in Missouri can garnish only 10% of after-tax wages if the debtor is the head of their household, though the burden to assert these protections is typically on the debtor and take-up is relatively low. There is no federal law limiting the amount of savings that can be seized from a debtor's bank accounts.

While not directly related to debt collection, other protections have been put in place to protect consumers in the credit market. For example, the Equal Credit Opportunity Act (ECOA) enacted in 1974 makes it illegal for creditors to discriminate against any applicant on the basis of race, color, religion, national origin, sex, marital status, age, or participation in a public assistance program.[52] The law applies to everyone who regularly participates in a credit decision, including banks, retail and department stores, bankcard companies, finance companies, and credit unions. The ECOA applies both to the decision to grant credit as well as setting the terms of credit.

Furthermore, the Fair Credit Reporting Act of 1970 promotes the accuracy, fairness, and privacy of consumer information contained in the files of consumer reporting agencies. It was intended to protect consumers from the willful and/or negligent inclusion of inaccurate information in their credit reports. More recently, the Credit Card Accountability Responsibility and

[52] This law is enforced by the Federal Trade Commission (FTC), the nation's consumer protection agency.

Disclosure (CARD) Act of 2009 established fair and transparent credit card practices. Key provisions include giving consumers enough time to pay their bills, prohibiting retroactive rate increases, making it easier to pay down debt, eliminating “fee harvester cards”, and eliminating excessive marketing to young people.

Despite these protections, abusive debt collection practices still exist and, as we will show, minority neighborhoods are disproportionately impacted by debt collection judgments.

## 3.3 Data

We construct a zip code level panel dataset to document racial disparities in debt collection lawsuits across black and non-black neighborhoods. This panel dataset is constructed by combining multiple different data sources, the first of which documents debt collection court cases filed in Missouri from 2004 to 2013.[53] For each zip code in our sample, we know the number of debt collection lawsuits filed and the number of judgments arising from these lawsuits. We also know the number of cases that resulted in a default judgment (meaning the debtor did not show up to court), a consent judgment (meaning the debtor showed up to court and admitted to owing the debt), and the number of cases that were contested (meaning that some aspect of the debt was disputed). We know if the defendant was represented by an attorney and the plaintiff type

[53] This data was generously provided by Kiel and Waldman (2015). They acquired individual court case data from the state court administration. Their white paper focuses on three jurisdictions: Cook County, Illinois (composed of Chicago and surrounding suburbs), St. Louis City and St. Louis County, Missouri, and Essex county, New Jersey (composed of Newark and suburbs). The data included basic case information such as the plaintiff, the defendant, and the defendant's address. The race of the defendant is not reported.

(categorized into the following groups: auto, debt buyer, high-cost lender, major bank, medical, utility, and miscellaneous).

We also use Experian credit report data to control for credit scores, default rates, and other debt characteristics. This is an anonymous quarterly longitudinal panel of individuals who have an Experian credit report and spans from 2004 to 2013. The data contains many variables which allows us to track all aspects of individuals' financial liabilities, detailed delinquencies, various types of debt with the number of accounts and balances, as well as an individual's credit score. The data also contains each individual's zip code. We calculate the median credit score, the average number of delinquent accounts, and other credit measures for each zip code in our sample. We supplement these data with zip code tabulation data from the 2009-2013 American Community Survey to control for racial composition, median household income, the unemployment rate, and other socioeconomic variables of interest at the zip code level.[54]

We also document the number of lending institutions that are accessible to each neighborhood as an additional proxy for a neighborhood's financial well-being.[55] We use Census ZIP Code Business Patterns (ZCBP) data to get access to the number of banks and payday lenders in each zip code. The ZCBP data measure the number of establishments, number of employees and total payroll by zip code and detailed industry code. Following Butta (2014) we use the following two North American Industrial Classification System (NAICS) codes to capture payday lending establishments: non-depository consumer lending (establishments primarily engaged in

[54] All dollar values are adjusted to be in terms of 2013 dollars. USPS ZIP Codes are not areal features used by the Census but a collection of mail delivery routes that identify the individual post office or metropolitan area delivery station associated with mailing addresses. ZIP Code Tabulation Areas (ZCTAs) are generalized areal representations of United States Postal Service (USPS) ZIP Code service areas.

[55] This measure can alternatively be thought of as a measure of access to credit markets. However, payday lenders could be the result of financial distress as opposed to the cause.

making unsecured cash loans to consumers) and other activities related to credit intermediation (establishments primarily engaged in facilitating credit intermediation, including check cashing services and money order issuance services).[56] For each zip code, we use arcGIS to create a weighted average (based on land area) of the number of banks and payday lenders that exist within a five mile radius from each zip codes centroid. Lastly, we use Fixed Broadband Deployment Data from Federal Communications Commission to document access to online credit markets for every zip code in our sample.

### 3.3.1 Race Variables

In this paper, our primary focus is differences in the number of judgments per 100 people across majority black and majority non-black neighborhoods.[57] As such, we use neighborhood racial composition as our primary independent variable and classify a zip code as a majority black zip code if more than 50% of its residents are black. Information on the racial and ethnic composition of the U.S. population by geography comes from the Summary File 1 (SF1) from the 2010 Census, which provides counts of enumerated individuals by race and ethnicity for various geographic area definitions, including zip code tabulation areas.[58] However, one may wonder about the racial composition of the defendant pool specifically. In this section, we discuss various methods used by statisticians to estimate race when it is not available in administrative data. These

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[56] Barth et al. (2016) discuss how this proxy could likely overstate the number of payday lenders. We adjust these measures to correct for states that prohibit payday lending to help reduce this bias.

[57] The Equal Credit Opportunity Act (ECOA) generally prohibits a creditor from inquiring about the race, color, religion, national origin, or sex of an applicant, and as a result we lack information about race in both the Experian and debt collection datasets. One exception is applications for home mortgages covered under the Home Mortgage Disclosure Act (HMDA).

[58] Census block are the highest level of disaggregation (the smallest geography).

methods generally use publicly available demographic information associated with an individual's surname and place of residence from the U.S. Census Bureau to construct proxies for race.

Our first proxy uses only surnames to predict the race of an individual, and thus the racial composition of the defendant pool. Information used to calculate the probability of belonging to a specific race given an individual's surname is based on data from the 2010 Census. This dataset provides each surname held by at least 100 enumerated individuals, along with a breakdown of the percentage of individuals with that name belonging to one of six race and ethnicity categories: Hispanic; non-Hispanic White; non-Hispanic Black or African American; non-Hispanic Asian/Pacific Islander; non-Hispanic American Indian and Alaska Native; and non-Hispanic Multiracial. In total, the surname list provides information on the 162,253 surnames covering approximately 90% of the population. While this proxy works well for Hispanic and Asian names, it is less accurate at predicting black-white differences since blacks and whites tend to have more similar surnames. We classify a defendant pool as being majority black if at least 50% of the defendants in the debt collection data were predicted to be black.

Our second proxy for the racial composition of defendants is constructed using Bayesian Improved Surname Geocoding (BISG) (Elliott et al. 2009).[59] This method combines geography- and surname-based information into a single proxy probability for race using the Bayes updating rule. This method involves constructing a probability of assignment to race based on demographic information associated with surname and then updating this probability using the demographic characteristics of the zip code associated with place of residence. The updating is performed through the application of a Bayesian algorithm, which yields an integrated probability that can be

[59] Consumer Finance Protection Bureau's Office of Research (OR), the Division of Supervision, Enforcement, and Fair Lending (SEFL) rely on a Bayesian Improved Surname Geocoding (BISG) proxy method.

used to proxy for an individual's race and ethnicity.[60] We once again classify the defendant pool as being majority black if at least 50% of the defendants in the debt collection data were predicted to be black. Research has found that this approach produces proxies that correlate highly with self-reported race and national origin and is more accurate than relying only on demographic information associated with a borrower's last name or place of residence alone (CFBP Report, 2014).

### 3.3.2 Sample Selection

Our main specifications focus only on debt collection cases from Missouri. Aside from the fact that Missouri has a centralized database of cases tried in different circuit courts, previous research has documented that Missouri is a representative state in terms of collection (Ratcliffe et al., 2014 and Cheng et al., 2019). More specifically, Missouri is representative in terms of percentage of consumers who are delinquent and the average amount of debt in collections (Ratcliffe et al., 2014). Missouri is also not particularly exceptional with regards to the law surrounding collections, its share of black residents, or its level of inequality (Cheng et al., 2019). Finally, debt collectors in Missouri are obligated to file cases in the court associated with the borrower's address and their centralized database provides the defendants surname, both of which assists in our calculation of racial proxies of the defendant pool.

We have two additional sources of debt collection judgment data. The first is from all counties from New Jersey and Cook County, Illinois (composed of Chicago and surrounding suburbs). However, it is less detailed than the Missouri data. This data includes the number of

[60] Details of this algorithm are discussed in Section C2 of the Appendix.

cases filed in a five-year window from 2008-2012, but we don't have breakdowns about the type of judgment (default, consent, contested), we don't know the defendants' names, and we don't know if they were represented by an attorney. Our second additional source of judgment data comes from the Experian Credit Report data. This data once again lacks breakdowns by judgment and plaintiff type, as well as information about attorney representation. All of our main specifications use data from Missouri due to its representative nature, the high level of detail in the data, and because we want to keep our sample consistent across specifications. However, when comparable information is available, we use these additional data sources to test the robustness of our results.

### 3.4 Empirical Strategy

There are important differences in observable characteristics across majority black and non-majority black neighborhoods, which is documented in Figure 3.1. This figure shows kernel density estimates of various covariates that are used throughout this analysis. High share black neighborhoods tend to have lower median credit scores, lower median household incomes, lower median house values, higher unemployment rates and a higher share of divorced individuals. These disparities cause concern that there could also be important differences in unobservable characteristics that vary with racial composition of neighborhoods. We mitigate this concern by limiting our sample to only neighborhoods with common support over observables (Crump et al. 2009).

Specifically, we use a logistic regression to restrict our dataset to a common support. We estimate:

$$\Phi(M_{ict}) = \beta_0 + \theta X_{ic} + \epsilon_{ic} \tag{3.1}$$

where $M_{ict}$ is an indicator variable equal to one if neighborhood i in county c in year t is a majority black neighborhood and $X_{ict}$ is a vector of other controls for neighborhood i in county c in year t which includes quintiles of the income and credit score distributions, median income, median credit score, the gini index of income inequality, 90+ days-past-due debt balances, unemployment and divorce rates, population density, median house value, and education attainment levels such as fraction with at least a bachelor's degree and fraction with less than a high school diploma. We plot the propensity score distribution in Figure C1 and restrict our sample to the intersection of the two curves. This drops very high-income non-black neighborhoods and very low-income black neighborhoods from our sample.

To further limit omitted variable bias, we use a rich set of control variables combined with both county and year fixed effects. County fixed effects will control for any time invariant unobservable characteristics of counties and year fixed effects control for any time varying changes that impact all of our neighborhoods.

### 3.4.1 Summary Statistics

Our common support sample consists of over 250 zip codes observed over 10 years. Table 3.1 presents summary statistics for our variables of interest across both majority black and majority non-black zip codes. Panel A shows our judgment data. On average, majority black neighborhoods had higher judgment rates than non-black neighborhoods (2.7 judgments per 100 people as opposed to 1.4). Black neighborhoods had a higher share of cases result in default judgments and a lower share of cases in which the defendant was represented by an attorney. Panel B summarizes differences in credit characteristics and lending institutions across black and non-black neighborhoods. On average, majority black neighborhoods have lower median credit scores and more debt balances that are 90 days past due. Lastly, Panel C summarizes other baseline control variables, including median income, median housing values, and educational attainment variables. In general, black neighborhoods have lower median household incomes, lower median house values, higher unemployment rates, and a lower share of college educated individuals. Most of these differences are significant at the 1% level.

### 3.4.2 Empirical Specification

We use a fixed effect framework with our common support sample to estimate the impact of racial composition on the debt collection judgment rate of neighborhoods. Our empirical specification is given by the following equation:

$$y_{ict} = \alpha + \beta M_{ic} + \theta X_{ict} + \gamma_c + \lambda_t + \epsilon_{ict} \tag{3.2}$$

where $y_{ict}$ is the number of judgments per every 100 people in neighborhood i in county c in year t , $M_{ict}$ is an indicator variable equal to one if neighborhood i in county c in year t has a black population greater than 50%, and $X_{ict}$ is a vector of other controls for neighborhood i in county c in year t. Our main specification also includes county fixed effects to control for any time invariant differences across counties and year fixed effects to control for any time varying changes that impact all of our neighborhoods (like the Credit Card Accountability Responsibility and Disclosure Act of 2009). All regressions are weighted by population and standard errors are clustered at the county level.

To limit omitted variable bias and to better understand the mechanisms driving the racial disparity in debt collection judgments, we include a vast set of control variables. Aside from income and credit score, we add measures of debt balances by type of debt (credit card, medical, student loans, etc.), debt composition (type of debt as share of total debt balances), delinquent balances by length of delinquency (30 days, 60 days, 90 days), and bankruptcy/collection flags to $X_{ict}$. We also add controls for the number of banks and payday lenders within a five-mile radius of each zip code and explore the results by plaintiff and judgment type.

### 3.5 Results

To establish a baseline judgment gap, we begin by documenting the racial disparity in judgments across black and non-black neighborhoods controlling only for county and year fixed effects. Figure 3.2 shows the relationship between the judgment rate and the percentage of blacks in a zip code. This figure classifies zip codes into one of a hundred bins based on their share of black residents and plots the average share of black of each bin against the average judgment rate of each bin. The size of the bubbles corresponds to the number of zip codes in each of the bins; as expected, there are many low share black neighborhoods and relatively less high share black observations. The regression line represents the fit between the average judgment rate in the bin and the share of black population weighted by the number of observations in the bin. We see that the judgment rate is positively correlated with the share of black residents residing in the zip code.

This relationship is formalized in Column (1) of Table 3.2. Column (1) shows that majority black neighborhoods have about 1.2 more judgments per every 100 people compared to non-black neighborhoods; this implies that the judgment rate in black neighborhoods is almost double that of non-black neighborhoods where the average judgment rate is 1.4 judgments per 100 people.

### 3.5.1 Income and Credit Score Distributions

One important difference in the observable characteristics between black and non-black neighborhoods is differences in income. Panel (a) of Figure 3.3 plots neighborhoods by their median income and the judgment rate per 100 people, with darker red circles representing neighborhoods with a higher share of black residents and darker blue circles representing neighborhoods with a higher share of white residents. This figure documents a negative relationship between the judgment rate and median income, with the judgment rate decreasing as median income increases. However, even looking at neighborhoods with similar income levels, we see higher judgment rates for majority black neighborhoods.

Column (2) of Table 3.2 adds controls for income quintiles and median income to the previous specification. After controlling for differences in the income distribution across black and non-black neighborhoods, we see that black neighborhoods are associated with 0.9 more judgments per 100 people. This implies that differences in the income distribution across black and non-black neighborhoods can explain 23% if the racial disparity in debt collection.

A second important difference in the observable characteristics between black and non-black neighborhoods is differences in credit scores. Panel (b) of Figure 3.3 plots neighborhoods by their median credit score and the judgment rate per 100 people, with darker red circles once again representing neighborhoods with a higher share of black residents and darker blue circles representing neighborhoods with a higher share of white residents. This figure documents a negative relationship between the judgment rate and median credit scores. Once again, racial bias is evident in this figure, with majority black neighborhood having higher judgment rates than non-black neighborhoods with similar median credit scores, suggesting that differences in credit scores may not be the primary mechanism driving the racial disparity in debt collection cases.

Column (3) in Table 3.2 adds credit score quintiles and median credit score to the baseline specification. We see that majority black zip codes are associated with 0.75 more judgments per 100 individuals, a 50% increase of the average judgment rate of non-black zip codes. This means that differences in the credit score distribution can explain 40% of the judgment gap between black and non-black neighborhoods. Column (4) adds income and credit score controls into the same specification and coefficient changes very little, suggesting that 60% of the judgment gap remains unexplained after controlling for differences in the income and credit score distributions across black and non-black communities.

Column (5) adds controls for total delinquent debt balances, unemployment rate, median house value, the fraction of the population with a college education, and population density.[61] In this specification, we see that majority black neighborhoods have a 40% higher judgment rate than non-black neighborhoods. It is primarily the inclusion of the unemployment rate and median housing values that cause the decline in the estimated coefficient on our majority black indicator. This suggests that these variables can explain away an additional 10% of the baseline judgment disparity. Column (6) uses a one year lag of our baseline controls. Even after adding controls for these observable characteristics, 50% of the baseline judgment disparity still remains.

[61] According to estimates provided by the United States Census Bureau in 2016, the typical black household has a net worth of $12,920, while that of a typical white household is $114,700. This difference could play an important role in driving the racial gap in debt collection. Since a majority of wealth accumulated to middle- or low-income households is through home ownership, we use housing values to help control for differences in wealth levels across black and non-black neighborhood.

### 3.5.2 Debt Characteristics

We also explore whether differences in debt portfolios of black and non-black neighborhoods are driving the racial disparity in debt collection judgments independent of credit score. For example, it could be the case that majority black neighborhoods tend to acquire the type of debt that is more likely to be collected in court. To explore this hypothesis, we use the plethora of information from the Experian Credit Report data to control for differences in debt characteristics across black and non-black communities. Such controls include total debt levels, debt composition, payment amounts, utilization ratios, and delinquency rates.

Our results are presented in Table 3.3. Each specification includes the income, credit scores, and baseline controls discussed in Table 3.2, as well as county and year fixed effects. Column (1) adds additional controls for total debt levels, including breakdowns for the type of debt such as credit card debt, mortgage debt, and student loan debt. Column (2) includes controls for payment amounts and utilization rates. Column (3) includes debt composition controls, such as credit card debt as a share of total debt. Column (4) adds additional delinquency and collection controls, including the total debt balances that are 30 days, 60 days, or 90 days delinquent, as well as bankruptcy and collection flags. Lastly, Column (5) includes all of these controls together. In each specification we see that majority black neighborhoods have an additional 0.6 judgments per 100 individuals, a 40% higher judgment rate compared to non-black neighborhoods. These results indicate that after controlling for differences in credit scores, differences in debt characteristics cannot explain any additional share of the racial disparity in judgment rates.

### 3.5.3 Lending Institutions

We have shown that a racial disparity in debt collection judgments exists, even after controlling for differences in the income and credit score distributions across black and non-black neighborhoods. In this section, we explore another potential mechanism that could be driving the racial disparity in debt collection judgments - differences in lending institutions across black and non-black neighborhoods.[62]

To explore this potential explanation, we use arcGIS to create an index that measures the number of banks and payday lenders within 5 and 10 mile radii from each zip codes' centroid.[63] We also use broadband access as a proxy for access to online credit markets and the share of credit reports that are unscored as proxy for access to credit.[64] We add these variables as controls to our main specification.

The results are presented in Table 3.4. Column (1) shows our main result is present on this subsample of data. In Columns (2) and (3), we add controls for broadband access and the share of unscored accounts in a zip code respectively; both measures have no impact on our coefficient of interest. In Columns (4) and (5) we add our controls for banks and payday lenders with 5 and 10 mile radii. We see that the number of banks is negatively correlated with the judgment rate while the number of payday lenders is positively correlated with the number of judgments. Adding these

[62] The presence of payday lenders is likely the result of financial distress as opposed to the cause. As such, we view these controls as an additional measure of the financial well-being of neighborhoods as opposed to a measure of credit access.

[63] This analysis only used data from 2008-2012 and thus our sample size is slightly smaller. We replicate the main results on this subsample of the data for context.

[64] An unscored credit report is one in which there is not enough information on a consumer's credit report to issue a formal credit score. This could serve as an access to credit if unscored reports are correlated with limited access to credit markets as opposed to a limited desire to obtain credit.

controls decreases the coefficient on black majority by 15%, though, a large racial gap in the number of judgments issued across black and non-black communities remains.

### 3.5.4 Attorney Representation and Judgment Type

We next investigate whether debt collectors target neighborhoods where defendants are less likely to have an attorney or to contest the debt. It could be the case that debt collectors target their collection efforts in areas where defendants are less likely to show up to court, resulting in a default judgment, or in areas where defendants tend to acknowledge they owe the debt. In other words, debt collectors might avoid collecting in areas where defendants tend to argue some aspect of the debt owed, which could result in the plaintiff exerting more effort or spending money to collect the debt.

To explore the extent to which differences in attorney representation are driving our result, we document the disparity in attorney representation and show how this disparity impacts the judgment rate. These results are presented in Columns (1) and (2) of Table 3.5. Note that each specification in this table includes the income, credit score, and baseline controls discussed in Table 3.2, as well as county and year fixed effects. The outcome variable in Column (1) is the share of debt collection court cases where the defendant was represented by an attorney; this result shows that defendants in majority black neighborhoods are less likely to have an attorney represent them in a debt collection court case. However, as seen in Column (2) where our dependent variable is once again judgments per 100 people, controlling for the share of cases in which defendants are represented by an attorney cannot explain the racial disparity in debt collection cases.

We take this as evidence that attorney representation does not impact the number of debt collection judgments; this does not imply that attorney representation is not meaningful or important in debt collection court cases. Debt collection laws often place the burden to assert various legal protections, including the share of the debtor's wages that can be garnished as the result of a judgment, on the debtor. Attorney representation is likely important in protecting debtors' rights throughout the debt collection process, even if such cases ultimately end in judgments.

