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The long run effects of de jure discrimination in the credit market: How redlining increased crime

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ABSTRACT

Today in the United States the welfare costs of crime are disproportionately borne by individuals living in predominately African-American or Hispanic neighborhoods. This paper shows that redlining practices established in the wake of the Great Depression made lasting contributions to this inequity. First I use an unannounced population cutoff that determined which cities were redline mapped to show that redline mapping increased present-day city level crime. Secondly, I use a spatial regression discontinuity to show that redlining influenced the present-day neighborhood level distribution of crime in Los Angeles, California. I also identify channels though which redline mapping influenced crime including increasing racial segregation and decreasing educational attainment.

1. Introduction

Today in the United States the social costs of crime exceed 2 trillion dollars.[2] These welfare costs are not distributed evenly across racial and ethnic categories: nearly 60% of murder victims, for example, are either African-American or Hispanic.[3] These welfare costs are also unevenly distributed across neighborhoods. Predominantly African-American neighborhoods have 5 times as many violent crimes as predominantly Non-Hispanic White neighborhoods; predominantly Latino neighborhoods have about 2.5 times as many violent crimes as predominantly Non-Hispanic White neighborhoods (Peterson and Krivo, 2010).

Because variation in crime is associated with a vast number of factors including income, racial segregation (Chetty et al., 2014), school quality (Chetty et al., 2011) and pollution (Stretesky and Lynch, 2004), researchers face a significant challenge in trying to identify the causes of these inequalities in the distribution of crime. In this paper, I utilize discontinuities in credit access arising from Great Depression era federal housing policies which are colloquially called "redlining".

To stabilize housing markets in the 1930's, a newly formed federal agency, the Home Owner's Loan Corporation (HOLC), constructed maps of 239 US cities; these maps purported to grade neighborhoods in terms of lending risk, the riskiest neighborhoods being labeled in red and colloquially said to have been "redlined". Neighborhoods assigned low grades faced decades of reduced credit access relative to neighborhoods assigned higher grades (Jackson, 1987). Thus, redlining policy provides a context in which a researcher can identify the long run effects of restricting credit access to a neighborhood.

Beginning in 1936, HOLC surveyors and administrators classified neighborhoods on the basis of housing characteristics such as home value, home age, construction-type and rental values, as well as demographic characteristics such as the occupation of residents and, most controversially, the race and ethnicity of residents. In particular, HOLC surveyors were asked to detail whether or not it was expected that certain "inharmonious" or "subversive" racial and ethnic groups were present in a neighborhood. Accordingly, the term "redlining" has come to denote the practice of credit-market discrimination on the basis of neighborhood characteristics such as racial demographics, rather than individual loan-applicant credit-worthiness. Whether or not there still exist *de facto* forms of discrimination, the legal use of HOLC maps from roughly 1938 to 1968 created widespread de jure racially discriminatory practices in the credit market. This de jure discrimina-

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[2] United States. Senate Committee on the Judiciary. Hearing on The Costs of Crime. September 19, 2006 (cited in Barr and Smith (2017)).

[3] Author calculations from NIBRS 2010 Crime Victimization data.

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tion restricted credit access to neighborhoods which were given low grades for at least this 30 year period.

In this paper, I use discontinuities in credit access to show that redlining policies established in the wake of the Great Depression have increased crime up through the present-day. Using NIBRS data from 2000–2015 I find, in particular, that redlining is responsible for nearly doubling Black crime victimization. I also find evidence of increased Hispanic crime victimization and increased violent crime victimization across all racial and ethnic groups. Using Census data from across the twentieth century, I identify mechanisms through which redlining increased present-day crime, which include harming Black labor market outcomes as well as increasing racial segregation. While the nature of the variation does not allow me to demonstrate uniqueness of channel, the evidence in this paper suggests a labor market story: restricting credit access by race increased racial segregation, which harmed local educational attainment, which, in turn, influenced job market outcomes and altered the likelihood of criminal perpetration and victimization by race.

This paper contributes to the growing literature on redlining as well as to the larger literatures on the determinants of crime and the effects of credit access. Concerning redlining, this paper is the first to estimate the causal effect of redlining on crime. This paper is also the first to show that this effect on crime comes through increases in racial segregation and decreases in Black educational attainment. These results build on Aaronson et al. (2021), which uses within-city variation to show that redlining increased racial segregation, decreased home ownership, home values and credit scores. Because this paper produces both a city level and a neighborhood level estimate, it is also able to show that redline mapping may have *improved* crime outcomes in some neighborhoods, increasing overall city level crime in part by transferring would-be crimes from predominantly White neighborhoods into redlined neighborhoods.

There is a large literature on the long-run effects of credit access to both children and communities.[4] This literature provides evidence that if a neighborhood were cut off from the credit market for an extended period of time, this lack of investment would be associated with worsening neighborhood quality across many dimensions, which, in turn, would increase poverty and all its associated ills, so that these neighborhoods and their residents would likely enter into a poverty trap. Concerning this literature on the determinants of crime and the effects of credit access more broadly, this paper is the first to show the long-run, persistent effects of credit access on crime.

2. Background

2.1. Institutional history of redlining

Prior to the housing policies enacted in Franklin Roosevelt's "New Deal", homeownership was difficult for most middle class households. Home loans were neither amortized nor federally insured, and consequently most lenders offered home loans that were between 5 and 10 years in duration and required down payments of 30% or more (Jackson, 1987). Moreover, the terms of these loans needed to be renegotiated every five years, leaving would-be homeowners subject to fluctuating interest rates. In the midst of the Great Depression, the home ownership market contracted even further as financially strapped families lost their homes and vacancies increased (Rothstein, 2017). In an effort to stabilize the housing market, the Roosevelt administration created the Home Owner's Loan Corporation (HOLC) in 1933. HOLC bought up billions of dollars of mortgages which were on the brink of foreclosure and renegotiated 15 to 25 year mortgages with uniform, amortized loan schedules; nearly 40% of eligible Americans sought HOLC assistance (Jackson, 1987). In order to make such a large volume of loans, HOLC needed to gauge the riskiness of these new loan offers. Accordingly, HOLC hired local real estate agents to survey parts of a given city, dividing the city into neighborhoods and assigning to each of these neighborhoods a color-coded "security risk" grade. These HOLC neighborhoods were not based on preexisting Census designations such as Wards or Enumeration Districts and were drawn at the discretion of the agency.

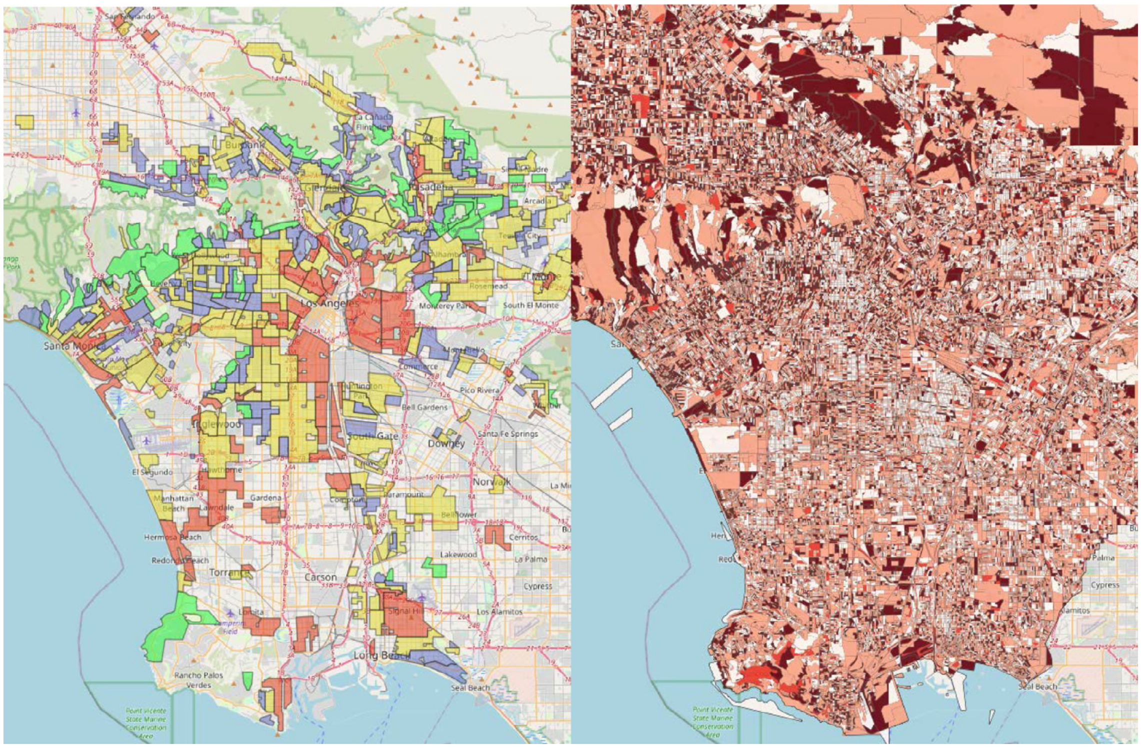
The resulting "Residential Security Maps" contained four ordinally ranked risk grades: A(green), B(blue), C(yellow), D(red) (e.g. Fig. 1). The highest ranked neighborhoods, graded "A" and colored in green, were described as "new, homogeneous", while the lowest ranked neighborhoods, graded "D" and colored red, were described as "hazardous" (Jackson, 1987). HOLC surveyors assigned quality categories and accordingly classified neighborhoods on the basis of housing characteristics such as home value, home age, construction-type and rental values as well as demographic characteristics such as the occupation of residents and, most controversially, the race and ethnicity of residents. In particular, HOLC surveyors were asked to detail whether or not it was expected that certain "inharmonious" or "subversive" groups were likely to move into the neighborhood.[5] Because surveyors recorded demographics, expected demographics and explicitly expressed preferences about which races and ethnicities were more or less advantageous to neighborhood quality and more or less risky to lenders, many observers and researchers have claimed that this practice and its associated maps not only reflected existing racial discrimination but further institutionalized this racial animus in the public and private credit market (Jackson, 1987). Thus the term "redlining" has come to denote the practice of credit-market discrimination on the basis of neighborhood characteristics such as racial demographics, rather than individual loan-applicant credit-worthiness.

Alongside these efforts to reduce foreclosures, the Roosevelt administration took further measures to increase homeownership by creating the Federal Housing Administration (FHA) in 1934. The FHA was tasked with insuring private home loans so long as they were amortized, had a long enough term and were deemed to be suitably low risk by the FHA. When a bank applied for FHA insurance on a prospective loan, the FHA hired an appraiser and guided the appraisal process by detailing procedures in its 1935 "*Underwriting Manual*". The "*Underwriting Manual*" instructed appraisers to rate loan risk partly based on current and expected racial composition of the surrounding neighborhood, since the presence or introduction of "adverse influences" such as "inharmonious racial or nationality groups" were likely to lead to "instability and a reduction in values" (Rothstein, 2017). While insuring loans incentivized lenders to make more loans and increased access to credit, it did so differentially by race, leading many observers to see in FHA loan-insurance practices explicit discrimination against African-Americans loan-seekers even conditional on applicant creditworthiness (Jackson, 1987; Rothstein, 2017).

Because the Department of Veteran Affairs later adopted FHA appraisal practices as it gave out millions of loans to veterans returning from World War II, these FHA practices affected a substantial volume of loans for decades. Because the HOLC security maps were very widely distributed to FHA appraisers and FHA appraisers were explicitly encouraged to use them, the pervasive and longstanding influence of FHA insurance practices were them-

[4] The literature is too large to do it full justice, but some studies that are particularly relevant include Lochner and Monge-Naranjo (2011); Lovenheim (2011), Brown et al. (2016); Krivo and Kaufman (2004).

[5] See the "Shifting or Infiltration" item in Fig. A1, as well as Table H1.



(a) HOLC grades (1939) (b) Crime Rates (2010)

Fig. 1. Residential security map, crime rates for Los Angeles. **Note:** Figure shows a georeferenced version of the Residential Security Maps constructed for Los Angeles by the Home Owners Loan Corporation (HOLC) in 1939 (panel a) compared to 2010 crime rates (panel b). HOLC assignments: areas colored green were considered the best and to bear the least risk; blue were considered next best, followed by yellow and finally red. Areas colored red were considered the most risky and least deserving of credit access and, accordingly are said to have been "redlined". Crime rates: crime rates are calculated at the block level as the total serious property and violent crimes divided by the residential population. Panel b shows a heat map of crime-rate quartiles. The darkest areas have the highest crime rates, while the lightest have the lowest.

selves influenced by the decisions of HOLC surveyors as they delineated and graded neighborhoods (Jackson, 1987). In short, even though HOLC did influence neighborhood credit access through its own loan granting practices, its long-run influence is due to its influence on other institutions. In particular, HOLC and its Residential Security Maps influenced loan access in two ways: (1) by influencing private lenders[6] and (2) influencing FHA loan-insurance appraisals. Through these two channels HOLC maps influenced credit access in hundreds of US cities for decades.

The use of these maps and loan practices have since been made illegal, first in the 1968 Fair Housing Act (FHA), but later in the 1974 Equal Credit Opportunity Act (ECOA) as well as later revisions to the FHA such as the Fair Housing Amendments Act of 1988, which strengthened penalties for discriminatory lending practices (Fig. 2). Nevertheless, there exists an active debate about whether or not such discriminatory practices still take place.[7] Whether or not there still exist *de facto* forms of discrimination, the legal use of HOLC maps from 1938 to 1968 created widespread de jure racially discriminatory practices in the credit market. This de jure discrimination restricted credit access to neighborhoods which were given low grades for at least the duration of this 30 year period.

2.2. Existing evidence on redlining and crime

While there is a large, interdisciplinary body of work exploring how housing policy in the 1930's may have shaped present-day neighborhood characteristics, the literature has not yet identified the effects of redlining on crime nor has it used used this massive federal policy to address questions about the the determinants of crime and the effects of credit access more broadly.

Jackson's seminal book, "*Crabgrass Frontier*" chronicles the activities of the HOLC and FHA in relation to several broader narratives he weaves together which include urbanization, suburbanization and the racially motivated history of United States housing policy. More recently, Appel and Nickerson (2016) uses a spatial regression discontinuity design to show that homes just across the border of a lower HOLC security grade have less value in 1990. To establish the identification assumption that home values did not exhibit jumps prior to the policy, the paper uses home value data from 1940, which is soon after the maps were constructed.

Most recently, Aaronson et al. (2021) engages in a groundbreaking and ambitious project to chart the effects of redlining maps in over one hundred US cities across decades. Using a variety of empirical approaches including the construction of counterfactual boundaries that experienced the same pre-existing trends, they identify the causal effect of the HOLC maps on the racial composition and housing development of urban neighborhoods. In particular, Aaronson et al. (2021) shows that being on the lower graded side of D-C (red-yellow) boundaries increased racial segregation from 1930 until about 1970 or 1980 before starting to decline thereafter, even though some gaps persist until 2010. They find that the effects on homeownership rates and house values dissipate over time along the D-C (red-yellow) boundary, but persist along the C-B (yellow-blue) boundaries. This work is the first to explore and identify causal effects for a vast number of outcomes across over one hundred cities over three quarters of a century. Their work is also the first to highlight the importance of the C-B (yellow-blue) boundary and identify the long-run effects of "yellow-lining".

6 Aaronson et al. (2021) offer an extended discussion of the debate in the literature concerning how widely the HOLC maps were used.

7 See Reibel (2000) and references. Aaronson et al. (2021) points out that even today there are lawsuits which allege this sort of discrimination.

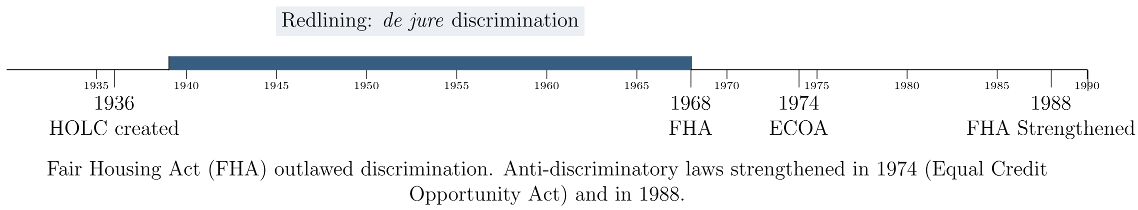


Fig. 2. Timeline of *de jure* discrimination implemented by redlining. **Note:** The figure shows the period during which it was legal to discriminate (“*de jure* discrimination”) in the loan market based on neighborhood demographics rather than loan-applicant creditworthiness.

There still remain important gaps in the literature on redlining. I complement the existing literature by identifying the impact of redlining on crime. Furthermore, I add to the literature by focusing on a population cutoff that determined whether a city would be redline mapped[8] to produce a an estimate of the causal impact of redline mapping on crime, racial segregation and educational attainment. Given the findings in Aaronson et al. (2021), we would expect racial segregation to be one of the main mechanisms through which redlining influenced the formation of cities, which is indeed what I find.

Concerning the literature on the determinants of crime and the effects of credit access more broadly, this paper is the first to show the long-run, persistent effects of credit access on crime. Previous studies have identified effects of childhood exposure to credit access on adulthood credit scores (Brown et al., 2016), and the local effects of reduced local competition between banks on property crime (Garmaise and Moskowitz, 2006). Furthermore, there exists evidence that racial segregation is responsible for lower educational attainment for Blacks (Ananat, 2007; Billings et al., 2013) and that lower educational attainment is responsible for increased crime (Lochner and Moretti, 2004).

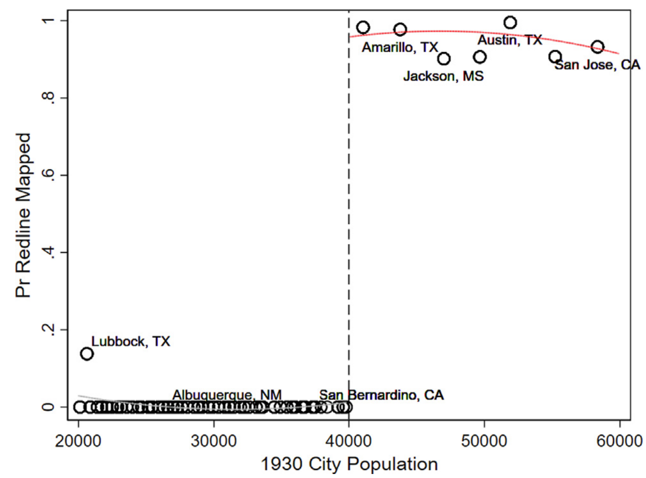


Fig. 3. 1930 population and redline mapping: between-city first stage. **Note:** The figure shows a regression discontinuity diagram where the outcome variable is the likelihood that HOLC constructed a Residential Security Map (“Pr Redline-Mapped”) for a given city in the 1930’s. Observations are at the city level. The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. There are 315 cities in the 1930 Census with population between 20,000 and 60,000, 231 of which have population between 20,000 and 40,000. Bandwidth size is chosen to be 20,000 people. Bin numbers are chosen optimally following Calonico (2017). Data sources are the 1930 Census and Home Owner Loan Corporation (HOLC) archival records.

3. Data

3.1. Administrative data

To analyze the determinants of redline mapping assignments at both the city level and the neighborhood level, I compile two novel datasets from HOLC Administrative documents. For the city level study, I use archival HOLC data to determine whether HOLC constructed a redlining map for a given city.[9] As I discuss in detail below (Section 4), an analysis of this dataset reveals an unannounced population cutoff that determined whether or not a city were redline mapped (Fig. 3). For the neighborhood level study, I generated a geocoded dataset of HOLC security grades and their purported determinants. To create this dataset, I obtained all 416 surveyor “area description” documents for Los Angeles,[10] coded the information contained in each document and assigned the resulting data to a georeferenced HOLC map of Los Angeles (Fig. A1).

3.2. Census data

For the city level study, I take city level HOLC administrave data and introduce decennial Census data from 1890 to 2010 (Ruggles et al., 2019). For the neighborhood level study, I digitize neighborhood level HOLC administrative data and geocode the 1920 and 1930 address level Census micro data. I observe housing variables such as self-reported home-value and rental amounts as well as demographic information such as the race and ethnicity of residents.[11]

3.3. Crime data

For the city level study, I use individual level National Incident Based Reporting System (NIBRS) crime victimization[12] data, restrict to UCR classified Part 1 property and violent crimes and collapse by reporting agency, assigning each agency to the city which it polices. I also use agency-month level FBI Uniform Crime Reports (UCR) from Kaplan (2018), which I collapse to obtain city-year level arrest rates.[13] Summary statistics are reported in Tables A1 and A2.

For the neighborhood level study, I use geocoded crime data from the city of Los Angeles. These data contain exact location of

[8] Aaronson et al. (2021) also utilizes the population cutoff to perform a supplemental analysis, to compare with their main within-city estimates. Hillier (2005) originally discovered archival materials that document this 40,000 population cutoff. I independently discovered the cutoff in the raw data.

[9] These documents reside in National Archive Group 31.

[10] T-RACES (2019) publishes HOLC administrative documents for many cities in California.

[11] I do not observe migration or education attainment, since these were not introduced to the Census surveys until 1940.

[12] Victimizations measure only those crimes reported to the police. I use the term “victimization” rather than “incident” or “arrest”, since I use victimization measures (V4000 variables) in National Archive of Criminal Justice Data (2018). The codebook explains that while each offense always has at least one victim, there are crime incidents in which victim demographics such as race are non-missing even though offender demographics are missing.

[13] Following the codebook in Kaplan (2018) I use the term “arrest” to describe these crime measures.

the crime as well as a description of the crime for the universe of crimes in Los Angeles in 2010.[14] I then use string searches over crime descriptions to classify crimes as UCR Part 1 property or violent crimes.[15] Summary statistics are reported in Table A3. Fig. A2 presents a Gini coefficient diagram which displays how crime in Los Angeles in 2010 is distributed across the neighborhoods HOLC delineated in 1939. The most dangerous 10 percent of neighborhoods bear 80 percent of the total crime burden (Fig. A2).

4. City-level effects of redline mapping on crime

In this section, I utilize an unannounced population cutoff which determined whether or not a city was redline mapped to show that redline mapping increased city level crime. In Section 5, I use this same between-city variation to identify mechanisms for how redline mapping increased crime which include increasing racial segregation and decreasing educational attainment.

4.1. Which cities were mapped and why?

HOLC residential security maps were made for 239 US cities including most modern, major metropolitan areas. Despite the broad coverage of the maps, hundreds of cities and smaller towns were never mapped. As Fig. 3 shows, having a 1930 population above 40,000 nearly guaranteed that a city would be mapped, while having a population below 40,000 nearly guaranteed that a city would not be mapped. While smaller in population than the largest and most often studied metropolitan areas, cities within a reasonable bandwidth about the 1930 population cutoff of 40,000 are still home to significant numbers of US residents. In 1930 approximately one third of the US population lived in cities with 50,000 or less people. In California, cities whose 1930 population was near the cutoff include Stockton, Fresno and San Jose (which were redline mapped) as well as Santa Barbara, Santa Monica and San Bernardino (which were not redline mapped); in Texas, representative cities include Austin, Galveston and Waco (which were redline mapped) as well as Lubbock, Laredo and Corpus Christi (which were not redline mapped). Table A4 contains a list of cities in the bandwidth, and Fig. A3 shows the regional breakdowns of cities near the threshold. Midwestern and Northeastern cities are slightly overrepresented, but there are cities from each region in the main bandwidths.[16] The between-city estimates are local to these largely mid-sized cities, and external validity concerns arise if we apply them to much larger cities. (See Section 6.5.).

4.2. Estimation

I use a regression discontinuity design to identify the city level impact of being redline mapped on crime. I estimate regressions of the form:

$$Crime_c = \tau Above_c + \beta f(Pop30_c) + \gamma Above_c \times f(Pop30_c) + \epsilon_c. \qquad (1)$$

where $Crime_c$ is the crime rate in city c, $Pop30$ is the 1930 population of city c. This regression uses 1930 city population as the running variable variable and fits a local linear polynomial on either side of the mapping population cutoff of 40,000 people. I am primarily interested in τ, the coefficient on *Above*, an indicator variable which equals one when city's population is above the population mapping cutoff (40,000 people) and zero otherwise; τ, the coefficient on *Above*, measures the average jump that occurs at the population cutoff conditional on the local linear polynomials.

4.3. Pre-period balancing

Ex ante it seems unlikely that cities with slightly more than 40,000 people and those with slightly less than 40,000 would exhibit pre-existing jumps across this threshold for any covariate associated with crime, however, to be cautious, I implement a density manipulation test, and perform a series of balancing tests. I implement a density manipulation test to test whether the crime results are driven by the policing agency's decision to report their data to the NIBRS database. Fig. A4 shows that the density of agencies reporting to NIBRS is smooth across the population threshold. Moreover, Fig. A5 shows that the results pass a standard manipulation test Additional tests are discussed in Appendix B. I conclude that the results are not driven by the agency level decision to report to the NIBRS database.

To implement balancing tests, I use 1920–1930 Census data to show that observable city level covariates are smooth across the threshold. I focus my balancing tests on the percent of households that are Black, the percent that are Hispanic, as well as self-reported home values and rental values. I am most concerned about these covariates because any pre-period discontinuity in racial composition or socio-economic status would lead us to question whether that pre-existing racial or socio-economic difference were the common cause of both the choice of the population-cutoff and present-day crime volume. Fig. A6 shows RD diagrams for these four covariates. We can see that they do not exhibit significant jumps about the population cutoff. Fig. A7 shows RD estimates for these covariates across a range of bandwidths. Across a wide range of bandwidths, none of these covariates exhibit evidence of a pre-existing jump.

To the best of my knowledge there does not exist crime data from 1930 or earlier which covers large numbers of cities.[17] Thus, I cannot directly test for discontinuities in crime in the pre-period.[18] Nevertheless, the covariate tests (Figs. A6 and A7) show that *if* there were some unobserved factor influencing present-day crime in cities just above the mapping threshold, this factor would have to be correlated with present-day crime and yet uncorrelated with the racial and socio-economic measures observed prior to mapping. Even if pre-period crime correlates with present-day crime, it is

[14] The lacity.org website notes two limitations to its data: (1) "This data is transcribed from original crime reports that are typed on paper and therefore there may be some inaccuracies within the data", and (2) "Address fields are only provided to the nearest hundred block in order to maintain privacy." Thus, (1) the LA crime data contains some measurement error and (2) using the address field to locate, say, the residence of a burglary would introduce additional imprecision. I do not use the address variable. But the latitude/longitude field that I use to locate crimes, likely also contains measurement error at least because of (1). The RD estimates would still hold if the measurement error did not systematically vary across spatial cutoffs.

[15] Property crimes include burglary and motor vehicle theft, while violent crimes include murder, robbery as well as physical and sexual assault.

[16] Conditional on 1930 city population, census region does not predict whether or not a city was redline mapped.

[17] UCR historical data, which predates 1960, contains at most 400 agencies, all of which lie in very large metropolitan areas.