To explore the extent to which differences in the share of contested versus uncontested cases could be driving our result, we document the impact of neighborhood racial composition on the share of different types of judgments.[65] Our outcome variable in Column (3) is the share of cases in which the defendant admitted to owing the debt, our outcome variable in Column (4) is the share of cases that were contested, and our outcome variable in Column (5) is the share of cases resulting in default judgments. We see no racial differences along these dimensions. These results suggest that it is unlikely that debt collectors are targeting areas without attorney representation or areas where defendants are less likely to show up to court.

### 3.5.5 Non-linearities and Higher Order Interactions

In this section, we investigate if machine learning techniques that allow for high order interactions of observable characteristics can help inform what mechanisms are driving the

[65] We also document the share of cases that resulted in a judgment. These results are presented in Table C12 in the Appendix. While only marginally significant, we see that majority black neighborhoods are 2 percentage points more likely to have a case result in a judgment. This is primarily driven by a lower share of cases being settled before a case is tried. This translates to a 10% decrease from the non-black settlement rate. Since settling a case often requires a one-time lump sum payment, defendants who settle tend to have worse subsequent credit outcomes (Cheng et al. 2019). This suggests that a lower propensity to settle cases before trial could actually help defendants from majority black neighborhoods. This can also be seen as suggestive evidence that defendants from majority non-black neighborhoods are better able to mitigate negative shocks. We see no statistical difference in the share of cases that are dismissed.

remaining racial disparity in judgments. More specifically, we implement Gradient Boosted Trees (GBT) which is an ensemble learning method that recursively combines the forecasts of many shallow decision trees.[66] The theory behind boosting is that a collection of weak learners as a whole creates a single strong learner with improved stability over a single complex tree. There are pros and cons to using machine learning approaches to explore the racial disparity in debt collection. Two of the key benefits of applying GBT is that it is particularly well suited to capturing interactions between variables in the data, without ex-ante specifying what interactions to add and that it increases our predictive power.[67] The downside of this technique is that interpretability becomes more difficult and less precise.

#### 3.5.5.1 Explanatory Power of Variables

We use SHapley Additive exPlanations (SHAP), a unified framework for interpreting associations, to explain the output of our Gradient Boosted Trees (Nonlinear Model).[68] SHAP uses a game theoretical concept to assign each feature a local importance value for a given prediction. The SHAP value gives us individualized impacts for each predictor; positive SHAP values are associated with increased judgment rates and negative SHAP values are associated with decreased judgment rates. Figure 3.4 plots the distribution of the impact each predictor, including first order interactions, has on the model output for the fifteen most important predictors. These distributions are shaded based on the value of the independent variable with blue dots representing lower values

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[66] For more information about the GBT model, see Friedman (2001) and section C3 in Appendix C.

[67] Table C12 contrasts the predictive power of the GBT model with the linear model. The RMSE is computed using a regression of baseline covariates on judgment rates, and the results show that the nonlinear model is better able to explain the variation in judgment rates. Note $RMSE = \sqrt{1 - r^2} * \sigma_y$, and hence, a lower RMSE translates into higher predictive power

[68] For more on SHAP, see Lundberg and Lee (2017).

and red dots representing higher values.  This figure orders our independent variables in order of their importance as a predictor of judgments in the GBT procedure.  Neighborhood racial composition is the most important predictor of judgment rates.  Thus, allowing for a nonlinear model with higher order interactions does not mitigate the impact of neighborhood racial composition on judgment rates; if anything, allowing for this more flexible model highlights the importance of race in predicting judgments.

Aside from neighborhood racial composition, high median house value and credit score decrease predicted judgment rate. The divorce rate is also a significant predictor; neighborhoods with higher divorce rates are associated with higher judgment rates and neighborhoods with lower divorce rates are associated with lower judgment rates.  These results only point to correlations between the predictors and the judgment rate; they should not be interpreted causally. They are primarily used to understand the contribution each predictor on the final model output and to provide some comparative statics.

### 3.5.6 Differences in Plaintiff Type

Lastly, we explore if differences in judgment rates across black and non-black zip codes are driven by a specific plaintiff category.  For each zip code, we know the number of judgments awarded to each of the following plaintiff types:  auto, debt buyer, high-cost lender, major bank, medical, utility, and miscellaneous.  Debt buyers account for 48% of plaintiffs in our sample. Medical lenders, major banks, and high-cost lenders are the next largest plaintiff categories accounting for 20%, 13%, and 6% of plaintiffs respectively.  The other plaintiff categories are combined into the miscellaneous category.

Our results are presented in Table 3.6. Each specification includes the income, credit score, and baseline controls discussed in Table 3.2, as well as county and year fixed effects. Column (1) repeats the main analysis and includes judgments from all plaintiff types (this is the same result presented in Column (5) of Table 3.2). Column (2) limits the outcome variable to only judgments obtained by debt buyers, Column (3) to major banks, Column (4) to medical companies, Column (5) to high cost lenders, and Column (6) to any other lender. The racial gap in judgments is persistent across all plaintiff types.

The coefficients estimated across each specification should not be directly compared due to differences in the baseline judgment rates in non-black neighborhoods across these different plaintiff types. For example, the judgment rate in non-black neighborhoods was 0.58 judgments per 100 people for debt buyers, 0.46 judgments per 100 people for major banks, and 0.08 judgments per 100 people for high cost lenders. These baseline levels imply that majority black neighborhoods have a 33% higher judgment rate than non-black neighborhoods among debt buyers, a 9% higher judgment rate among major banks and a 128% higher judgment rate among high cost lenders. Thus, while the racial gap in debt collection judgments exists for every plaintiff type, high cost lenders consistently showed more of a racial imbalance in their lawsuits than others.

### 3.6 Robustness Checks

In this section, we provide various robustness checks including different measures of racial composition and exploring the impact of the racial composition of the defendant pool as opposed to the racial composition of the neighborhood. We also explore selection on unobservables, an alternative measure of credit score, and alternative judgment data sources.

### 3.6.1 Race Proxies

In this section we show that our results are robust to using the share of black residents in a neighborhood as opposed to a binary measure. Columns (1) and (2) of Table 3.7 present this result. Column (1) shows a positive and statistically significant coefficient on the share of black residents within a zip code and Column (2) shows our preferred specification from Table 3.2 which uses our binary measure for a black neighborhood. One potential concern with this analysis is that the racial composition of defendants within a neighborhood could be drastically different from the racial composition of the neighborhood itself. As such, we use our BISG measure of share black to estimate the racial composition of the defendant pool. Columns (3)-(4) present the results. Once again, the results are all positive and statistically significant. Column (5) uses only surname (and no information on zip code demographics) to predict the racial composition of defendants. The result is positive and statistically significant, although the effect size increases drastically.

Columns (6)-(7) of Table 3.7 present our results when the BISG method was used to estimate race but uses zip code fixed effects instead of county fixed effects. This is only possible with our proxies that utilize variation in defendants name because only these proxies give us variation in the racial composition of defendants over time. We once again get a positive and statistically significant coefficient, with black neighborhoods experiencing 0.6 more judgments per 100 people than comparable non-black neighborhoods. This is a 40% increase over the non-black neighborhood mean of 1.4 judgments per every 100 people.

#### 3.6.1.1 Other Races

One might wonder if this phenomenon is specific to the black population. In Table 3.8, we replicate Table 3.2 with additional controls for the share of Hispanic and Asian population within each zip code. While columns (1) and (2) of Table 3.8 show judgment gaps for both Asians and Hispanic neighborhoods (with share Asian being negatively related to judgments and share Hispanic being positively related to judgments), these disparities are completely explained away by differences in credit scores, income, and our other baseline controls; Columns (5) and (6) show no statistically significant coefficients for the share of Hispanic and Asian populations. The share of black residents remains positive and statistically significant in each of the specifications. These results indicate that there is something specific about black neighborhoods that is causing the gap in judgments.

### 3.6.2 Selection on Unobservables

We also investigated the impact of selection on unobservables on coefficient stability (Oster, 2019). In particular, we used Column (5) of Table 3.2 as our benchmark, and found that given a selection on unobservables that is half of the size of the selection on observables, our coefficient on black majority is reduced to 0.34 with a 95% confidence interval ranging from [0.13 to 0.54].[69] This suggests that 24% of our baseline judgment gap of 1.4 would remain after controlling for unobservable characteristics.[70] This finding suggests that a racial gap is unlikely to be zero, even after controlling for any unobservable characteristics.

[69] We bootstrapped our treatment coefficient estimates 100 times and assumed a maximum $R^2$ value of 0.9.

[70] We also examined the proportion of selection of unobservables to observables that would explain away our treatment effect. We found that a ratio of 1.08 with a 95% confidence interval ranging from [0.56, 1.6] is sufficient to explain away our findings.

### 3.6.3 Alternative Credit Score

In Table C2 in the Appendix, we add an alternative control for credit score, that was calculated using a deep learning algorithm. The model was shown to consistently outperform standard credit scoring models when predicting default rates (Albanesi and Vamossy, 2019). This alternative credit score has more predictive power then credit score in predicting default. However, it does not mitigate the racial bias we see in judgments across black and non-black communities. This provides additional support that differences in credit scores, which measure a borrower's likelihood of defaulting, is not the main factor driving the judgment gap between black and non-black communities.

### 3.6.4 Evolution of Disparity

Figure 3.5 plots the evolution of the racial disparity from 2004-2013. The racial disparity is present over our whole sample period, however it increases dramatically during the great recession. This could be taken as evidence that minority neighborhoods were disproportionately impacted by recession or that they had less wealth to help mitigate the negative shocks associated with the recession.

### 3.6.5 Alternative Data Sources

#### 3.6.5.1 New Jersey and Illinois Data

Tables 3.1 and 3.2 are replicated using data from New Jersey and Cook County, Illinois. The results are presented in Table C3 and C4 in the appendix. Table C3 shows that judgments per

100 people are larger in majority black neighborhoods compared to majority non-black neighborhoods, while median income and median credit score tend to be lower. Table C4 confirms that the racial gap in debt collection judgments cannot be explained by differences in median income or median credit score. These results suggest that judgments are 30% higher in majority black neighborhoods compared to majority white ones. Differences in other observable characteristics, such as default rates, can explain some of this disparity, although even after controlling for these differences, judgments are still 22% higher in black neighborhoods compared to non-black neighborhoods.

#### 3.6.5.2 Individual Data

Tables 3.1, 3.2, and 3.3 are replicated using merged Experian-ACS data. All the credit variables, including judgments, are individual specific. Racial composition and other control variables from the census are imputed by zip code. The results are presented in Tables C5, C6, and C7 in the appendix. About 75% of the racial disparity can be explained by differences in income and debt portfolios; being from a black neighborhood is associated with 0.02 more judgments, a 22% increase over the baseline rate of 0.07 judgments per 100 people.[71]

## 3.7 Conclusion

Our estimates suggest that there are 40% more debt collection judgments in majority black neighborhoods compared to non-black neighborhoods even after controlling for differences in

[71] Only 24% of the baseline judgment gap of 0.069 remains after the inclusion of income and credit controls. This is the same share of the judgment gap that remains unexplained from the Oster test presented above.

incomes and credit scores. This racial disparity exists for different racial measures and cannot be fully explained away by the share of contested versus uncontested cases across black and non-black communities or by differences in debt characteristics. The racial gap in debt collection judgments cannot be explained by differences in lending institutions and exists for every plaintiff type, however, certain types of plaintiffs consistently showed more of a racial imbalance in their lawsuits than others.

There are two potential explanations that we cannot explore using our current data: differences in wealth and discrimination. It is unclear where discrimination would occur during the legal process, as most cases are fairly algorithmic and heard by a judge with no jury necessary. Furthermore, Keil and Waldman (2015) quote Lance LeCombs, the Metropolitan St. Louis Sewer District's spokesman, who claims his company has no demographic data on its customers and treated them all the same. The racial disparity in its suits, he said, is the result of "broader ills in our community that are outside of our scope and exceed our abilities and authority to do anything about." According to estimates provided by the United States Census Bureau in 2016, one such broader ill is that the typical black household has a net worth of $12,920, while that of a typical white household is $114,700 - this is a $101,780 difference in wealth that could have important implications for a household's ability to mitigate negative income shocks. About 35,000 of this wealth gap is not driven by home equity. By translating this wealth gap into a difference in annual income and using our estimates of the relationship between income and judgements, we calculate that a wealth gap of this size would explain almost all of our most conservative estimate of the judgment gap across black and non-black communities.[72]

[72] Our most conservative estimate of the judgment gap is 0.34 more judgments per 100 people in majority black neighborhoods compared to majority white ones and is derived from Oster (2016). We computed the difference in annual savings needed over a 40-year horizon to generate a wealth gap of $35,000. We found that an annual

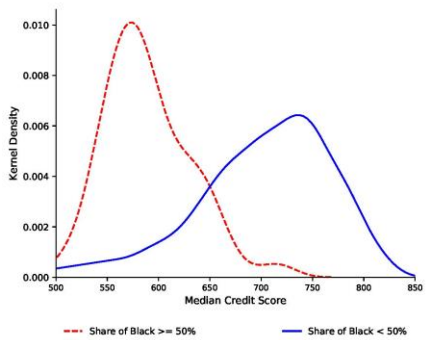
As the number of debt collection cases rise, identifying both the extent to which racial disparities exist and how they are entering the debt collection system are crucial. Future research should explore policies meant to provide more protections to consumers and how they impact the racial disparity in debt collection judgments. Such reforms could require debt buying companies to prove they own the debt before they can sue a debtor, preventing companies from winning judgments when the statute of limitations has expired on a debt[73], or require collection attorneys to prove they have a legal right to collect attorney fees and provide an itemized list of their work on the case in order to win an attorney's fee through a default judgment[74]. When states do provide legal protections for debtors, such as allowing those with children to keep more of their pay under a head of family exemption, the burden is typically on the debtor to assert these protections. Another policy reform could require a clear notice that these are provided to debtors.

difference of $2,910 is sufficient to generate the wealth gap in net present value. For interest rate, we applied the historical return of the stock market, which between 1957 through 2018 is roughly 8%. Consistent with estimates from the U.S. Bureau of Economic Analysis, we assume an 8% personal savings rate. This translates into an annual income difference of $36,375. Increasing the median income of majority black neighborhoods by this amount would decrease the judgment rate by 0.25 judgments per 100 people.

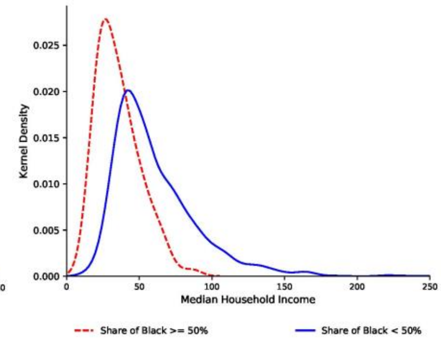
[73] In most states, the law currently requires defendants to know that the statute of limitations has expired and raise it as a defense in court.

[74] Currently, when companies sue, they often request such fees, which are usually granted and passed on to the debtor as part of the judgment. For example, in Missouri, the fees are usually set at 15 percent of the debt owed, even though attorneys may spend only a few minutes on a suit.

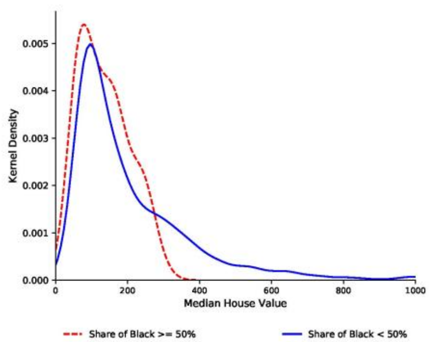
## 3.8 Figures and Tables



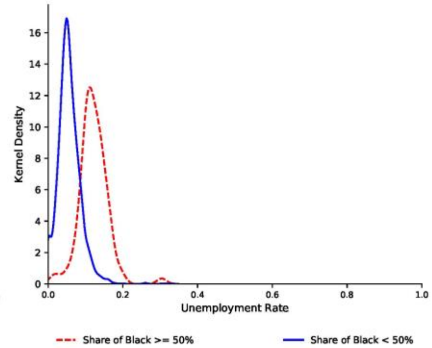

(a) Credit Score



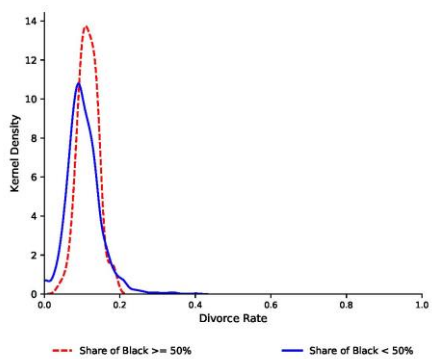

(b) Household Income



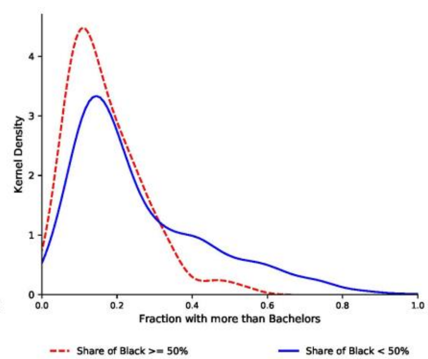

(c) House Value




(d) Unemployment Rate




(e) Divorce Rate




(f) >=Bachelor Degree

**Figure 3.1: Kernel Density Estimates of Select Covariates**

Notes: This figure shows the kernel densities of select variables of interest broken down by the racial composition of neighborhoods.

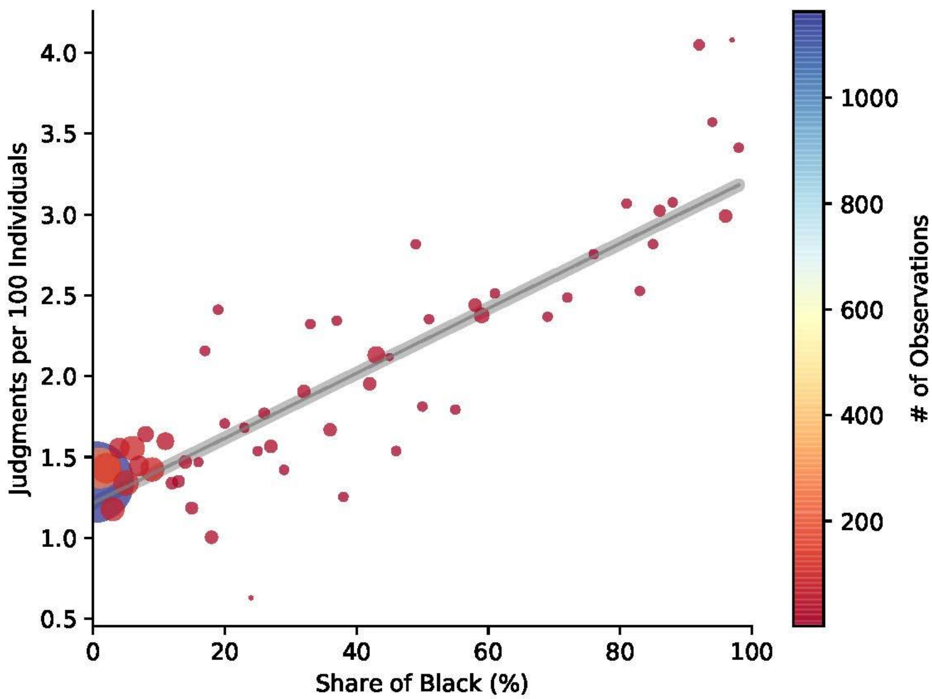
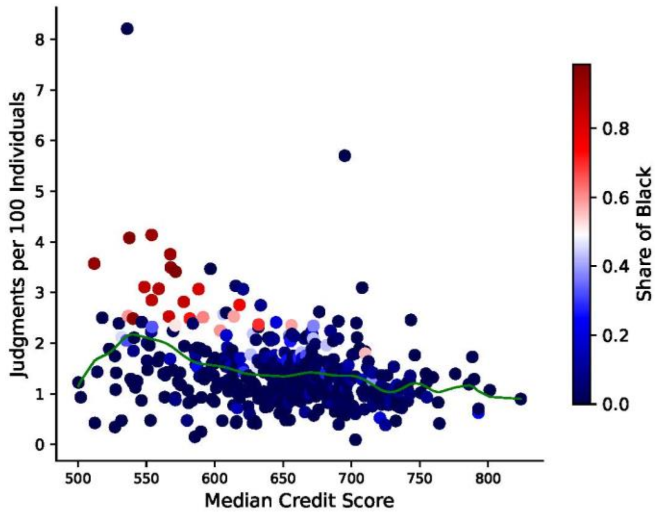
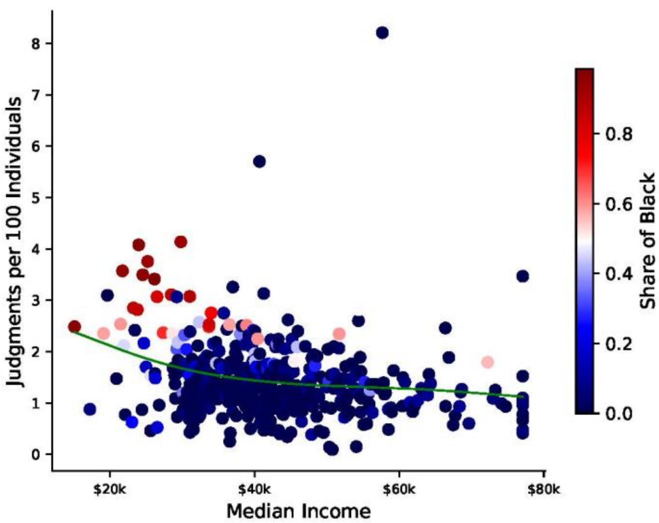



**Figure 3.2: Judgments and Demographic Composition**

Notes: Linear regression illustrates the relationship between share of black population and judgment rate. We categorized zip codes into one of a hundred bins based on their share of black residents and plotted the average share black of each bin against the average judgment rate of each bin. The size of the bubbles corresponds to the number of observations in each of the bins. The regression line represents the fit between the average judgment rate in the bin and the share of black population weighted by the number of observations in the bin.