[18] Despite this data limitation, in Fig. A15, I use the group quarters variable from the 1930 Census to test for city level pre-period differences in the share residing in institutional group quarters. Because this variable measures not only individuals who are incarcerated, but also many non-incarcerated individuals (see note to Fig. A15) it does not constitute an ideal variable to measure pre-period criminal activity. Nevertheless, if more individuals in cities just above the population threshold are incarcerated in the pre-period, and the institutional group quarters variable measures this difference across the threshold, this could be an indication of higher pre-existing crime rates in the cities just above the threshold if we believe that the percent incarcerated is an increasing function of criminal perpetration. However, the results in Fig. A15 show that, if anything, mapped cities had a smaller share of incarcerated individuals and incarcerated black individuals compared to non mapped cities. Both because there is considerable variation in these bins and the group quarters variable in 1930 measures certain non-incarcerated individuals together with the incarcerated, these estimates should be treated with caution.

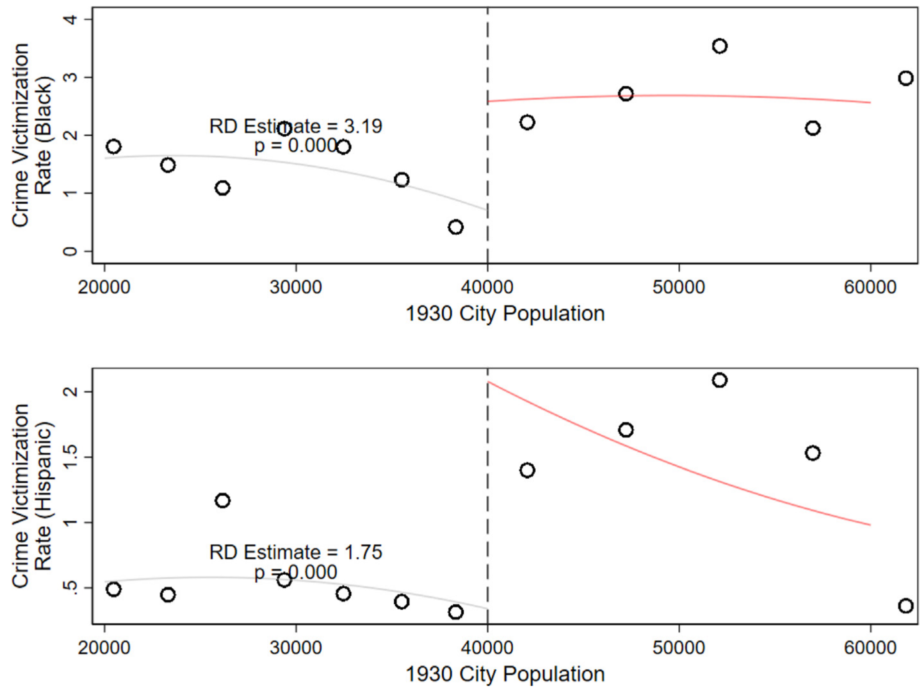


Fig. 4. Impact of redline mapping on crime: between-city estimates. **Note:** The figure shows a regression discontinuity diagram where the outcome variable is the rate of Black or Hispanic crime victimization in a given city per 1,000 persons policed. The estimates are drawn from 2010–2015 pooled NIBRS data. Observations are at the agency level. The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following Calonico (2017). Data sources are individual level NIBRS crime victimization data and Home Owner Loan Corporation (HOLC) archival records.

also likely to be correlated with pre-period racial and socioeconomic measures.

4.4. Crime effects

Because of the racialized nature of redlining, I focus on Black and Hispanic crime victimization. (See Appendix H for details.) Fig. 4 shows evidence that HOLC mapping increased present-day city level crime victimizations. In particular, Fig. 4 uses pooled from 2010–2015 NIBRS data to show that Black crime victimizations double across the mapping threshold, increasing by approximately 3 victimizations per 1,000 persons. Fig. D1 shows estimated increases in victimizations annually from 2000–2015, while Fig. A8 and Table 1 show the regression discontinuity diagram for 2015 data. In 2015, Black crime victimizations double across the mapping threshold, increasing by nearly 2 victimizations per 1,000 persons. The estimates reported in Fig. A8 (and listed in Table 1) imply that redline mapping added 176 Black crime victimizations to a city in 2015. The estimates for Hispanic crime victimization in 2015 are smaller than those for Black crime victimization and less precisely estimated, suggesting that Hispanic crime victimizations increase by 70% or 65 Hispanic crime victimizations per city in 2015 (Fig. A10). Fig. A11 shows estimates for Black crime victimization broken down by crime type, and reveals that the crime increases are larger for property crime than for violent crime. I also find evidence of increased violent crime victimization across all racial and ethnic groups. (See Appendix F for further discussion.) In the main figures and tables, bandwidth and bin number are chosen optimally using (Calonico, 2017). Fig. A12 shows that these results are robust to the choice of more than the optimal number of bins. Lastly, Fig. A13 shows that these estimates are robust across a wide array of bandwidths.

Results using UCR crime data are comparable. (See Fig. A14 and the Appendix "Comparing NIBRS and UCR Results" for details.) Aside from providing a useful comparison to measures in the NIBRS dataset, the main reason to utilize the UCR measures of crime is that the UCR dataset spans many decades and allows me to understand long-run dynamics. Estimating Eq. (1) using UCR decadal data, Fig. A9 shows the dynamics of the impact of redline mapping on crime over the entire period after the Fair Housing Act.[19] The decadal estimates reported in Fig. A9 show that the effects of redline mapping peaked in the period around the passage of major anti-discriminatory laws (such as the Fair Housing Act) and, while

Table 1
Impact of redline mapping on crime: between-city estimates, by crime-type, race.

	(1) All Crime	(2) Property Crime	(3) Violent Crime
Panel A: Black Crime Victimization			
Impact of Redline Mapping	1.92**	2.33***	0.41
	(0.92)	(0.73)	(0.28)
Observations	966	966	966
Mean (Bandwidth)	1.56	1.24	.35
Mean (Non-Mapped)	1.21	.93	.27
Bandwidth (1930 Population)	7,977	8,209	7,202
Panel B: Hispanic Crime Victimization			
Impact of Redline Mapping	0.72	1.22	0.21
	(1.29)	(0.87)	(0.23)
Observations	966	966	966
Mean (Bandwidth)	.90	.55	.20
Mean (Non-Mapped)	.41	.33	.07
Bandwidth (1930 Population)	11,070	9,888	16,633

Note: Table shows regression discontinuity estimates of the impact of redline mapping on crime with standard errors reported in parentheses. Observations are at the agency level. The outcome variable is the rate of crime victimizations in a given city in 2015. Reported means are city level counts of crime victimizations in 2015. The running variable is always 1930 city population. Bandwidth size is chosen optimally following Calonico (2017). Data sources are NIBRS Crime Victimization Data (2015) and HOLC archival documents.

[19] See Fig. 2 for the timing of major anti-discriminatory laws such as the Fair Housing Act.

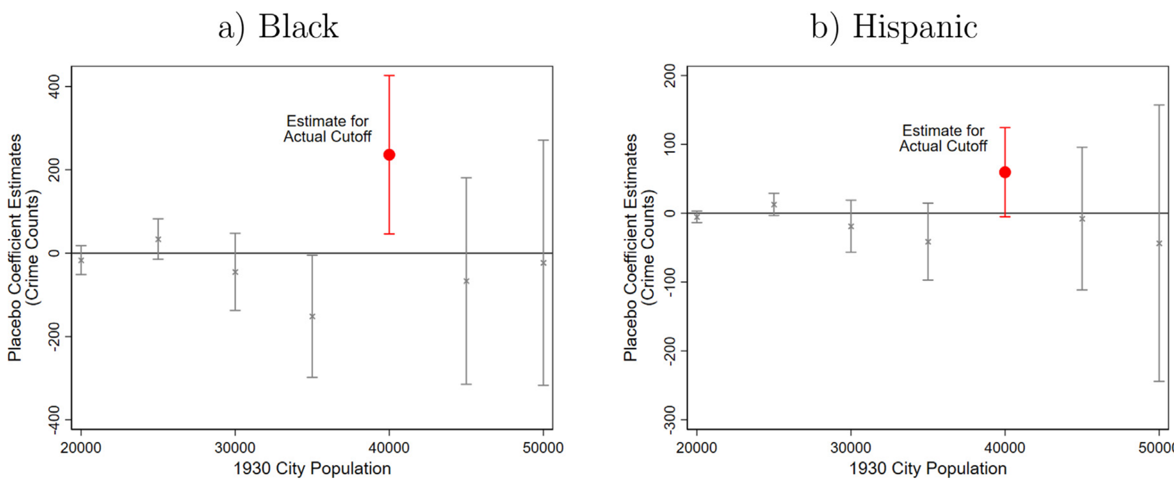


Fig. 5. Impact of redline mapping on crime: placbo tests. **Note:** Each figure shows a series of placebo tests which report estimates of Eq. (1) at simulated or "placebo" cutoff values. The left panel presents these results for 2015 Black crime victimization, while the left panel presents these results for 2015 Hispanic crime victimization. Estimates at the actual cutoff (40,000) imply that redline mapping added 176 Black crime victimizations and 65 Hispanic crime victimizations.

having decreased in subsequent decades, the effects nevertheless persist into the present-day. If legislation such as the Fair Housing Act did mitigate the effects on crime, it is not ex ante clear *exactly* when the estimates ought to decline since city level crime volume is likely to persist even after the enactment of anti-discriminatory legislation. Nevertheless, it is clear from Fig. A9 that the increases in crime attributable to redline mapping decrease in the period after the Fair Housing Act and related legislation, when the use of redlining maps became illegal.

These longstanding impacts are large in magnitude. Restricting to cities within the bandwidth, I use UCR data in a back of the envelope calculation and compare the overall rise in the black crime rate from the 1980s to the peak in the 1990s with the magnitude of the increase attributable to redlining as measured in the 1980s and 1990s (Fig. A9). I find that that the magnitude of the redlining estimate is 74% as large as the overall rise in crime from the 1980s to the 1990s.

4.5. Placebo tests

Fig. 5 shows the results from a series of placebo tests for Black and Hispanic crime victimization in panels (a) and (b) separately. These placebo tests introduce false population threshold cutoffs and test whether there are any "effects" on crime at these false cutoffs. I consider populations cutoffs in 1930 city population at every 5,000 people, starting at 20,000 and advancing through 50,000. Both for Black and Hispanic crime victimization, I find positive and statistically significant estimates at the actual cutoff, but not at the simulated, placebo cutoffs.

5. Mechanisms: how did redlining increase crime?

In this section, I discuss evidence that redline mapping increased crime through influencing racial segregation and educational attainment. In Appendices E-G I consider the channels of housing (E), migration (F), and police officer expenditures (G), and find less evidence in their favor.

5.1. Racial segregation as a mechanism

There exists evidence that present-day racial segregation is correlated with reduced intergenerational mobility (Chetty et al., 2014), is associated with increases in the Black-White SAT test score gap (Card and Rothstein, 2007), and that racial segregation is causally responsible for lower income and educational attainment for Blacks as well as increased crime (Ananat, 2007; Billings et al., 2013).[20] Thus, one way redline mapping may have increased crime is by increasing racial segregation. Indeed, Aaronson et al. (2021) uses within-city variation in HOLC mapping assignments to show that redlining increased racial segregation.

To empirically test the hypothesis that racial segregation is a channel through which redline mapping increased crime, I consider racial segregation as an outcome variable in Eq. (1). I measure racial segregation using the White-Black Dissimilarity Index (a standard measure of racial segregation in cities).[21] I pool decadal measures of racial segregation from 1890–2010 based on whether they were (a) in the period prior to any redline mapping (1890–1930), (b) in the period after redline mapping was first implemented (1940–2010) or (c) in the period after both redline mapping and the Fair Housing Act (1970–2010).[22] Fig. A18, subfigure (a), presents a placebo test which finds that there is not a significant difference in Black-White racial segregation across the population threshold prior to redline mapping. Fig. A18, subfigure (b), presents a regression discontinuity diagram that uses pooled city-decade level data from the entire period after redline mapping was implemented (1940–2010); the reported estimate suggests that redline mapping is responsible for an increase in racial segregation of 11.4 dissimilarity points (a 24% increase off the mean).

Fig. A18, subfigure (c), presents a regression discontinuity diagram that uses pooled city-decade level data from the period after both redline mapping was implemented and the Fair Housing Act was passed (1970–2010). During this period (1970–2010), even though there was no *de jure* discrimination, there may have been *de facto* discrimination as well as lagged effects of prior *de jure* discrimination. If the Fair Housing Act and the subsequent anti-discrimination laws, which ended *de jure* discrimination, mitigated the increases in racial segregation due to redline mapping, we

[20] Shertzer et al. (2018) and Shertzer and Walsh (2016) highlight the importance of studying racial segregation in the pre-World War II period (1900–1930): while the relocation decisions of white households from 1900–1930 ("White Flight") explain a large share of racial segregation, policies concerning zoning and public transit infrastructure have also affected racial segregation in the prewar era.

[21] A Dissimilarity Index of n implies that n percent of one race would have to move within the city and between neighborhoods in order for the neighborhood composition to reflect the overall city demography.

[22] For more on the timing of redline mapping, the Fair Housing Act and other anti-discriminatory legislation, see the timeline in Fig. 2.

would expect the estimates from (c) to be attenuated versions of those from (b). The estimates from 1970–2010 (reported in Fig. A18, subfigure (c)) are both smaller in magnitude and less strongly significant than those from 1940–2010 (reported in Fig. A18, subfigure (b)).[23] *If* we attribute this reduction in the estimate (namely, the reduction from Fig. A18, subfigure (b), to Fig. A18, subfigure (c)) to the Fair Housing Act and other subsequent anti-discriminatory legislation, then we would conclude that the Fair Housing Act may have mitigated as much as 34% of the increase in racial segregation brought about by redline mapping.

I do *not* claim that increases in racial segregation are the *only* channel through which redline mapping increased crime; however, comparing the magnitude of the effect of redline mapping on segregation to the magnitude of the effect of redline mapping on crime can give a useful back of the envelope estimate of the impact of racial segregation on crime. My estimates suggest that, for Black individuals born into a racially segregated city, a 10 point increase in segregation is associated with a 1.02 percentage point increase in likelihood of arrest by adulthood.[24] These estimates are comparable to Billings et al. (2013), and build on an existing body of evidence that shows that grouping together individuals who are at a high risk of committing crime increases the overall level of crime.[25]

5.2. Education as a mechanism

Because there exists evidence that racial segregation is causally responsible for lower educational attainment for Blacks (Ananat, 2007; Billings et al., 2013) and that lower educational attainment is causally responsible for increased crime (Lochner and Moretti, 2004; Dell et al., 2019), and, as we just saw in Section 5.1, evidence that redline mapping increased racial segregation, reductions in educational attainment are a channel through which redline mapping may have increased crime. Most directly, Aaronson et al. (1930) use neighborhood level variation to estimate that growing up in lower graded HOLC neighborhoods (for both red vs yellow and yellow vs blue) reduces adulthood educational attainment by .2 years.

To empirically test whether reductions in educational attainment are a channel through which redline mapping increased crime, I consider various measures of educational attainment as outcome variables in Eq. (1). Fig. A17 shows evidence that prior to redline mapping there were not significant differences in literacy levels across the population threshold.[26] Fig. A19 tests whether redline mapping and the increases in racial segregation it caused influenced educational attainment at the city level.[27] The estimates in Fig. A19 imply that redline mapping caused Black individuals to be 4.4 percentage point less likely to finish high school (an 11% reduction off the mean) and 5.3 percentage points less likely to attend at least some college (a 25% reduction off the mean).

The estimates reported in Fig. A19 suggest that redline mapping decreased educational attainment for Black individuals (possibly in part by increasing Black-White racial segregation), which, in turn, increased crime. I do *not* claim that decreases in educational attainment are the *only* channel through which redline mapping increased crime; however, comparing the magnitude of the effect of redline mapping on educational attainment to the magnitude of the effect of redline mapping on crime gives a back of the envelope estimate of the impact of educational attainment on crime. My estimates suggest that a 4.4 percentage point reduction in the likelihood of a Black individual completing high school in a city in 1980 (see Fig. A19) is associated with 11.42 additional Black arrests per one thousand people in 2000, which, in turn, suggests that for every additional Black high school graduate,.26 fewer Black arrests will occur. When scaling for the share of arrests that result in incarceration, this estimate is larger than, but consistent with, Lochner and Moretti (2004).[28]

6. Within-city: neighborhood-level effects of redlining on crime

Section 4 showed that redline mapping increased city level crime. In this section, I complement the city level analysis with a neighborhood level analysis to understand where within a mapped city the crime increases attributable to redlining occurred.

6.1. Motivation for a spatial RD

I use contemporary crime data from the city of Los Angeles in a spatial regression discontinuity framework to estimate the causal effect of credit access restrictions on crime. In the absence of HOLC mapping, whatever discrimination there was in the loan market would have still existed and the racial composition of neighborhoods would likely still have impacted credit access. However, it is also likely that individual lenders in the private market would have had heterogeneous beliefs about *exactly* where the "good" and "bad" neighborhoods began and ended. This variation would, in all likelihood, have led to credit access being smooth across the would-be HOLC borders. Therefore, when HOLC created its borders, it aligned lenders' beliefs and expectations and introduced a sharp discontinuity where one likely would not have existed before (Fig. A20).

6.2. Estimation: neighborhood-level

I use a spatial regression discontinuity model to estimate the neighborhood level impact of being assigned a security grade "red" on crime:

$$Crime_{nd} = \tau Redlined_d + \beta f(DtoRedline_n) + \gamma Redlined_d \times f(DtoRedline_n) + \epsilon_{nd}. \tag{2}$$

where $Crime_{nd}$ is the count of crimes at a given distance d miles away from a given redlined neighborhood n, $DtoRedline$ is the run-

[23] In subfigure (c), I find an 18% effect as opposed to a 25% effect in subfigure (b). The estimate in subfigure (c) is significant only at the 15 percent level, as opposed to the estimate in subfigure (b) which is significant at the 10 percent level.

[24] My estimates suggest that an 11.15 dissimilarity point increase in 1980 is associated with 11.42 additional Black arrests per one thousand people in 2000 (see Fig. A9). Cohorts born in 1980 who commit crimes are likely to commit offenses that would be observed in 2000, thus my comparison is intended to be a back of the envelope estimate showing, for a black individual, the effect of being born into a city with more racial segregation on the likelihood of being arrested in adulthood.

[25] See citations in Billings et al. (2013). Billings et al. (2013) finds that a 10 percentage point increase in assigned school share minority led to a 1.3 percentage point increase in the probability of ever being arrested and ever being incarcerated for minority males.

[26] Literacy is the best available measure of education in the 1930 Census (Lleras-Muney and Shertzer, 2015; Margo, 1986).

[27] Cohorts born in the 1980s would likely commit crimes observed in present-day (2010–2015) crime data. Thus by choosing a 1980 measure, I am likely measuring parental educational attainment, which is strongly correlated with the educational attainment of the children whose criminal activity would be recorded in data from 2010–2015.

[28] Lochner and Moretti (2004) finds that completing high school reduces the probability of incarceration by about.76 percentage points for whites and 3.4 percentage points for Blacks. My estimate suggests that graduating high school reduces the likelihood of being arrested for Blacks by 26 percentage points, which implies a reduction of incarceration by Blacks of 12 percentage points. (I convert arrests to incarcerations using (Bureau of Justice Statistics, 2019).) The fact that my estimates are roughly four times larger than (Lochner and Moretti, 2004) is likely due to the fact that redline mapping worked through channels other than educational attainment.

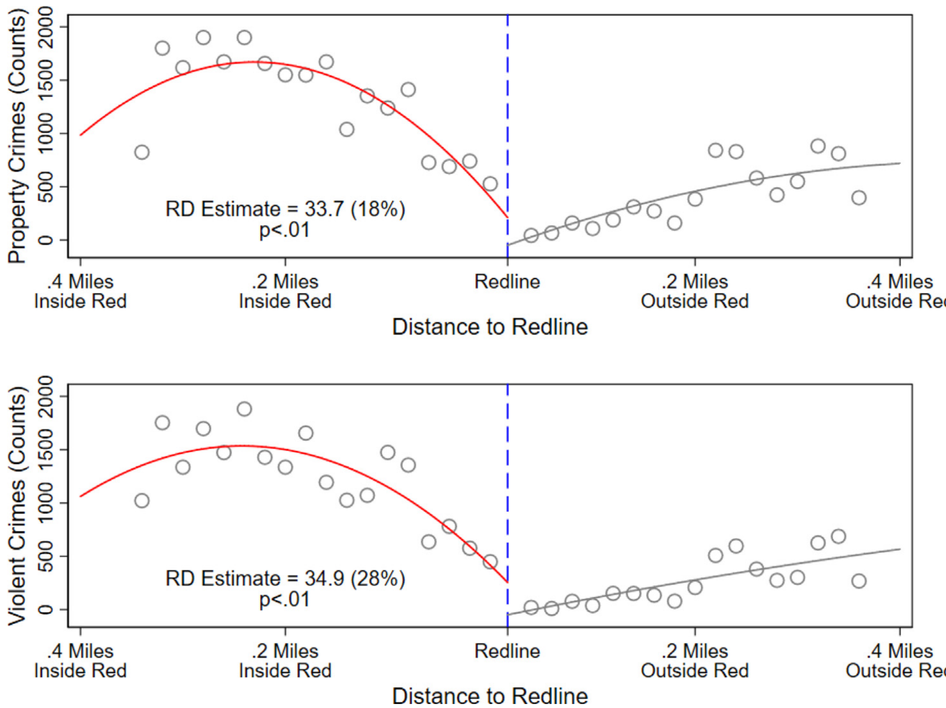


Fig. 6. Impact of redlining on crime: within-city estimates, by crime-type. **Note:** Each figure shows a spatial regression discontinuity diagram for crimes in Los Angeles in 2010. The top panel is restricted to property crimes, while the bottom panel is restricted to violent crimes. Property crimes are defined as those crimes the description of which contains words such as "burglary" and "larceny"; violent crimes are defined as those crimes the description of which contains words such as "murder" and "robbery". Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are both chosen optimally following Calonico (2017). The running variable is always the distance away from the redline on Home Owners Loan Corporation (HOLC) security maps, and the threshold is the redline itself. I dough-nut out a small region around the threshold to eliminate the small number of crimes committed inside the streets that divide neighborhoods. In all specifications, the sample is restricted to areas which received some HOLC color assignment in 1939. Data sources are city of Los Angeles crime data and HOLC archival records.

ning variable constructed as the distance from a given location in the city to the nearest redline on the map; *DtoRedline* is zero on the redline itself. This regression uses distance to the nearest redline as the running variable and fits a local linear polynomial on either side of the redline cutoff. I am primarily interested in τ, the coefficient on *Redlined*, an indicator variable which equals 1 when the point falls inside a redlined neighborhood, but is zero elsewhere.

6.3. Neighborhood-level pre-period balancing

In order to interpret the neighborhood level estimates as causal effects we must assume that, other than the redlining of a neighborhood, no determinant of crime is discontinuous at the redlining threshold. In order to test this assumption, I check whether before the HOLC maps were put in place these neighborhoods did not already exhibit jumps across the threshold for any covariate which could reasonably be said to be connected to contemporary crime volume. Using geocoded Census data from 1920 and 1930, I show smoothness across the threshold for a large set of covariates.

Following the city level balancing tests (Section 4.3), I focus on pre-period measures of the percent of households that are Black, the percent that are Hispanic, as well as home values and rent rates. None of these covariates exhibit significant jumps about the threshold prior to the introduction of the redlines (Fig. A22). Fig. A23 shows that these four covariates pass the balancing test for a wide range of bandwidths. For completeness, I estimate analogous balancing tests for every available covariate in the 1930 Census including measures of household demography, family formation, as well as literacy and labor market outcomes. Table A5 shows that nearly all of these pass the balancing test. When I use multiple inference methods to correct the p-values to account for the fact that I am testing for large numbers of covariates, I find that no covariate is statistically significant.[29]

6.4. Neighborhood-level crime effects: contemporary Los Angeles

Fig. 6 presents regression discontinuity diagrams for property and violent crime counts respectively in Los Angeles in 2010. We observe a higher volume of crime inside redlined neighborhoods than in neighborhoods that received some other color grade. Table 2 reports estimates of the discontinuity at the redlining threshold. I find that that, on average, crime jumps by approximately 34 property crimes and 35 violent crimes at the border of redlined neighborhoods. These represent increases of over 50% relative to crime within the bandwidth, and increases of 18% and 28% respectively relative to crime in the neighborhoods which were graded something other than red. Lastly, Fig. A24 shows that these estimates are robust to a large set of bandwidth choices. Appendix C shows further breakdowns by race and crime type (Fig. C6).[30]

[29] Aaronson et al. (2021), who are looking across over one hundred cities, find evidence of discontinuous jumps in several covariates which I find to be smooth. For example, they show (in their Figs. 4 and A3) that there are gaps across the red-yellow border in percent black as well as homeownership and home values (they do so using a bandwidth of.25 miles). There could several explanations for why I do not find these jumps. First, my running variable is distance from any non-red neighborhood to the nearest redline, whereas they are considering distance from any yellow neighborhood to a redline. Secondly, from examining the diagrams, it seems that some of the smaller jumps diagrams *might* turn out to be statistically insignificant after a multiple inference correction. Lastly, and most importantly, Aaronson et al. (2021) are looking at these covariates for over one hundred cities, whereas I am considering only Los Angeles.

[30] Even though the racialized nature of redlining suggests we should favor estimates by race, the Los Angeles crime data are, unfortunately, not well-suited to estimate race breakdowns since the race of the victim is missing in 81% of the observations.

Table 2
Impact of redlining on crime: within city estimates, by crime-type.

	(1) All	(2) Property	(3) Violent
Impact of Redlined Neighborhood	100.85***	33.72***	34.93***
	(19.68)	(8.02)	(6.63)
Observations	3423	3423	3423
Mean (Bandwidth)	171.56	60.10	51.58
Mean (Non-Red)	460.26	190.13	123.10

Note: The table reports spatial regression discontinuity estimates of the number of crime increases attributable to redlining by crime type. Standard errors are computed using a heteroskedasticity robust nearest neighbor variance estimator following Calonico (2017) and reported in parentheses. The running variable is always the distance away from the redline on Home Owners Loan Corporation (HOLC) security maps and the threshold is the redline itself. The outcome variable is crime in in Los Angeles in 2010. Property crimes are defined as those crimes the description of which contains words such as "burglary" and "larceny". Violent crimes are defined as those crimes the description of which contained words such as "murder" and "robbery". Bandwidth size and bin numbers are chosen optimally following Calonico (2017). The threshold is at the redline where the distance to the redlined neighborhood is zero. I dough-nut out a small region around the threshold to eliminate the small number of crimes committed inside the streets that divide neighborhoods. Two means are reported: means within the bandwidth and means across all neighborhoods, regardless of bandwidth, assigned a color grade other than red. The sample is restricted to areas which received some HOLC color assignment in 1939. Data sources are city of Los Angeles crime data and 1939 HOLC maps. Significance levels indicated by: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

6.5. Comparing neighborhood-level to city-level estimates

In this section, I compare the sizes of the neighborhood level and city level estimates to test for evidence that redlining *decreased* crime in predominately White neighborhoods. The intuition behind the comparison is that redline mapping likely influenced crime *both* by redistributing what would have been the city's volume of crime while concentrating crime into redlined neighborhoods *and* by increasing the city's overall volume of crime. That the city level estimates are non-zero is evidence that redlining increased the city's overall volume of crime by causing new crimes to be committed within the city. Accordingly, the neighborhood level estimates of crime increases likely consist of two components: new crimes added to city's overall volume of crime, and crimes transferred within the city away from non-red neighborhoods and into redlined neighborhoods. Evidence for a crime transfer would suggest that redline mapping *decreased* crime in neighborhoods not graded red by effectively taking crime that would have been in these predominantly White neighborhoods and transferring that crime into redlined neighborhoods. It is important to caveat this comparison by noting that it compares estimates from Los Angeles, a very large Metropolitan area, to estimates from mid-sized cities within redline mapping bandwidth.

The neighborhood level estimates imply that, on average, redlining caused 67 more crimes per redlined area,[31] implying that, for Los Angeles as a whole, redlining caused 6968 more crimes to be in redlined neighborhoods compared to neighborhoods not redlined (Section 6). The city level estimates imply that, on average, redlining caused 241 Black and Hispanic crime victimizations to occur in a city that was redline mapped than in a city not redline mapped.[32] Scaling these estimates based on differences in city population between Los Angeles and cities in the bandwidth for being redline mapped, I find that the between-city estimates are 30% the size of the within-city estimates (Fig. A25). This comparison provides evidence that, on average, crime in a neighborhood inside a city not redline mapped is greater than crime in a neighborhood inside a city that was redline mapped but was not itself redlined (i.e., was assigned a grade other than "red"). This suggests that redline mapping *reduced* crime in neighborhoods not graded "red" by effectively taking crime that would have been in these neighborhoods if a redlining map had *not* been drawn and transferring that crime away from these non-red neighborhoods into the redlined neighborhoods. These estimates should be treated with caution, however, because they are comparing estimates from Los Angeles, a very large Metropolitan area, to estimates from mid-sized cities within the bandwidth of the redline mapping population threshold.

7. Conclusion

In the United States today, the welfare costs of crime are disproportionately born by households living in predominately Black or Hispanic neighborhoods. This paper uses two regression discontinuity designs to show that federal housing policies established in the wake of the Great Depression and colloquially called "redlining" have increased crime up through the present-day. At the city level, I use an unannounced population cutoff that determined which cities were redline mapped to show that redline mapping increased present-day city level crime. At the neighborhood level, I use a spatial regression discontinuity to show that redlining influenced the present-day neighborhood level distribution of crime across neighborhoods in Los Angeles, California. Furthermore, I use the city level research design to identify channels through which redline mapping increased crime including increasing racial segregation and decreasing black educational attainment. These mechanisms enjoy ex ante motivation from the existing literature on racial segregation, education and crime.

At the city level, I find that redline mapping a city is responsible for adding 176 Black and 65 Hispanic crime victimizations to a city in the present-day. At the neighborhood level, I find that that, on average, crime jumps by approximately 34 property crimes and 35 violent crimes at the border of redlined neighborhoods (increases of approximately 50%). Assuming all new crime increases are born by Black residents, at least 6.5% of present-day racial disparities in crime are attributable to redlining.[33] Race specific estimates imply large increases in the Black-White gap in crime victimization, which I confirm by using race gap measures as the outcome variable.[34] Furthermore, I find that redline mapping increased Black-White residential segregation since 1940 by 11.4 dissimilarity points (24%) and decreased Black educational attainment by 1980, making Black individuals 4.4 percentage points (11%) less likely to finish high school and 5.3 percentage points (25%) less likely to attend college. While the nature of the variation does not allow me to demonstrate uniqueness of channel, the evidence in this paper suggests a labor market story: restricting credit access by race increased racial segregation, which harmed local educational attainment, which, in turn, influenced job market outcomes and altered the likelihood of criminal perpetration and victimization by race.

The main results of this paper show how a federal policy was able to radically alter the course of development of hundreds of cities by putting these cities on different paths in terms of housing, education and crime. In addition to showing how a policy can have lasting impact over three quarters of a century after its initial implementation, this paper also suggests lessons for present-day policy makers. First, the lasting harm done by redlining further

[31] Summing the point estimates in columns (2) and (3) of Table 2.

[32] Summing the Black crime increase (176) and the Hispanic crime increase (65). See the discussion in Section 4 for more details.

[33] 6.5% comes from taking the 22 "new crimes" divided by the 335 total crimes in non-red areas (Fig. A25).

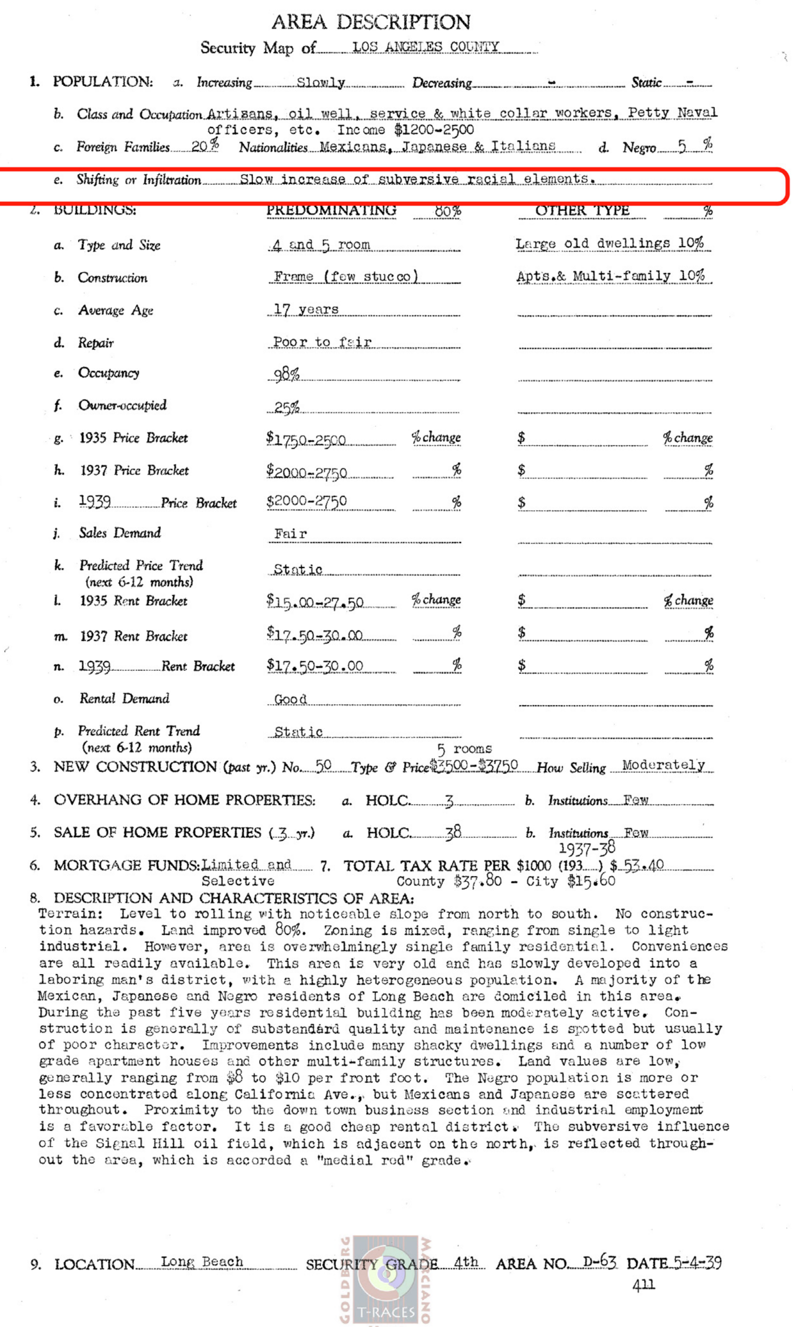
[34] I find that redline mapping increased the Black-White gap by 66%. I define the Black-White gap as the ratio of the Black rate to the White rate. When I use the gap measure as the outcome variable I find a noisy RD estimate of 4.78, which is 66% of the mean gap in non mapped cities within the population bandwidth.

underscores the importance of insuring that there exists a non-discriminatory credit market. Secondly, the mechanisms identified in this paper through which redlining increased crime suggest that racial segregation and educational attainment impact long-run city formation, so that a policy maker interested in reducing crime could target reductions in racial segregation and improvements in educational attainment.

Data availability

Data will be made available on request.

Appendix A. Appendix Tables and Figures


AREA DESCRIPTION
Security Map of LOS ANGELES COUNTY

1. POPULATION: a. Increasing Slowly Decreasing - Static -
b. Class and Occupation Artisans, oil well, service & white collar workers, Petty Naval officers, etc. Income $1200-2500
c. Foreign Families 20 % Nationalities Mexicans, Japanese & Italians d. Negro 5 %
e. Shifting or Infiltration Slow increase of subversive racial elements.

2. BUILDINGS:	PREDOMINATING 80%		OTHER TYPE %	
a. Type and Size	4 and 5 room		Large old dwellings 10%	
b. Construction	Frame (few stucco)		Apts.& Multi-family 10%	
c. Average Age	17 years			
d. Repair	Poor to fair			
e. Occupancy	98%			
f. Owner-occupied	25%			
g. 1935 Price Bracket	$1750-2500	% change	$	% change
h. 1937 Price Bracket	$2000-2750	%	$	%
i. 1939 Price Bracket	$2000-2750	%	$	%
j. Sales Demand	Fair			
k. Predicted Price Trend (next 6-12 months)	Static			
l. 1935 Rent Bracket	$15.00-27.50	% change	$	% change
m. 1937 Rent Bracket	$17.50-30.00	%	$	%
n. 1939 Rent Bracket	$17.50-30.00	%	$	%
o. Rental Demand	Good			
p. Predicted Rent Trend (next 6-12 months)	Static			

3. NEW CONSTRUCTION (past yr.) No. 50 Type & Price 5 rooms $3500-$3750 How Selling Moderately
4. OVERHANG OF HOME PROPERTIES: a. HOLC 3 b. Institutions Few
5. SALE OF HOME PROPERTIES (3 yr.) a. HOLC 38 b. Institutions Few
6. MORTGAGE FUNDS: Limited and Selective 7. TOTAL TAX RATE PER $1000 (193 1937-38) $ 53.40 County $37.80 - City $15.60
8. DESCRIPTION AND CHARACTERISTICS OF AREA:
Terrain: Level to rolling with noticeable slope from north to south. No construction hazards. Land improved 80%. Zoning is mixed, ranging from single to light industrial. However, area is overwhelmingly single family residential. Conveniences are all readily available. This area is very old and has slowly developed into a laboring man's district, with a highly heterogeneous population. A majority of the Mexican, Japanese and Negro residents of Long Beach are domiciled in this area. During the past five years residential building has been moderately active. Construction is generally of substandard quality and maintenance is spotted but usually of poor character. Improvements include many shacky dwellings and a number of low grade apartment houses and other multi-family structures. Land values are low, generally ranging from $8 to $10 per front foot. The Negro population is more or less concentrated along California Ave., but Mexicans and Japanese are scattered throughout. Proximity to the down town business section and industrial employment is a favorable factor. It is a good cheap rental district. The subversive influence of the Signal Hill oil field, which is adjacent on the north, is reflected throughout the area, which is accorded a "medial red" grade.

9. LOCATION Long Beach SECURITY GRADE 4th AREA NO. D-63 DATE 5-4-39
411

Fig. A1. Home owner's loan corporations survey report. **Note:** Figure shows a survey report produced for a neighborhood in Los Angeles by the Home Owner's Loan Corporation (HOLC) in May of 1939. This neighborhood is in the South of Los Angeles, in the Long Beach area; it was graded "4th" or "Red" and hence is said to be have been "redlined"; the "red" grade indicates that this neighborhood is considered to be among the riskiest neighborhoods for lenders. Surveyor expectations about neighborhood level racial demography can in found in item 1.e, "Shifting or Infiltration", which is boxed above.

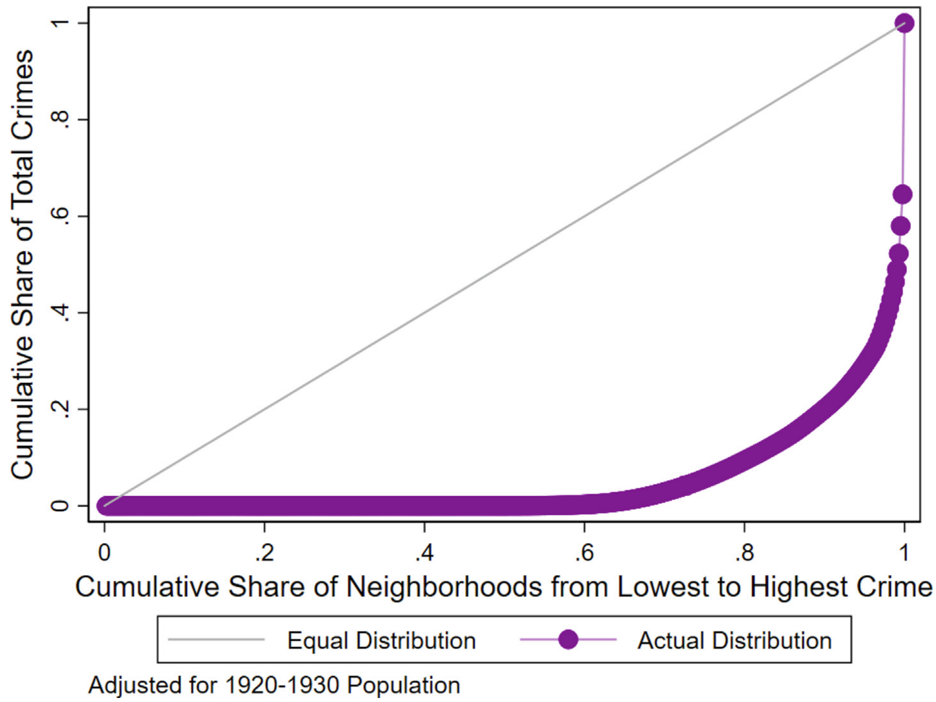
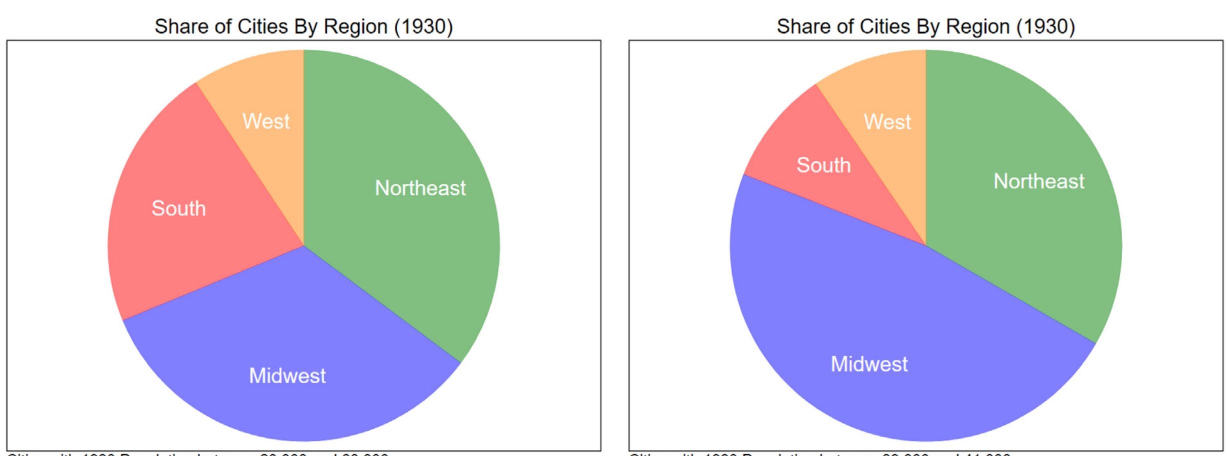


Fig. A2. Inequality in the distribution of crime in Los Angeles. **Note:** Figure shows a Gini or Inequality Curve for neighborhood level crime in Los Angeles in 2010. The sample is restricted to neighborhoods that received some Home Owners Loan Corporation (HOLC) color grade in 1939. Data sources are city of Los Angeles crime data and HOLC archival records.



(a) 1930 City Pop $\in [20,000, 60,000]$ (b) 1930 City Pop $\in [39,000, 41,000]$

Fig. A3. Regional breakdown of cities with redline mapping bandwidth: between-city regional breakdowns. **Note:** The figure shows the regional share of cities that lie in two small bandwidths around the redling mapping population threshold: in the left panel the regional shares for cities with 1930 population between 20,000 and 60,000 are shown, while in the right panel the regional shares for cities with 1930 population between 39,000 and 41,000 are shown. (The redline mapping threshold was 40,000 people.) Data sources are the 1930 Census and Home Owner Loan Corporation (HOLC) archival records.

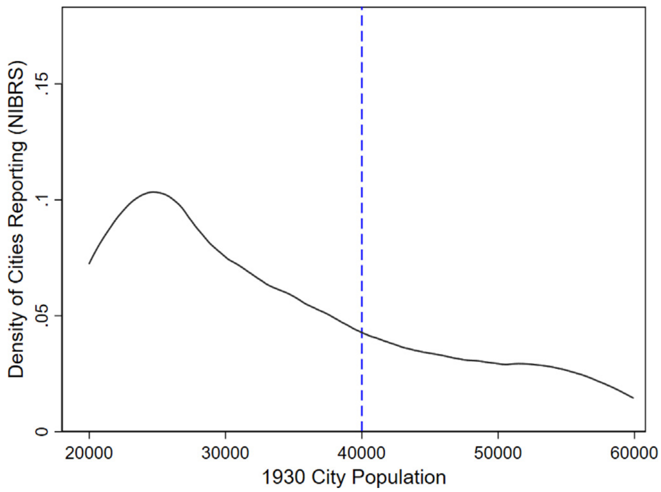


Fig. A4. Density of agencies reporting to NIBRS: between-city crime data. **Note:** The figure shows the density of agencies reporting to the National Incident Based Reporting System (NIBRS) in 2015 across the 1930 city population in which the agency operates. Data sources are individual level NIBRS data from 2015 and Home Owner Loan Corporation (HOLC) archival records.

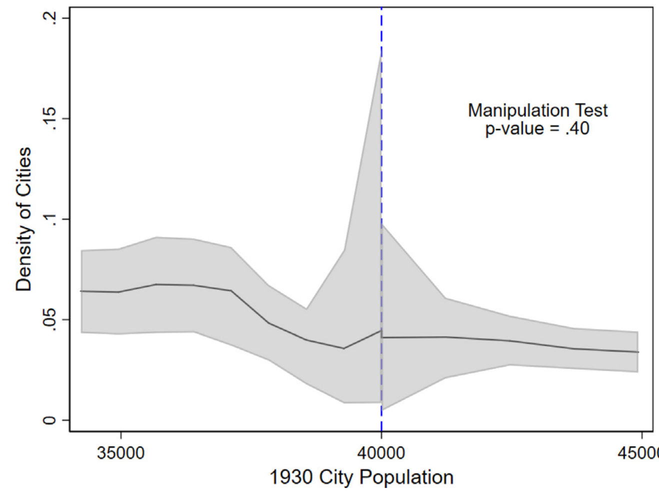


Fig. A5. Manipulation test. **Note:** The figure shows the results of a manipulation test following the methods of Cattaneo et al. (2018).

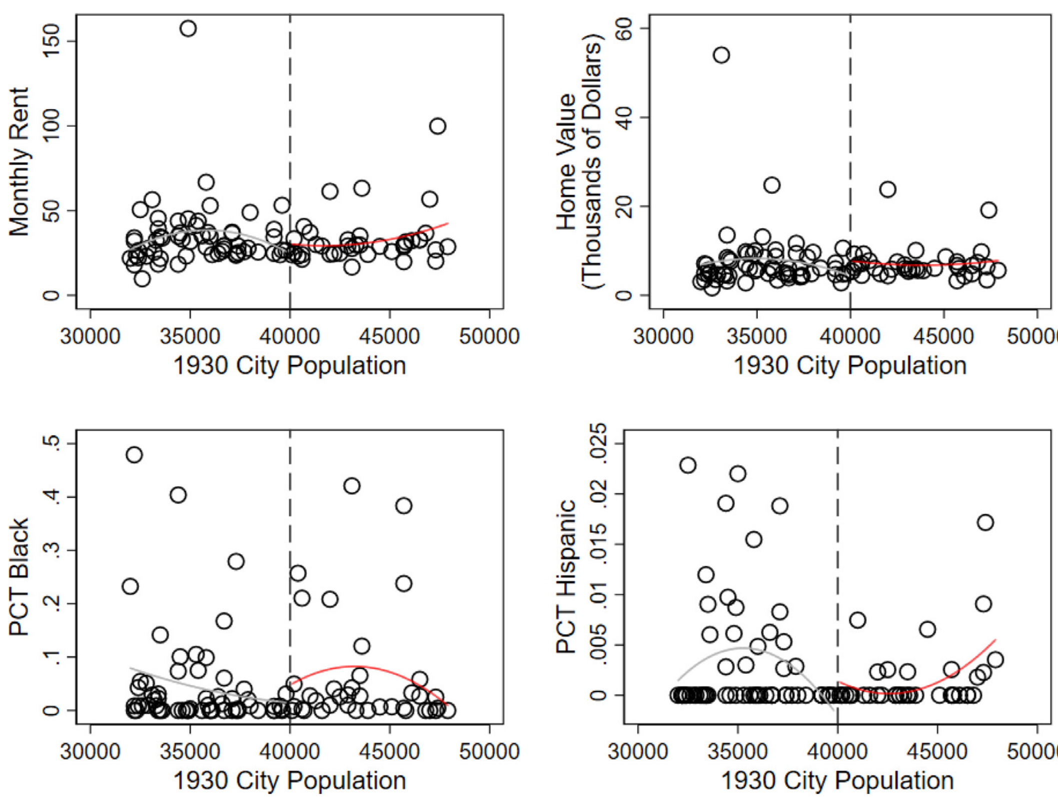


Fig. A6. Balancing tests: between-city 1920–1930 covariates. **Note:** Each figure shows a regression discontinuity diagram where the dependent variable is given city level pre-period covariate measured in 1920–1930. The top panels show results for self-reported monthly rent and home value, respectively, while the bottom panels show results for the percent of a city's population that is Black and the percent that is Hispanic, respectively. Circles represent bin means, while lines represent fitted quadratic curves. Bin number is fixed at 80 cities to ease comparison. The running variable is always 1930 city population. Bandwidth size is fixed at 7,000 people to ease comparison. Data sources are 1920–1930 Census and Home Owner Loan Corporation (HOLC) archival records.

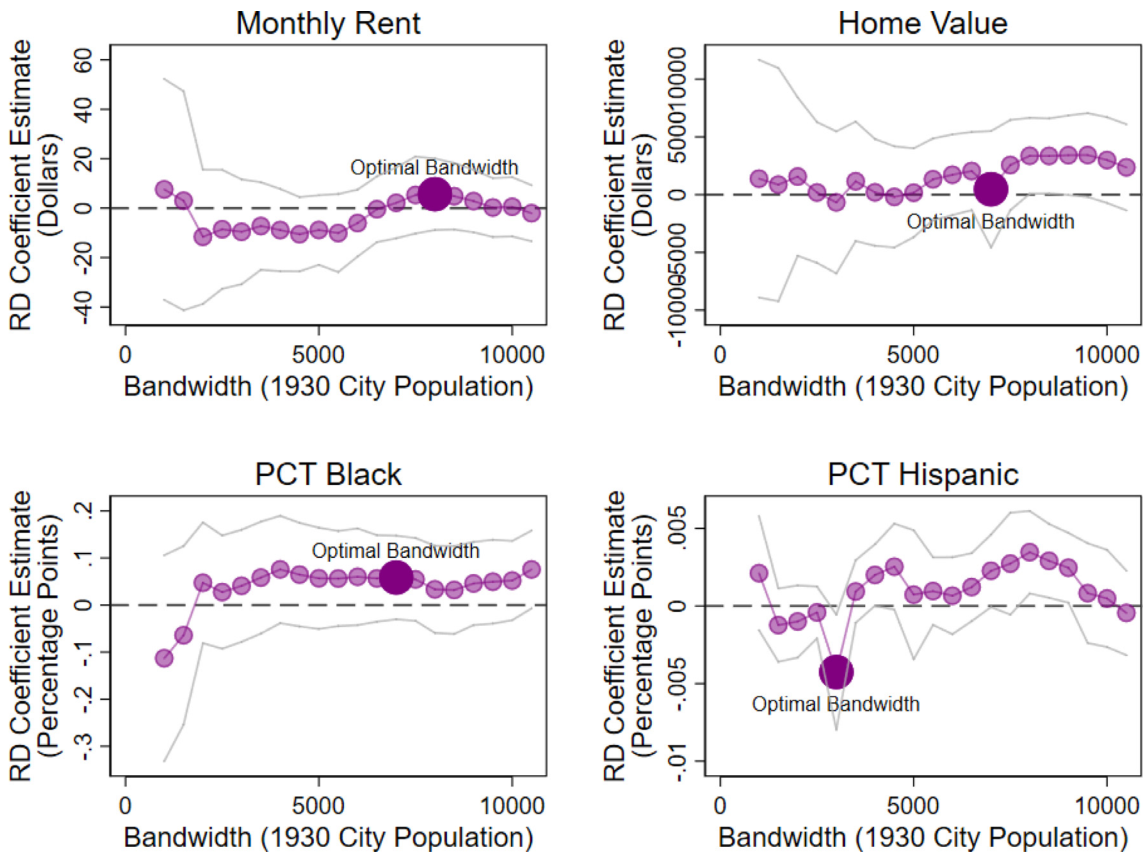


Fig. A7. Balancing tests: between-city 1920–1930 covariates, bandwidth sensitivity. **Note:** Each figure shows a profile of regression discontinuity coefficient estimates across a range of bandwidth selections. The top panels show results for self-reported monthly rent and home value, respectively, while the bottom panels show results for the percent of an area that is Black and the percent that is Hispanic, respectively. Circles represent estimates, with the large circle representing the estimate for the optimal bandwidth. The running variable is always 1930 city population. Data sources are 1920–1930 Census and Home Owner Loan Corporation (HOLC) archival records.