(a) Median Income and Judgment Rate




(b) Median Credit Score and Judgment Rate

**Figure 3.3: Income, Credit Scores, and Judgment Rate**

Notes: The green line represents the non-parametric locally weighted regression line (LOESS) showing the smoothed fit curve of the data. Income is winsorized at the 98% level to mitigate the impact of outliers.

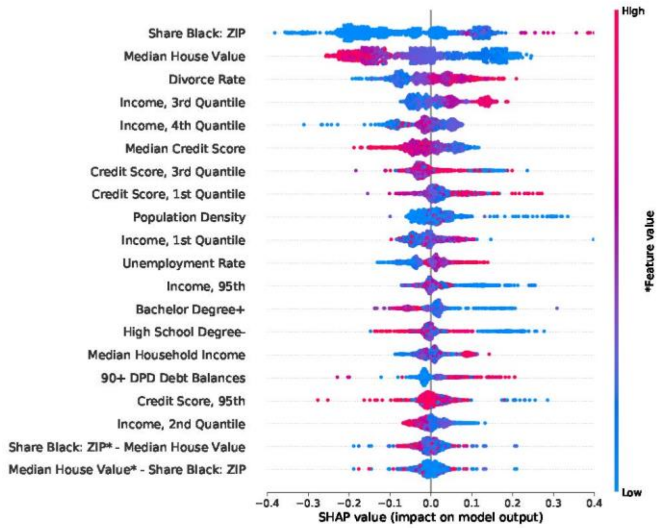



**Figure 3.4: GBT Feature Explanations**

Notes: This figure orders our independent variables in order of their importance as a predict of judgments in our Gradient Boosted Trees procedure. We use Shapley Additive exPlanations (SHAP) to explain the output of our Gradient Boosted Trees nonlinear model. The SHAP value gives us individualized impacts for each predictor; positive SHAP values are associated with increased judgment rates and negative SHAP values are associated with decreased judgment rates.

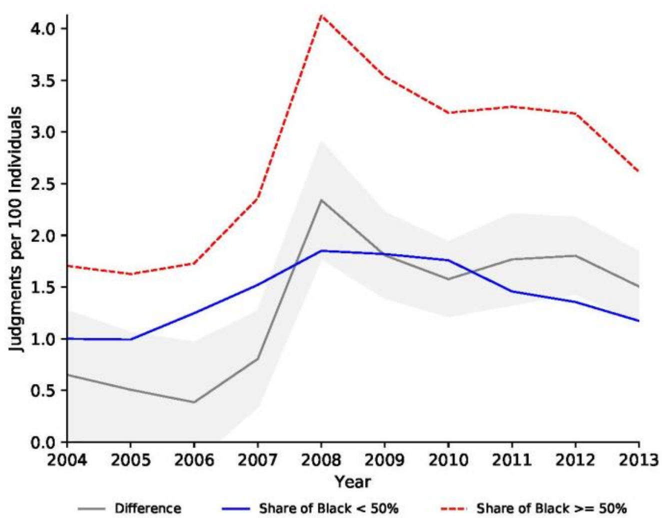



**Figure 3.5: Disparity over Time**

Notes: In this figure we graph the judgment rate for both majority black and majority non-black neighborhoods. We estimate the disparity in judgments by year and graph this disparity along with the 95% confidence interval.

**Table 3.1: Summary Statistics**

| | Black | White | t-test |
|---|---|---|---|
| **Panel A: Judgments** | | | |
| Judgments per 100 people | 2.73 | 1.43 | -1.30*** |
| | (1.31) | (0.91) | |
| Share of Default Judgments | 0.45 | 0.38 | -0.06*** |
| | (0.07) | (0.12) | |
| Share of Consent Judgments | 0.16 | 0.16 | 0.01 |
| | (0.07) | (0.10) | |
| Share of Contested Judgments | 0.06 | 0.05 | -0.01* |
| | (0.04) | (0.06) | |
| Share w/ Attorney | 0.04 | 0.10 | 0.06*** |
| | (0.02) | (0.07) | |
| **Panel B: Credit Variables** | | | |
| Median Credit Score | 606.21 | 647.30 | 41.09*** |
| | (38.55) | (49.16) | |
| 90+ DPD Debt Balances | 3347.35 | 2336.32 | -<br>1011.03*** |
| | (2690.36) | (5619.73) | |
| Banks (5 miles | 86.22 | 23.24 | -62.99*** |
| | (40.75) | (42.50) | |
| Payday Lenders (5 miles) | 30.49 | 7.29 | -23.20*** |
| | (9.51) | (11.51) | |
| **Panel C: Census Data** | | | |
| Median Household Income (000s) | 32.07 | 42.86 | 10.79*** |
| | (12.04) | (12.07) | |
| Gini Index | 0.46 | 0.42 | -0.04*** |
| | (0.06) | (0.05) | |
| Unemployment Rate | 0.11 | 0.07 | -0.04*** |
| | (0.03) | (0.04) | |
| Divorce Rate | 0.13 | 0.12 | -0.01*** |
| | (0.02) | (0.04) | |
| Median House Value (000s) | 88.95 | 105.82 | 16.87*** |
| | (35.83) | (41.42) | |
| Fraction with Bachelor's Degree | 0.17 | 0.19 | 0.02* |
| | (0.10) | (0.12) | |
| Fraction without High School Degree | 0.19 | 0.15 | -0.04 |
| | (0.06) | (0.07) | |
| Observations | 227 | 2446 | |

Notes: Summary statistics for observations on the common support sample. Data is drawn from Missouri. Standard deviations are in parenthesis.

**Table 3.2: Judgments, Income, and Credit Score**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Black Majority: ZIP | 1.2091*** | 0.9204*** | 0.7503*** | 0.7262*** | 0.5819*** | 0.6454*** |
| | (0.0437) | (0.0888) | (0.0857) | (0.0682) | (0.1175) | (0.0552) |
| Median Household Income | | 0.0006 | | 0.0137 | 0.0174 | |
| | | (0.0294) | | (0.0180) | (0.0181) | |
| Median Credit Score | | | -0.0101*** | -0.0085* | -0.0057 | |
| | | | (0.0035) | (0.0044) | (0.0034) | |
| County Fixed Effects | X | X | X | X | X | X |
| Year Fixed Effects | X | X | X | X | X | X |
| Baseline Controls | | | | | X | |
| Income Quintiles | | X | | X | X | |
| Credit Quintiles | | | X | X | X | |
| Lagged Baseline Controls | | | | | | X |
| Observations | 2673 | 2673 | 2673 | 2673 | 2673 | 2407 |
| R-squared | 0.5943 | 0.6354 | 0.6418 | 0.6529 | 0.6704 | 0.6825 |

Notes: Robust standard errors clustered at the county level are in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population and estimated on the common support sample.

**Table 3.3: Judgments and Debt Portfolios**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Black Majority: Zip | 0.5853*** | 0.5873*** | 0.5957*** | 0.5614*** | 0.5671*** |
| | (0.1193) | (0.1161) | (0.1123) | (0.1303) | (0.1339) |
| County Fixed Effects | X | X | X | X | X |
| Year Fixed Effects | X | X | X | X | X |
| Baseline Controls | X | X | X | X | X |
| Debt Levels | Yes | | | | Yes |
| Monthly Payment and Utilization | | Yes | | | Yes |
| Debt Composition | | | Yes | | Yes |
| Delinquency/Bankruptcy/Collections | | | | Yes | Yes |
| Observations | 2673 | 2673 | 2673 | 2673 | 2673 |
| R-squared | 0.6812 | 0.6733 | 0.6725 | 0.6745 | 0.6891 |

Notes: Robust standard errors clustered at the county level are in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population and estimated on the common support sample.

**Table 3.4: Judgments and Lending Institutions**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Black Majority: Zip | 0.8265*** | 0.8248*** | 0.8263*** | 0.7108*** | 0.6965*** |
| | (0.2801) | (0.2786) | (0.2803) | (0.2121) | (0.2861) |
| Broadband | | 0.0930** | | | |
| | | (0.0431) | | | |
| Unscored | | | 0.3830 | | |
| | | | (0.3923) | | |
| Banks (5 miles) | | | | -0.0068*** | |
| | | | | (0.0020) | |
| Payday Lenders (5 miles) | | | | 0.0219*** | |
| | | | | (0.0032) | |
| Banks (10 miles) | | | | | -0.0021*** |
| | | | | | (0.0001) |
| Payday Lenders (10 miles) | | | | | 0.0099*** |
| | | | | | (0.0013) |
| County Fixed Effects | X | X | X | X | X |
| Year Fixed Effects | X | X | X | X | X |
| Baseline Controls | X | X | X | X | X |
| Observations | 1703 | 1703 | 1703 | 1703 | 1703 |
| R-squared | 0.8463 | 0.8474 | 0.8463 | 0.8597 | 0.8557 |

Notes: Robust standard errors clustered at the county level are in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population and estimated on the common support sample.

**Table 3.5: Attorney Representation and Judgment Type**

| | (1) Attorney | (2) Judgments | (3) Consent | (4) Contested | (5) Default |
|---|---|---|---|---|---|
| Black Majority: Zip | -0.013*** | 0.579*** | 0.006 | 0.002 | 0.010 |
| | (0.004) | (0.121) | (0.004) | (0.006) | (0.008) |
| Attorney | | -0.190 | -0.015 | 0.058 | -0.234*** |
| | | (0.691) | (0.059) | (0.050) | (0.057) |
| County Fixed Effects | X | X | X | X | X |
| Year Fixed Effects | X | X | X | X | X |
| Baseline Controls | X | X | X | X | X |
| Observations | 2673 | 2673 | 2673 | 2673 | 2673 |
| R-squared | 0.667 | 0.670 | 0.661 | 0.431 | 0.532 |

Notes: Robust standard errors clustered at the county level are in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$. All regressions are weighted by population and estimated on the common support sample

**Table 3.6: Judgments by Plaintiff Type**

| | (1) Judgments | (2) Debt Buyer | (3) Major Bank | (4) Medical | (5) High-Cost | (6) Misc. |
|---|---|---|---|---|---|---|
| Black Majority: Zip | 0.582*** | 0.188*** | 0.041* | 0.076* | 0.105** | 0.193*** |
| | (0.118) | (0.045) | (0.022) | (0.040) | (0.047) | (0.049) |
| Mean | 1.299 | 0.580 | 0.456 | 0.288 | 0.083 | 0.161 |
| Effect Size | 44.8% | 32.5% | 8.9% | 26.4% | 125.7% | 119.7% |
| County Fixed Effects | X | X | X | X | X | X |
| Year Fixed Effects | X | X | X | X | X | X |
| Baseline Controls | X | X | X | X | X | X |
| Observations | 2673 | 2673 | 2673 | 2673 | 2673 | 2673 |
| R-squared | 0.670 | 0.699 | 0.751 | 0.641 | 0.614 | 0.553 |

Notes: Robust standard errors clustered at the county level are in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$. All regressions are weighted by population and estimated on the common support sample.

**Table 3.7: Judgments and Other Measures of Racial Composition**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Share Black: ZIP | 1.5253*** | | | | | | |
| | (0.1532) | | | | | | |
| Majority Black: ZIP | | 0.5819*** | | | | | |
| | | (0.1175) | | | | | |
| Share Black: BISG | | | 1.5163*** | | | | 7.1962*** |
| | | | (0.1389) | | | | (1.5909) |
| Black Majority: BISG | | | | 0.5955*** | | 0.5553** | |
| | | | | (0.0905) | | (0.2170) | |
| Share Black: Names | | | | | 5.6546*** | | |
| | | | | | (0.4711) | | |
| County Fixed Effects | X | X | X | X | X | | |
| Zip Code Fixed Effects | | | | | | X | X |
| Year Fixed Effects | X | X | X | X | X | X | X |
| Baseline Controls | X | X | X | X | X | | |
| Income Quintiles | X | X | X | X | X | | |
| Credit Quintiles | X | X | X | X | X | X | X |
| Observations | 2673 | 2673 | 2671 | 2671 | 2671 | 2671 | 2671 |
| R-squared | 0.6847 | 0.6704 | 0.6898 | 0.6769 | 0.6839 | 0.7296 | 0.7384 |

Notes: Robust standard errors clustered at the county level in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population and estimated on the common support sample.

**Table 3.8: Judgments and Other Demographic Groups**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Share Black: ZIP | 2.1829*** | 1.8677*** | 1.6103*** | 1.6150*** | 1.4976*** | 1.6356*** |
| | (0.0574) | (0.1594) | (0.1687) | (0.1021) | (0.1551) | (0.1052) |
| Share Asian: ZIP | -6.2263*** | -3.2578*** | -5.7555*** | -3.3496*** | -1.5249 | -1.3548 |
| | (1.2408) | (1.1516) | (1.0673) | (0.9776) | (1.3861) | (1.0703) |
| Share Hispanic: BISG | 1.3408*** | 0.1660 | 00.7119* | 0.1395 | -0.0104 | 0.1370 |
| | (0.4118) | (0.3155) | (0.4169) | (0.3216) | (0.4319) | (0.3732) |
| Median Income | | 0.0193 | | 0.0245** | 0.0281** | |
| | | (0.0170) | | (0.0122) | (0.0129) | |
| Median Credit Score | | | -0.0061 | -0.0051 | -0.0033 | |
| | | | (0.0037) | (0.0043) | (0.0039) | |
| County Fixed Effects | X | X | X | X | X | X |
| Year Fixed Effects | X | X | X | X | X | X |
| Baseline Controls | | | | | X | |
| Income Quintiles | | X | | X | X | |
| Credit Quintiles | | | X | X | X | |
| Lagged Baseline Controls | | | | | | X |
| Observations | 2673 | 2673 | 2673 | 2673 | 2673 | 2407 |
| R-squared | 0.6521 | 0.6694 | 0.6689 | 0.6769 | 0.6850 | 0.6993 |

Notes: Robust standard errors clustered at the county level are in parentheses. ∗ $p < 0.10$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population and estimated on the common support sample.

# Appendix A - The Long-Run Effects of Urban Renewal and Slum Clearance

## Appendix A.1 Additional Figures


**Figure A.1: Urban Renewal Projects in Chicago**

Notes: This figure shows the digitized project locations in Chicago, Illinois.

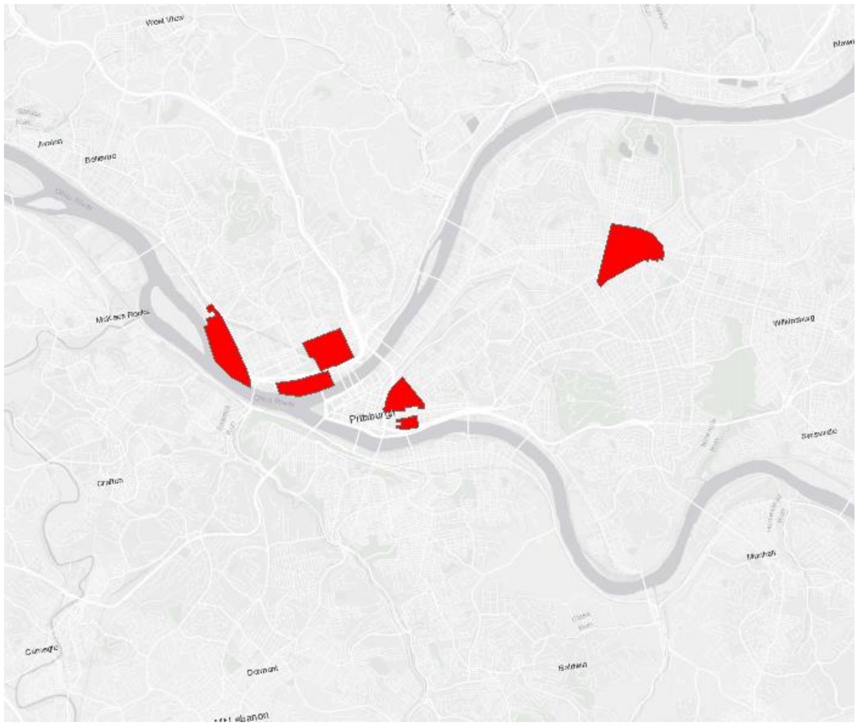
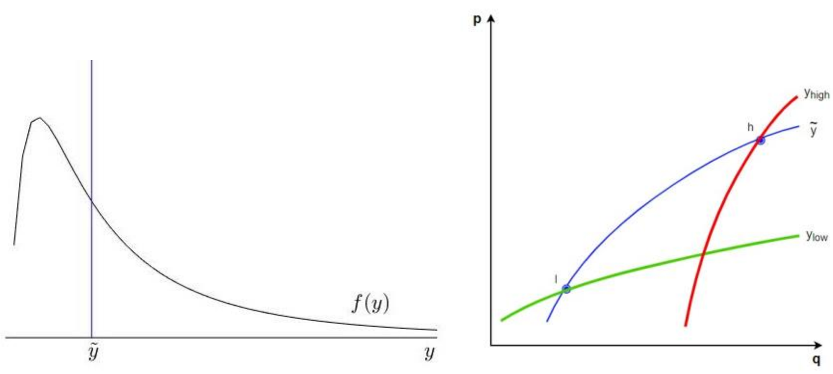



**Figure A.2: Urban Renewal Projects in Pittsburgh**

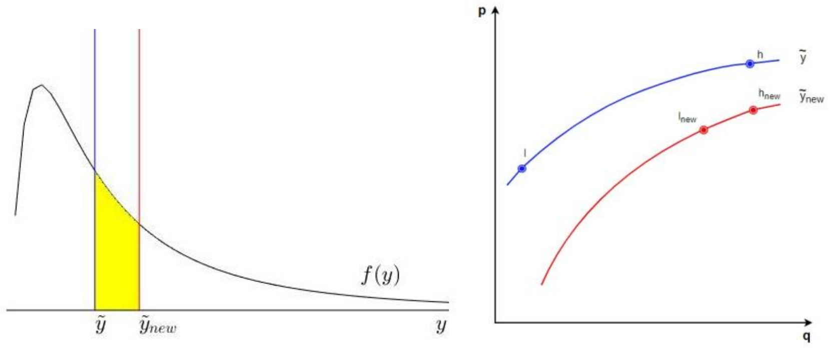
Notes: This figure shows the digitized project locations in Pittsburgh, Pennsylvania.




(a) Income Distribution (b) Indirect Indifference Curves

**Figure A.3: Single Crossing Property**

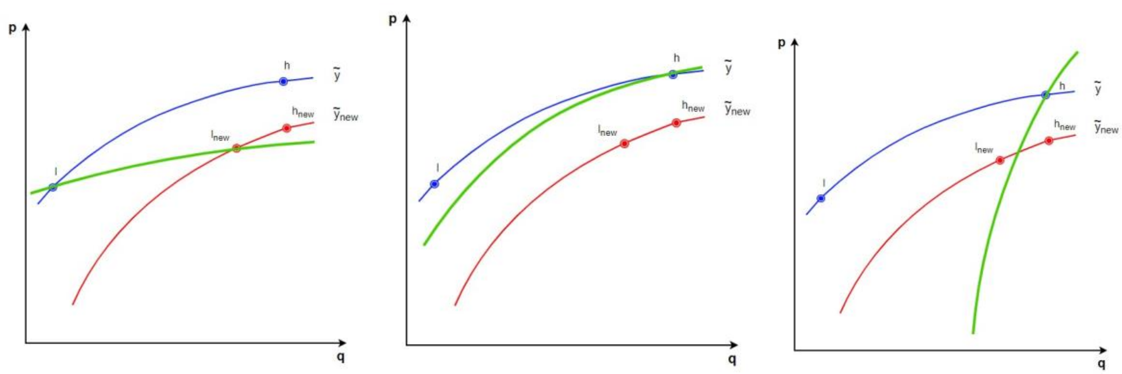
Notes: This figure provides a visual representation of the equilibrium income sorting that is induced by the single crossing property. Panel (a) shows an example of an income distribution and Panel (b) shows the two neighborhood option in the (q,p) plane and the direct indifference curves associated with three different households.