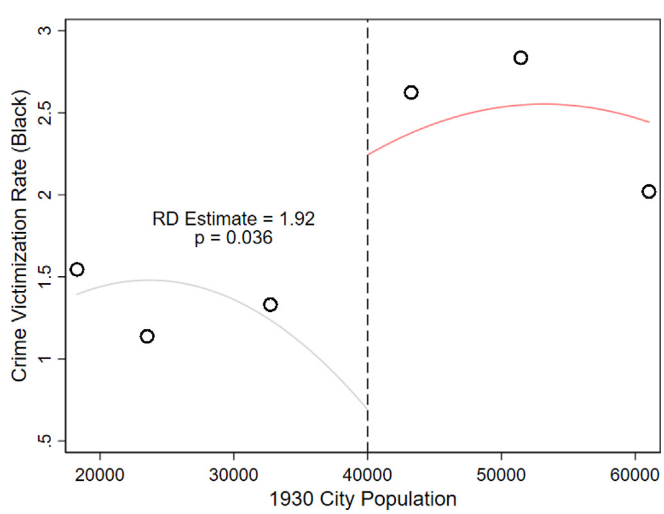


Fig. A8. Impact of redline mapping on crime: between-city estimates. **Note:** The figure shows a regression discontinuity diagram where the outcome variable is the rate of Black or Hispanic crime victimization in a given city per 1,000 persons policed. The estimates are drawn from 2015 NIBRS data. Observations are at the agency level. The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following Calonico (2017). Data sources are individual level NIBRS crime victimization data and Home Owner Loan Corporation (HOLC) archival records.

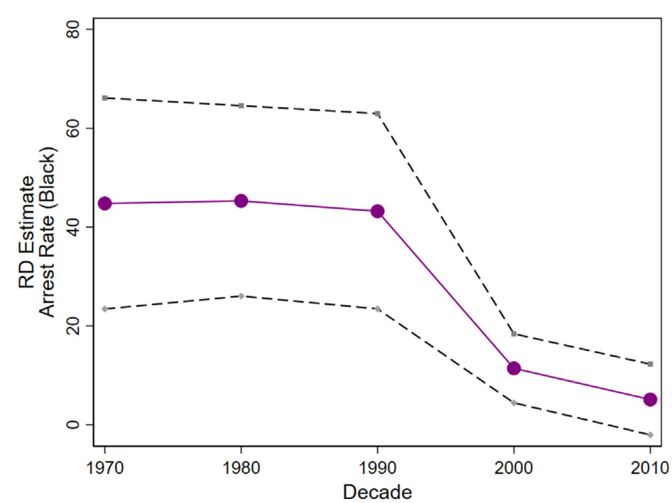


Fig. A9. Impact of redline mapping on arrests: between-city estimates over decades. **Note:** The figure shows a profile of regression discontinuity estimates and 95% confidence intervals obtained by estimating Eq. (1) on decadal UCR data. (Decadal UCR data is obtained by pooling monthly UCR data across decades.) In each estimate the the outcome variable is black arrest rate per 1,000 people in a given city in a given decade. Data sources are UCR arrest data (1974–2016) and Home Owner Loan Corporation (HOLC) archival records.

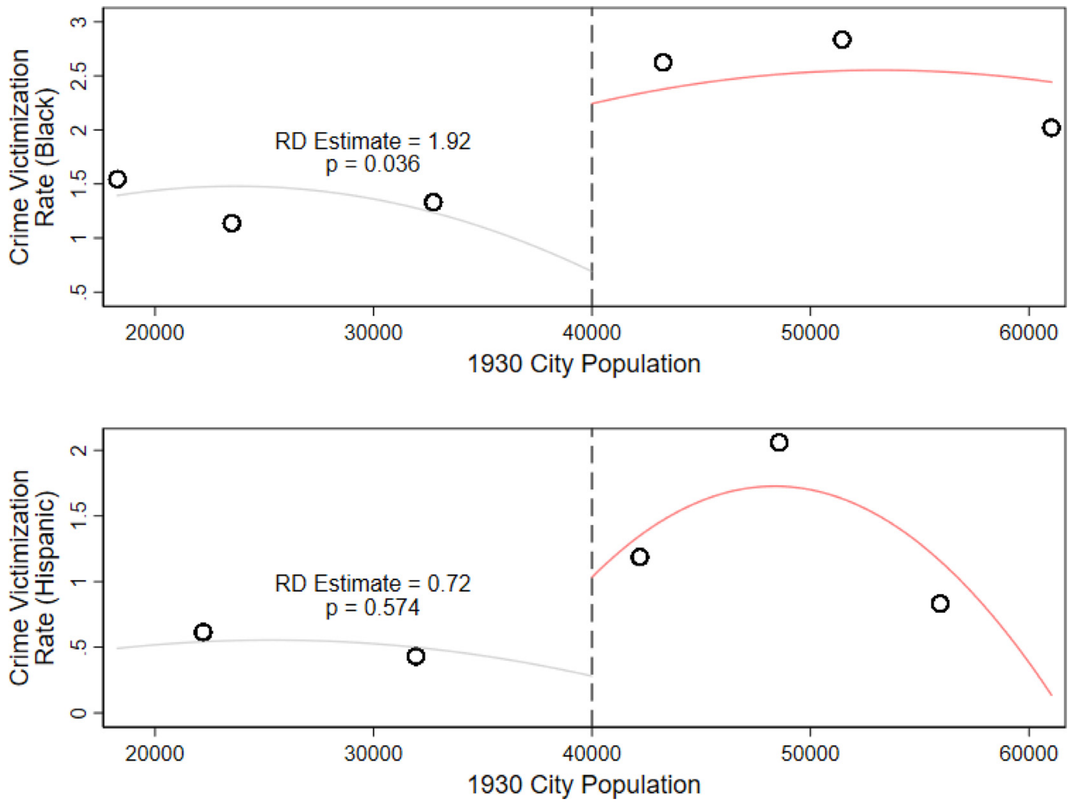


Fig. A10. Impact of redline mapping on crime: between-city estimates by race. **Note:** Each figure shows a regression discontinuity diagram where the outcome variable is the rate of Black and Hispanic crime victimizations respectively in a given city in 2015. Observations are at the agency level. The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following Calonico (2017). There are 133 agencies included in NIBRS 2015 data who report crime outcomes for cities whose 1930 population places them within the optimal bandwidth; there are 84 reporting agencies on the left-hand side and 49 on the right-hand side. The estimates imply that 176 Black and 65 Hispanic crime victimizations per city in 2015 are attributable to redline mapping. Data sources are individual level NIBRS crime victimization data and Home Owner Loan Corporation (HOLC) archival records.

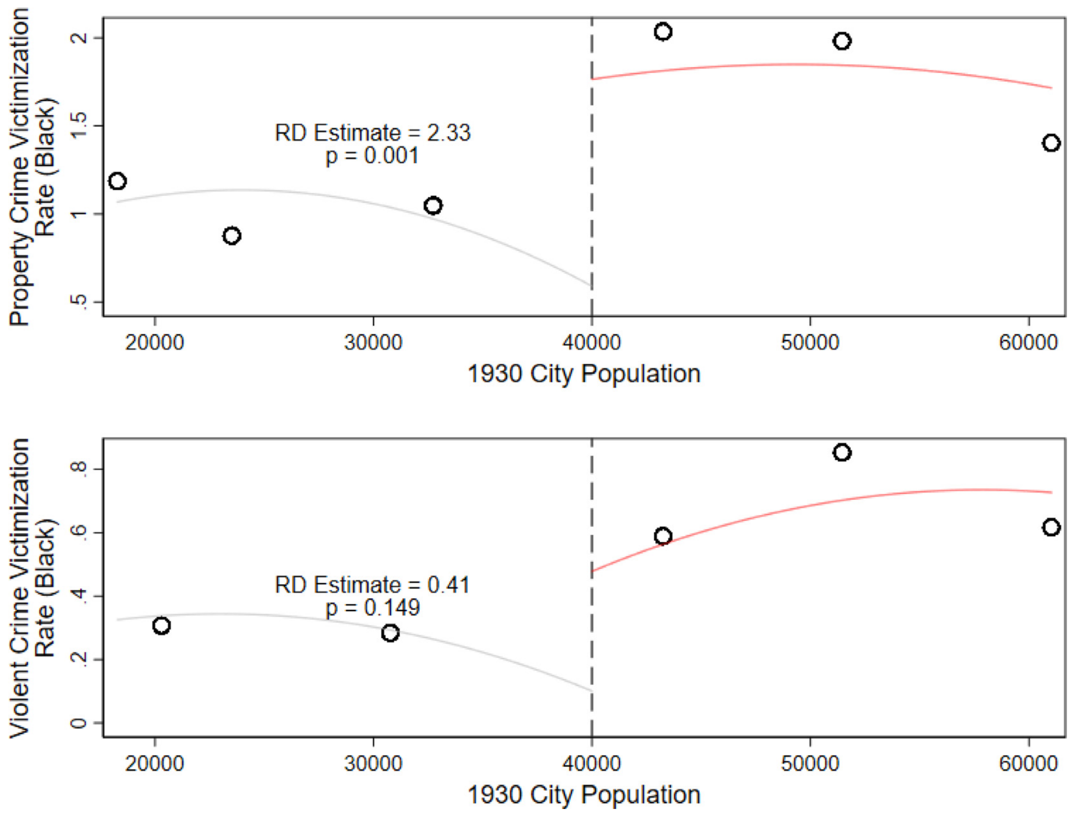


Fig. A11. Impact of redline mapping on crime: between-city estimates by crime type. **Note:** Each figure shows a regression discontinuity diagram where the outcome variable is the Black rate of property and violent crime victimizations respectively in a given city in 2015. Observations are at the agency level. The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following Calonico (2017). There are 133 agencies included in NIBRS 2015 data who report crime outcomes for cities whose 1930 population places them within the optimal bandwidth; there are 84 reporting agencies on the left-hand side and 49 on the right-hand side. Data sources are individual level NIBRS crime victimization data and Home Owner Loan Corporation (HOLC) archival records.

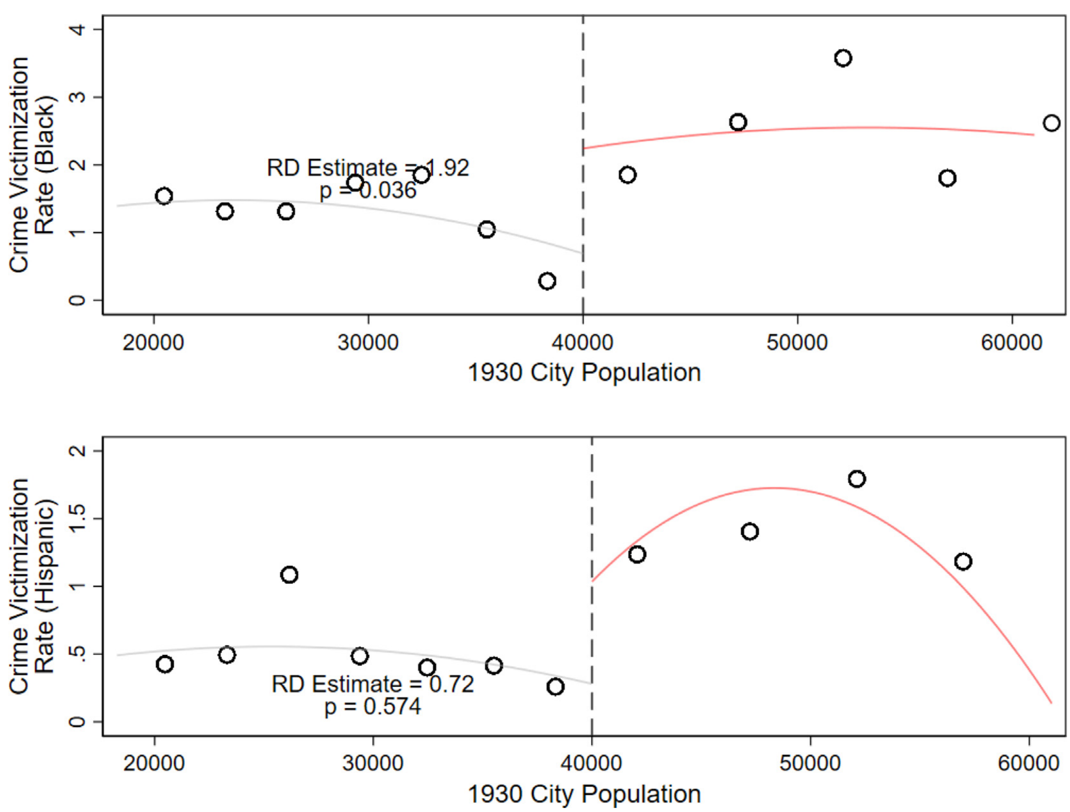


Fig. A12. Impact of redline mapping on crime: between-city estimates (non-optimal bin number). **Note:** These figures differ from those in Fig. A10 in only one way: in Fig. A10 techniques from Calonico (2017) are used to select the number of bins on each side of the cutoff optimally, whereas in these figures the bin size is manually selected to show more of the variation in the outcome variable across the running variable.

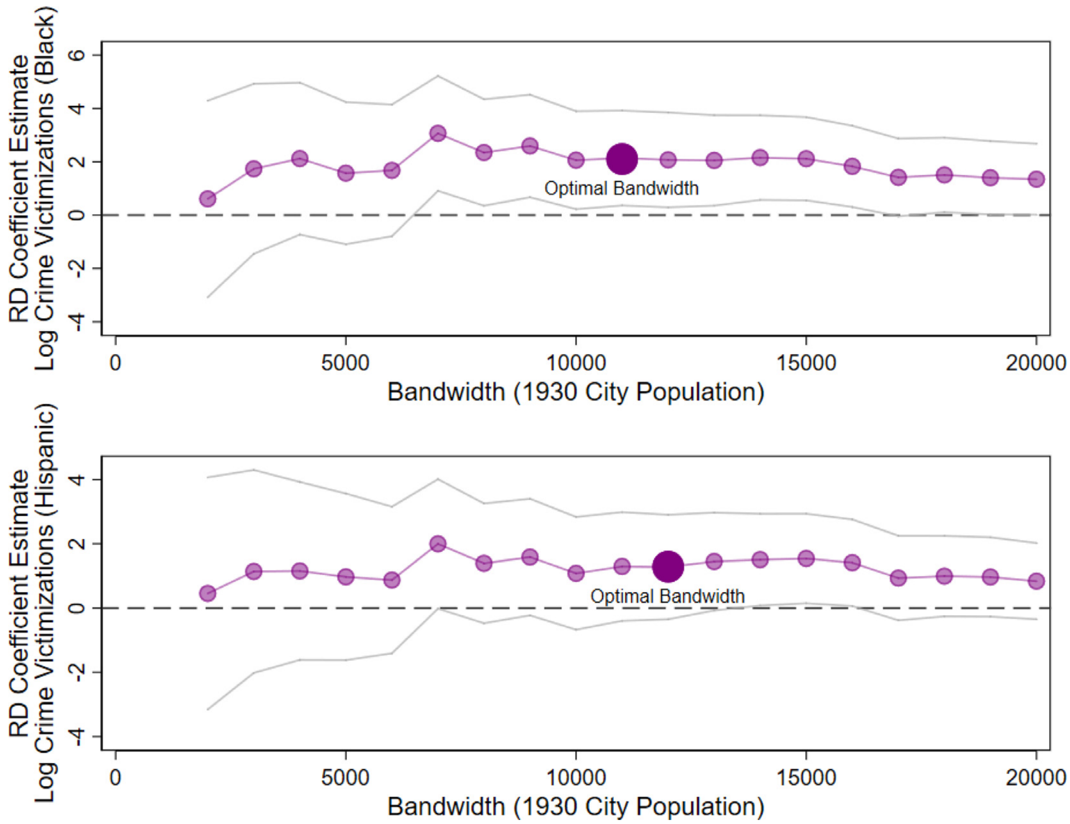


Fig. A13. Impact of redline mapping on crime: between-city estimates, by bandwidth. **Note:** Each figure shows a profile of regression discontinuity coefficent estimates and 95% confidence intervals across a range of bandwidth selections. The outcome variable is the log of crime victimizations in a given city in 2015. The top panel show results for the log of Black crime victimizations, while the bottom panel shows results for the log of Hispanic crime victimizations. The running variable is always 1930 city population. Circles represent estimates, with the large circle representing the estimate for the optimal bandwidth. Data sources are individual level NIBRS crime victimization data and Home Owner Loan Corporation (HOLC) archival records.

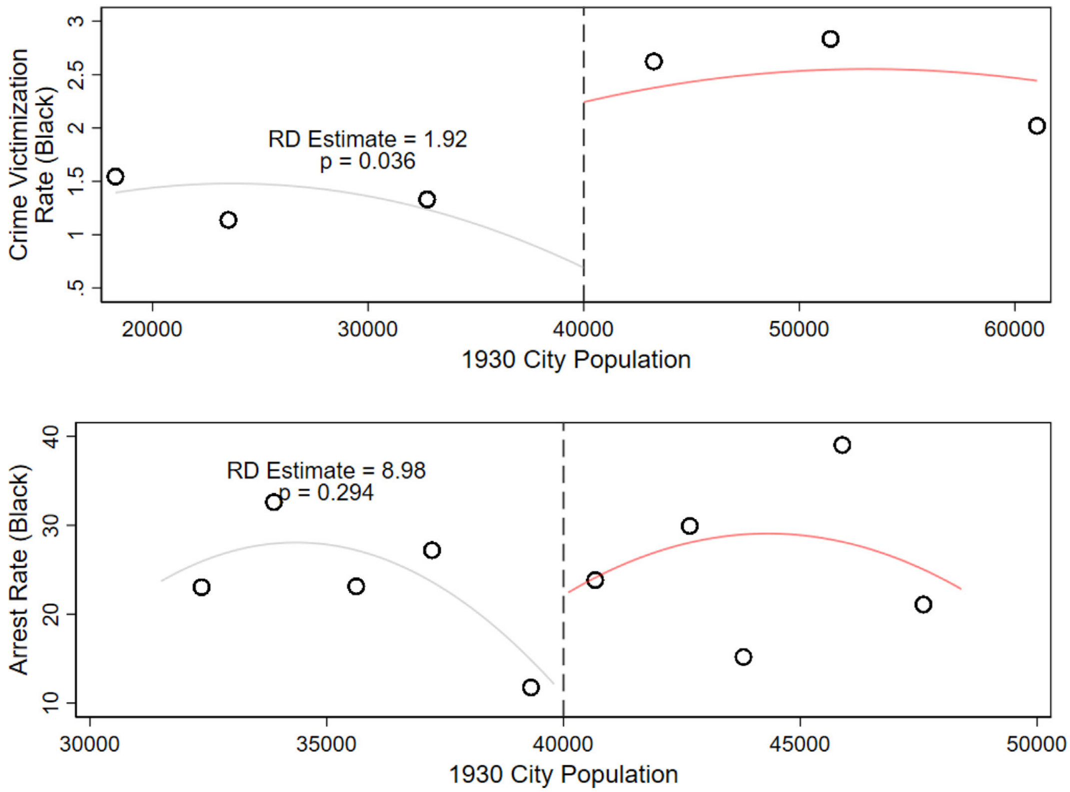
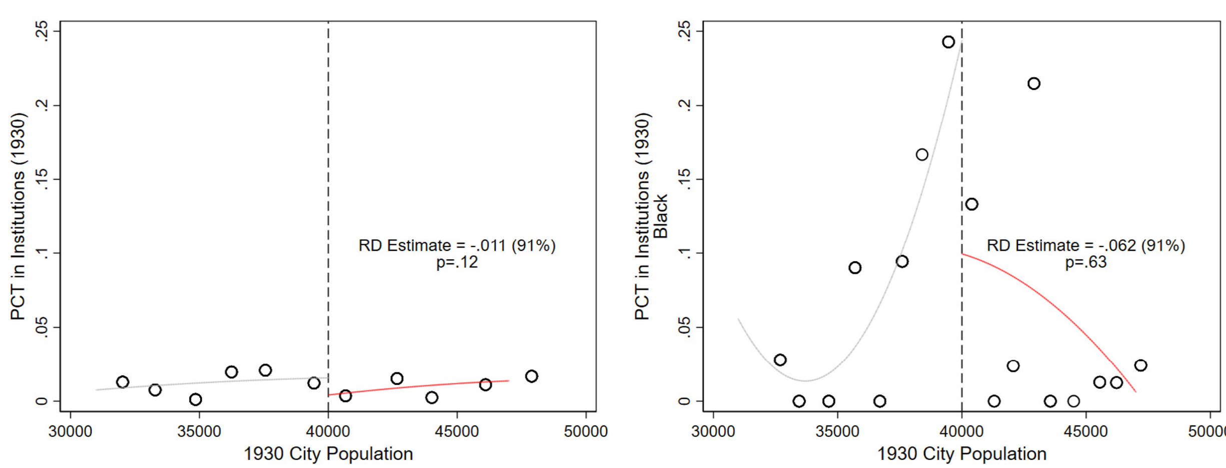


Fig. A14. Impact of redline mapping on crimes and arrests: between-city estimates (UCR vs NIBRS). **Note:** Each figure shows a regression discontinuity diagram. In the top panel, the outcome variable is the black crime victimization rate in a given city in 2015, while in the bottom panel the outcome variable is the black arrest rate in a given city in 2015. In both panels, observations are at the agency level and the running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following Calonico (2017). In the top panel, there are 133 agencies included in NIBRS 2015 victimization data who report crime outcomes for cities whose 1930 population places them within the optimal bandwidth; there are 84 reporting agencies on the left-hand side and 49 on the right-hand side. The estimates in the top panel imply that 176 Black crime victimizations per city in 2015 are attributable to redline mapping. In the bottom panel, there are 131 agencies included in UCR 2015 arrest data who report crime outcomes for cities whose 1930 population places them within the optimal bandwidth; there are 82 reporting agencies on the left-hand side and 49 on the right-hand side. The estimates in the bottom panel imply that 61 Black arrests per city in 2015 are attributable to redline mapping. Data sources are UCR arrest data and NIBRS victimization data, as well as and Home Owner Loan Corporation (HOLC) archival records.



(a) Pre-Period Incarceration (b) Pre-Period Incarceration: Black

Fig. A15. Impact of redline mapping on incarcerated population: placebo tests with institutional group quarters. **Note:** Each figure shows a regression discontinuity diagram. In the top panel, the outcome variable is the share of individuals who report living in an institutional group quarter in a given city in 1930, while in the bottom panel the outcome variable is the share of black individuals who report living in an institutional group quarter in a given city in 1930. Institutional group quarters include correctional facilities, nursing homes and mental hospitals. Starting in 1980, institutional group quarters excludes persons living in non-institutional group quarters such as college dormitories, miliary barracks, group homes, mission and shelters. However, in the 1930 Census, institutionalized group quarters includes "non-inmates" who would have been classified as living in non-institutional group quarters after 1980 (See the IPUMS documentation for the variable "GQ".) In both panels, The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth and bin numbers are chosen optimally following Calonico (2017). Data sources are the 1930 Census, as well as and Home Owner Loan Corporation (HOLC) archival records.

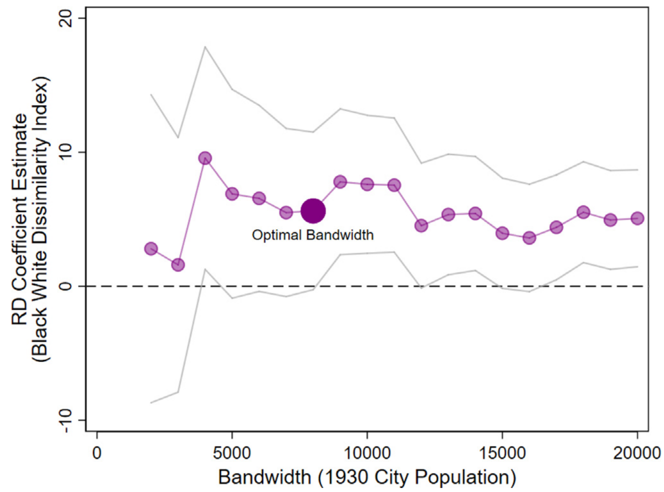


Fig. A16. Impact of redline mapping on segregation: bandwidth sensitivity. **Note:** The figure shows a profile of regression discontinuity estimates and 95% confidence intervals obtained by estimating Eq. (1) on decadal segregation measures. These estimates constitute a bandwidth sensitivity test for panel (b) of Fig. A18. Data sources are Cutler et al. (1999); Logan (2014), and Home Owner Loan Corporation (HOLC) Archival records.

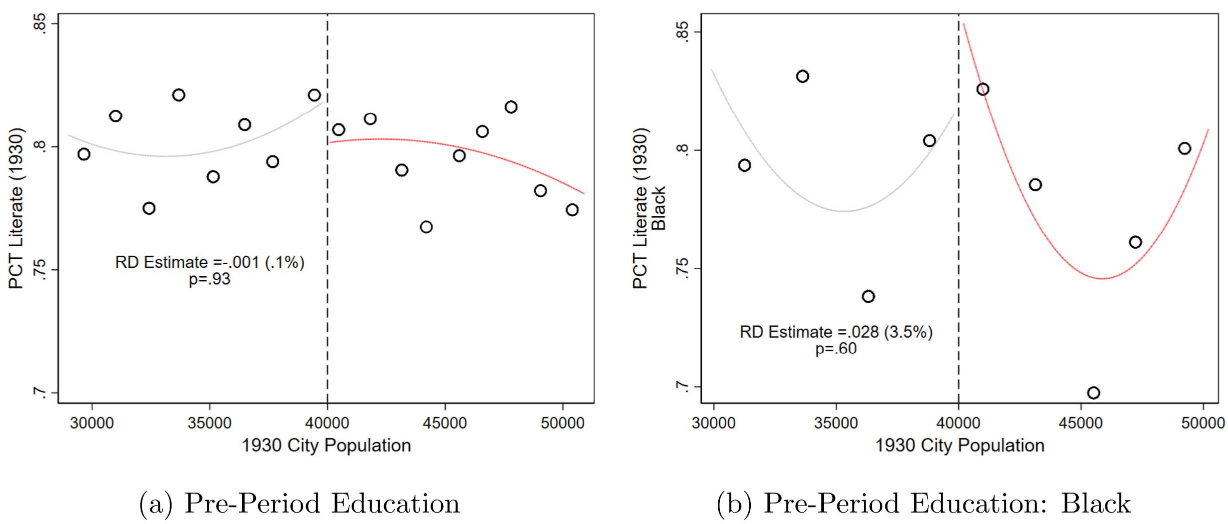


Fig. A17. Impact of redline mapping on educational attainment: placebo tests with literacy. **Note:** Each figure shows a regression discontinuity diagram. In the top panel, the outcome variable is the share of individuals who report being literate in a given city in 1930, while in the bottom panel the outcome variable is the share of black individuals who report being literate in a given city in 1930. In both panels, The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth and bin numbers are chosen optimally following Calonico (2017). Data sources are the 1930 Census, as well as and Home Owner Loan Corporation (HOLC) archival records.

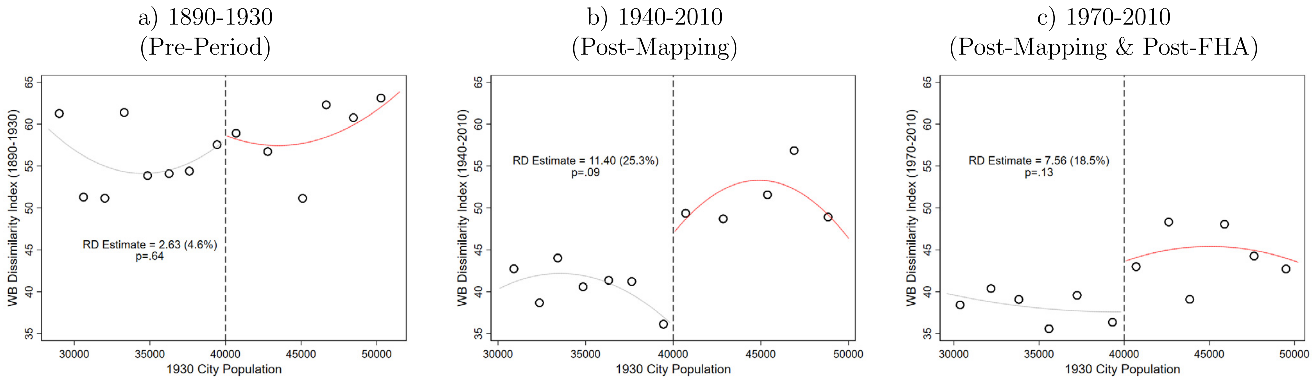


Fig. A18. Impact of redline mapping on racial segregation: pooled between-city estimates. **Note:** Figure shows a regression discontinuity diagram where the outcome variable is White-Black Dissimilarity Index for a given city in a given year The running variable is always 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following Calonico (2017). Subfigure (a) shows a placebo test for White-Black segregation in the period prior to redline mapping, pooling data from 1890 to 1930. Subfigure (b) shows the impact of redline mapping on White-Black segregation over the entire modern period, pooling data from 1940 to 2010. Subfigure (c) shows the impact of redline mapping on White-Black segregation on the period after the Fair Housing Act (FHA) which first outlawed *de jure* discrimination in the credit market. Data sources are Cutler et al. (1999); Logan (2014), and Home Owner Loan Corporation (HOLC) Archival records.

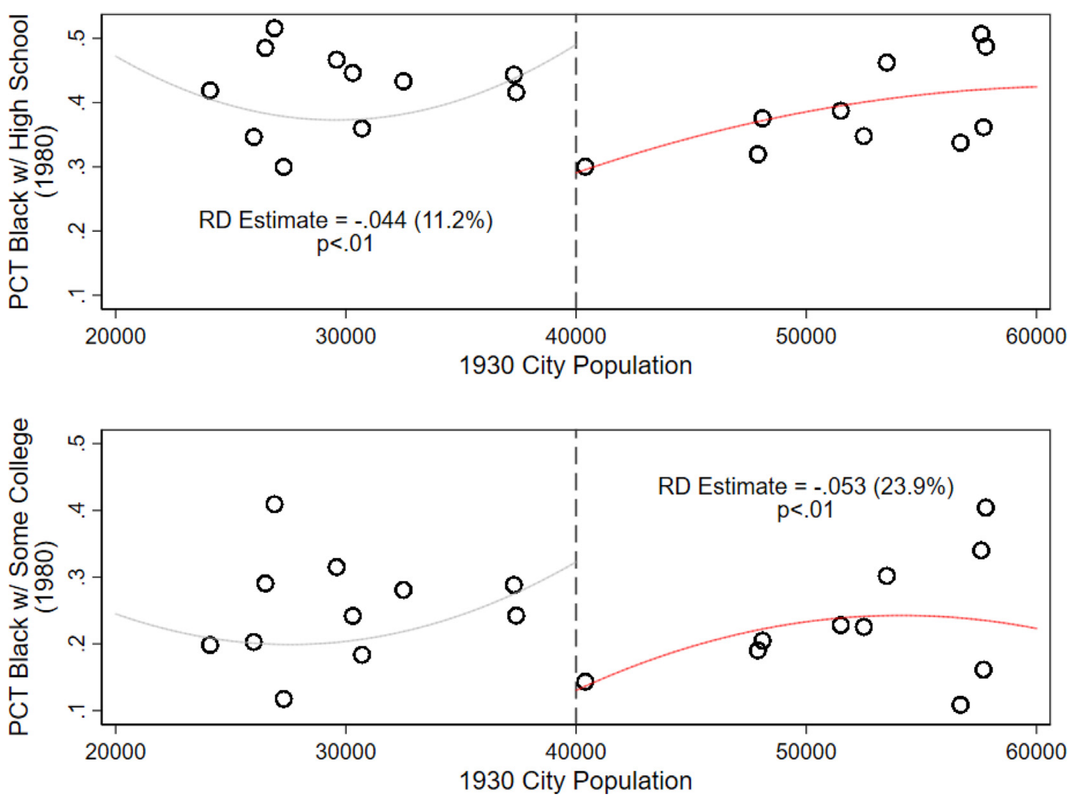
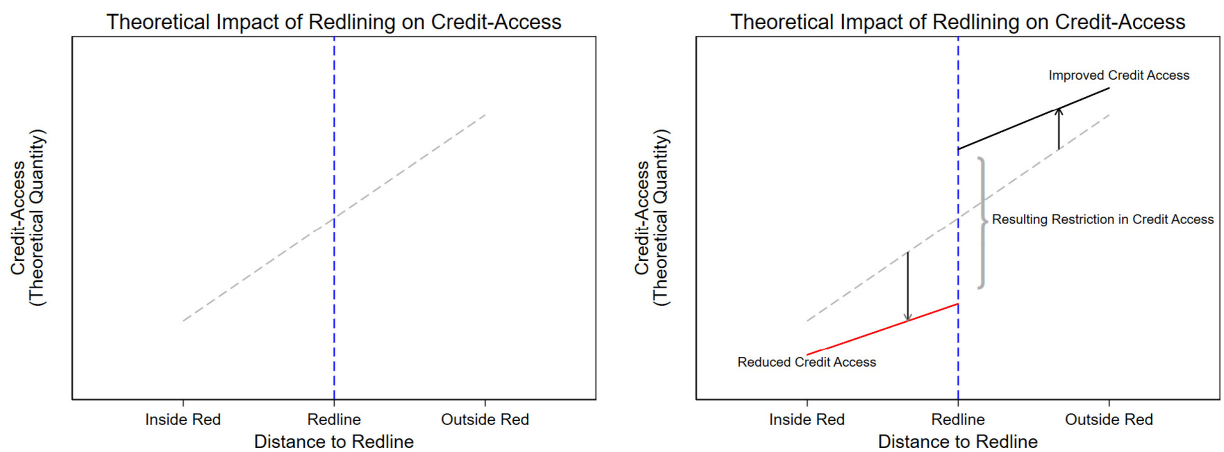
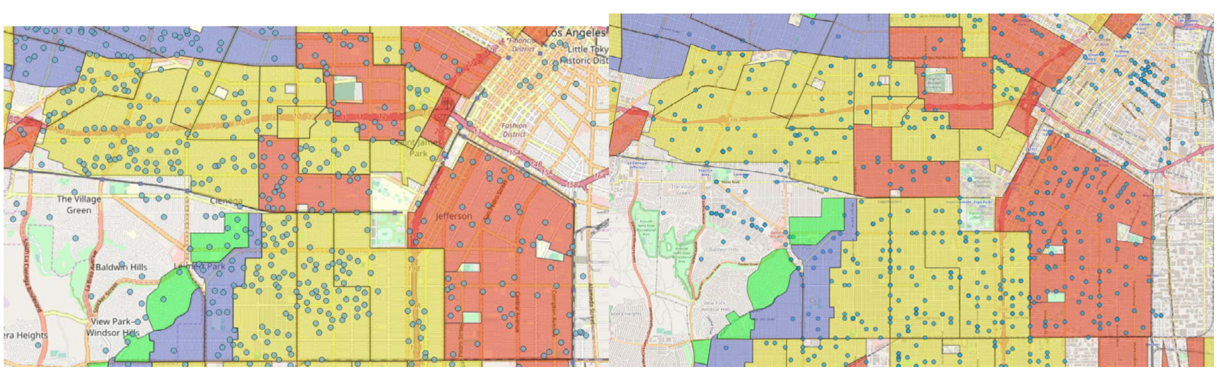


Fig. A19. Impact of redline mapping on educational attainment: high school, some college. **Note:** Each figure shows a regression discontinuity diagram. In the left panel, the outcome variable is the share of black individuals who report having graduated high school in a given city in 1980, while in the right panel the outcome variable is the share of black individuals who report having attended at least some college in a given city in 1980. In both panels, The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bin numbers are chosen optimally following Calonico (2017), but bandwidth was set at 20,000 population to ease visual comparison (the optimal bandwidths being slightly over 22,000 and 28,000 population respectively). Data sources are the 1980 Census, as well as and Home Owner Loan Corporation (HOLC) archival records.



(a) Pre-Period credit access (b) Post Mapping credit access

Fig. A20. Impact of redlining on credit access: within-city theoretical diagrams. **Note:** Each figure shows a theoretical regression discontinuity diagram. In both panels, the running variable is the distance away from the redline on Home Owners Loan Corporation (HOLC) security maps, and the threshold is the redline itself. In the left panel, I depict credit access as a continuous and linear function of distance away the redline; this is the situation we expect to hold prior to the creation of a redline border. In the right panel, I depict credit access having been differentially affected by the creation of a HOLC border.



(a) Predicted Murders (2010) (b) Actual Murders (2010)

Fig. A21. Residential security map, murder in selected Los Angeles neighborhoods. **Note:** Figure shows the location of murders relative to the Residential Security Maps constructed for selected Los Angeles neighborhoods. Panel a shows murder as predicted by block population counts. Panel b shows observed murders in 2010. See Fig. 1 for the full city.

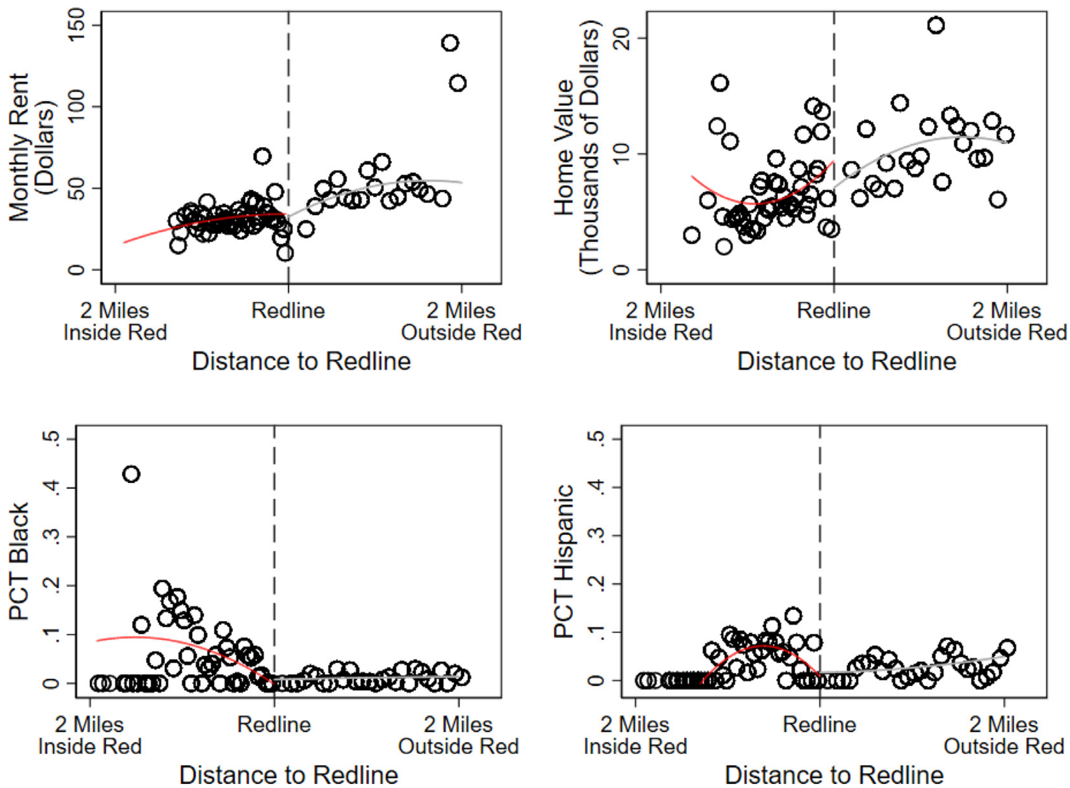


Fig. A22. Balancing tests: within-city 1920–1930 covariates. **Note:** Each figure shows a spatial regression discontinuity diagram where the dependent variable is a given pre-period covariate, measured in Los Angeles from 1920–1930. The top panels show results for self-reported monthly rent and home value, respectively, while the bottom panels show results for the percent of a neighborhood that is Black and the percent that is Hispanic, respectively. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size is fixed at 2 miles to ease comparison. The running variable always is the distance away from the redline on Home Owners Loan Corporation (HOLC) security maps and the threshold is the redline itself. The sample is restricted to areas which received some HOLC color designation in 1939. Data sources are 1920 and 1930 Census data and HOLC archival records.

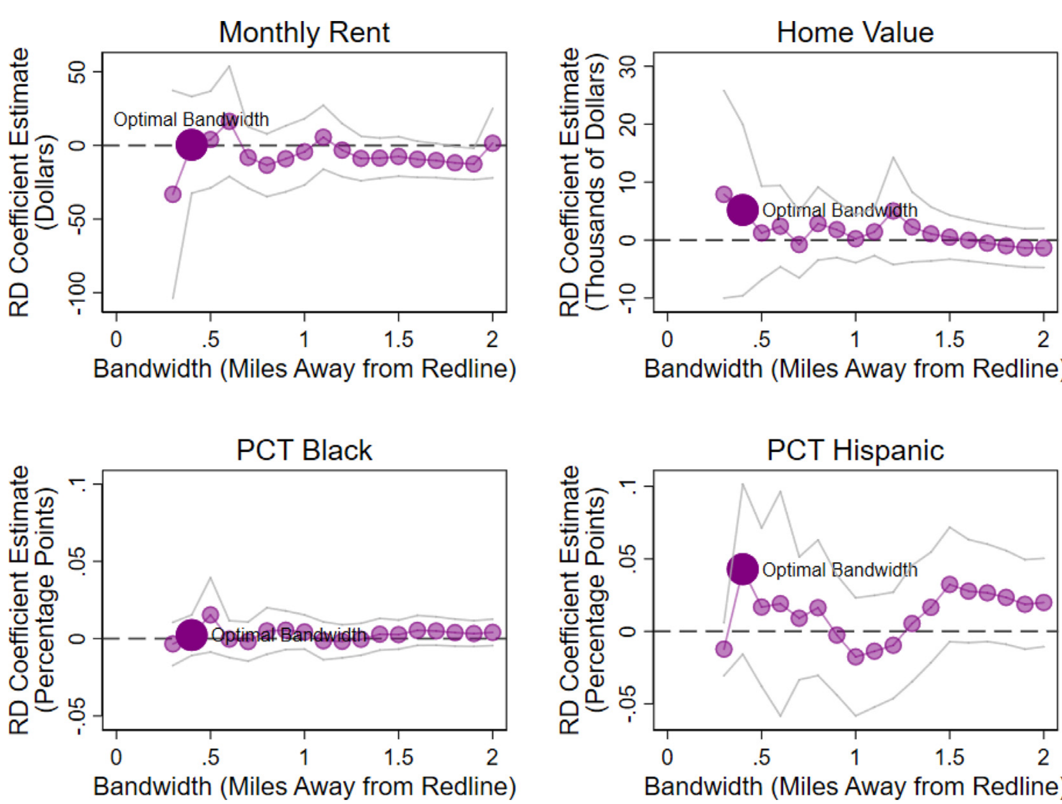


Fig. A23. Balancing tests: within-city 1920–1930 covariate estimates, by bandwidth. **Note:** Each figure shows a profile of spatial regression discontinuity coefficient estimates across a range of bandwidth selections. The top panels show results for self-reported monthly rent and home value, respectively, while the bottom panels show results for the percent of a neighborhood that is Black and the percent that is Hispanic, respectively. Circles represent estimates, with the large circle representing the estimate for the optimal bandwidth, In all specifications, the sample is restricted to areas which received some HOLC color designation in 1939. Data sources are 1920 and 1930 Census data and HOLC archival records.

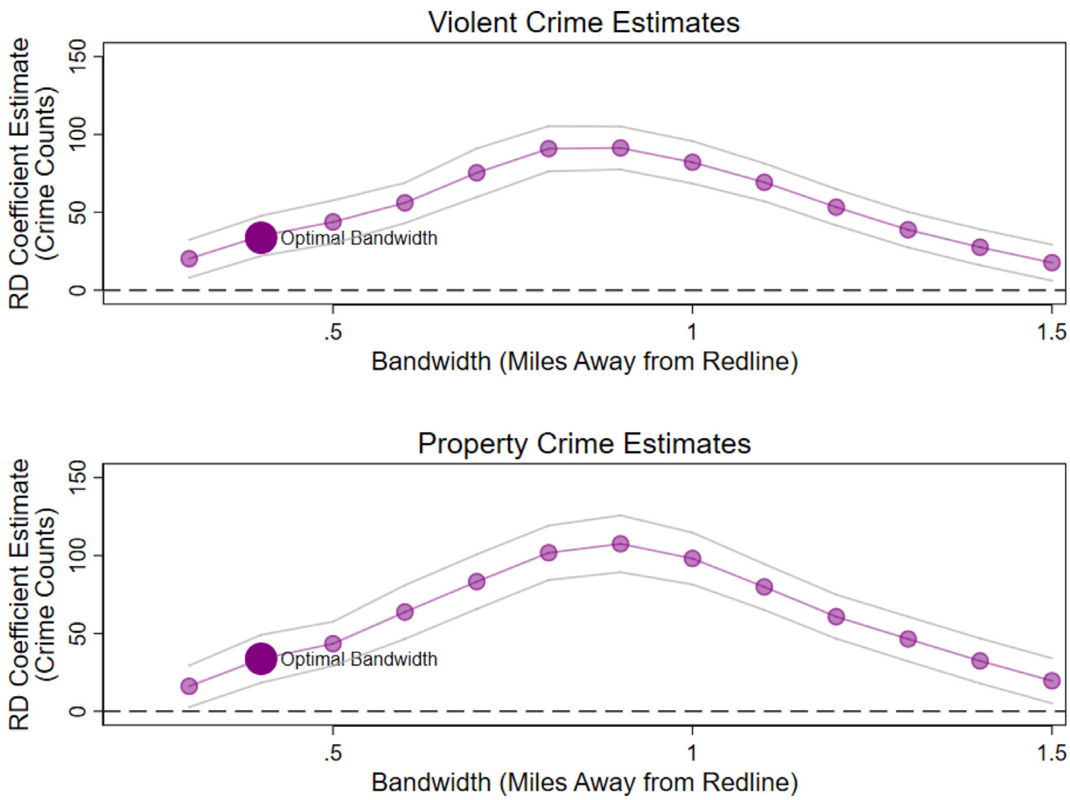


Fig. A24. Impact of redlining on crime: within-city estimates, bandwidth sensitivity. **Note:** Each figure shows a profile of regression discontinuity coefficient estimates across a range of bandwidth selections. The outcome is always crimes in Los Angeles in 2010. The top panel is restricted to property crimes, while the bottom panel is restricted to violent crimes. Property crimes are defined as those crimes the description of which contains words such as "burglary" and "larceny"; violent crimes are defined as those crimes the description of which contained words such as "murder" and "robbery". Circles represent estimates, with the large circle representing the estimate for the optimal bandwidth, the bandwidth displayed in Fig. 6 and reported in Table 2. In all specifications, the sample is restricted to areas which received some HOLC color designation in 1939. Data sources are city of Los Angeles crime data and HOLC archival records.

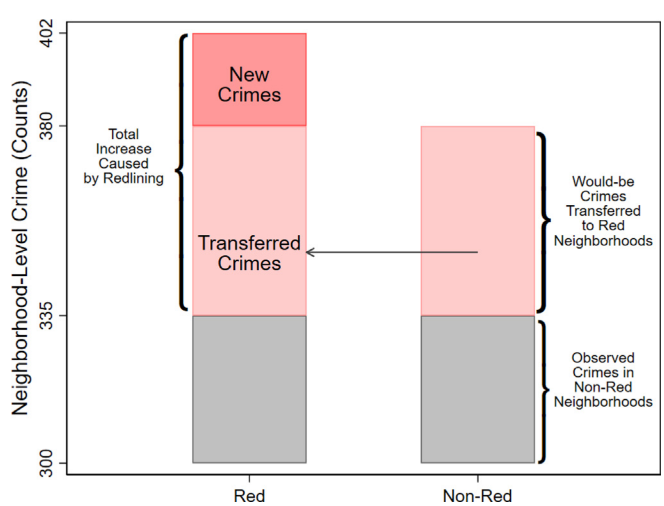


Fig. A25. Comparing size of between-city and within-city estimates. **Note:** Figure depicts a back of the envelope comparison between the city level estimates and neighborhood level estimates of the impact of redlining on crime. Crime volumes in a representative redlined neighborhood and neighborhood assigned some color designation other than red are compared. The within-city or neighborhood level estimate (a 67 crime increase) is depicted as the "Total Increase Caused by Redlining". The (population scaled) between-city or city level estimate (a 22 crime increase) is depicted as the "New Crimes" bar. The "Transferred Crimes" bar is the difference.

Table A1
Summary statistics.

Panel A: City Level	Mapped	Non-Mapped	Total
Crime Victimizations			
All (White)	765.61	477.11	569.15
	(1385.38)	(1060.01)	(1179.46)
Violent (White)	101.03	57.19	71.17
	(179.96)	(129.90)	(148.89)
Property (White)	664.58	419.93	497.98
	(1223.03)	(936.79)	(1041.33)
All (Black)	262.74	95.59	148.92
	(729.44)	(323.91)	(496.21)
Violent (Black)	70.61	22.20	37.65
	(194.54)	(75.94)	(128.21)
Property (Black)	192.13	73.39	111.27
	(543.87)	(249.91)	(373.33)
All (Hispanic)	90.97	47.74	61.54
	(309.95)	(175.13)	(227.46)
Violent (Hispanic)	18.48	9.12	12.11
	(52.81)	(29.42)	(38.63)
Property (Hispanic)	72.50	38.62	49.43
	(260.30)	(149.61)	(192.26)
Observations	119	254	373
1930 Demography			
City Population	48,640.00	28,954.92	33,343.31
	(6,933.42)	(7,893.42)	(11,238.84)
PCT White	0.91	0.94	0.93
	(0.12)	(0.11)	(0.11)
PCT Black	0.09	0.06	0.07
	(0.12)	(0.11)	(0.11)
PCT Naturalized Citizens	0.07	0.08	0.08
	(0.06)	(0.06)	(0.06)
PCT Married (Spouse Present)	0.42	0.42	0.42
	(0.04)	(0.05)	(0.05)
PCT HH Having a Radio	0.45	0.48	0.47
	(0.16)	(0.18)	(0.18)
PCT in School	0.22	0.22	0.22
	(0.03)	(0.04)	(0.04)
PCT Literate	0.80	0.79	0.80
	(0.04)	(0.05)	(0.05)
PCT in Labor Force	0.42	0.41	0.41
	(0.04)	(0.05)	(0.05)
PCT Wage Workers	0.39	0.37	0.37
	(0.04)	(0.05)	(0.05)
Average Home Value ($)	6,976.20	6,636.16	6,711.97
	(2,955.04)	(4,276.88)	(4,018.21)
Average Rental Amount ($)	31.19	31.04	31.07
	(11.51)	(13.67)	(13.20)
Observations	70	244	314

Note: Means reported with standard errors in parentheses. Sample is restricted to observations for cities with a 1930 population between 20,000 and 60,000 people. Distributions are reported separately for cities which were redline mapped and for those not mapped. Panel C Crime Victimization: Source is NIBRS 2015 Crime Victimization Data. Observations are at the agency level. Panel C 1930 Demography: Source is address level 1920–1930 Census Data. Observations are at the city level.

Table A2
Summary statistics.

Panel B: City Level, Continued	Mapped	Non-Mapped	Total
Criminal Arrests			
All (White)	438.25	300.83	347.68
	(911.49)	(547.62)	(695.45)
Violent (White)	114.87	70.57	85.67
	(271.90)	(163.89)	(207.89)
Property (White)	323.38	230.26	262.00
	(683.40)	(415.58)	(523.61)
All (Black)	271.60	116.70	169.50
	(511.23)	(231.70)	(359.80)
Violent (Black)	95.70	36.40	56.62
	(200.39)	(71.26)	(133.28)
Property (Black)	175.90	80.30	112.89

Table A2 (*continued*)

Panel B: City Level, Continued	Mapped	Non-Mapped	Total
	(337.32)	(167.95)	(243.40)
Observations	150	290	440

Note: Means reported with standard errors in parentheses. Sample is restricted to observations for cities with a 1930 population between 20,000 and 60,000 people. Distributions are reported separately for cities which were Redline-Mapped and for those not mapped. Panel B Criminal Arrests: Source is UCR 2015 Arrest Data. Observations are at the agency level.

Table A3
Summary statistics, Los Angeles.