(a) Income Distribution (b) Indirect Indifference Curves

**Figure A.4: Quality Effect Dominates Price Effect**

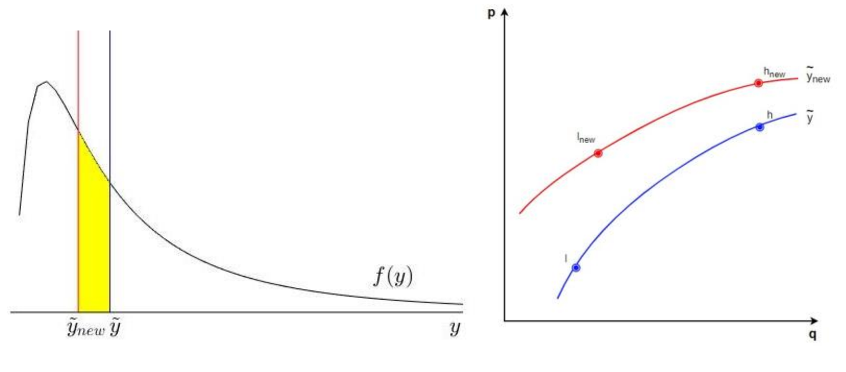
Notes: This figure provides a visual representation of the equilibrium income sorting that is induced by the single crossing property when the quality effect dominates the supply effect, which induces households to move out of neighborhood h into neighborhood l. Panel (a) shows an example of an income distribution and Panel (b) shows the neighborhood option in the (q,p) plane both before and after urban renewal was introduced.




(a) Low Income (b) Middle Income (c) High Income

**Figure A.5: Welfare Implications when Quality Effect Dominates**

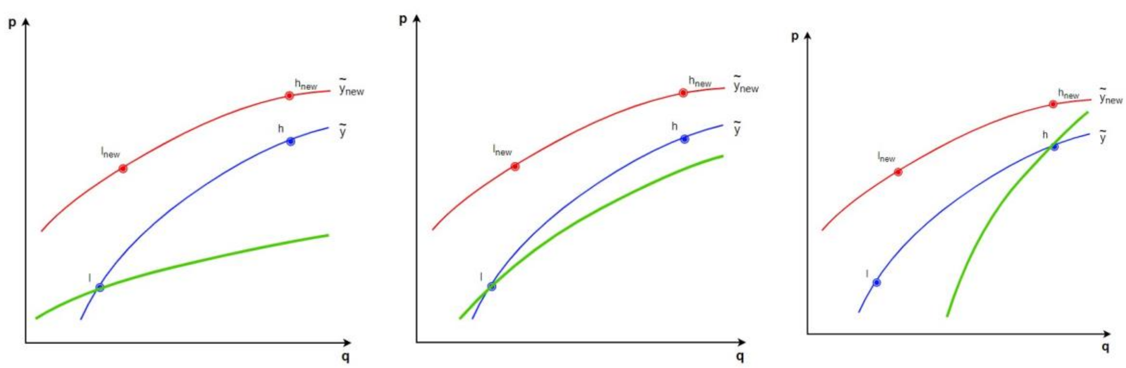
Notes: This figure provides a visual representation of the welfare implications of urban renewal when the quality effect dominates the supply effect. Each panel includes the neighborhood options and the indirect indifference curve of the boundary household both before and after urban renewal occurred. Panel (a) shows an indirect indifference curve for a low-income household who remains in neighborhood l, Panel (b) shows an indirect indifference curve for a household who moves from neighborhood h to neighborhood l, and Panel (c) shows an indirect indifference curve for a high income household who remains in neighborhood h.




(a) Income Distribution (b) Indirect Indifference Curves

**Figure A.6: Price Effect Dominates Quality Effect**

Notes: This figure provides a visual representation of the equilibrium income sorting that is induced by the single crossing property when the supply effect dominates the quality effect, which induces households to move out of neighborhood l into neighborhood h. Panel (a) shows an example of an income distribution and Panel (b) shows the neighborhood option in the (q,p) plane both before and after urban renewal was introduced.




(a) Low Income (b) Middle Income (c) High Income

**Figure A.7: Welfare Implications when Supply Effect Dominates**

Notes: This figure provides a visual representation of the welfare implications of urban renewal when the supply effect dominates the quality effect. Each panel includes the neighborhood options and the indirect indifference curve of the boundary household both before and after urban renewal occurred. Panel (a) shows an indirect indifference curve for a low-income household who remains in neighborhood l, Panel (b) shows an indirect indifference curve for a household who moves from neighborhood l to neighborhood h, and Panel (c) shows an indirect indifference curve for a high income household who remains in neighborhood h.

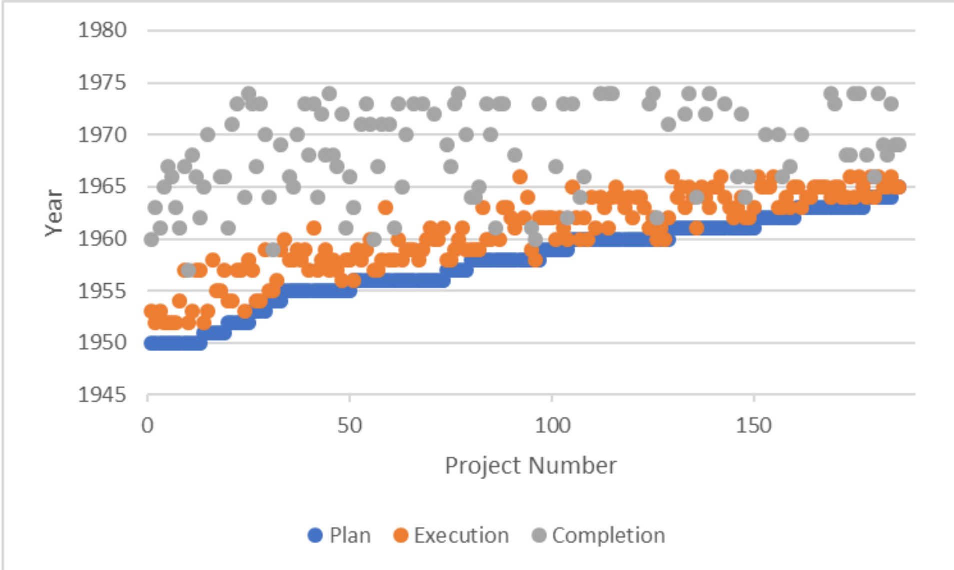
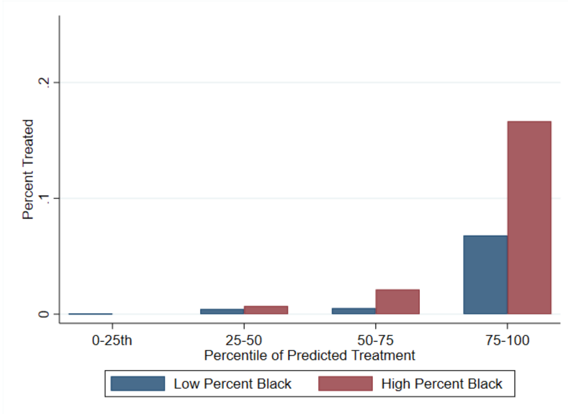
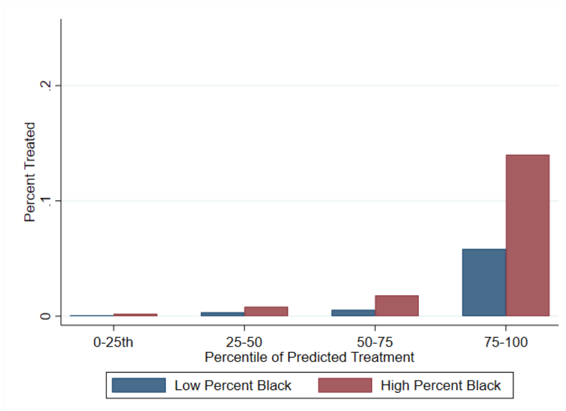



**Figure A.8: Project Timing**

Notes: This figure plots the timing variables associated with the urban renewal projects used in this analysis. Data comes from the "Urban Renewal Directory" (June 30,1974) U.S. Department of Housing and Urban Development - Community Planning and Development.



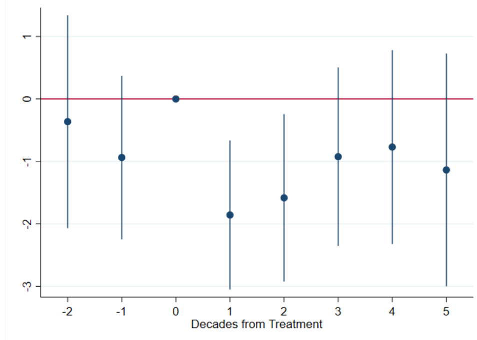

(a) Full Predicted Treatment Distribution



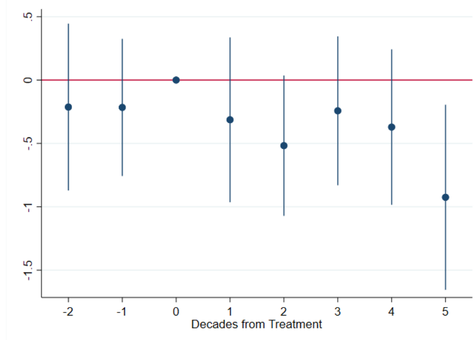

(b) Top 10% of Predicted Treatment Distribution

**Figure A.9: Racial Bias in Site Selection - Alternative Specifications**

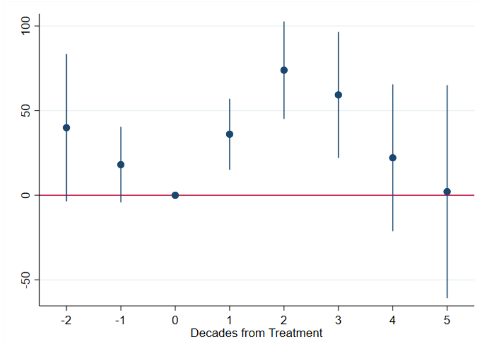
Notes: This figure graphs the share of tracts that received an urban renewal project by quartile of predicted treatment and the share of black residents in a neighborhood. High and low percent black are defined as being above and below the average share of black residents in the sample. Predicted treatment was calculated using a probit regression of treatment on all observable characteristics of neighborhood except racial composition. These specifications correspond to the specifications used in column (1) and column (2) of Table 1.3.



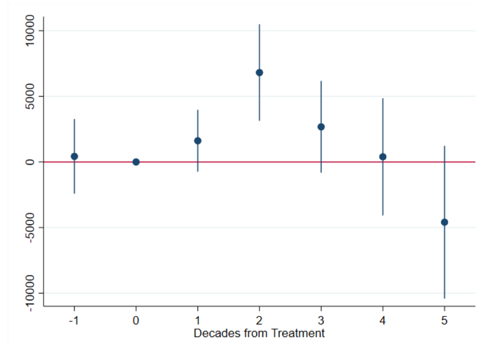

(a) Population Density



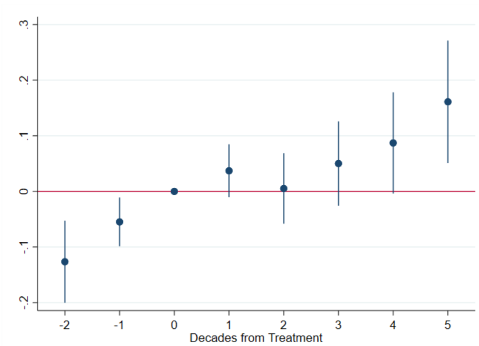

(b) Housing Density




(c) Median Rent



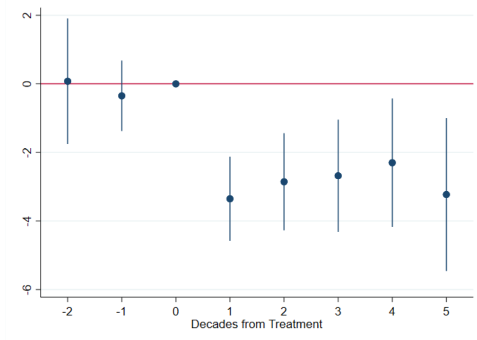

(d) Median Income



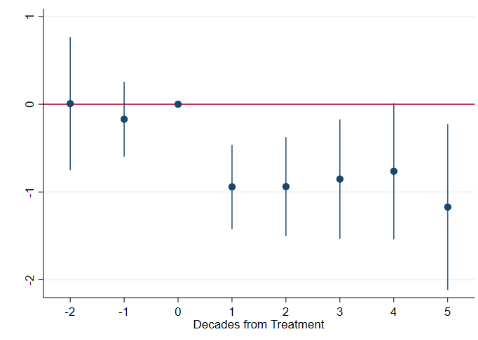

(e) Percent Black

**Figure A.10: Direct Effects of Urban Renewal - Residential Projects Only**

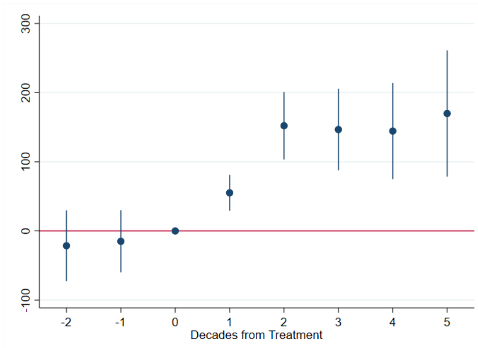
Notes: This figure shows the regression results on the $\tau_k$ coefficients from equation (1.14). In this specification, k=5 was used in the k-nearest neighbors technique to identify urban blight in control cities. Robust standard errors are clustered at the neighborhood level. Only residential projects were used in this specification.



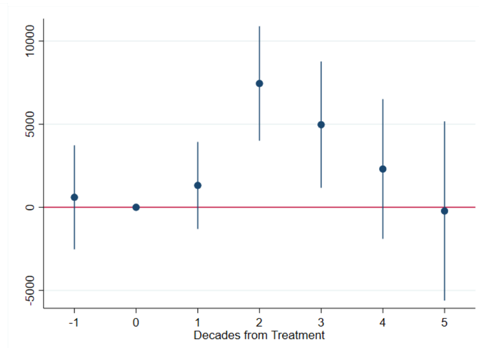

(a) Population Density



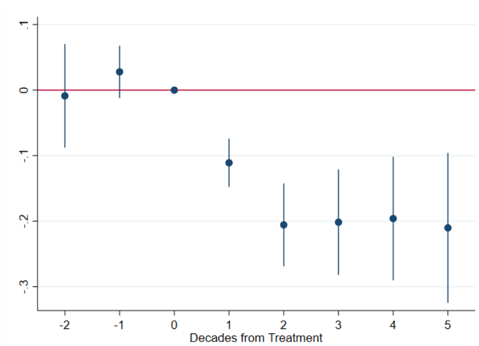

(b) Housing Density




(c) Median Rent



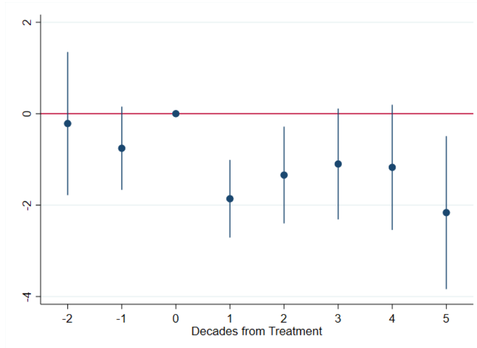

(d) Median Income



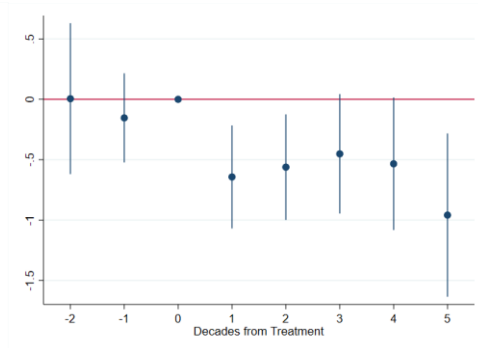

(e) Percent Black

**Figure A.11: Direct Effects of Urban Renewal - Non-Resid. Projects Only**

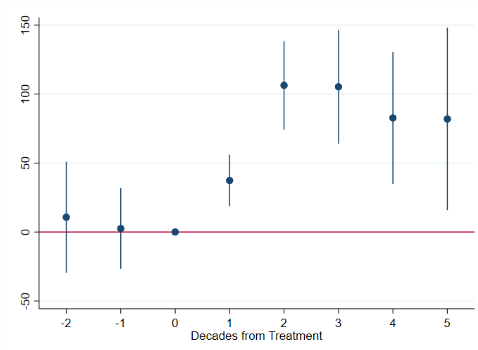
Notes: This figure shows the regression results on the $\tau_k$ coefficients from equation (1.14). In this specification, k=5 was used in the k-nearest neighbors technique to identify urban blight in control cities. Robust standard errors are clustered at the neighborhood level. Only non-residential projects were used in this specification.



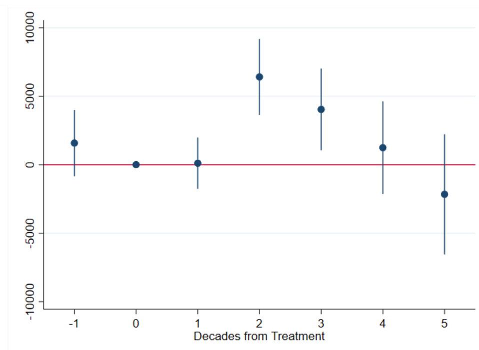

(a) Population Density



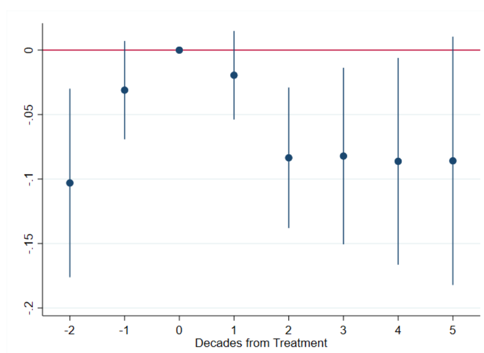

(b) Housing Density




(c) Median Rent



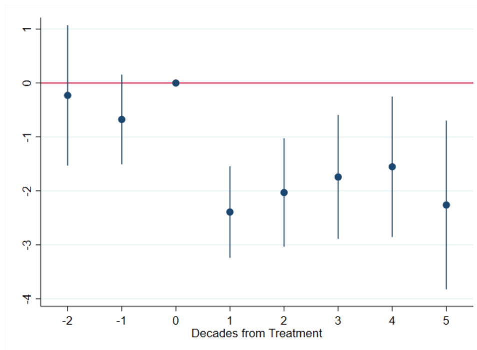

(d) Median Income



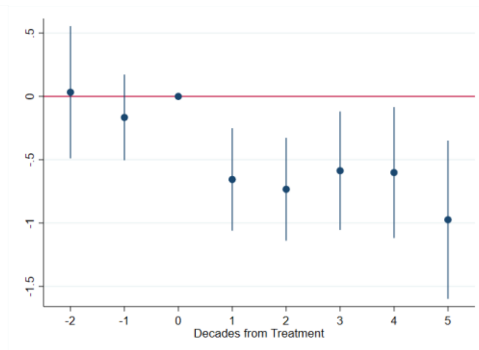

(e) Percent Black

**Figure A.12: Direct Effects of Urban Renewal (knn=3)**

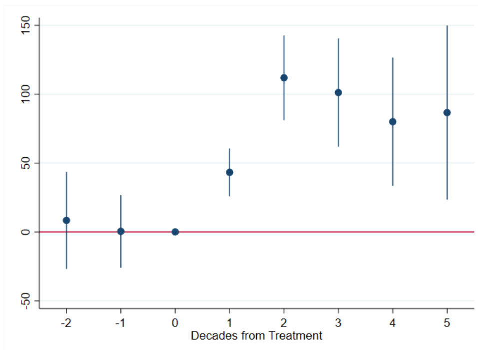
Notes: This figure shows the regression results on the $\tau_k$ coefficients from equation (1.14). In this specification, k=3 was used in the k-nearest neighbors technique to identify urban blight in control cities. Robust standard errors are clustered at the neighborhood level.



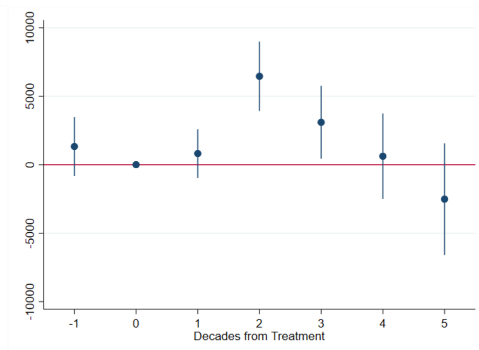

(a) Population Density



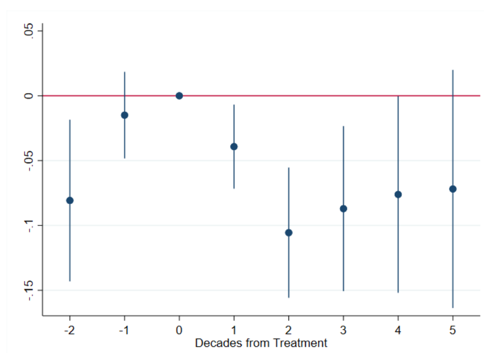

(b) Housing Density




(c) Median Rent



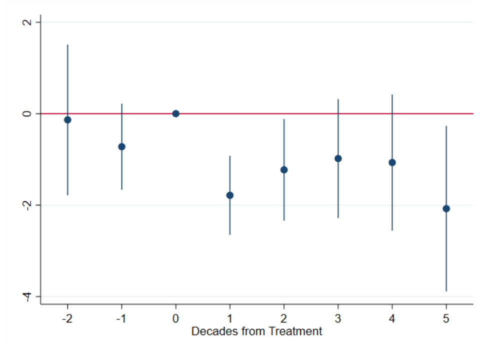

(d) Median Income

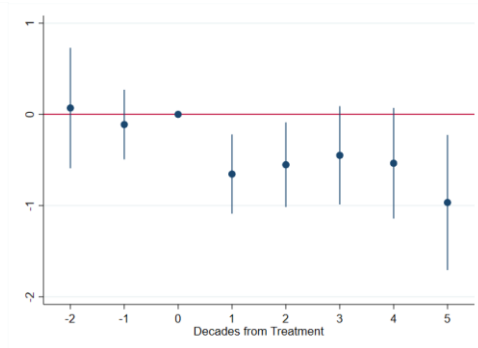



(e) Percent Black

**Figure A.13: Direct Effects of Urban Renewal (knn=7)**

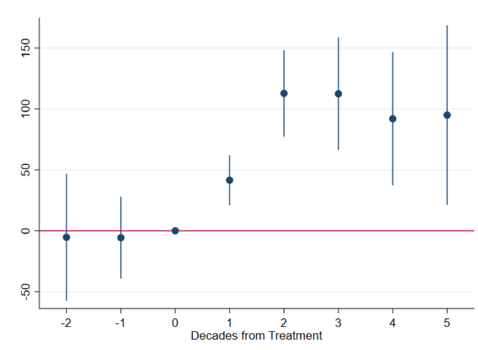
Notes: This figure shows the regression results on the τk coefficients from equation (1.14). In this specification, k=7 was used in the k-nearest neighbors technique to identify urban blight in control cities. Robust standard errors are clustered at the neighborhood level.

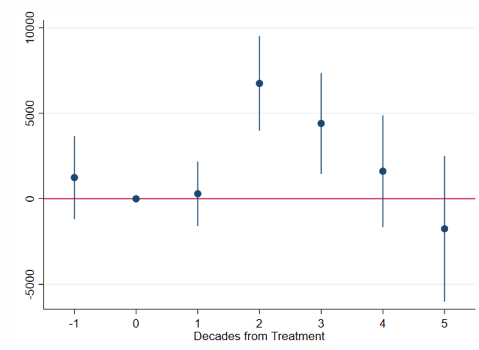



(a) Population Density

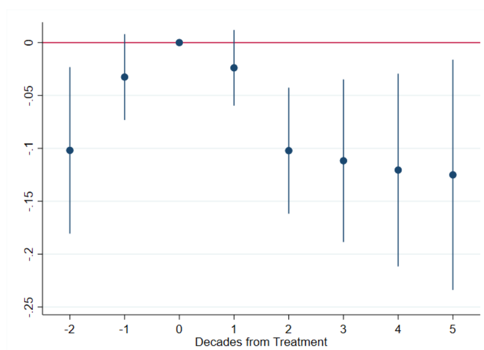
(b) Housing Density




(c) Median Rent

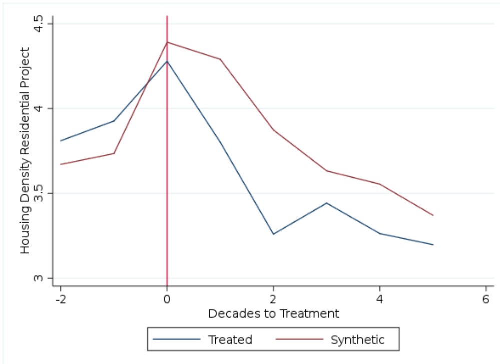
(d) Median Income



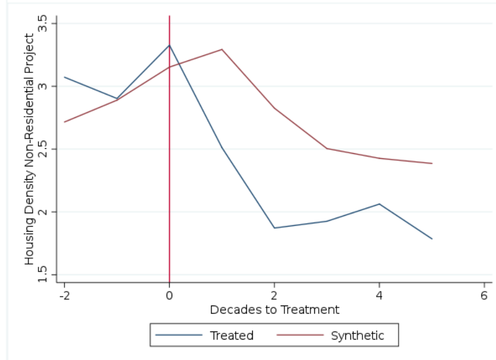

(e) Percent Black

**Figure A.14: Direct Effects of Urban Renewal (knn=5, year=1950)**

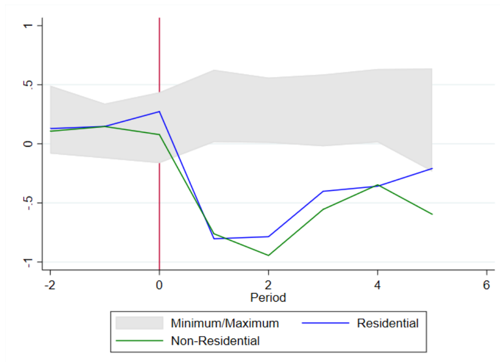
Notes: This figure shows the regression results on the $\tau_k$ coefficients from equation (1.14). In this specification, k=5 was used in the k-nearest neighbors technique to identify urban blight in control cities. In this specification, data was only matched on 1950 values. Robust standard errors are clustered at the neighborhood level.




(a) Residential Projects



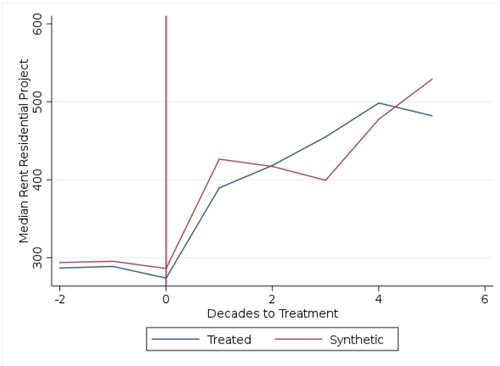

(b) Non-Residential Projects



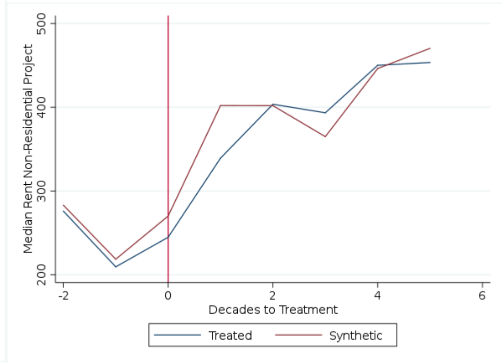

(c) Treatment Effects

**Figure A.15: Relative Effects on Housing Density by Project Type**

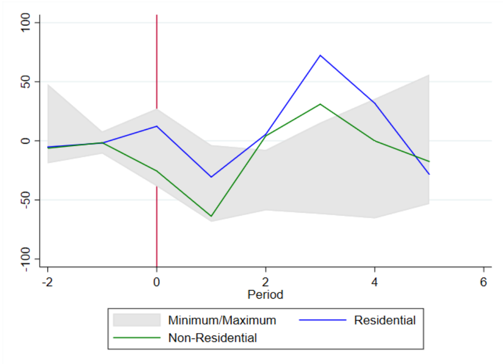
Notes: The outcome variable of interest in this figure is houses per 1000 sq meters. Panel (a) and (b) of this figure shows the averaged data for treated neighborhoods and the synthetic control groups across residential and non-residential projects separately. A different synthetic control group was constructed for each treatment neighborhood in my sample. The synthetic control group was constructed to minimize the pretreatment differences in observable characteristics between the treatment and control groups. Panel (c) of this figure shows the average differences between treated neighborhoods and the synthetic control groups. The shaded area shows the range of placebo effects estimated when treatment is randomly assigned to neighborhoods.




(a) Residential Projects



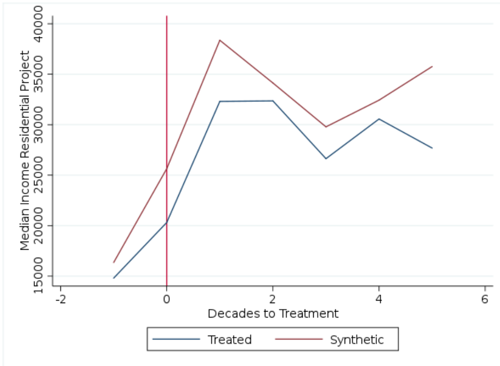

(b) Non-Residential Projects



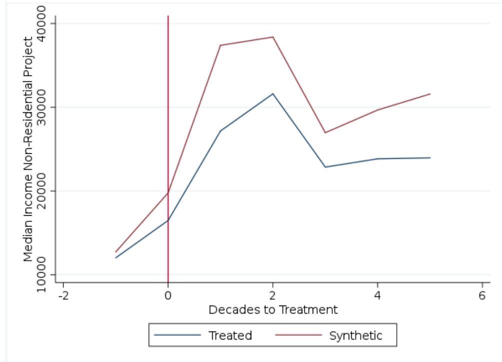

(c) Treatment Effects

**Figure A.16: Relative Effects on Median Rent by Project Type**

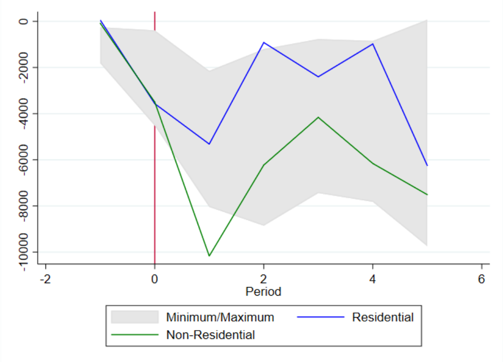
Notes: The outcome variable of interest in this figure is median rent. Panel (a) and (b) of this figure shows the averaged data for treated neighborhoods and the synthetic control groups across residential and non-residential projects separately. A different synthetic control group was constructed for each treatment neighborhood in my sample. The synthetic control group was constructed to minimize the pretreatment differences in observable characteristics between the treatment and control groups. Panel (c) of this figure shows the average differences between treated neighborhoods and the synthetic control groups. The shaded area shows the range of placebo effects estimated when treatment is randomly assigned to neighborhoods.




(a) Residential Projects




(b) Non-Residential Projects




(c) Treatment Effects

**Figure A.17: Relative Effects on Median Income by Project Type**

Notes: The outcome variable of interest in this figure is median income. Panel (a) and (b) of this figure shows the averaged data for treated neighborhoods and the synthetic control groups across residential and non-residential projects separately. A different synthetic control group was constructed for each treatment neighborhood in my sample. The synthetic control group was constructed to minimize the pretreatment differences in observable characteristics between the treatment and control groups. Panel (c) of this figure shows the average differences between treated neighborhoods and the synthetic control groups. The shaded area shows the range of placebo effects estimated when treatment is randomly assigned to neighborhoods.

## Appendix A.2 Additional Tables

**Table A.1: Cities in Sample**

| City | Projects | Funding (Millions of $) | Locations Identified | Control City |
|---|---|---|---|---|
| Baltimore | 15 | 82 | 15 | |
| Boston | 9 | 243 | 9 | |
| Buffalo | 2 | 33 | 2 | Yes |
| Chicago | 31 | 180 | 31 | |
| Cincinnati | 5 | 92 | 2 | |
| Cleveland | 7 | 97 | 1 | |
| Columbus | 6 | 24 | 4 | |
| Dallas | 0 | 0 | 0 | Yes |
| Detroit | 16 | 79 | 16 | |
| Denver | 4 | 7 | 4 | Yes |
| Washington, DC | 6 | 96 | 6 | |
| Indianapolis | 0 | 0 | 0 | Yes |
| Kansas City | 2 | 2 | 1 | Yes |
| Los Angeles | 1 | 20 | 1 | Yes |
| Louisville | 6 | 90 | 3 | |
| Memphis | 6 | 29 | 1 | |
| Milwaukee | 5 | 24 | 3 | |
| Minneapolis | 6 | 41 | 6 | |
| Newark | 11 | 121 | 11 | |
| New Orleans | 2 | 2 | 1 | Yes |
| New York | 25 | 247 | 25 | |
| Oakland | 2 | 15 | 0 | Yes |
| Philadelphia | 21 | 43 | 21 | |
| Pittsburgh | 6 | 111 | 6 | |
| Portland | 2 | 11 | 2 | Yes |
| San Francisco | 4 | 118 | 4 | Yes |
| Seattle | 2 | 9 | 2 | Yes |
| St. Louis | 4 | 70 | 4 | Yes |
| Total | 206 | 1657 | 183 | 12 |

Source: "Urban Renewal Directory" (June 30,1974) U.S. Department of Housing and Urban Development - Community Planning and Development.

**Table A.2: Project Characteristics - Housing and Race**

| City | Projects | Sub-Standard | Standard | White | Non-white |
|---|---|---|---|---|---|
| Baltimore | 15 | 6477 | 2514 | 2225 | 6251 |
| Boston | 9 | 11761 | 2841 | 6962 | 3268 |
| Buffalo | 2 | 3000 | 814 | 1632 | 1612 |
| Chicago | 31 | 24320 | 4583 | 8103 | 14491 |
| Cincinnati | 5 | 7255 | 2286 | 328 | 3943 |
| Cleveland | 7 | 4147 | 2027 | 716 | 5173 |
| Columbus | 6 | 1876 | 400 | 712 | 767 |
| Dallas | 0 | - | - | - | - |
| Detroit | 16 | 8677 | 1780 | 1970 | 5397 |
| Denver | 4 | 462 | 80 | 500 | 139 |
| Washington, DC | 6 | 6070 | 2156 | 1925 | 5620 |
| Indianapolis | 0 | - | - | - | - |
| Kansas City | 2 | 0 | 0 | 23 | 16 |
| Los Angeles | 1 | 3413 | 1674 | 1294 | 48 |
| Louisville | 6 | 4641 | 1099 | 1931 | 1756 |
| Memphis | 6 | 2729 | 580 | 811 | 1393 |
| Milwaukee | 5 | 2433 | 1287 | 911 | 367 |
| Minneapolis | 6 | 2016 | 478 | 1738 | 710 |
| Newark | 11 | 5499 | 1334 | 1952 | 3620 |
| New Orleans | 2 | 323 | 19 | 6 | 162 |
| New York | 25 | 35859 | 2158 | 15400 | 9936 |
| Oakland | 2 | 1735 | 55 | 0 | 0 |
| Philadelphia | 21 | 4853 | 1682 | 674 | 3132 |
| Pittsburgh | 6 | 5506 | 1934 | 2191 | 1819 |
| Portland | 2 | 1108 | 682 | 444 | 156 |
| San Francisco | 4 | 9858 | 3932 | 1633 | 3051 |
| Seattle | 2 | 243 | 163 | 65 | 10 |
| St. Louis | 4 | 9591 | 249 | 1609 | 5285 |
| Total | 206 | 163,849 | 36,807 | 56,355 | 78,122 |

Notes: This information was obtained from "Urban Renewal Project Characteristics" (June 30,1966) U.S. Department of Housing and Urban Development - Renewal Assistance Administration

**Table A.3: Direct Effects of Urban Renewal on Population Density**

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated – knn(3) | -1.42*** | -1.21** | -2.66*** |
| | (0.43) | (0.51) | (0.61) |
| Treated – knn(5) | -1.79*** | -1.47*** | -3.02*** |
| | (0.41) | (0.51) | (0.60) |
| Treated – knn(7) | -2.00*** | -1.60*** | -3.20*** |
| | (0.41) | (0.51) | (0.60) |
| Treated – match on 1950 | -1.36*** | -1.17*** | -2.58*** |
| | (0.44) | (0.52) | (0.63) |
| Neighborhood Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| City Specific Linear Time Trend | Yes | Yes | Yes |
| Pretreatment Mean of the Treated | 14.00 | 15.39 | 12.58 |
| Sample | Full | Residential | Non. Resid. |

Notes: Robust standard errors are clustered at the neighborhood level. *p < .10, **p < .05, ***p < .01. The outcome variable in all columns is population per 1000sq meters. Each table entry corresponds to a separate regression. Column (1) uses all treated tracts while column (2) uses only treated tracts where the majority of the land was used for residential purposes and column (s) uses only treated tracts were the majority of the land was used for non-residential purposes.

**Table A.4: Direct Effects of Urban Renewal on Housing Density**

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated – knn(3) | -.054*** | -0.29 | -0.82*** |
| | (0.20) | (0.28) | (0.24) |
| Treated – knn(5) | -0.54*** | -0.24 | -0.85*** |
| | (0.19) | (0.27) | (0.23) |
| Treated – knn(7) | -0.59*** | -0.27 | -0.92*** |
| | (0.18) | (0.27) | (0.22) |
| Treated – match on 1950 | -0.56*** | -0.30 | -0.83*** |
| | (0.21) | (0.29) | (0.25) |
| Neighborhood Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| City Specific Linear Time Trend | Yes | Yes | Yes |
| Pretreatment Mean of the Treated | 4.42 | 4.82 | 4.01 |
| Sample | Full | Residential | Non. Resid. |

Notes: Robust standard errors are clustered at the neighborhood level. *p < .10, **p < .05, ***p < .01. The outcome variable in all columns is housing units per 1000sq meters. Each table entry corresponds to a separate regression. Column (1) uses all treated tracts while column (2) uses only treated tracts where the majority of the land was used for residential purposes and column (s) uses only treated tracts were the majority of the land was used for non-residential purposes.

**Table A.5: Direct Effects of Urban Renewal on Median Rent**

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated – knn(3) | 55.37*** | 41.44*** | 78.48*** |
| | (10.60) | (12.26) | (15.84) |
| Treated – knn(5) | 64.68*** | 52.10*** | 87.43*** |
| | (10.62) | (12.17) | (16.08) |
| Treated – knn(7) | 64.96*** | 50.58*** | 88.25*** |
| | (10.44) | (12.05) | (16.09) |
| Treated – match on 1950 | 58.94*** | 49.41*** | 65.14*** |
| | (11.59) | (13.42) | (16.52) |
| Neighborhood Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| City Specific Linear Time Trend | Yes | Yes | Yes |
| Pretreatment Mean of the Treated | 267 | 287 | 245 |
| Sample | Full | Residential | Non. Resid. |

Notes: Robust standard errors are clustered at the neighborhood level. *p < .10, **p < .05, ***p < .01. The outcome variable in all columns is median rents. Each table entry corresponds to a separate regression. Column (1) uses all treated tracts while column (2) uses only treated tracts where the majority of the land was used for residential purposes and column (s) uses only treated tracts were the majority of the land was used for non-residential purposes.

**Table A.6: Direct Effects of Urban Renewal on Median Income**

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated – knn(3) | 1883** | 3655*** | 2848*** |
| | (938) | (1268) | (1235) |
| Treated – knn(5) | 2878*** | 4423*** | 3755*** |
| | (901) | (1274) | (1214) |
| Treated – knn(7) | 3069*** | 4398*** | 3863*** |
| | (892) | (1276) | (1215) |
| Treated – match on 1950 | 2002** | 4292*** | 2717** |
| | (947) | (1238) | (1262) |
| Neighborhood Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| City Specific Linear Time Trend | Yes | Yes | Yes |
| Pretreatment Mean of the Treated | 15949 | 17072 | 14662 |
| Sample | Full | Residential | Non. Resid. |

Notes: Robust standard errors are clustered at the neighborhood level. *p <.10, **p < .05, ***p < .01. The outcome variable in all columns is median income. Each table entry corresponds to a separate regression. Column (1) uses all treated tracts while column (2) uses only treated tracts where the majority of the land was used for residential purposes and column (s) uses only treated tracts were the majority of the land was used for non-residential purposes.

**Table A.7: Direct Effects of Urban Renewal on Share Black**

| | (1) | (2) | (3) |
|---|---|---|---|
| Treated – knn(3) | -0.03 | 0.04 | -0.12*** |
| | (0.02) | (0.03) | (0.02) |
| Treated – knn(5) | -0.05*** | 0.02 | -0.15*** |
| | (0.02) | (0.03) | (0.02) |
| Treated – knn(7) | -0.06*** | 0.02 | -0.15*** |
| | (0.02) | (0.03) | (0.02) |
| Treated – match on 1950 | -0.04* | 0.04 | -0.12*** |
| | (0.02) | (0.03) | (0.03) |
| Neighborhood Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| City Specific Linear Time Trend | Yes | Yes | Yes |
| Pretreatment Mean of the Treated | 0.31 | 0.29 | 0.34 |
| Sample | Full | Residential | Non. Resid. |

Notes: Robust standard errors are clustered at the neighborhood level. *p < .10, **p < .05, ***p < .01. The outcome variable in all columns is percentage black. Each table entry corresponds to a separate regression. Column (1) uses all treated tracts while column (2) uses only treated tracts where the majority of the land was used for residential purposes and column (s) uses only treated tracts were the majority of the land was used for non-residential purposes.