Panel C: Neighborhood Level (Los Angeles)	Red	Non-Red	Total
2010 Crime Victimizations			
Distance to Redline (Miles)	−0.51	1.70	1.36
	(0.30)	(1.09)	(1.29)
Property Crimes	1991.00	1021.06	1170.83
	(2718.07)	(2102.43)	(2230.74)
Violent Crimes	1739.69	683.29	846.41
	(2853.51)	(2535.18)	(2609.44)
All Crimes	5756.83	2531.23	3029.30
	(8582.86)	(6936.69)	(7291.70)
1930 Demography			
PCT White	0.94	0.98	0.97
	(0.16)	(0.08)	(0.10)
PCT Black	0.02	0.01	0.01
	(0.05)	(0.02)	(0.03)
PCT Japanese	0.01	0.01	0.01
	(0.02)	(0.07)	(0.07)
PCT Non-Hispanic	0.92	0.98	0.97
	(0.12)	(0.06)	(0.08)
PCT Mexican	0.07	0.02	0.03
	(0.11)	(0.06)	(0.07)
PCT Native Born	0.53	0.62	0.60
	(0.19)	(0.21)	(0.21)
PCT Married (Spouse Present)	0.42	0.46	0.45
	(0.12)	(0.17)	(0.16)
PCT Have Children in HH	0.15	0.17	0.16
	(0.09)	(0.11)	(0.11)
PCT Have Children $\leqslant$5 in HH	0.03	0.02	0.02
	(0.04)	(0.05)	(0.05)
PCT Have a Radio	0.58	0.77	0.73
	(0.23)	(0.24)	(0.24)
PCT In School	0.20	0.21	0.20
	(0.10)	(0.13)	(0.12)
PCT Literate	0.85	0.85	0.85
	(0.08)	(0.12)	(0.11)
PCT In Labor Force	0.46	0.44	0.44
	(0.15)	(0.17)	(0.17)
PCT Self-Employed	0.06	0.11	0.10
	(0.04)	(0.15)	(0.14)
PCT Works for Wages	0.40	0.34	0.35
	(0.15)	(0.14)	(0.15)
House Value	8089.84	14539.23	13218.89
	(5519.34)	(22786.69)	(20614.56)
Rent Value	31.83	55.75	50.09
	(12.43)	(56.83)	(50.97)
PCT No Mortgage - Own Free and Clear	0.04	0.03	0.03
	(0.05)	(0.10)	(0.09)
PCT Have a Mortgage	0.05	0.03	0.03
	(0.05)	(0.05)	(0.05)
Observations	42	230	272

Note: Means reported with standard errors in parentheses. All observations are at the neighborhood level, neighborhoods being delineated accordingly to boundaries drawn by HOLC surveyors. Distributions are reported separately for neighborhoods assigned red ("redlined") and those assigned any other color. Panel C (2010 Crime Victimizations): Source is City of Los Angeles geocoded crime data (2010). The distance to redline variable measures the distance from a crime to the nearest redline (distances inside a red neighborhood out to its border being coded with negative values). Crimes are limited to UCR Type 1 crimes and broken down by property and violent crimes, respectively. Panel C (1930 Demography): Source is geocoded 1920–1930 decennial Census.

Table A4
List of cities in main redline mapping bandwidth.

City	Population	White	Black	Hispanic	Rent	Home Value
Alton, Il	30,100	0.94	0.06	0.01	$15	$956
Santa Ana, Ca	30,300	0.99	0.00	0.00	$18	$1,900
Everett, Wa	30,500	0.99	0.00	0.00	$8	$838
Newark, Oh	30,500	0.99	0.01	0.00	$8	$1,887
Baton Rouge, La	30,700	0.60	0.40	0.01	$12	$4,224
Bellingham, Wa	30,800	1.00	0.00	0.00	$4	$1,523
Hagerstown, Md	30,800	0.96	0.03	0.00	$60	$1,217
Bloomington, Il	30,900	0.97	0.03	0.00	$8	$2,201
Marion, Oh	31,000	0.99	0.01	0.00	$12	$1,198
Newburgh, Ny	31,200	0.96	0.04	0.00	$14	$2,506
Port Huron, Mi	31,300	0.97	0.03	0.00	$46	$1,105
Fort Smith, Ar	31,400	0.85	0.15	0.00	$8	$1,811
Nashua, Nh	31,400	1.00	0.00	0.00	$14	$1,847
Pensacola, Fl	31,500	0.69	0.31	0.02	$13	$1,967
Meridian, Ms.	31,900	0.62	0.38	0.00	$10	$1,648
Muskogee, Ok	32,000	0.72	0.24	0.00	$9	$2,270
Moline, Il	32,200	0.99	0.01	0.00	$22	$2,108
Watertown, Ny	32,200	1.00	0.00	0.00	$13	$1,839
Wilmington, Nc	32,200	0.55	0.45	0.00	$12	$1,969
Rome, Ny	32,300	0.99	0.01	0.00	$11	$2,053
Norwich, Ct	32,400	0.98	0.02	0.00	$15	$1,169
Richmond, In	32,400	0.94	0.06	0.00	$20	$1,577
Tucson, Az	32,500	0.94	0.05	0.01	$22	$1,384
Laredo, Tx	32,600	1.00	0.00	0.01	$9	$508
Kokomo, In	32,800	0.95	0.05	0.00	$8	$2,361
Elkhart, In	32,900	0.99	0.01	0.00	$7	$3,857
Greenwich, Ct	33,100	0.97	0.03	0.00	$93	$18,760
Colorado Springs, Co	33,200	0.98	0.02	0.00	$6	$2,697
Sioux Falls, Sd	33,300	0.99	0.00	0.00	$12	$2,028
Joplin, Mo	33,400	0.97	0.03	0.01	$8	$1,696
Norwood, Oh	33,400	1.00	0.00	0.01	$70	$5,522
Waukegan, Il	33,400	0.97	0.03	0.00	$30	$2,057
Mansfield, Oh	33,500	0.98	0.02	0.00	$22	$2,174
Paducah, Ky	33,500	0.84	0.16	0.00	$11	$1,921
Santa Barbara, Ca	33,600	0.97	0.00	0.01	$13	$3,498
Easton, Pa	34,400	0.99	0.01	0.00	$12	$3,482
Newport News, Va	34,400	0.61	0.38	0.00	$11	$716
Plainfield, Nj	34,400	0.92	0.08	0.00	$23	$3,194
New Brunswick, Nj	34,500	0.92	0.08	0.01	$14	$3,841
West Allis, Wi	34,600	1.00	0.00	0.00	$17	$2,930
Amsterdam, Ny	34,800	1.00	0.00	0.01	$16	$1,921
Lewiston, Me	34,900	0.99	0.01	0.00	$19	$2,879
Watertown, Ma	34,900	1.00	0.00	0.00	$22	$7,233
Alameda, Ca	35,000	0.97	0.01	0.01	$14	$2,972
Orange, Nj	35,300	0.85	0.15	0.00	$22	$2,765
Steubenville, Oh	35,400	0.93	0.07	0.00	$22	$2,610
Revere, Ma	35,600	1.00	0.00	0.00	$24	$1,688
Norristown Borough, Pa	35,800	0.95	0.05	0.00	$9	$3,407
White Plains, Ny	35,800	0.93	0.07	0.02	$41	$4,669
Elgin, Il	35,900	0.99	0.01	0.00	$9	$4,036
Arlington, Ma	36,000	1.00	0.00	0.00	$18	$5,447
Norwalk, Ct	36,000	0.97	0.03	0.01	$17	$1,656
Superior, Wi	36,100	0.99	0.00	0.00	$28	$919
Zanesville, Oh	36,400	0.96	0.04	0.00	$6	$3,262
Auburn, Ny	36,600	0.99	0.01	0.01	$5	$2,496
Danville, Il	36,700	0.93	0.07	0.00	$13	$2,779
Hazleton, Pa	36,700	1.00	0.00	0.00	$25	$3,492
High Point, Nc	36,700	0.79	0.21	0.00	$10	$3,937
Santa Monica, Ca	37,100	0.97	0.01	0.01	$24	$2,211
West New York, Nj	37,100	1.00	0.00	0.01	$52	$1,515
Raleigh, Nc	37,300	0.61	0.39	0.01	$19	$2,787
Taunton, Ma	37,300	0.99	0.00	0.01	$22	$1,081
Green Bay, Wi	37,400	0.99	0.00	0.00	$12	$2,351
San Bernardino, Ca	37,400	0.98	0.01	0.00	$7	$1,587
Cumberland, Md	37,700	0.97	0.03	0.00	$18	$4,776
Rock Island, Il	37,900	0.99	0.01	0.00	$17	$1,600
Bloomfield, Nj	38,000	0.98	0.02	0.00	$36	$3,750
Meriden, Ct	38,400	1.00	0.00	0.00	$12	$2,371
Quincy, Il	39,200	0.99	0.01	0.00	$6	$1,973
Sheboygan, Wi	39,200	1.00	0.00	0.00	$12	$3,722
Waltham, Ma	39,200	1.00	0.00	0.00	$20	$1,434
Butte, Mt	39,500	0.97	0.01	0.00	$10	$1,692
East Cleveland, Oh	39,600	1.00	0.00	0.00	$60	$2,877
La Crosse, Wi	39,600	1.00	0.00	0.00	$18	$2,989

(*continued on next page*)

Table A4 (*continued*)

City	Population	White	Black	Hispanic	Rent	Home Value
Anderson, In	39,800	0.96	0.04	0.00	$6	$3,542
Oshkosh, Wi	40,100	1.00	0.00	0.00	$6	$3,167
Ogden, Ut	40,200	0.99	0.00	0.00	$19	$2,616
Poughkeepsie, Ny	40,200	0.97	0.03	0.00	$10	$5,710
Saint Petersburg, Fl	40,400	0.79	0.21	0.00	$10	$8,595
Fitchburg, Ma	40,600	1.00	0.00	0.00	$24	$1,434
Lynchburg, Va	40,600	0.75	0.25	0.00	$10	$2,560
Kearny, Nj	40,700	1.00	0.00	0.00	$22	$3,503
Warren, Oh	41,000	0.94	0.06	0.00	$18	$1,430
Muskegon, Mi	41,300	0.98	0.02	0.00	$9	$2,475
Dubuque, Ia	41,600	1.00	0.00	0.00	$12	$1,123
Council Bluffs, Ia	42,000	0.99	0.01	0.00	$11	$2,100
Montclair, Nj	42,000	0.82	0.18	0.00	$52	$8,608
Lima, Oh	42,200	0.96	0.04	0.00	$18	$1,620
Portsmouth, Oh	42,500	0.96	0.04	0.01	$15	$3,031
Cranston, Ri	42,900	0.99	0.01	0.00	$9	$2,946
Joliet, Il	42,900	0.97	0.03	0.00	$13	$2,467
Amarillo, Tx	43,100	0.96	0.04	0.00	$20	$1,347
Columbus, Ga	43,100	0.63	0.37	0.00	$16	$641
Salem, Ma	43,300	1.00	0.00	0.00	$15	$1,872
Battle Creek, Mi	43,500	0.96	0.04	0.00	$29	$2,259
Perth Amboy, Nj	43,500	0.97	0.03	0.01	$18	$2,441
Wichita Falls, Tx	43,600	0.89	0.11	0.00	$13	$1,299
Chicopee, Ma	43,900	1.00	0.00	0.00	$17	$1,685
Lorain, Oh	44,500	0.97	0.03	0.00	$6	$4,594
Jamestown, Ny	45,100	0.99	0.01	0.00	$8	$4,244
Lexington, Ky	45,700	0.71	0.29	0.00	$26	$2,026
Portsmouth, Va	45,700	0.60	0.40	0.00	$11	$893
Williamsport, Pa	45,700	0.98	0.02	0.00	$15	$2,611
Chelsea, Ma	45,800	0.99	0.01	0.00	$24	$756
Waterloo, Ia	46,100	0.97	0.03	0.00	$8	$2,660
Aurora, Il	46,500	0.98	0.02	0.00	$21	$3,294
Muncie, In	46,500	0.92	0.07	0.00	$5	$3,522
Clifton, Nj	46,800	1.00	0.00	0.00	$7	$7,702
Berwyn, Il	47,000	1.00	0.00	0.00	$20	$5,126
Bay City, Mi	47,300	1.00	0.00	0.00	$10	$1,418
Elmira, Ny	47,300	0.97	0.03	0.01	$14	$1,985
Brookline, Ma	47,400	0.99	0.01	0.00	$62	$9,149
Stockton, Ca	47,900	0.94	0.01	0.01	$21	$943
Phoenix, Az	48,100	0.93	0.07	0.00	$30	$1,295
Jackson, Ms.	48,200	0.59	0.41	0.00	$18	$1,989
Everett, Ma	48,400	0.99	0.01	0.00	$24	$1,657
New Castle, Pa	48,600	0.97	0.03	0.00	$14	$2,235
Haverhill, Ma	48,700	1.00	0.00	0.00	$13	$1,912
Woonsocket, Ri	49,300	1.00	0.00	0.00	$26	$5,074
Pittsfield, Ma	49,600	0.99	0.01	0.01	$20	$2,760
Pueblo, Co	50,000	0.98	0.02	0.00	$28	$1,516

Note: Reported cities all have a 1930 population between 30,000 and 50,000, the redline mapping cutoff being 40,000. There are 315 cities in the 1930 Census with population between 20,000 and 60,000, 231 of which have population between 20,000 and 40,000. Data sources are 1930 Census and HOLC archival records.

Table A5
Further balancing tests: within-city 1920–1930 covariates.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Household Variables								
	Have Radio	Nmb Families	Nmb Subfamilies	Nmb Married	Nmb Mothers	Nmb Fathers	Own Home	Rent Home
RD Estimate	0.16	0.68	0.25	−0.15	0.90	1.13	−0.61	1.15
	(0.900)	(0.335)	(0.440)	(0.804)	(0.361)	(0.243)	(0.458)	(0.182)
	[0.974]	[0.748]	[0.748]	[0.945]	[0.748]	[0.748]	[0.748]	[0.748]
Observations	7449	10140	10140	10140	10140	10140	10140	10140
Mean (Bandwidth)	.54	1.6	.09	.76	.70	.55	.33	.58
Mean (Non-Red)	.61	1.54	.09	.84	.78	.64	.44	.51
Panel B: Family Formation Variables								
	Nmb Family Members	Nmb Children	Have Children <5	Female	Spouse Present	Spouse Absent	Divorced	Single
RD Estimate	1.19	0.35	0.05	0.36	−0.46	−0.10	0.01	0.14
	(0.751)	(0.722)	(0.254)	(0.377)	(0.321)	(0.461)	(0.865)	(0.751)
	[0.929]	[0.929]	[0.748]	[0.748]	[0.748]	[0.748]	[0.974]	[0.929]
Observations	10140		10140	10140	10140	10140	10140	10140
Mean (Bandwidth)	3.41	.54	.02	.51	.41	.04	.03	.43
Mean (Non-Red)	3.64	.63	.03	.5	.44	.04	.02	.42
Panel C: Race and Class Variables								
	White	Chinese	Japanese	Asian/Pacific Islander	Cuban	Native Born	Mother Foreign Born	Foreign Born
RD Estimate	−0.00	−0.00	0.18	−0.13	−0.01	−0.07	0.02	−0.01
	(0.988)	(0.322)	(0.342)	(0.271)	(0.368)	(0.911)	(0.872)	(0.984)
	[0.988]	[0.748]	[0.748]	[0.748]	[0.748]	[0.974]	[0.974]	[0.988]
Observations	10140	10140	10140	10140	10140	10140	10140	10140
Mean (Bandwidth)	.96	0	.01	.01	0	.52	.03	.2
Mean (Non-Red)	.96	0	.02	0	.01	.6	.03	.17
Panel D: Education and Labor Force								
	Not In School	In School	Illiterate	Literate	Not in Labor Force	In Labor Force	Self-Employed	Works for Wages
RD Estimate	−0.34	0.34	−0.03	−0.36	0.20	−0.59	−0.82*	0.25
	(0.413)	(0.413)	(0.739)	(0.127)	(0.586)	(0.247)	(0.076)	(0.551)
	[0.748]	[0.748]	[0.929]	[0.748]	[0.835]	[0.748]	[0.715]	[0.810]
Observations	10140	10140	10140	10140	10140	10140	10140	10140
Mean (Bandwidth)	.82	.18	.02	.86	.31	.49	.08	.42
Mean (Non-Red)	.79	.21	.01	.84	.34	.43	.07	.36

Note: The reported coefficients are spatial regression discontinuity estimates of whether there is an "effect" of redlining on the given pre-period variable, with p-values reported underneath. The running variable is always the distance away from the redline on Home Owners Loan Corporation (HOLC) security maps and the threshold is the redline itself. Two p-values are reported: first, the standard p-value for a two-tailed hypothesis test is reported in parentheses, and, second, multiple-inference corrected p-values are reported in brackets. Bandwidth size is fixed at.4 miles to ease comparison. The sample is restricted to areas which received some HOLC color grade in 1939. Sample size is smaller for "Have Radio" because some households in the sample did not answer this survey question. Two means are reported: means within the bandwidth and means across all neighborhoods, regardless of bandwidth, given a color grade other than red. Data sources are 1920–1930 Census and HOLC archival records. Significance levels for standard p-values indicated by: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B1
Population and agency counts of NIBRS reporting agencies.

	(1) Raw Measure	(2) Residualized
Population Served (2015)		
RD Estimate	22857.0	−1911.2
	(30031.1)	(31267.9)
Mean (Bandwidth)	49526.0	−277.5
Census Population (2015)		
RD Estimate	23103.4	−2574.2
	(48051.4)	(42687.1)
Mean (Bandwidth)	134610.8	−30796.0
Count Agencies (2015)		
RD Estimate	0.689	−0.0233
	(0.909)	(0.282)
Mean (Bandwidth)	1.00	0.08
Count Cities (Bandwidth)	198	198

Note: Each cell represents a separate regression discontinuity estimate of Eq. (1). In each cell of column (1), the variable listed in each row is the outcome variable. In each cell of column (2), the variable listed in each row is first regressed against state fixed effects, and the residuals from that equation are used as the outcome variable in the regression discontinuity estimate.

Appendix B. Comparing NIBRS to UCR results

The overall tradeoff between NIBRS and UCR measures is that while UCR measures exist for earlier years and for all cities in the bandwidth in more recent years, NIBRS measures are generally considered higher quality (Lantz, 2021). In this section, I show that the city level choice to report to the NIBRS database is not driving the main results, that UCR results are similar to NIBRS results (even when restricting the UCR sample to be the NIBRS sample), and that estimates are similar when using *Opportunity Atlas* data (which is compiled from Census and IRS data and is entirely independent of the UCR and NIBRS datasets).

B.1. City choice to report to NIBRS not driving results

RD estimates using the share of non-missing cities with a NIBRS report in 2015 as the outcome variable show a very noisy 8 percentage point (14%) increase in the likelihood of having a non-missing agency report as 1930 population crosses 40,000 (p=.78). The choice to fail to report to NIBRS is largely due to state level NIBRS participation rather than city level choices. When we residualize the non-missing reporting shares on state fixed effects, the increase across the threshold goes down to 2 percentage points (3%) and becomes even noisier (p=.96). Thus the state level decision to NIBRS report explains almost all of the already noisy increase in reporting. Results are similar if instead of share non-missing, we use total population served by reporting agencies in 2015, total census population for reporting agencies in 2015 and the county of reporting agencies in 2015. Table B1 shows that regression discontinuity results change from a noisy increase to a smaller and noisier decrease when we introduce state fixed effects.

B.2. NIBRS and UCR give comparable results

Fig. A14 shows that the city level estimates of the impact of redline mapping on crime using NIBRS are comparable to the estimates obtained using UCR data, which measure arrests by city by race. The estimates reported in Fig. A14 (obtained by estimating Eq. (1) on UCR data) imply that 61 additional Black arrests per city in 2015 are attributable to redline mapping. Thus, if we assume

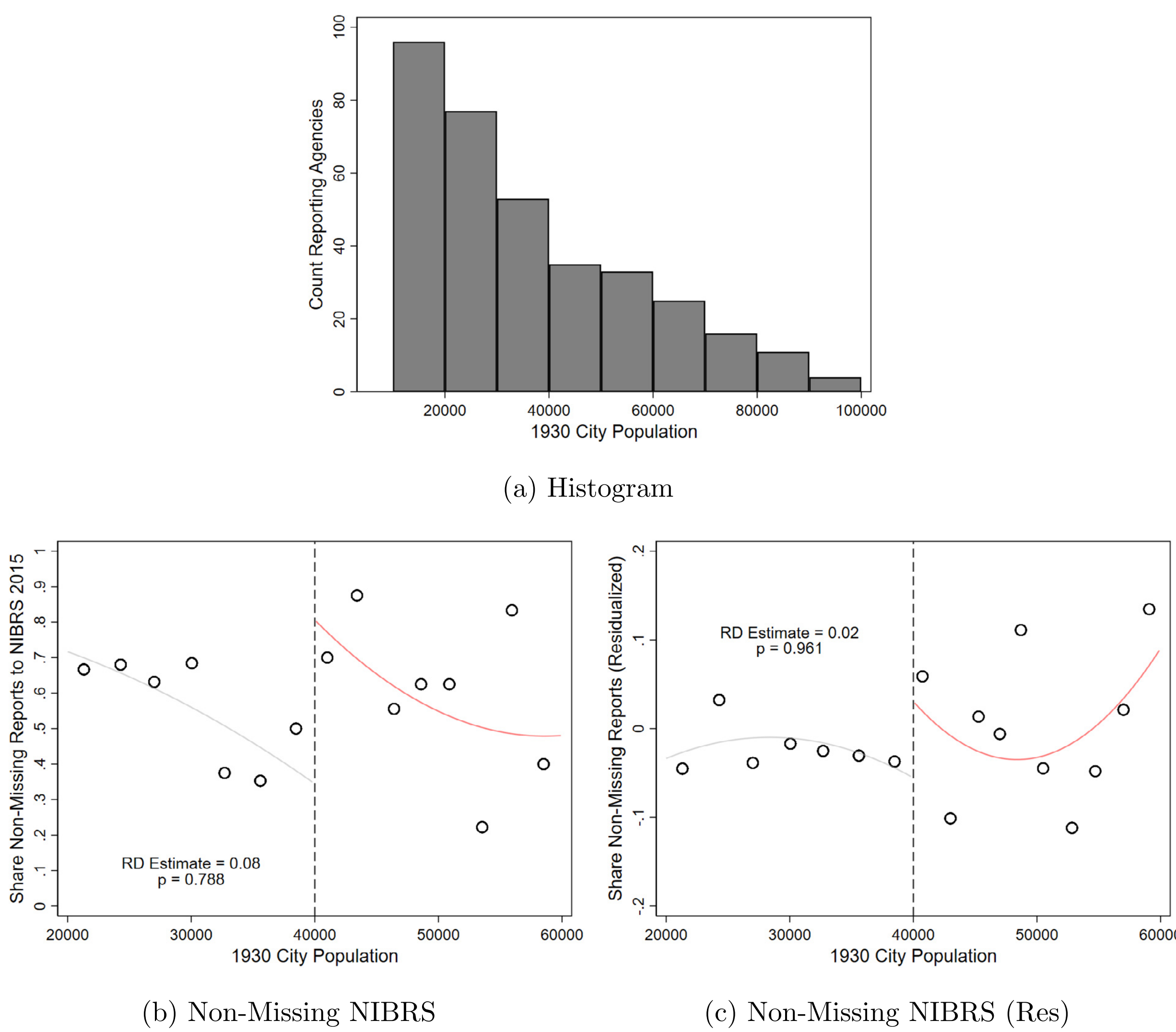
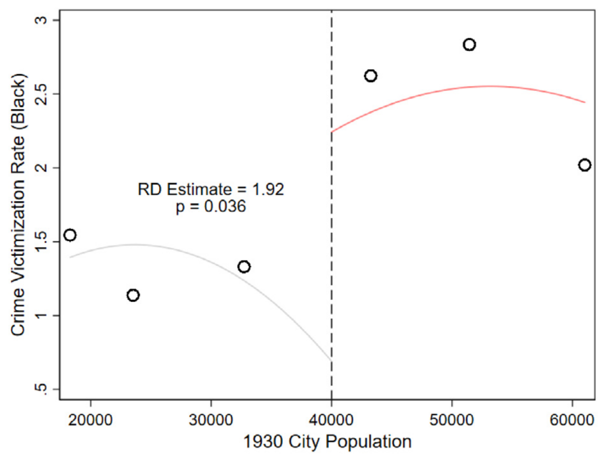
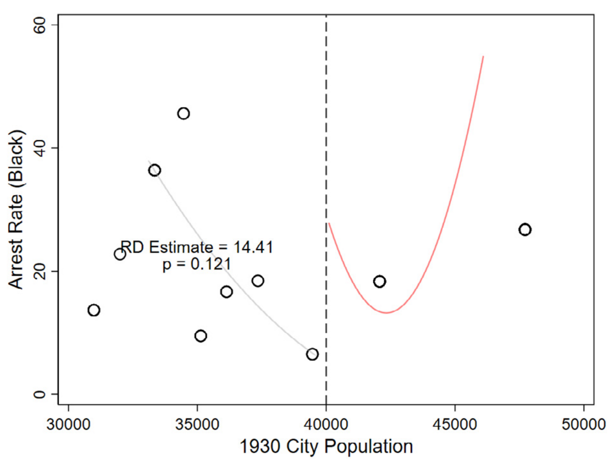


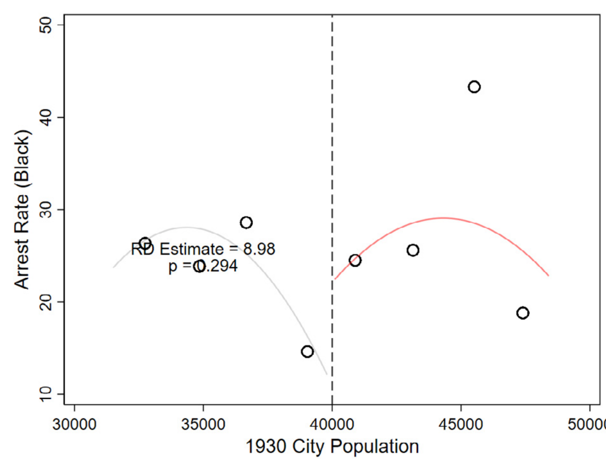
Fig. B1. City level choice to report to NIBRS. **Note:** Data sources are individual level NIBRS crime victimization data and Home Owner Loan Corporation (HOLC) archival records.



(a) NIBRS 2015



(b) UCR 2015 (NIBRS Subsample)



(c) UCR 2015 (Full UCR Sample)

Fig. B2. UCR estimates 2015 (restricted to NIBRS sample). **Note:** Each figure shows a regression discontinuity diagram where the outcome variable is a measure of Black crime. Panel a repeats Fig. 4. Panel C repeats Fig. A14. Panel B shows the same estimate as in Panel C, but where I restrict to those cities which reported to NIBRS in 2015 The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following Calonico (2017). Data sources are individual level NIBRS crime victimization data, UCR crime data and Home Owner Loan Corporation (HOLC) archival records.

that the additional Black arrests are for crime perpetrated against Black victims, these estimates would suggest an arrest rate of roughly 35%, which is not far from the national average for UCR Type 1 crimes.[35]

Furthermore, preliminary tests on the distribution of crime as measured by NIBRS and UCR reveal that these datasets give consistent measures of criminality by race. To test consistency, for example, I construct a variable that measures the difference between Black (UCR Type 1) crime victimizations reported to NIBRS and Black (UCR Type 1) criminal arrests reported to the UCR. This variable, which measures consistency between the two datasets, is very nearly mean zero, and, more importantly, does not jump at the 40,000 population threshold. This suggests that whatever noise there is in these data at the city level is an instance of classical measurement error, or at least not systematically connected to redline mapping.

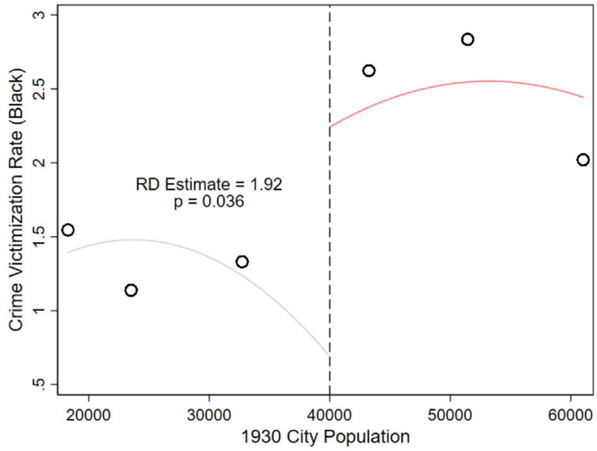
B.3. UCR estimates on NIBRS sample

To further ensure that the NIBRS estimates are similar to the UCR estimates and that city level choices to report to NIBRS are not driving the main results, I take the 2015 UCR sample and restrict the full UCR sample to the 2015 NIBRS sample, which is a proper subset of the UCR dataset. Fig. B2 compares RD diagrams for Black crimes for three specifications: 2015 NIBRS (main results), 2015 UCR on the full UCR sample, and 2015 UCR on the 2015 NIBRS sample. I find that increases across the threshold increase from 9 arrests per 1,000 to 14 arrests per 1,000 as I restrict the UCR data to the NIBRS subsample. These results suggest that NIBRS and UCR data yield comparable results.

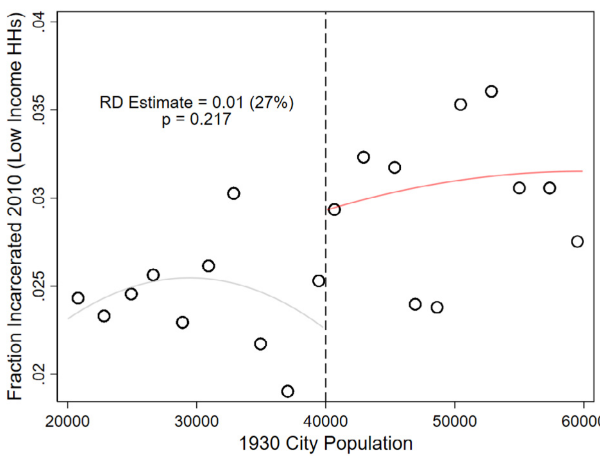
B.4. Comparison to Opportunity Atlas incarceration measures

As a final test that my estimates are not driven by anything idiosyncratic to the NIBRS sample, I use *Opportunity Atlas* (OI) measures of incarceration, which are based on the 2010 Census merged with tax records. While these measures are derived from the same Group Quarters variable in other decennial releases, these mea-

[35] Federal Bureau of Investigation (2010).



(a) NIBRS (2015)

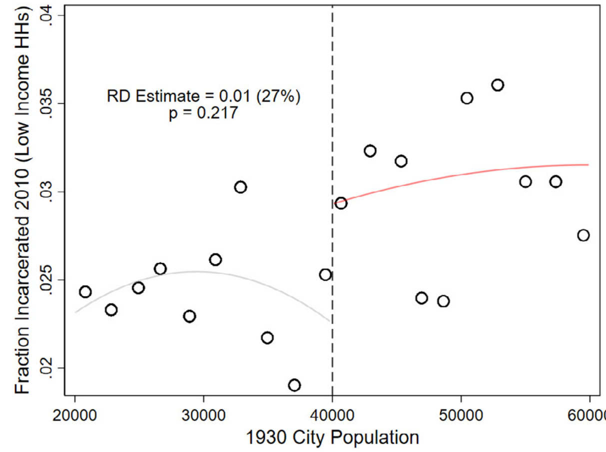


(b) Opportunity Atlas (April 1, 2010)

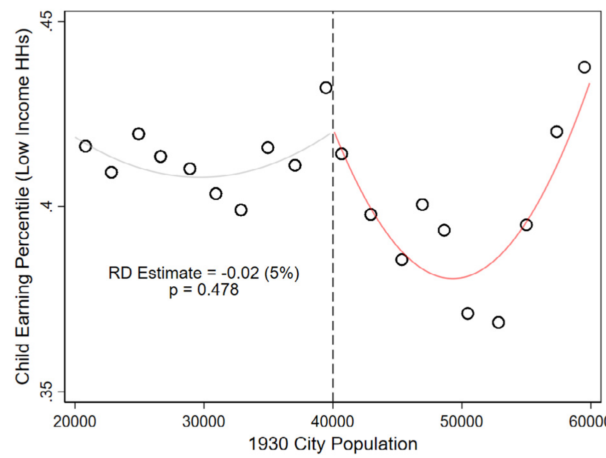
Fig. B3. Opportunity Atlas estimates. **Note:** Each figure shows a regression discontinuity diagram. Panel A repeats Fig. 4. Panel B estimates the same specification as in Fig. 4, but using *Opportunity Atlas* incarceration measures as the outcome variable. I use the incarceration measure which shows the share of children born into a given area into low-income households, pooling all races and genders. In the *Opportunity Atlas* data, a "low income" household is a household in which the parents were in the 25th percentile of their cohort's earnings distribution. Data are at the county level. Data sources are county level OI incarceration data and Home Owner Loan Corporation (HOLC) archival records.

sures are preferable since they show the fraction of children who *grew up in an area* who were in prison or jail on April 1, 2010. While OI measures enjoy coverage over all 1930 cities, they feature less than ideal birth cohorts (1978–1983).

Fig. B3 shows that share incarcerated jumps across the threshold by approximately 1 percentage point (27%), but that this estimate is imprecise. This imprecision is likely due to two sources: first, the birth cohorts (1978–1983) have already experienced two generations of migrations since redline mapping was first implemented, and second incarceration for those born into an area is merely a proxy for criminal perpetration in that area since children could move into or away from redlined cities before adulthood. Accordingly, I interpret the fact that OI incarceration measures gives comparable results to crime measures in NIBRS and UCR merely as further confirmation that the core estimates are not driven by anything idiosyncratic to the NIBRS sample.



(a) Opportunity Atlas Incarceration



(b) Opportunity Atlas Earnings

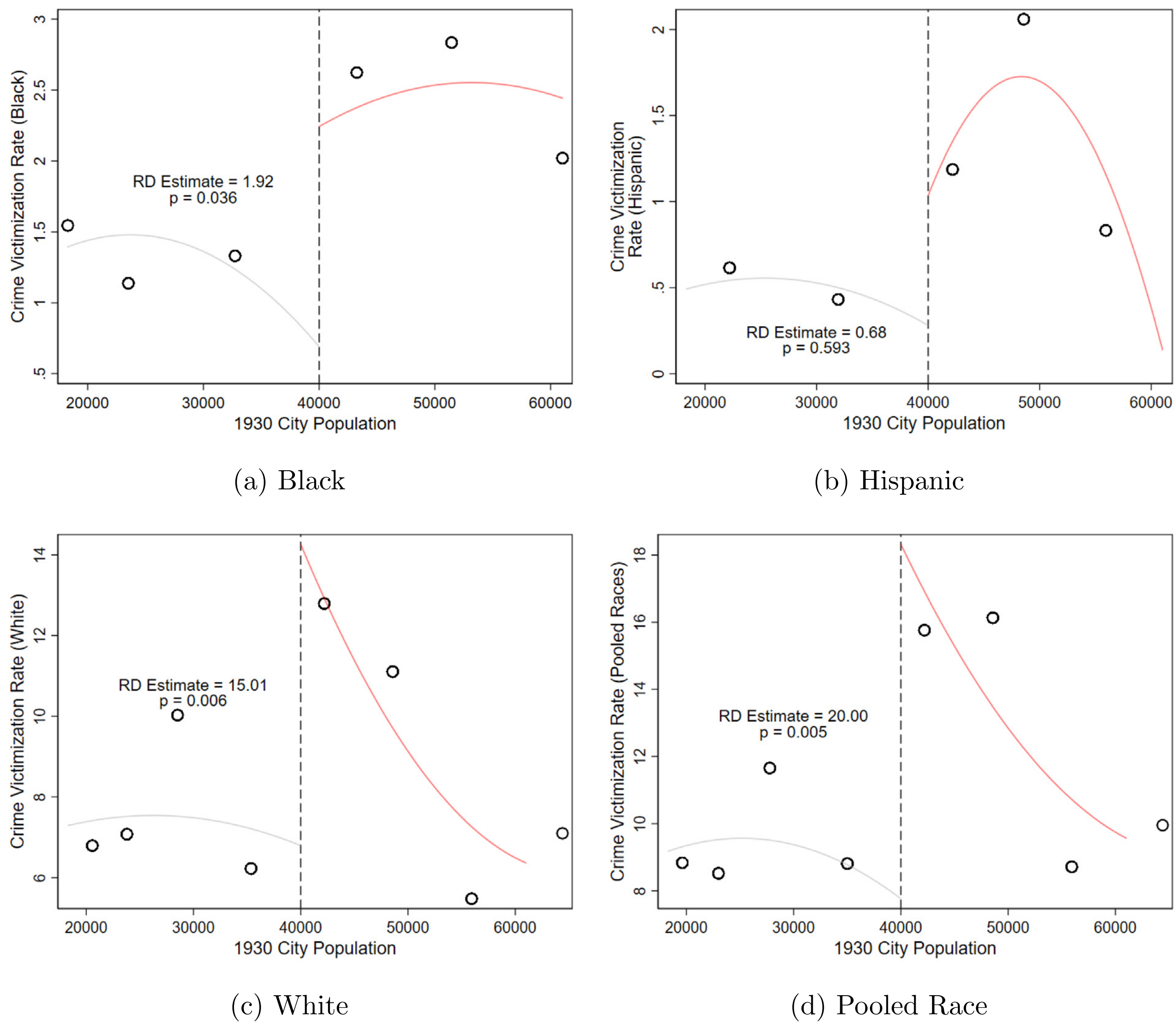
Fig. B4. Opportunity Atlas estimates. **Note:** Each figure shows a regression discontinuity diagram. Panel A repeats Fig. B3b. Panel B estimates the same specification as in Fig. B3b, but using *Opportunity Atlas* earnings measures as the outcome variable. I use the child's earnings percentile which shows that average earnings percentile for children who born into a given area into low-income households, pooling all races and genders. In the *Opportunity Atlas* data, a "low income" household is a household in which the parents were in the 25th percentile of their cohort's earnings distribution. Data are at the county level. Data sources are county level OI incarceration and earnings data and Home Owner Loan Corporation (HOLC) archival records.

Appendix C. White and pooled race crime results: complete race by type interactions

C.1. City-level, by race by crime type

The main between-city results in Table 1 do not include White crime victimization results or pooled race victimization results. The ex ante motivation for focusing on racial minority groups lies in the racialized nature of redlining (See Appendix H.) However, if redlining increased overall crime volume and affected large segments of hundreds of cities, it is reasonable to think that redlining affected White crime victimization.[36] The ex post motivation for not

[36] Note that ex ante it would be reasonable to think that redlining increased White crime victimization even as it increased racial segregation, since it we might expect that White residents of once redlined and now predominately Black neighborhoods would experience increased crime as a result of redlining.



(a) Black

(b) Hispanic

(c) White

(d) Pooled Race

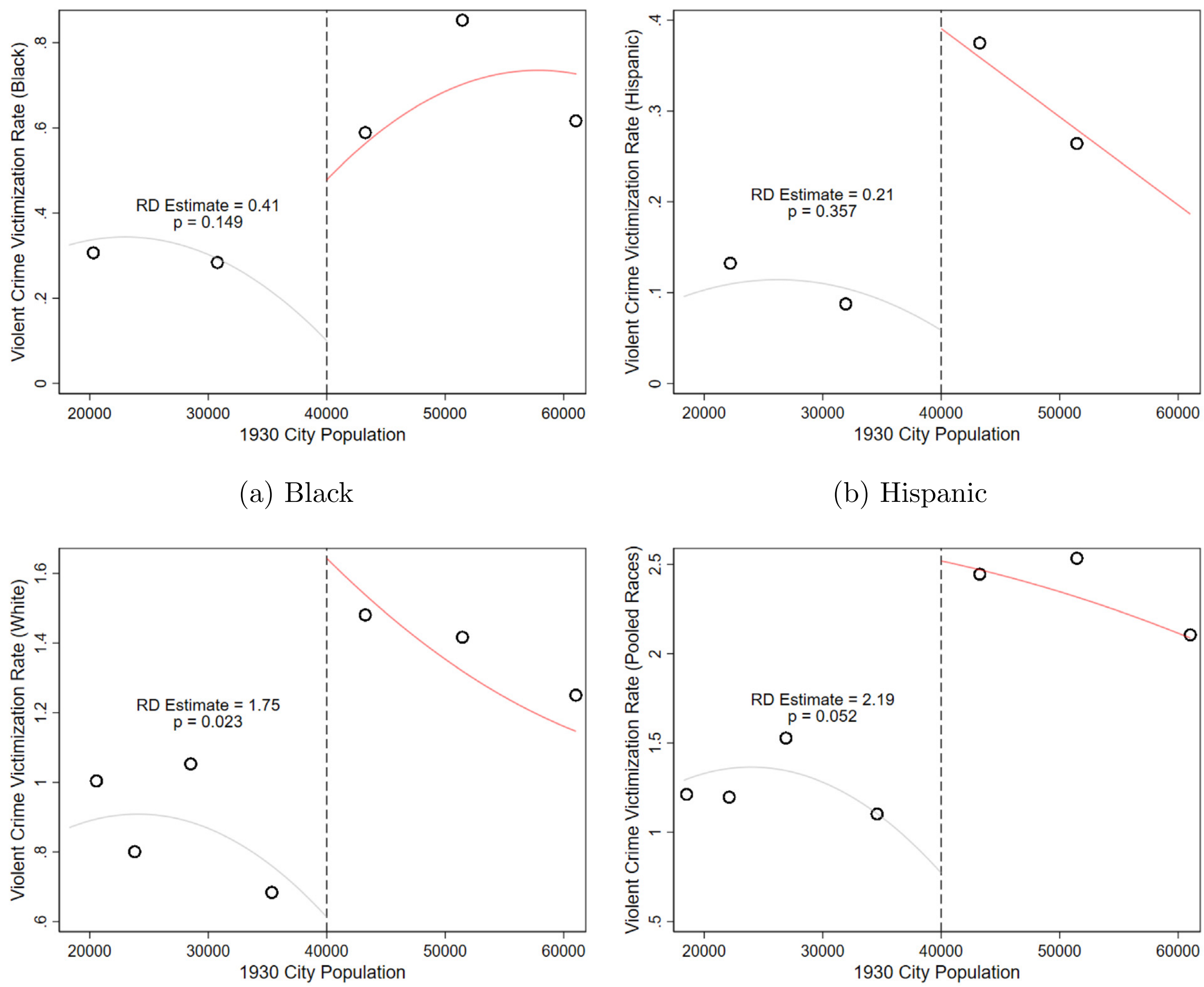
Fig. C1. Crime victimization, by race. **Note:** Each figure shows a regression discontinuity diagram where the outcome variable is the rate of violent crime victimization in a given city in 2015 per 1,000 persons policed. Panel a shows Black violent crime victimization, panel b shows Hispanic violent crime victimization, panel c shows White crime victimization, and panel d shows pooled-race violent crime victimization. See the note to Fig. 4 for more details.

focusing upon White or pooled race regression results is that the RD diagrams do not *unambiguously* suggest a causal effect.

In this section, diagrams for each race by crime type interactive cut are discussed. As Fig. C4 below shows, while there is a jump in White crime victimization just after the 40,000 population threshold, the jump is not a sustained level jump that persists for more populous cities within the bandwidth, but rather White crime rates fall down to levels comparable to the control group even within the main bandwidth (50,000–60,000 population). A similar pattern can be seen in the pooled race estimates (Fig. C5): if there is an effect on pooled race crime victimization is does not persist across the entire bandwidth. These result do not prove that redlining failed to increase White crime victimization, in fact they provide some evidence that they did. However, these results confirm the ex ante expectation that effects would be most pronounced for racial minority groups.

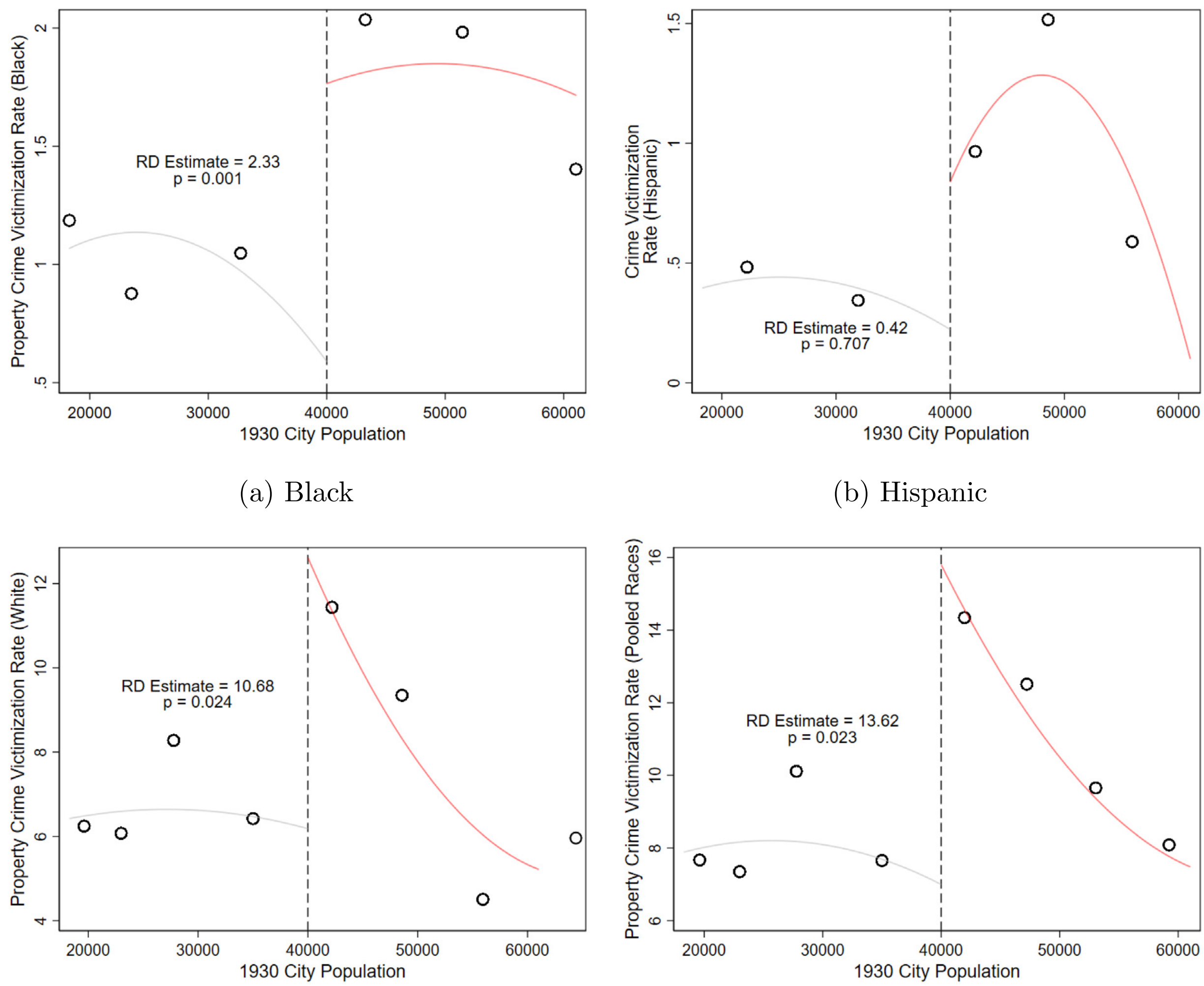
The exception to this overall finding is with violent crime victimization. Black, Hispanic, White and pooled-race violent crime victimization each exhibit pronounced and sustained increases across the bandwidth from 40,000 to 60,000 population. In other words, there is a sustained, level increase in violent crime victimization across each racial and ethnic group. This same pronounced increase is *not* present in property crime victimization. Since there is no ex ante reason to expect redlining to affect violent crime instead of property crime, I do not focus my analysis on these breakdowns.

C.2. Within-city, by race by crime type



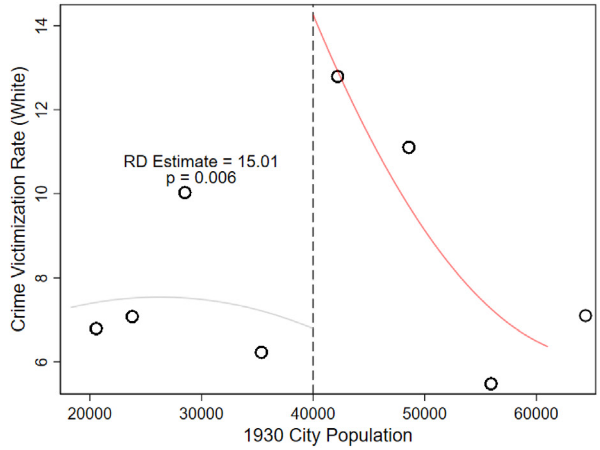
(a) Black (b) Hispanic (c) White (d) Pooled Race

Fig. C2. Violent crime victimization, by race. **Note:** Each figure shows a regression discontinuity diagram where the outcome variable is the rate of violent crime victimization in a given city in 2015 per 1,000 persons policed. Panel a shows Black violent crime victimization, panel b shows Hispanic violent crime victimization, panel c shows White crime victimization, and panel d shows pooled-race violent crime victimization. See the note to Fig. 4 for more details.

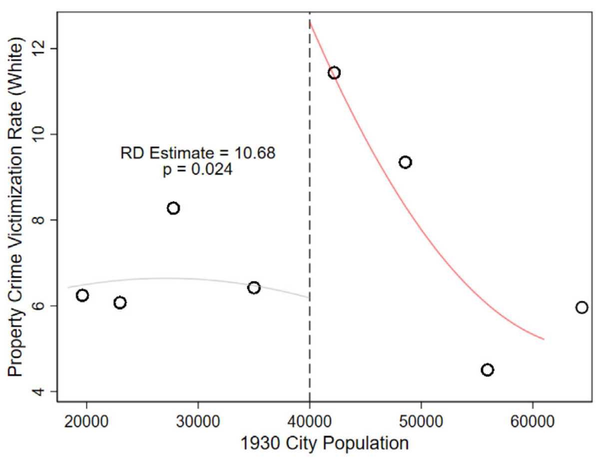


(a) Black (b) Hispanic (c) White (d) Pooled Race

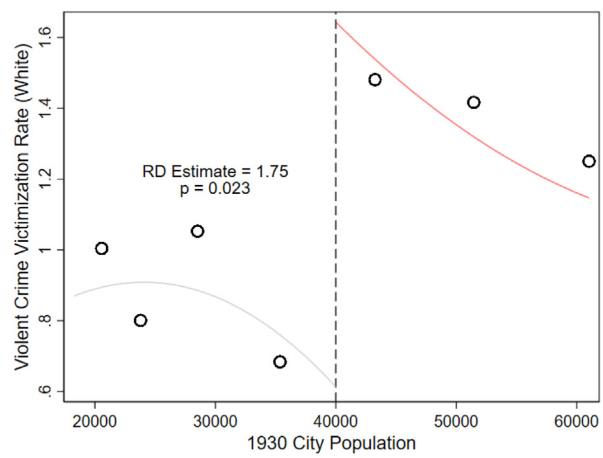
Fig. C3. Property crime victimization, by race. **Note:** Each figure shows a regression discontinuity diagram where the outcome variable is the rate of property crime victimization in a given city in 2015 per 1,000 persons policed. Panel a shows Black property crime victimization, panel b shows Hispanic property crime victimization, panel c shows White property crime victimization, and panel d shows pooled-race property crime victimization. See the note to Fig. 4 for more details.



(a) All Serious Crimes

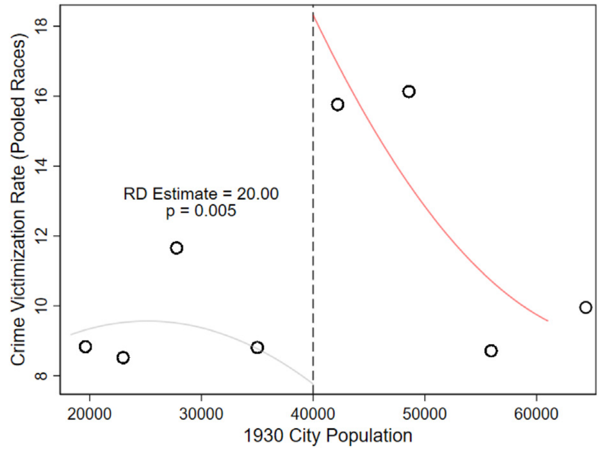


(b) Property Crimes

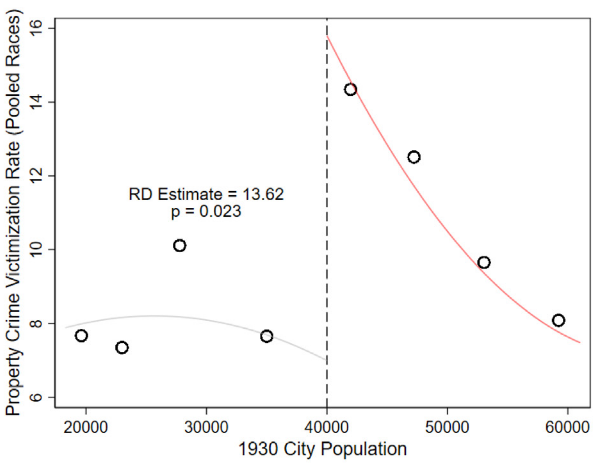


(c) Violent Crimes

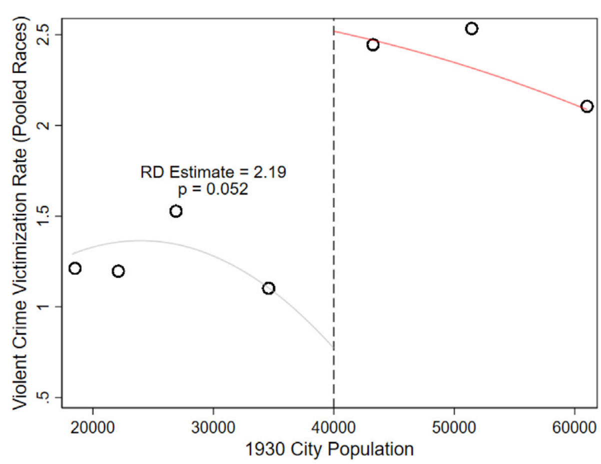
Fig. C4. White crime victimization. **Note:** Each figure shows a regression discontinuity diagram where the outcome variable is the rate of White crime victimization in a given city in 2015 per 1,000 persons policed. Panel a shows all serious crimes (property and violent pooled), panel b shows only property crime rates, while panel c shows only violent crime rates. See the note to Fig. 4 for more details.