# Appendix B - Race, Risk, and the Emergence of Federal Redlining

## Appendix B.1 Additional Figures

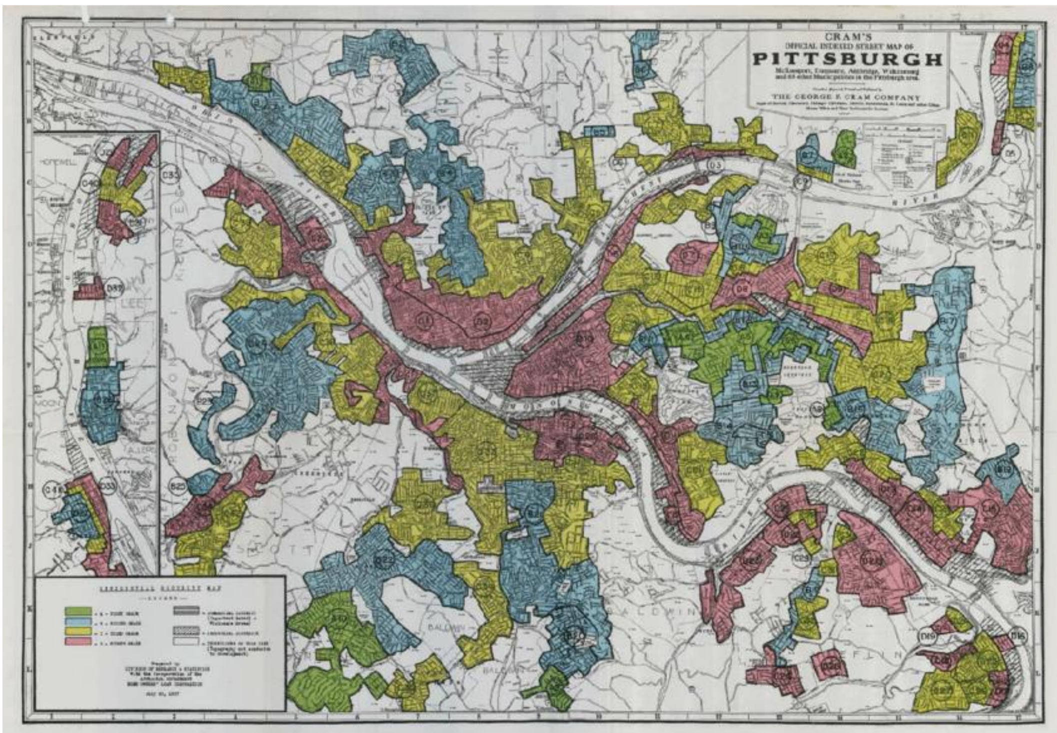
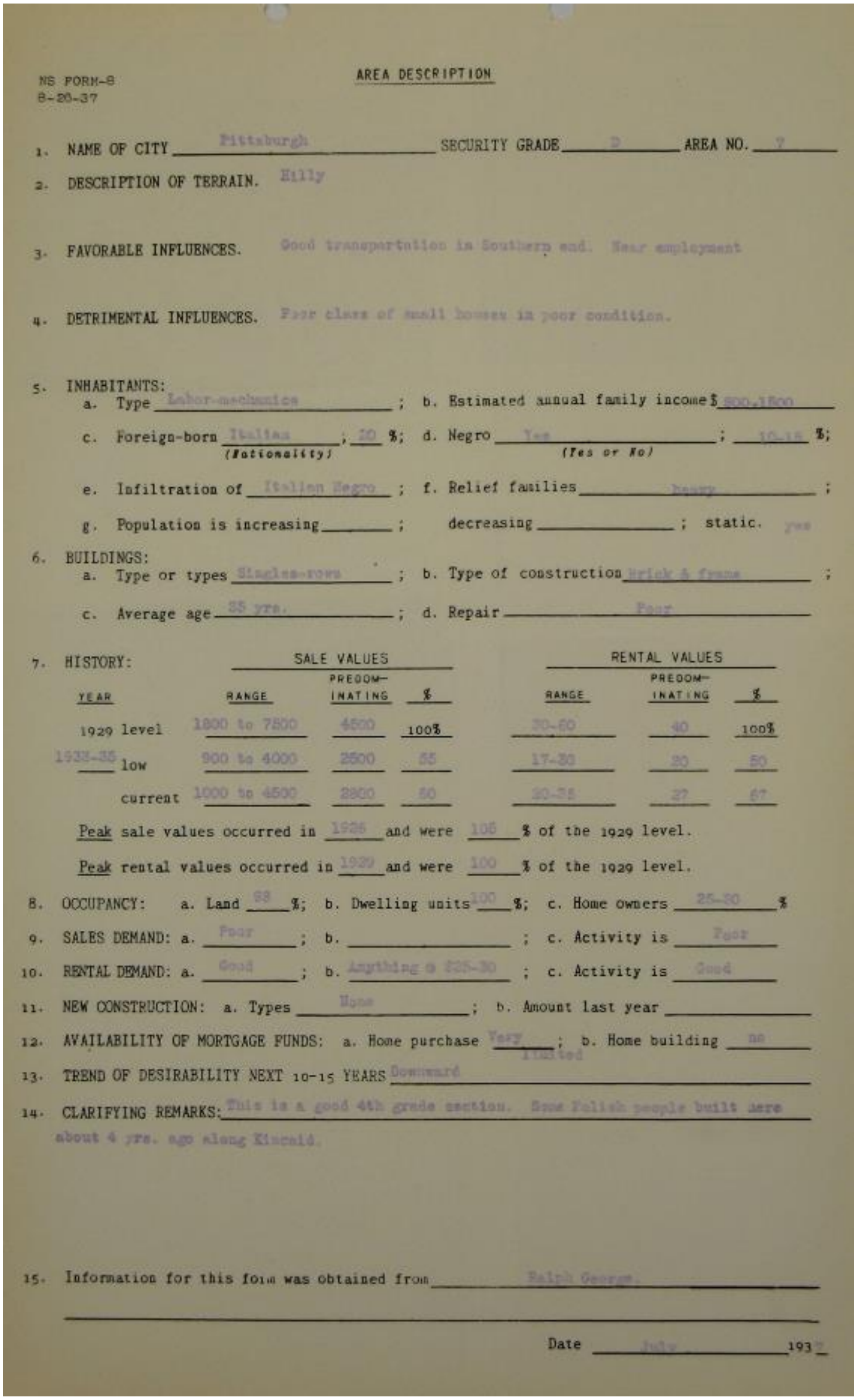



**Figure B.1: Pittsburgh Home Owners Loan Corporation Map**



NS FORM-8
8-26-37

AREA DESCRIPTION

1. NAME OF CITY Pittsburgh SECURITY GRADE D AREA NO. 7
2. DESCRIPTION OF TERRAIN. Hilly
3. FAVORABLE INFLUENCES. Good transportation in Southern end. Near employment
4. DETRIMENTAL INFLUENCES. Poor class of small houses in poor condition.
5. INHABITANTS:
   a. Type Labor-mechanics ; b. Estimated annual family income $ 800-1800
   c. Foreign-born Italian (Nationality) ; 20 %; d. Negro Yes (Yes or No) ; 10-15 %;
   e. Infiltration of Italian Negro ; f. Relief families heavy ;
   g. Population is increasing ; decreasing ; static. yes
6. BUILDINGS:
   a. Type or types Singles-rows ; b. Type of construction Brick & frame ;
   c. Average age 35 yrs. ; d. Repair Poor
7. HISTORY:

| YEAR | SALE VALUES RANGE | PREDOMINATING | % | RENTAL VALUES RANGE | PREDOMINATING | % |
|---|---|---|---|---|---|---|
| 1929 level | 1800 to 7500 | 4500 | 100% | 30-60 | 40 | 100% |
| 1933-35 low | 900 to 4000 | 2500 | 55 | 17-30 | 20 | 50 |
| current | 1000 to 4500 | 2800 | 60 | 20-35 | 27 | 67 |

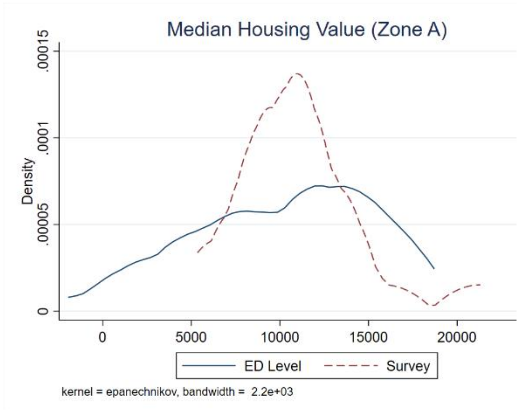
Peak sale values occurred in 1926 and were 105 % of the 1929 level.

Peak rental values occurred in 1929 and were 100 % of the 1929 level.

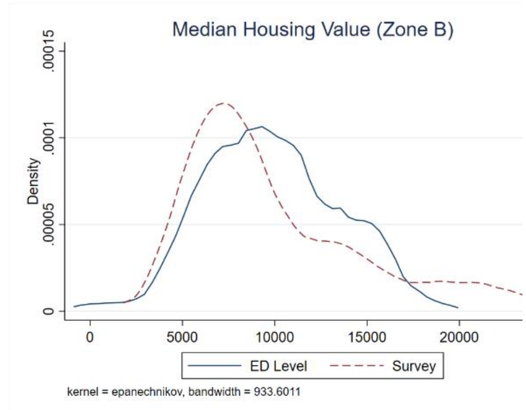
8. OCCUPANCY: a. Land 98 %; b. Dwelling units 100 %; c. Home owners 25-30 %
9. SALES DEMAND: a. Poor ; b. ; c. Activity is Poor
10. RENTAL DEMAND: a. Good ; b. Anything @ $25-30 ; c. Activity is Good
11. NEW CONSTRUCTION: a. Types None ; b. Amount last year
12. AVAILABILITY OF MORTGAGE FUNDS: a. Home purchase Very limited ; b. Home building no
13. TREND OF DESIRABILITY NEXT 10-15 YEARS Downward
14. CLARIFYING REMARKS: This is a good 4th grade section. Some Polish people built here about 4 yrs. ago along Kincaid.
15. Information for this form was obtained from Ralph George.

Date July 1937

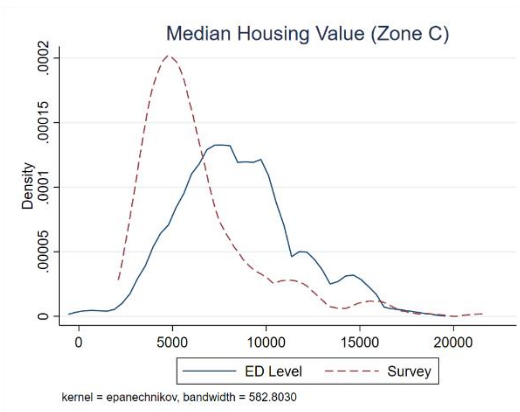
**Figure B.2: Example of HOLC Zone Survey (Pittsburgh D7 Zone)**



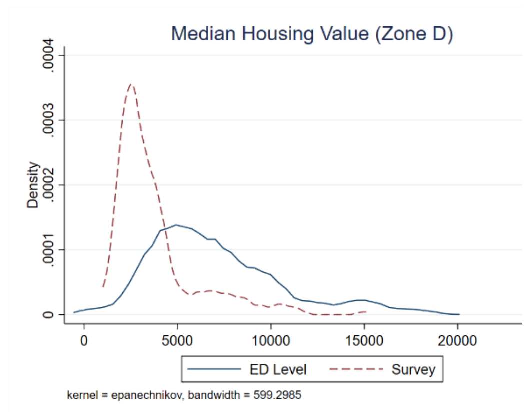

(a) Zone A




(b) Zone B



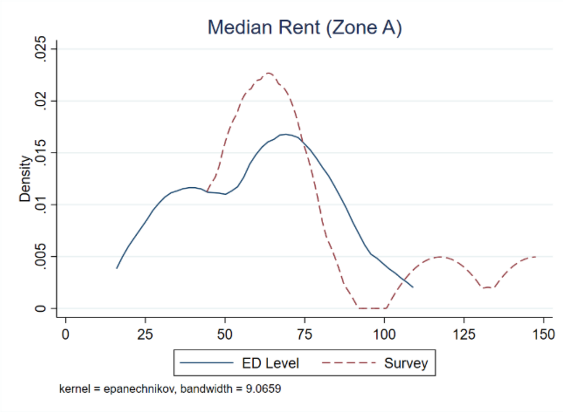

(c) Zone C



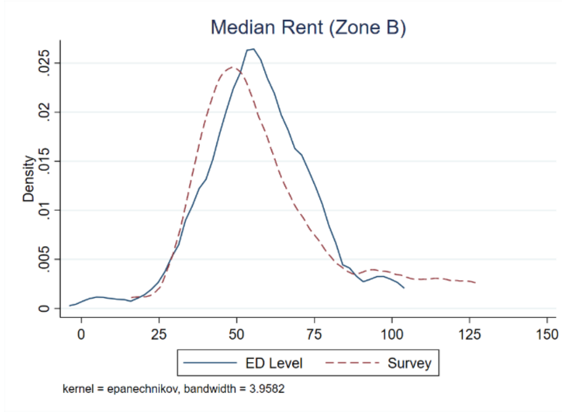

(d) Zone D

**Figure B.3: Survey verse Census Houshing Values**

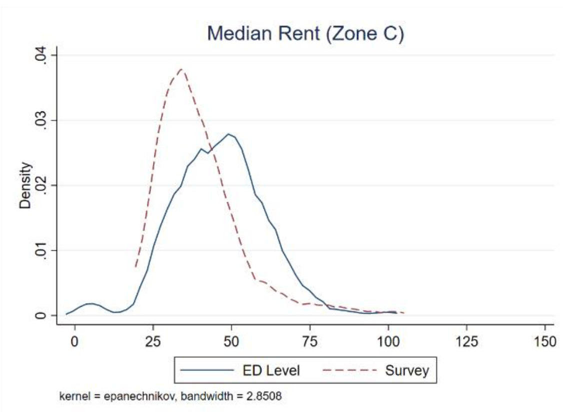
Notes: To analyze the accuracy of the surveys, we compare the kernel densities of survey measures of housing values with census measures of housing values. We use enumeration districts as our unit of observation for the census data and restrict our sample to only enumeration districts that lie completely within a residential security zone to prevent bias in our estimates.



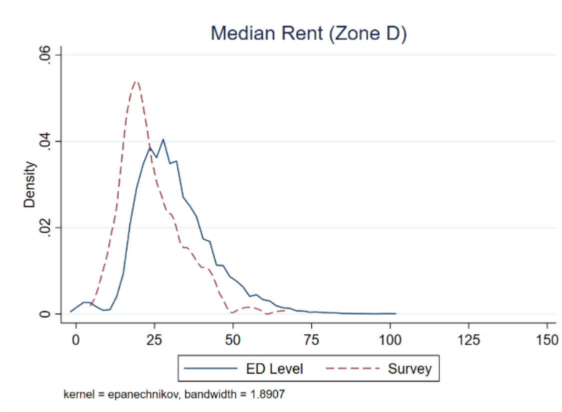

(a) Zone A




(b) Zone B



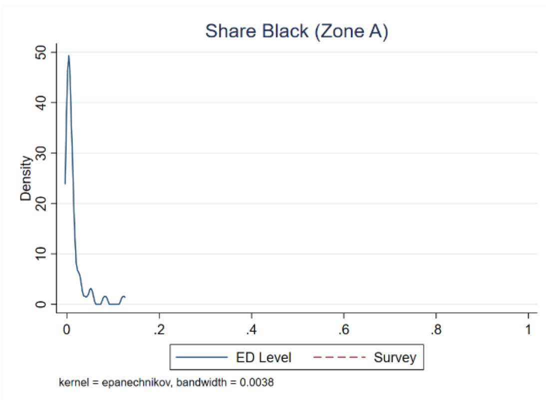

(c) Zone C



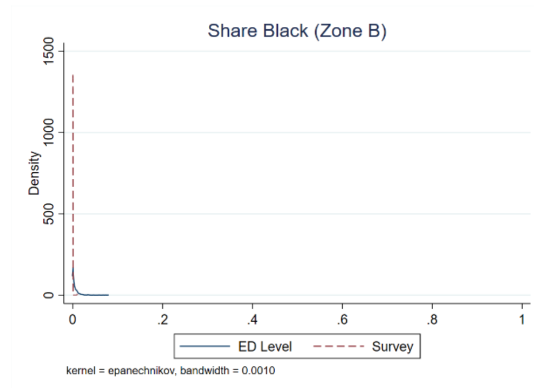

(d) Zone D

**Figure B.4: Survey verse Census Rents**

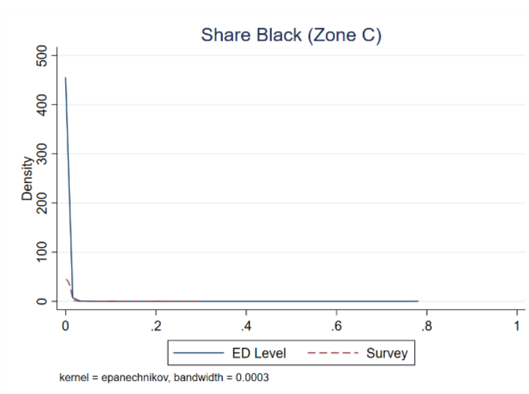
Notes: To analyze the accuracy of the surveys, we compare the kernel densities of survey measures and census measures of average rents. We use enumeration districts as our unit of observation for the census data and restrict our sample to only enumeration districts that lie completely within a residential security zone to prevent bias in our estimates.



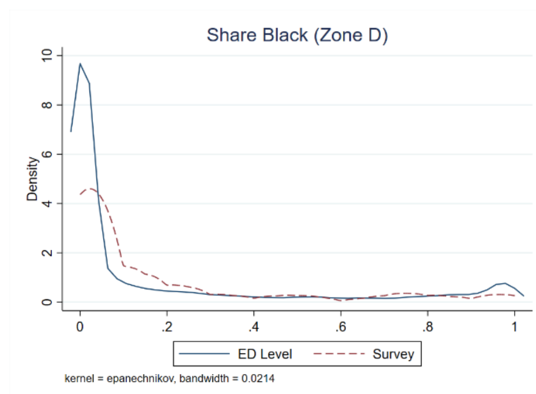

(a) Zone A




(b) Zone B



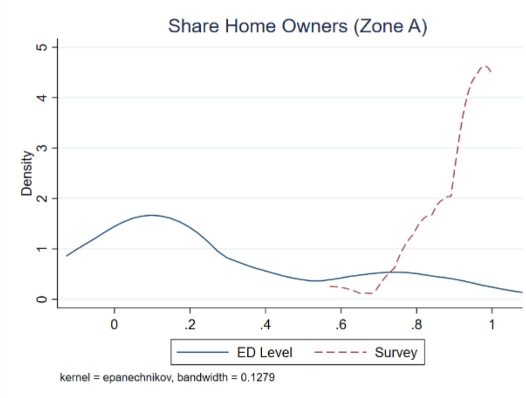

(c) Zone C



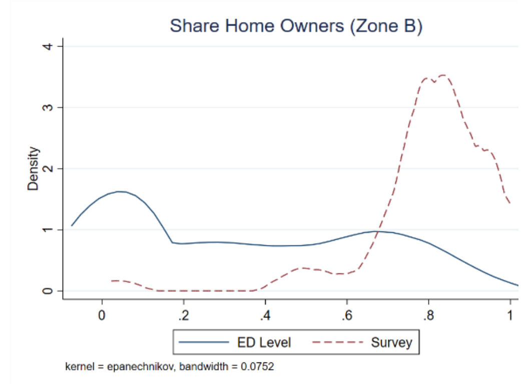

(d) Zone D

**Figure B.5: Survey verse Census Share Black**

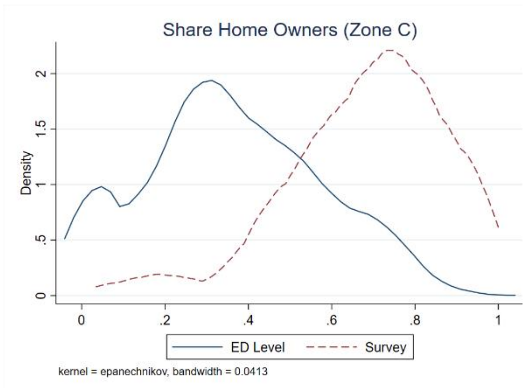
Notes: To analyze the accuracy of the surveys, we compare the kernel densities of survey and census measures of neighborhood racial composition. We use enumeration districts as our unit of observation for the census data and restrict our sample to only enumeration districts that lie completely within a residential security zone to prevent bias in our estimates.



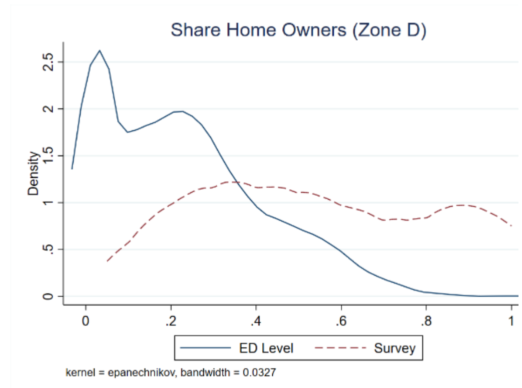

(a) Zone A




(b) Zone B



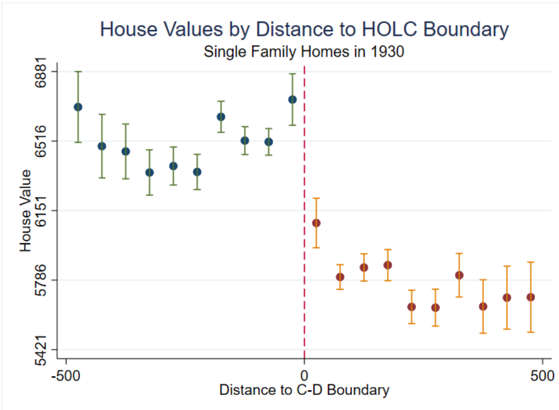

(c) Zone C



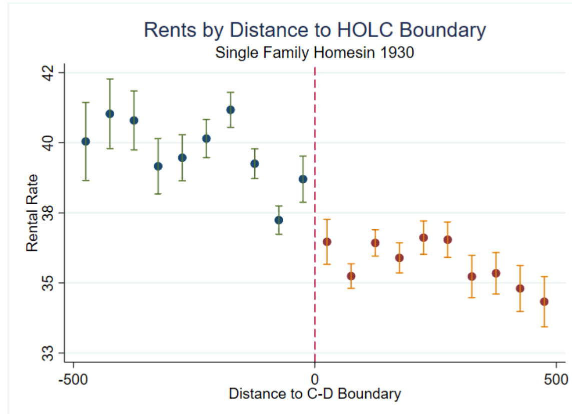

(d) Zone D

**Figure B.6: Survey verse Census Home Ownership Rates**

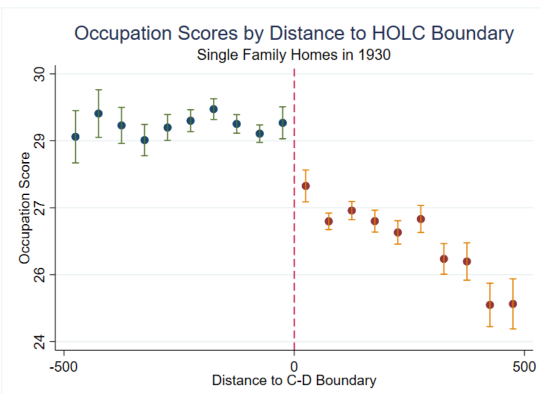
Notes: To analyze the accuracy of the surveys, we compare the kernel densities of survey and census measures of home ownership rates. We use enumeration districts as our unit of observation for the census data and restrict our sample to only enumeration districts that lie completely within a residential security zone to prevent bias in our estimates.



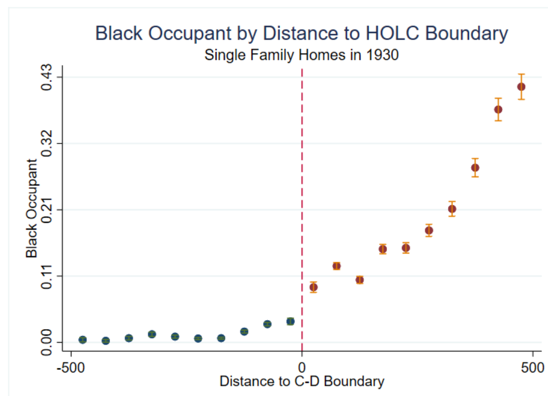

(a) House Values




(b) Rents



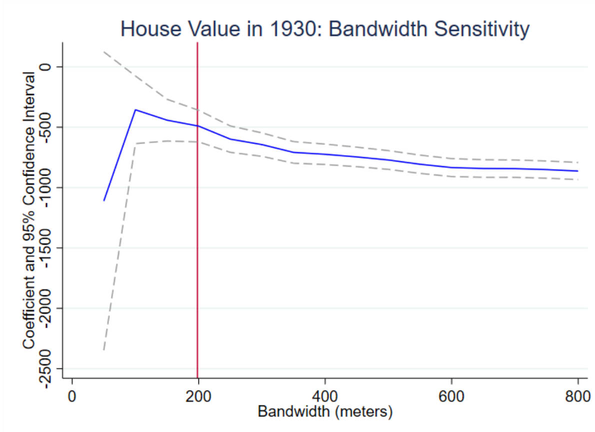

(c) Occupation Scores



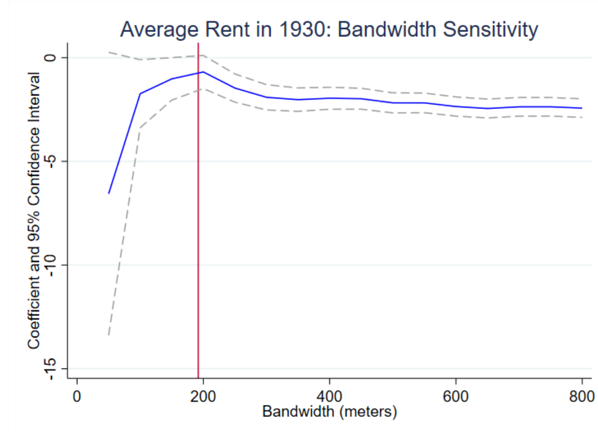

(d) Racial Composition

**Figure B.7: 1930 Levels by Distance to HOLC Boundary**

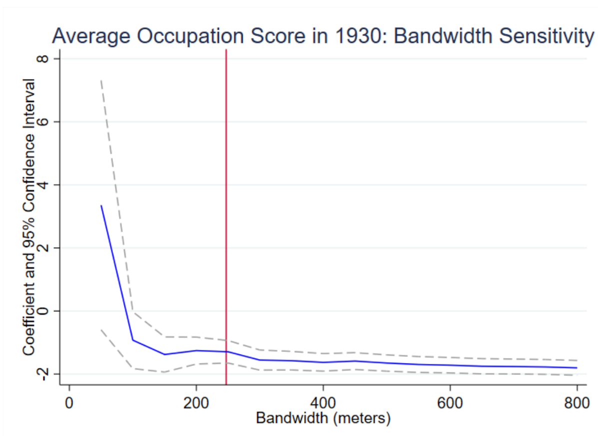
Notes: This figure shows binned averages of 1930 census data for single family households by distance to a C-D HOLC boundary. The red dotted line represents the HOLC boundary, positive distances represent households in the redlined zone, and negative distances represent houses in the yellow-lined zones. All distances are measured in meters.



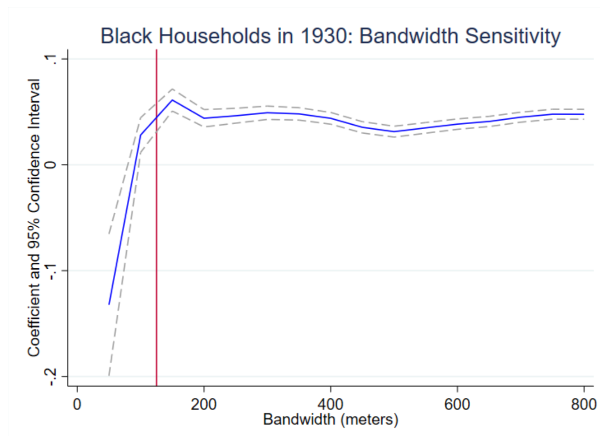

(a) House Values




(b) Rents



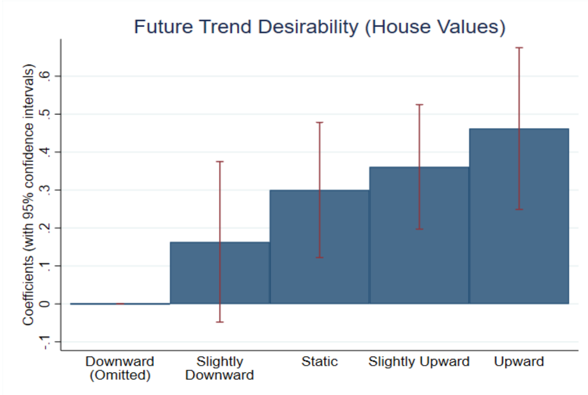

(c) Occupation Scores



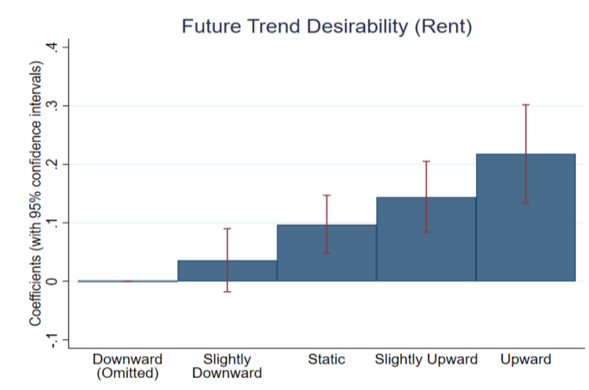

(d) Racial Composition

**Figure B.8: Optimal Bandwidth and Bandwidth Sensitivity for 1930 Levels**

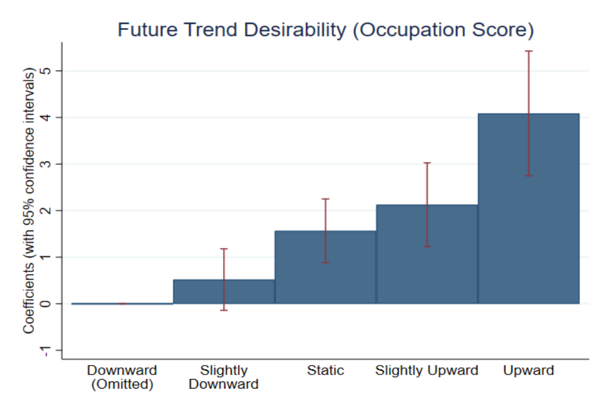
Notes: This figure graphs the estimated coefficients and 95% confidence intervals of β from equation (2.1) as we vary the bandwidth around a HOLC boundary. The red line represents the optimal bandwidth selection procedure proposed by Calonico, Cattaneo, and Titiunik (2014).



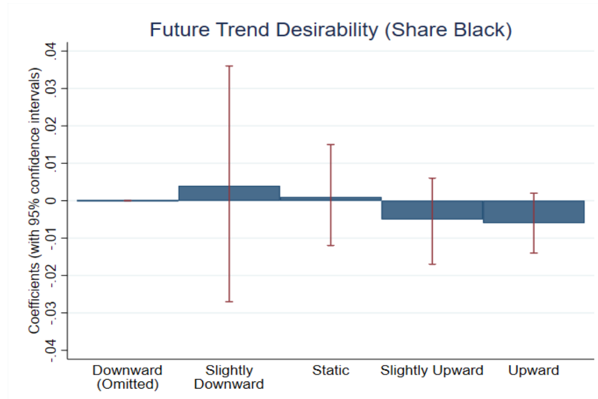

(a) House Values




(b) Rents




(c) Occupation Scores




(d) Racial Composition

**Figure B.9: Determinates of Future Trened Desirability Code**

Notes: This figure shows the coefficient and 95% confidence interval for the indicator variables related to the future trend desirability of a neighborhood with downward being the omitted category. Each regression controls for the 1930 value of the outcome variable. No zone fixed effects are included in this specification.

## Appendix B.2 Additional Tables

**Table B.1: Survey Summary Statistics**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Zone A | Zone B | Zone C | Zone D |
| | All | All | All | All |
| Family Income | 16760 | 6116 | 2969 | 1426 |
| | (20274) | (7526) | (5492) | (771) |
| House Value | 15566 | 10005 | 6787 | 3888 |
| | (10047) | (4805) | (8785) | (2312) |
| Rent | 76.78 | 60.94 | 39.74 | 24.58 |
| | (33.83) | (24.50) | (13.82) | (10.38) |
| Share Black | 0 | 0 | 0.004 | 0.168 |
| | (0.000) | (0.001) | (0.023) | (0.288) |
| Share Foreign | 0.02 | 0.07 | 0.24 | 0.44 |
| | (0.05) | (0.14) | (0.27) | (0.29) |
| Share of Houses Occupied | 98.47 | 97.35 | 97.38 | 95.59 |
| | (2.51) | (6.94) | (2.83) | (4.76) |
| Share Owner Occupied | 56.76 | 54.82 | 58.35 | 44.93 |
| | (47.11) | (40.86) | (32.24) | (35.17) |
| Construction Type | | | | |
| Brick | 0.873 | 0.714 | 0.489 | 0.395 |
| | (0.336) | (0.453) | (0.501) | (0.490) |
| Frame | 0.073 | 0.240 | 0.495 | 0.586 |
| | (0.262) | (0.428) | (0.501) | (0.494) |
| Other | 0.055 | 0.046 | 0.016 | 0.020 |
| | (0.229) | (0.209) | (0.126) | (0.139) |
| Repair | | | | |
| Excellent | 0.389 | 0.080 | 0.008 | 0 |
| | (0.492) | (0.273) | (0.091) | (0.000) |
| Good | 0.611 | 0.776 | 0.220 | 0.035 |
| | (0.492) | (0.418) | (0.415) | (0.184) |
| Fair | 0 | 0.138 | 0.747 | 0.490 |
| | (0.000) | (0.346) | (0.435) | (0.501) |
| Poor | 0 | 0.006 | 0.025 | 0.475 |
| | (0.000) | (0.076) | (0.156) | (0.500) |
| Future Trend Desirability | | | | |
| Upward | 0.419 | 0.186 | 0.030 | 0 |
| | (0.502) | (0.391) | (0.171) | (0.000) |
| Slightly Upward | 0.226 | 0.216 | 0.024 | 0.031 |
| | (0.425) | (0.414) | (0.154) | (0.173) |
| Static | 0.355 | 0.392 | 0.217 | 0.321 |
| | (0.486) | (0.491) | (0.413) | (0.469) |
| Slightly Downward | 0 | 0.093 | 0.114 | 0.084 |
| | (0.000) | (0.292) | (0.319) | (0.278) |
| Downward | 0 | 0.113 | 0.614 | 0.565 |
| | (0.000) | (0.319) | (0.488) | (0.498) |
| Observations | 57 | 187 | 378 | 268 |

Notes: This data was obtained from HOLC surveys.

**Table B.2: Census Data Summary Statistics (Household Level)**

| | 1930 | | | | 1940 | | | | % Change | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Zone A | Zone B | Zone C | Zone D | Zone A | Zone B | Zone C | Zone D | Zone A | Zone B | Zone C | Zone D |
| Household Size | 3.78 | 4.09 | 4.31 | 4.57 | 3.64 | 3.85 | 4 | 4.24 | -0.04 | -0.06 | -0.07 | -0.07 |
| | (1.54) | (1.70) | (1.85) | (2.08) | (1.46) | (1.60) | (1.73) | (1.96) | | | | |
| Family Size | 3.62 | 3.92 | 4.09 | 4.17 | 3.47 | 3.69 | 3.82 | 3.94 | -0.04 | -0.06 | -0.07 | -0.06 |
| | (1.49) | (1.67) | (1.82) | (2.06) | (1.40) | (1.57) | (1.72) | (1.94) | | | | |
| Occupation Score | 33.2 | 31.48 | 29.52 | 26.52 | 34.7 | 31.55 | 29.3 | 26.07 | 0.05 | 0.00 | -0.01 | -0.02 |
| | (11.12) | (10.04) | (9.41) | (9.38) | (12.52) | (10.57) | (9.63) | (8.92) | | | | |
| House Value | 9397 | 8227 | 7075 | 5377 | 7588 | 5706 | 4280 | 3071 | -0.19 | -0.31 | -0.40 | -0.43 |
| | (4057) | (3688) | (3368) | (2968) | (3888) | (2853) | (2191) | (1788) | | | | |
| Rent | 48.75 | 48.40 | 42.32 | 33.65 | 47.99 | 39.48 | 31.89 | 24.08 | -0.02 | -0.18 | -0.25 | -0.28 |
| | (19.80) | (18.68) | (17.09) | (15.03) | (21.19) | (16.60) | (13.24) | (10.44) | | | | |
| Share Black | 0.002 | 0.003 | 0.009 | 0.135 | 0.002 | 0.002 | 0.008 | 0.153 | 0.00 | -0.33 | -0.11 | 0.13 |
| | (0.046) | (0.051) | (0.093) | (0.342) | (0.041) | (0.046) | (0.089) | (0.360) | | | | |
| Share Foreign | 0.19 | 0.27 | 0.38 | 0.41 | 0.16 | 0.23 | 0.33 | 0.35 | -0.16 | -0.15 | -0.13 | -0.15 |
| | (0.39) | (0.44) | (0.49) | (0.49) | (0.37) | (0.42) | (0.47) | (0.48) | | | | |
| Sh Owner Occupied | 0.82 | 0.79 | 0.7 | 0.56 | 0.79 | 0.67 | 0.6 | 0.48 | -0.04 | -0.15 | -0.14 | -0.14 |
| | (0.39) | (0.41) | (0.46) | (0.50) | (0.41) | (0.47) | (0.49) | (0.50) | | | | |
| Observations | 9866 | 120232 | 289011 | 244049 | 17743 | 136744 | 280366 | 213587 | | | | |

Notes: This table includes all households from the 1930 and 1940 census.

**Table B.3: Future Trend Desirability Determination**

| | (1) Log Value | (2) Log Value | (3) Log Rent | (4) Log Rent | (5) Occupation Score | (6) Occupation Score | (7) Share Black | (8) Share Black |
|---|---|---|---|---|---|---|---|---|
| Downward (omitted) | - | - | - | - | - | - | - | - |
| Slightly Downward | 0.163 | 0.035 | 0.034 | 0.011 | 0.519 | 0.679** | 0.005 | 0.003 |
| | (0.108) | (0.105) | (0.028) | (0.031) | (0.336) | (0.342) | (0.016) | (0.017) |
| Static | 0.300*** | 0.163** | 0.097*** | 0.044 | 1.566*** | 1.641*** | 0.001 | 0.002 |
| | (0.091) | (0.081) | (0.025) | (0.028) | (0.347) | (0.352) | (0.006) | (0.008) |
| Slightly Upward | 0.361*** | 0.160 | 0.144*** | 0.056 | 2.127*** | 2.160*** | -0.005 | -0.005 |
| | (0.083) | (0.105) | (0.031) | (0.040) | (0.458) | (0.502) | (0.006) | (0.007) |
| Upward | 0.462*** | 0.305** | 0.218*** | 0.104** | 4.088*** | 3.777*** | -0.006 | -0.002 |
| | (0.108) | (0.125) | (0.043) | (0.047) | (0.680) | (0.686) | (0.004) | (0.006) |
| City Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Zone Fixed Effects | No | Yes | No | Yes | No | Yes | No | Yes |
| Observations | 425 | 425 | 425 | 425 | 425 | 425 | 425 | 425 |
| R-squared | 0.235 | 0.483 | 0.780 | 0.807 | 0.696 | 0.708 | 0.890 | 0.891 |

Notes: Notes: Robust standard errors are shown in parenthesis. *p < .10, **p < .05, ***p < .01. These results are presented graphically in figures (7) and (A9).

## Appendix C - Racial Disparities in Debt Collection

### Appendix C.1 Judgment Data

We obtained our judgment data from Paul Kiel and Annie Waldman at ProPublica. This data included all debt collection judgments in New Jersey, Missouri, and Cook County Illinois from 2008 to 2012.[75] Both Missouri and New Jersey have state-wide databases. The Missouri dataset was provided by Missouri's Office of the State Courts Administrator (OSCA) and included all debt collection cases filed in Associate Circuit Court for which OSCA has an electronic record through early 2014.[76] For each case in Missouri, the data contained the following information: court (judicial circuit), county, case ID, filing Date, case type, disposition, plaintiff, plaintiff attorney, defendant, defendant date of birth, defendant address, defendant attorney, judgment amount, date of judgment satisfaction, date of first garnishment attempt.[77] Kiel and Waldman added two fields: a standard name for each plaintiff and a plaintiff type. St. Louis County joined the state's online system in 2007 and St. Louis City has been online since 2000. Missouri's court system has some variation among the judicial circuits in how case types are categorized, so, in consultation with OSCA employees, Keil and Waldman selected a range of case types that could

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[75] In their ProPublica articles, Paul and Annie focus on Essex County, St. Louis City, St. Louis County, and Cook County due to the cities high segregation indexes. Due to a peculiarity of the court system database, the Essex County window is slightly different: July 1, 2007, through June 30, 2012. Furthermore, various circuits in Missouri came online at different times, but all circuits were online by 2008.

[76] The max amount sought in associate circuit courts in Missouri is $25K.

[77] The judgment amount was determined to be unreliable and is not used throughout this analysis.

be reasonably construed as debt collection cases. For St. Louis City and County courts, these were: Breach of Contract, Promissory Note, Suit on Account, Contract /Account (Bulk), Misc. Associate Civil-Other, Small Claims under \$100, Small Claims over \$100.[78] They limited the dataset to cases that had resulted in a judgment.

## Appendix C.2 BISG Algorithm

Vectors of six racial/ethnic probabilities for each listed surname (corrected for suppression and for low-frequency surnames) are used as the first input into the BISG algorithm. This information is used to calculate a prior probability of an individual's race/ethnicity. The algorithm updates these prior probabilities with geocoded ZCTA proportions for these groups from the 2010 Census SF1 files to generate posterior probabilities. Let J equal the number of names on the enhanced surname list plus one to account for names not on the list and let K equal the number of ZCTA in the 2010 census with any population. We define the prior probability of a person's race on the basis of surname, so that for a person with surname $j = 1, ..., J$ on the list, the prior probability for race, $i = 1,...,6$, is $p(i|j)$ which is equal to the proportion of all people with surname j who report being of race i in the enhanced surname file (the probability of a selected race given surname). This probability is updated on the basis of ZCTA residence. For ZCTA $k = 1,...,K$, $r(k|i)$ = proportion of all people in redistributed SF1 file who self-report being race i who reside in ZCTA k (the probability of a selected ZCTA of residence given race/ethnicity). Let $u(i, j, k) = p(i|j) * r(k|i)$. According to Bayes' Theorem and the assumption that the probability of residing in a given

[78] Together, the small claims and "misc. associate" cases comprised less than four percent of cases.

ZCTA given a person's race does not vary by surname, the updated (posterior) probability of being of race/ethnicity i given surname j and ZCTA of residence k can be calculated as follows:

$$q(i|j,k) = \frac{u(i,j,k)}{u(1,j,k) + u(2,j,k) + u(3,j,k) + u(4,j,k) + u(5,j,k) + u(6,j,k)}$$

Note that all parameters needed for BISG posterior probabilities are derived only from Census 2010 data, and that none are derived from administrative sources.

## Appendix C.3 Gradient Boosted Trees

Gradient Boosted Trees (GBT) is an ensemble learning approach that mitigates the tendency of tree-based models to overfit to training data. This is accomplished by recursively combining the forecasts of many over-simplified trees. The theory behind boosting proposes that a collection of weak learners as an ensemble create a single strong learner with improved stability over a single complex tree.

At each step m, $1 \leq m \leq M$, of gradient boosting, an estimator, $h_m$, is computed on the residuals from the previous models' predictions. A critical part of gradient boosting method is regularization by shrinkage as proposed by Friedman (2001). This consists in modifying the update rule as follows:

$$F_m(x) = F_{m-1}(x) + \nu\gamma_m h(x)_m$$

where $h_m(x)$ represents a weak learner of fixed depth, $\gamma_m$ is the step length and $\nu$ is the learning rate or shrinkage factor.

The estimation procedure begins with fitting a shallow tree (e.g., with depth L = 1). Using the prediction residuals from the first tree, you then fit a second tree with the same shallow depth

L. Weight the predictions of the second tree by $\nu \in (0,1)$ to prevent the model from overfitting the residuals, and then aggregate the forecasts of these two trees. At each step k, fit a shallow tree to the residuals from the model with k-1 trees, and add its prediction to the forecast of the ensemble with a shrinkage weight of $\nu$. Do this until a total of K trees is reached in the ensemble. For our GBT model, we split the data into three chunks: training set (60%), holdout set (20%), and testing set (20%). We relied on XGBoost for the implementation of our GBT model (Chen and Guestrin, 2016).

## Appendix C.4 Expanded Sample

For the interested reader, we relax the common support assumption and replicate Table 3.1, Table 3.2, Table 3.3, and Table 3.4 on the entire MO sample. Table C8, Table C9, Table C10, and Table C11 report the results. We find very similar results to our main specification which uses only the common support sample, suggesting that omitted variables that are correlated with observable neighborhood characteristics are not biasing our results in any particular direction.

## Appendix C.5 Additional Figures

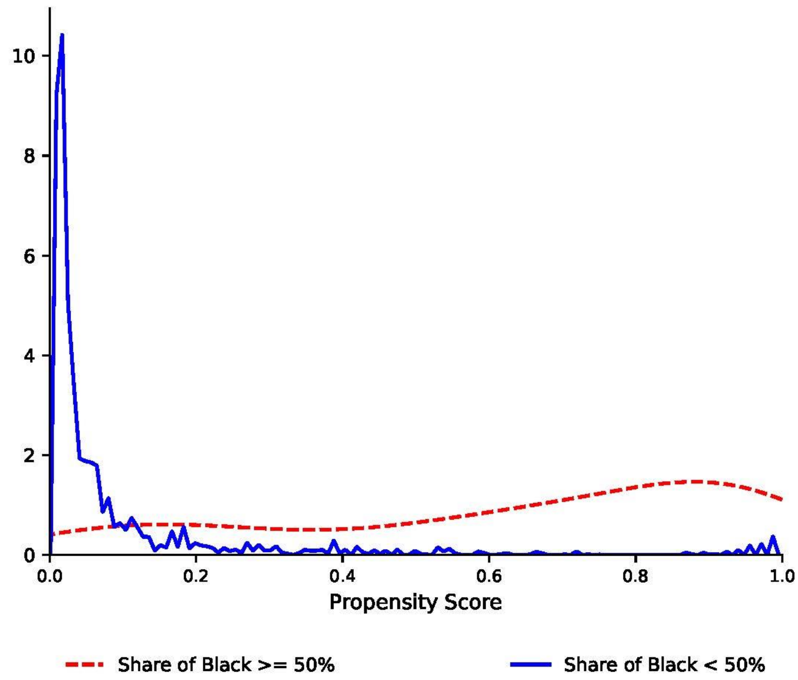


**Figure C.1: Propensity Score Distributions**

Notes: The common support for the propensity score distributions: [0.0013, 0.9750].

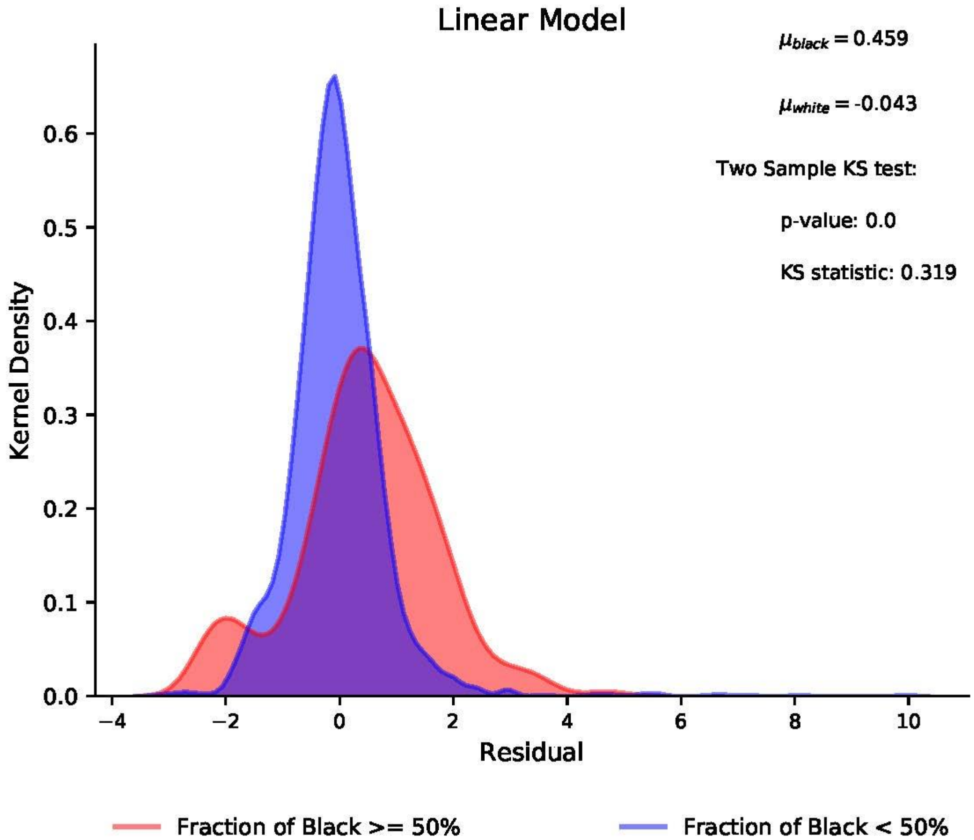


**Figure C.2: Residual Distributions by Race**

Notes: We fit a linear model using our baseline controls and compute corresponding fitted values. We then obtain and plot the residuals separately for black majority and non-black majority ZIP codes. We test whether the distribution of black majority ZIP codes is to the right of the non-black majority distribution and report the results of the KS-test in the upper right corner.

## Appendix C.6 Additional Tables

**Table C.1: Judgment Rates**

| | (1)<br>Judgment Rate | (2)<br>Dismissed | (3)<br>Settle |
|---|---|---|---|
| Black Majority: Zip | 0.017* | 0.007 | -0.017*** |
| | (0.009) | (0.011) | (0.003) |
| Median Household Income | -0.002 | -0.001 | 0.002** |
| | (0.001) | (0.001) | (0.001) |
| Median Credit Score | -0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) |
| Attorney | -0.192*** | 0.036 | 0.204*** |
| | (0.068) | (0.067) | (0.075) |
| County Fixed Effects | X | X | X |
| Year Fixed Effects | X | X | X |
| Baseline Controls | X | X | X |
| Observations | 2673 | 2673 | 2673 |
| R-squared | 0.490 | 0.737 | 0.686 |

Notes: Robust standard errors clustered at the county level are in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$. All regressions are weighted by population and estimated on the common support sample.

**Table C.2: Judgments and Alternative Credit Score**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Black Majority: ZIP | 1.3307*** | 0.8139*** | 0.8310*** | 0.8139*** | 0.6317*** | 0.7410*** |
| | (0.1444) | (0.1383) | (0.1317) | (0.1383) | (0.1511) | (0.0519) |
| Median Household Income | | -0.0040 | | -0.0040 | 0.0029 | |
| | | (0.0259) | | (0.0259) | (0.0203) | |
| Predicted Probability | | 1.6121** | 2.3538*** | 1.6121** | 1.1066*** | |
| | | (0.6432) | (0.5456) | (0.6432) | (0.3345) | |
| Median Credit Score | | | | | -0.0026 | |
| | | | | | (0.0039) | |
| County Fixed Effects | X | X | X | X | X | X |
| Year Fixed Effects | X | X | X | X | X | X |
| Baseline Controls | | | | | X | |
| Income Quintiles | | X | | X | X | |
| Lagged Baseline Controls | | | | | | X |
| Observations | 2204 | 2204 | 2204 | 2204 | 2204 | 1949 |
| R-squared | 0.6175 | 0.6821 | 0.6687 | 0.6821 | 0.7090 | 0.7707 |

Notes: Robust standard errors clustered at the county level are in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population and estimated on the common support sample.

**Table C.3: Summary Statistics (NJ and IL Sample)**

| | Black | White | t-test |
|---|---|---|---|
| Judgments per 100 people | 2.81 | 2.10 | -0.70*** |
| | (2.05) | (2.24) | |
| Median Credit Score | 605.52 | 644.63 | 39.10 |
| | (25.99) | (39.71) | |
| 90+ DPD Debt Balances | 7326.86 | 6330.69 | -996.18 |
| | (7004.39) | (11535.76) | |
| Median Household Income (000s) | 41.37 | 51.64 | 10.26*** |
| | (15.31) | (16.85) | |
| Gini Index | 0.46 | 0.44 | -0.01*** |
| | (0.05) | (0.06) | |
| Unemployment Rate | 0.12 | 0.09 | -0.03*** |
| | (0.02) | (0.03) | |
| Median House Value (000s) | 161.47 | 201.05 | 39.58*** |
| | (65.88) | (107.93) | |
| Fraction with Bachelor's Degree | 0.19 | 0.25 | 0.07*** |
| | (0.10) | (0.17) | |
| Fraction without High School Degree | 0.18 | 0.14 | -0.04*** |
| | (0.08) | (0.08) | |
| Observations | 224 | 596 | |

Notes: Summary statistics for observations on the common support sample. Data is drawn from NJ & IL.

**Table C.4: Judgments, Income, and Credit Scores (NJ and IL Data)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Black Majority: ZIP | 0.8525*** | 0.6745*** | 0.6049*** | 0.6338*** | 0.4700*** | 0.4453*** |
| | (0.1294) | (0.1269) | (0.1601) | (0.1532) | (0.1372) | (0.1436) |
| Median Household Income | | 0.0625* | | 0.0609 | 0.0331 | |
| | | (0.0370) | | (0.0397) | (0.0372) | |
| Median Credit Score | | | 0.0056 | 0.0086 | 0.0066 | |
| | | | (0.0072) | (0.0072) | (0.0068) | |
| County Fixed Effects | X | X | X | X | X | X |
| Year Fixed Effects | X | X | X | X | X | X |
| Baseline Controls | | | | | X | |
| Lagged Baseline Controls | | | | | | X |
| Observations | 820 | 820 | 820 | 820 | 820 | 579 |
| R-squared | 0.9268 | 0.9406 | 0.9329 | 0.9412 | 0.9472 | 0.9539 |

Notes: Robust standard errors clustered at the county level are in parentheses. ∗ $p < 0.10$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$. Observations on the common support sample from NJ and IL; estimated by a logistic regression, ran separately for each year. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population and estimated on the common support sample.

**Table C.5: Summary Statistics (Individual Judgment Data)**

| | Black | White | t-test |
|---|---|---|---|
| Judgments per 100 people | 0.14 | 0.17 | -0.07*** |
| | (0.47) | (0.36) | |
| Household Income (000s) | 60.28 | 83.75 | 23.46*** |
| | (41.12) | (57.67) | |
| Credit Score | 611.93 | 683.13 | 71.20*** |
| | (107.69) | (107.84) | |
| Observations | 399723 | 6722540 | |

Notes: Summary statistics for observations on the common support sample. Data is a 1% representative sample of the U.S. for individuals with a credit report.

**Table C.6: Judgments, Income, and Credit Scores (Individual Judgment Data)**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Black Majority: ZIP | 0.0689** | 0.0559*** | 0.0148* | 0.0158* | 0.0162* |
| | (0.0012) | (0.0006) | (0.0014) | (0.0015) | (0.0013) |
| Household Income | | -0.0006*** | | 0.0002*** | 0.0002*** |
| | | (0.0000) | | (0.0000) | (0.0000) |
| Credit Score | | | -0.0008*** | -0.0008*** | -0.0008*** |
| | | | (0.0000) | (0.0000) | (0.0000) |
| County Fixed Effects | X | X | X | X | X |
| Year Fixed Effects | X | X | X | X | X |
| Baseline Controls | | | | | X |
| Observations | 7122263 | 7122263 | 7122263 | 7122263 | 7122263 |
| R-squared | 0.0026 | 0.0102 | 0.0530 | 0.0541 | 0.0567 |

Notes: Robust standard errors clustered at the county level are in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$ Observations on the common support sample. Data is a 1% representative sample of the U.S. for individuals with a credit report. Dependent variable: current judgments.

**Table C.7: Judgments and Debt Portfolios (Individual Judgment Data)**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Black Majority: Zip | 0.0125* | 0.0136* | 0.0157* | 0.0158* | 0.0126* |
| | (0.0015) | (0.0020) | (0.0014) | (0.0014) | (0.0016) |
| County Fixed Effects | X | X | X | X | X |
| Year Fixed Effects | X | X | X | X | X |
| Baseline Controls | X | X | X | X | X |
| Debt Levels | Yes | | | | Yes |
| Monthly Payment and Utilization | | Yes | | | Yes |
| Debt Composition | | | Yes | | Yes |
| Delinquency/Bankruptcy/Collections | | | | Yes | Yes |
| Observations | 7122263 | 7122263 | 7122263 | 4122263 | 7122263 |
| R-squared | 0.0601 | 0.0580 | 0.0541 | 0.0629 | 0.0691 |

Notes: Robust standard errors clustered at the county level are in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$ Observations on the common support sample. Data is a 1% representative sample of the U.S. for individuals with a credit report. Dependent variable: current judgments.

**Table C.8: Summary Statistics (Full Sample)**

| | Black | White | t-test |
|---|---|---|---|
| **Panel A: Judgments** | | | |
| Judgments per 100 people | 2.76 | 1.25 | -1.51*** |
| | (1.33) | (0.82) | |
| Share of Default Judgments | 0.45 | 0.37 | -0.08*** |
| | (0.07) | (0.14) | |
| Share of Consent Judgments | 0.06 | 0.05 | -0.01* |
| | (0.04) | (0.08) | |
| Share w/ Attorney | 0.04 | 0.10 | 0.07*** |
| | (0.02) | (0.09) | |
| **Panel B: Credit Variables** | | | |
| Median Credit Score | 603.63 | 680.50 | 76.86*** |
| | (37.89) | (66.59) | |
| 90+ DPD Debt Balances | 3224.61 | 1430.29 | -1794.31*** |
| | (2632.57) | (4275.62) | |
| Banks (5 miles) | 87.91 | 13.74 | -74.17*** |
| | (39.65) | (33.24) | |
| Payday Lenders (5 miles) | 30.74 | 3.72 | -27.01*** |
| | (9.27) | (8.27) | |
| **Panel C: Census Data** | | | |
| Median Household Income (000s) | 31.41 | 45.91 | 14.50*** |
| | (11.73) | (17.10) | |
| Gini Index | 0.46 | 0.40 | -0.05*** |
| | (0.05) | (0.03) | |
| Unemployment Rate | 0.12 | 0.05 | -0.06*** |
| | (0.03) | (0.03) | |
| Divorce Rate | 0.13 | 0.12 | -0.01*** |
| | (0.02) | (0.05) | |
| Median House Value (000s) | 86.47 | 116.08 | 29.61*** |
| | (35.28) | (67.95) | |
| Fraction with Bachelor’s Degree | 0.16 | 0.18 | 0.01* |
| | (0.10) | (0.13) | |
| Fraction without High School Degree | 0.19 | 0.15 | -0.04*** |
| | (0.06) | (0.08) | |
| Observations | 248 | 7865 | |

Notes: Summary statistics for observations for the entire Missouri sample.

**Table C.9: Judgments, Income, and Credit Scores (Full Sample)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Black Majority: ZIP | 1.5063*** | 1.0203*** | 1.0202*** | 0.8521*** | 0.7632*** | 0.8334*** |
| | (0.1460) | (0.1088) | (0.1016) | (0.0757) | (0.1082) | (0.0324) |
| Median Household Income | | -0.0101 | | -0.0046 | -0.0012 | |
| | | (0.0091) | | (0.0069) | (0.0071) | |
| Median Credit Score | | | -0.0002 | -0.0005 | 0.0003 | |
| | | | (0.0018) | (0.0019) | (0.0020) | |
| County Fixed Effects | X | X | X | X | X | X |
| Year Fixed Effects | X | X | X | X | X | X |
| Baseline Controls | | | | | X | |
| Lagged Baseline Controls | | | | | | X |
| Observations | 8113 | 8113 | 8113 | 8113 | 8113 | 7197 |
| R-squared | 0.5884 | 0.6545 | 0.6367 | 0.6660 | 0.6761 | 0.6835 |

Notes: Robust standard errors clustered at the county level are in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$. Observations for the entire Missouri sample. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population.

**Table C.10: Judgments and Other Measures of Racial Composition (Full Sample)**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Share Black: ZIP | 1.6431*** | | | | | | |
| | (0.1037) | | | | | | |
| Majority Black: ZIP | | 0.7632*** | | | | | |
| | | (0.1082) | | | | | |
| Share Black: BISG | | | 1.5541*** | | | | 5.1912*** |
| | | | (0.1189) | | | | (1.3026) |
| Black Majority: BISG | | | | 0.7144*** | | 0.4766** | |
| | | | | (0.0950) | | (0.2131) | |
| Share Black: Names | | | | | 4.5415*** | | |
| | | | | | (0.5285) | | |
| County Fixed Effects | X | X | X | X | X | | |
| Zip Code Fixed Effects | | | | | | X | X |
| Year Fixed Effects | X | X | X | X | X | X | X |
| Baseline Controls | X | X | X | X | X | | |
| Income Quintiles | X | X | X | X | X | | |
| Credit Quintiles | X | X | X | X | X | X | X |
| Observations | 8113 | 8113 | 8106 | 8106 | 8106 | 8106 | 8106 |
| R-squared | 0.6926 | 0.6761 | 0.6951 | 0.6802 | 0.6805 | 0.7491 | 0.7530 |

Notes: Robust standard errors clustered at the county level are in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$. Observations for the entire Missouri sample. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population.

**Table C.11: Judgments and Other Demographic Groups (Full Sample)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Share Black: ZIP | 2.4034*** | 1.8654*** | 1.9266*** | 1.6624*** | 1.6533*** | 1.7849*** |
| | (0.1141) | (0.1480) | (0.0909) | (0.1015) | (0.1003) | (0.0545) |
| Share Asian: ZIP | -4.6405*** | -2.2187** | -3.9005*** | -2.1501** | -0.6058 | -0.6912* |
| | (0.9563) | (0.8761) | (1.0624) | (0.9451) | (0.7972) | (0.4098) |
| Share Hispanic: BISG | 1.5121*** | 0.2103 | 0.9982*** | 0.1223 | 0.2984 | 0.3032* |
| | (0.3017) | (0.2648) | (0.2462) | (0.2330) | (0.2363) | (0.1805) |
| Median Income | | -0.0038 | | -0.0008 | 0.0013 | |
| | | (0.0062) | | (0.0056) | (0.0056) | |
| Median Credit Score | | | 0.0007 | 0.0002 | 0.0010 | |
| | | | (0.0016) | (0.0017) | (0.0018) | |
| County Fixed Effects | X | X | X | X | X | X |
| Year Fixed Effects | X | X | X | X | X | X |
| Baseline Controls | | | | | X | |
| Income Quintiles | | X | | X | X | |
| Credit Quintiles | | | X | X | X | |
| Lagged Baseline Controls | | | | | | X |
| Observations | 8113 | 8113 | 8113 | 8113 | 8113 | 7197 |
| R-squared | 0.6577 | 0.6814 | 0.6717 | 0.6860 | 0.6927 | 0.7029 |

Notes: Robust standard errors clustered at the county level are in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$. Observations for the entire Missouri sample. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population.

**Table C.12: Model Comparison**

| Model | RMSE: All | RSME: Black | RSME: White |
|---|---|---|---|
| Linear Model | 0.8079 | 1.2704 | 0.780+ |
| Non-Linear Model | 0.6294 | 0.9556 | 0.6106 |

Notes: We split our sample into training and test dataset according to an 80-20 split. Then, we fit both of our models on the training dataset and using the parameter estimates obtained from the training dataset, we compute fitted values on the test dataset. Then we compute the residual and obtain the corresponding Root Mean Square Error (RMSE). We repeat this procedure 10,000 times with distinct train-test splits and the statistics reported are the averages obtained from this exercise.

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