(a) All Serious Crimes

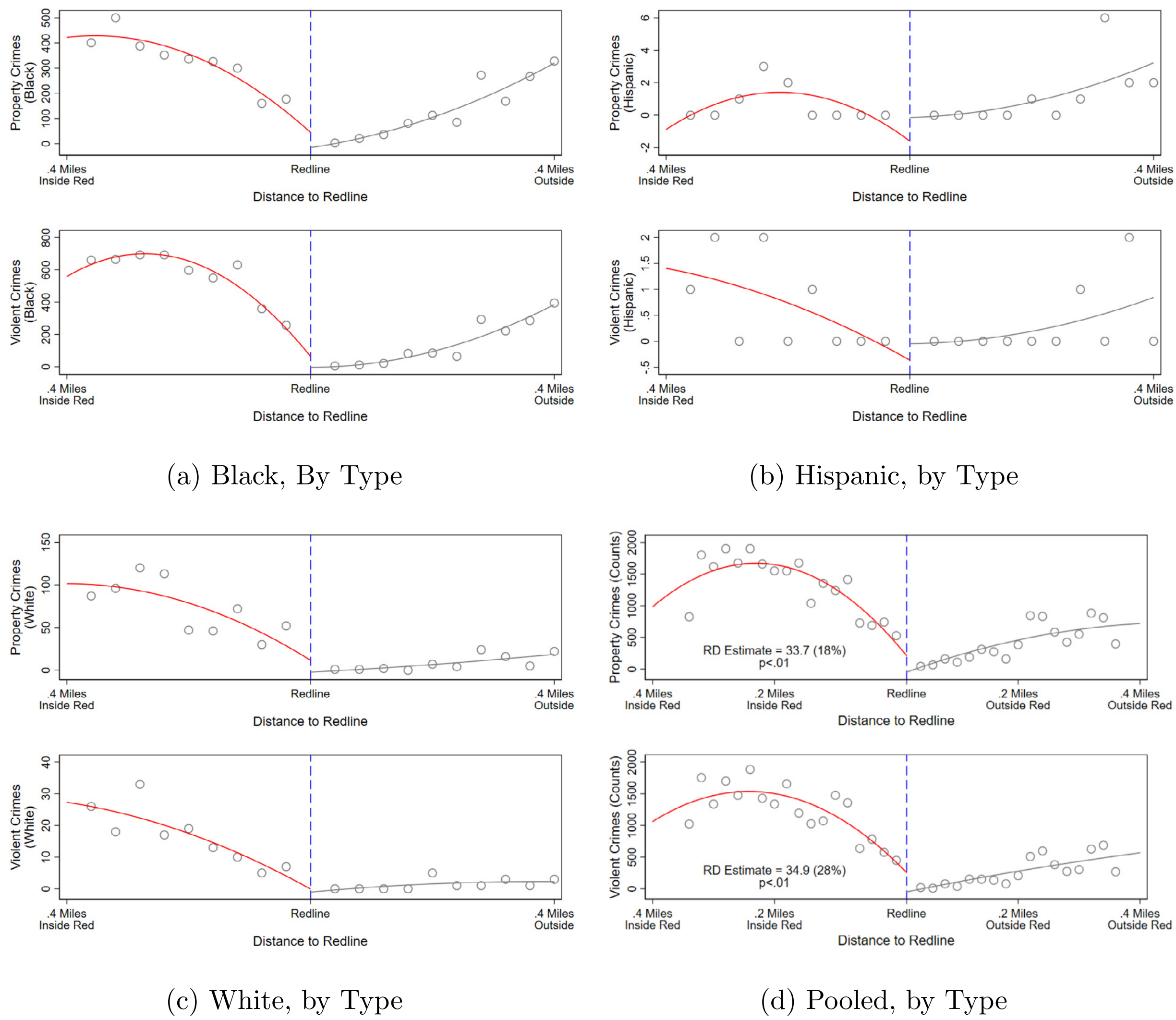


(b) Property Crimes



(c) Violent Crimes

Fig. C5. Pooled race crime victimization. **Note:** Each figure shows a regression discontinuity diagram where the outcome variable is the rate of crime victimization in a given city in 2015 per 1,000 persons policed. Rates are formed by pooling across White, Black and Hispanic victimizations together. Panel a shows all serious crimes (property and violent pooled), panel b shows only property crime rates, while panel c shows only violent crime rates. See the note to Fig. 4 for more details.



(a) Black, By Type (b) Hispanic, by Type

(c) White, by Type (d) Pooled, by Type

Fig. C6. Within-city: black, by type. **Note:** Each figure shows regression discontinuity diagrams analogous to Fig. 6, but with further breakdowns by race and crime type. Panel D repeats Fig. 6. Each other panel reiterates panel D, but restricting by the race or ethnicity of the victim of the crime. Because a 81% of the Los Angeles crime data do not report the race of the victim, the overall counts are larger than the sum of the race specific counts.

Appendix D. NIBRS estimates across years

The main between-city results in Table 1 are based on NIBRS 2015 crime measures. In choosing a year of NIBRS data, this project faces a tradeoff: while reporting to NIBRS grows every year, so does the time since redline mapping. As we get measures closer in time to the advent of HOLC, we lose observations. This appendix explores the sensitivity of the main results to using other years of the NIBRS database, as well as pooling across years.

Performing annual estimates, I find that 2000–2014 estimates for Black crime victimization from 2000–2015 range from 58% to 216% of the 2015 estimate, while estimates for Hispanic crime 2000–2015 range from −71% to 388% of the 2015 estimate (Fig. D1). If anything, the main estimates using 2015 NIBRS data are slightly smaller than those found using earlier years.

Fig. D2 shows estimates pooling NIBRS data from 2000–2009 and 2010–2015, respectively. Results are again similar to the main results in Table 1 and Fig. 4.

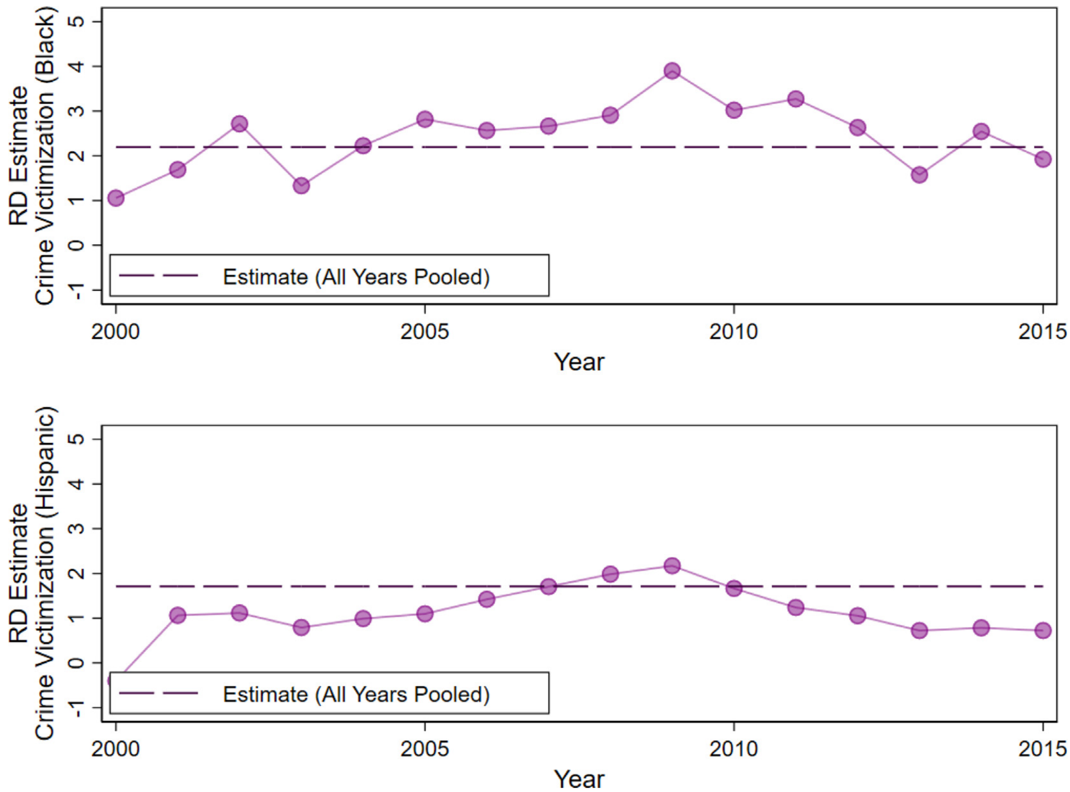


Fig. D1. Between-city estimates: annual NIBRS estimates 2000–2015. **Note:** Each figure shows a profile of regression discontinuity estimates from 2000–2015. In panel a the outcome variable is the rate of Black crime victimization in a given city in a given year per 1,000 persons policed. In panel b the outcome variable is the rate of Hispanic crime victimization in a given city in a given year per 1,000 persons policed. See the note to Fig. 4 for more details.

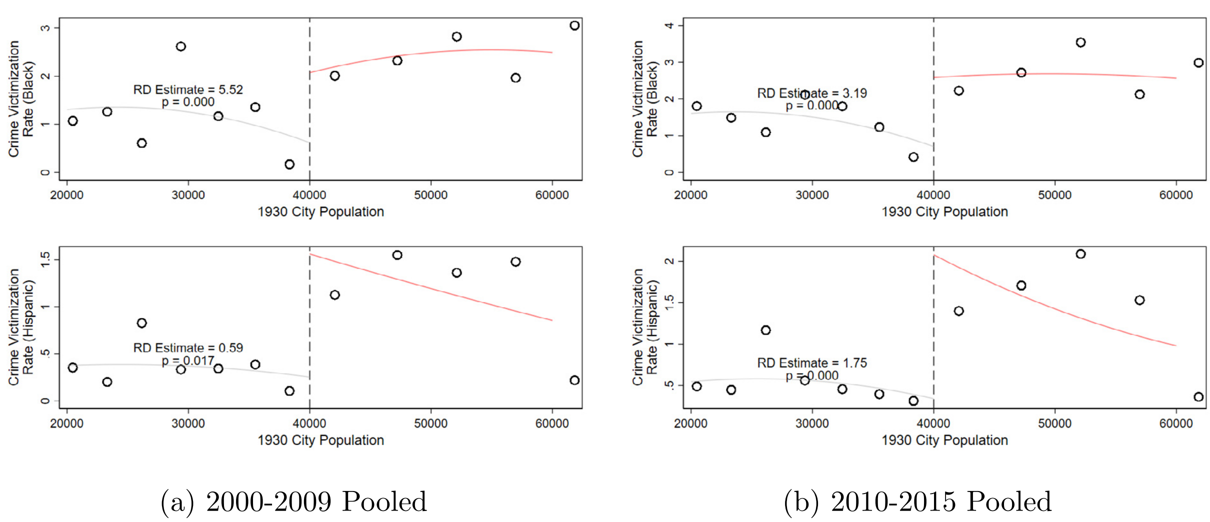


Fig. D2. Between-city estimates: NIBRS estimates pooling years. **Note:** Each figure shows a pair of regression discontinuity diagrams analogous to Fig. A10 where the outcome variable is the rate of crime victimization in a given city in 2015 per 1,000 persons policed. Panel a pools NIBRS data from 2000–2009, while panel b pools data from 2010–2015. See the note to Fig. A10 for more details.

Appendix E. Housing as a mechanism

Because there exists evidence that home vacancies are causally responsible for violent crime (Cui and Walsh, 2015) and that mortgage lending is responsible for decreased crime (Kubrin and Squires, 2004), harm to local housing markets that resulted in increased vacancies and decreased home ownership rates is a channel through which redline mapping may have increased crime. Indeed, Aaronson et al. (2021) uses within-city variation in HOLC mapping assignments to show that redlining reduced home ownership.

To empirically test whether harm to present-day housing markets is a channel through which redline mapping increased crime, I consider various measures of present-day housing market strength as outcome variables in Eq. (1). Table E1 shows regression discontinuity estimates, which suggest that redline mapping increased

Table E1
Impact of redline mapping on present-day housing market: between city estimates.

	(1) PCT Vacant	(2) PCT Mortgaged	(3) AVG Rent
Impact of Redline-Mapping	0.050***	−0.070***	−121.21***
	(0.009)	(0.009)	(26.61)
Observations	3203	3202	3184
Mean	.125	.691	$792.35

Note: Table shows regression discontinuity estimates of the impact of redline mapping on measures of housing market strength with standard errors reported in parentheses. Observations are at the city level. The outcome variable is the percent of vacant homes, the percent of homes under mortgage and average reported monthly rent in 2010 dollars in columns (1), (2) and (3) respectively. The running variable is always 1930 city population. Bandwidth size is chosen optimally in each column following Calonico (2017). Slight differences in the number of observations arise from there being different optimal bandwidths for each outcome variable. The reported mean is for non mapped cities within the optimal population bandwidth. Source: 2010 Census and HOLC archival documents. Significance levels indicated by: * ($p < 0.10$), ** ($p < 0.05$), ***($p < 0.01$).

Table E2
Impact of redline mapping on housing stock: between city estimates.

	(1) PCT Detached Single Family Homes	(2) PCT Attached Single Family Homes	(3) PCT 2–4 Family Housing Units	(4) PCT 5 + Family Housing Units	(5) PCT Mobile Home Units	(6) PCT Other Housing Stock
Panel A1: 1960 Housing Stock						
RD Estimate	0.1203	0.0146	−0.0507	−0.0633	−0.0140	−0.0000
	(0.0734)	(0.0272)	(0.0474)	(0.0404)	(0.0136)	(0.0003)
Mean	.4704	.0615	.1612	.0816	.02053	.0004
Panel A2: 1960 Housing Stock With Black Residents						
RD Estimate	0.2519	0.0621	−0.0974	−0.1668**	−0.0042	−0.0009
	(0.1954)	(0.0930)	(0.0778)	(0.0808)	(0.0139)	(0.0011)
Mean	.3568	.0864	.1571	.1039	.0245	.0006
Panel B1: 1980 Housing Stock						
RD Estimate	0.0367	−0.0089	0.0122	−0.0304	−0.0068	−0.0000
	(0.0398)	(0.0202)	(0.0165)	(0.0243)	(0.0098)	(0.0000)
Mean	.6493	.0457	.0993	.1619	.0140	.00005
Panel B2: 1980 Housing Stock With Black Residents						
RD Estimate	−0.0487	−0.0050	0.0315	0.0246	0.0011	.
	(0.0687)	(0.0335)	(0.0291)	(0.0765)	(0.0045)	.
Mean	.5487	.0610	.1246	.2256	.0034	.
Panel C1: 2000 Housing Stock						
RD Estimate	0.0423	−0.0149	−0.0227	−0.0040	−0.0016	−0.0031
	(0.0647)	(0.0261)	(0.0217)	(0.0467)	(0.0112)	(0.0033)
Mean	.6044	.0668	.1046	.1954	.01988	.0092
Panel C2: 2000 Housing Stock With Black Residents						
RD Estimate	−0.0335	−0.0333	0.0005	0.0479	0.0019	0.0001
	(0.0993)	(0.0380)	(0.0610)	(0.0818)	(0.0112)	(0.0071)
Mean	.4761	.0633	.1432	.3168	.00537	.0102
Observations	143	143	143	143	126	126

Note: The table shows regression discontinuity estimates of the impact of redline mapping on city level housing stock and city level black housing occupancy. The outcome variables are aggregated tabulations of the Census variable UNITSSTR. In panels A1, B1 and C1 the outcome variables measure available housing stock at the city year level; in panels A2, B2 and C2 the outcome variables measure housing stock with black residents at the city year level. The running variable is always 1930 city population. Bandwidth size is chosen optimally following Calonico (2017). The reported mean is for cities within the optimal population bandwidth. There is a small amount of variation in the number of cities reporting non-missing UNITSSTR values across decades; reported observations are for the 2000 sample. In 1980, the estimate for "other" housing stock with black residents is missing because there is not enough support in the outcome variable over the bandwidth to perform the estimation. The sources are the Decennial Census and HOLC archival documents. Significance levels indicated by: * ($p < 0.10$), ** ($p < 0.05$), ***($p < 0.01$).

home vacancy by 5 percentage points (43%), decreased the percentage of homes underwritten by a mortgage by 7 percentage points (10%) and decreased average monthly rental amounts by $121 (15%).

One way in which redline mapping could have influenced local housing markets is by changing the composition of housing stock. If minority would-be home buyers were prevented from accessing the credit market in redlined neighborhoods, there could arise an incentive for developers to favor large, multi-family housing units over single family homes in these redlined neighborhoods. Table E2 reports estimates obtained by using various housing stock measures in various decades as outcome variables in Eq. (1). The estimates in Table E2 provide little evidence that redline mapping changed the composition of housing stock at the city level. Hence further research at the neighborhood level is necessary to identify whatever effects there may be of redlining on housing stock, and, more generally, to further elaborate how redline mapping did lasting harm to local housing markets.

Appendix F. Redlining and migration

Redline mapping could cause both within-city migration across neighborhoods and between-city migration across cities. Thus, understanding both types of migration is important for interpreting the reduced form impact of redlining both at the within-city and between-city level.

The estimates reported in Table F1 provide evidence that redline mapping did not cause either significant within or between city migration in the short run.[37] If we saw evidence of differential

[37] The short run in this case is the period of time between April 1, 1935 and the date of the 1940 Census survey. Because this is the first year the Census began to ask this migration question it is not possible to run a similar specification using the 1930 Census. Redline mapping began in 1935 and continued through 1940. In Los Angeles, for example, city mapping occurred mainly in March of 1939 while the 1940 decennial Census surveys were given out so as to be reflective of conditions April 1, 1940. While there is variation in when cities were mapped, it is reasonable to think that the migration responses of a survey in April of 1940 could pick up migration patterns in the immediate aftermath of redline mapping.

Table F1
Impact of redline mapping on short run migration (1940): between-city estimates.

	(1) Same House	(2) Same Community	(3) Same City	(4) Moved Within County	(5) Moved Wthn St	(6) Btw St (Contig)
City HOLC Mapped	0.0670***	0.0617***	0.0617***	−0.00815*	0.00314	0.00114
	(0.00981)	(0.0123)	(0.0123)	(0.00408)	(0.00214)	(0.00270)
Observations	266	266	266	266	266	266
Mean	.2227	.6407	.6407	.4524	.0199	.0394

Note: Table reports estimates of the impact of redline mapping on various measures of short-run migration. Observations are at the city level. The estimates are obtained by regressing a given short-run migration measure against an indicator variable for whether a city were mapped. The sample is restricted to cities with a 1930 population between 20,000 and 60,000. Each measure is obtained from respondent's answer on the 1940 Census to questions about residency on April 1, 1935. In column (1) the outcome variable is an indicator for whether or not the respondent reports living in the same house at the time of survey as in 1935. Columns (2)–(4) use similar measures at the community, city and county level. Column (5) uses a measure of moving within the state of residence, and column (6) uses a measures of moving between contiguous states as the outcome variable. The reported mean is for non mapped cities within this population bandwidth. Source: 1940 Census and HOLC archival documents Significance levels indicated by: * ($p < 0.10$), ** ($p < 0.05$), ***($p < 0.01$).

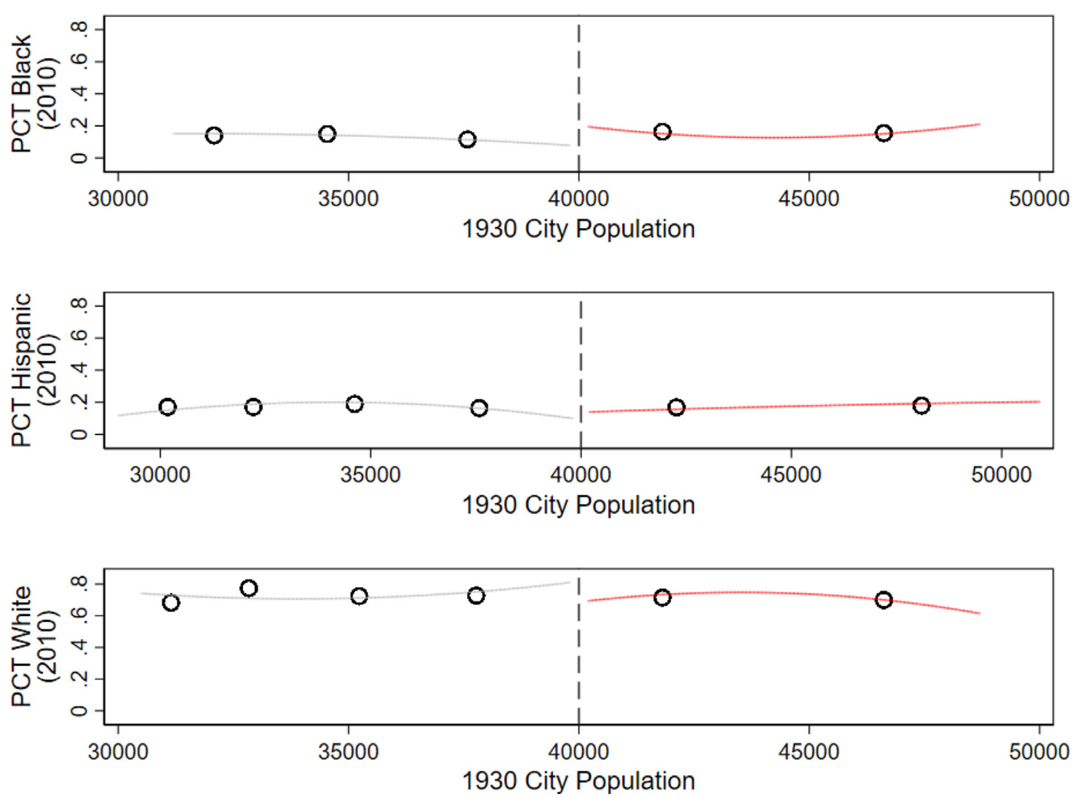


Fig. F1. Impact of redline mapping on demography: between-city estimates of compositional migration. **Note:** The figure shows a set of regression discontinuity diagrams depicting the possible impact of redline mapping on present-day racial demography. Coefficient estimates are reported in Table F2. Data sources are the 2010 Census and Home Owner Loan Corporation (HOLC) archival records.

Table F2
Impact of redline mapping on demography: between-city estimates of compositional migration.

	(1) PCT Black	(2) PCT Hispanic	(3) PCT White
Impact of Redline-Mapping	0.06	−0.03	−0.01
	(0.07)	(0.08)	(0.07)
Observations	559	559	559
Mean (Bandwidth)	.14	.17	.72

Note: Table shows regression discontinuity estimates of the possible impact of redline mapping on present-day racial demography. Corresponding regression discontinuity diagrams are displayed in Fig. F1. Observations are at the city level. Data sources are the 2010 Census and Home Owner Loan Corporation (HOLC) archival records.

within-city migration from the estimates in Table F1, this might suggest that the within-city effects of redlining on crime could be due largely to residents of a city sorting themselves between neighborhoods in response to the mapping. However, the estimates in columns (1)-(3) of Table F1 suggest that redline mapping may have *decreased* within-city moves by about 6 percentage points (a 10% decrease off the mean) in the short run; columns (4)-(6) suggest redline mapping did not affect between-cities moving rates in the short run.

It is still possible that redline mapping is responsible for shaping long-run between-city migration patterns. For example, it could be that some of the well known "Great Migration" patterns of black residents moving away from Southeastern states were influenced by redline mapping practices. Fig. F1 and Table F2 shows regression discontinuity estimates of the possible impact of redline mapping on present-day city level racial composition; they utilize the same city level identification strategy I describe in Section 4.2. The estimates provide suggestive evidence that redline mapping may have increased share black and decreased share white at the city level; these estimates are consistent with an account in which some, but not all,[38] of the reduced form effect of redline mapping on crime is due to between-city migration and accompanying shifts in the racial composition of cities.

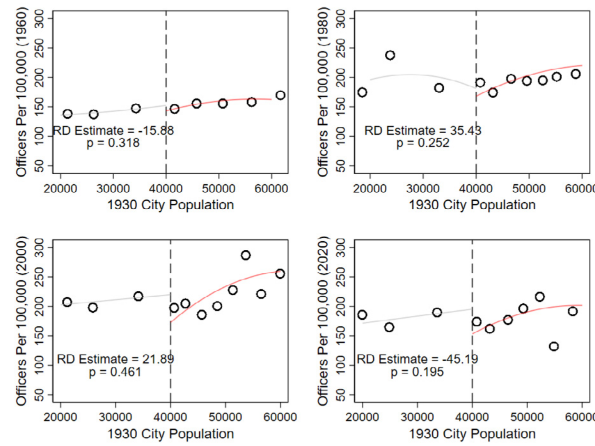
I am in the process of using restricted Census data which links individuals in various Census surveys to their place of birth to more definitively answer the question of whether between-city migration was affected by redline mapping.

Appendix G. Did redline mapping influence police officer volume and expenditures?

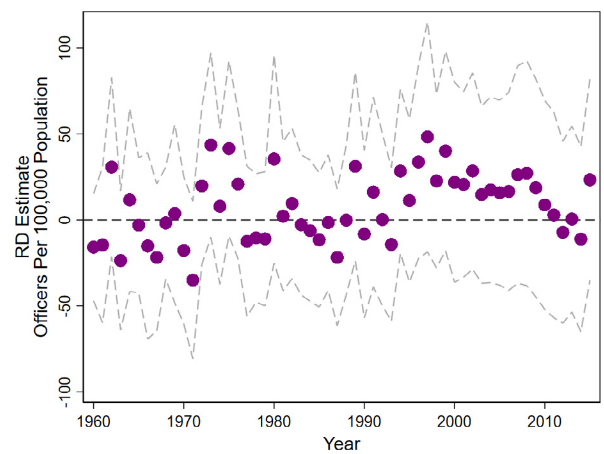
One channel through which redline mapping a city could affect crime rates is by influencing public expenditures on local police forces. For example, if redline mapped cities invested less in police by reducing the number of police officers relative to non mapped cities that might lead to a relative increase in crime. This appendix tests whether redline mapping influenced the number of police officers in a city, and the amount spent on police in a city. Overall, I find no evidence of effects. While it is still possible that redline mapped cities shifted their public safety investments away from neighborhoods graded "red" and towards those graded "green", this evidence suggests that *overall* city investment in public safety is not a mechanism through which redline mapping increased crime.

Police officer volume data are taken from Kaplan (2021) (LEOKA files), and measure the annual number of police officers employed by a city per 100,000 population from 1960 to 2020. These LEOKA files enjoy the same broad geographic coverage as the UCR data used in Fig. A9. Fig. G1 shows regression discontinuity estimates separately for selected years (panel a) and annual estimates for each year from 1960 to 2015. The annual estimates are consistently noisy and bounce around zero; the results do not show any systemic difference in police office volumes across the mapping threshold.

Police expenditure data are taken from Chalfin et al. (2022), and measure the annual payroll expenditures on police per 100,000 population by a city from 1972 to 2006. Relative to the LEOKA files, these data enjoy less coverage across years and less geographic coverage; approximately 70% of the cities in the LEOKA files are



(a) RD Diagrams, select years

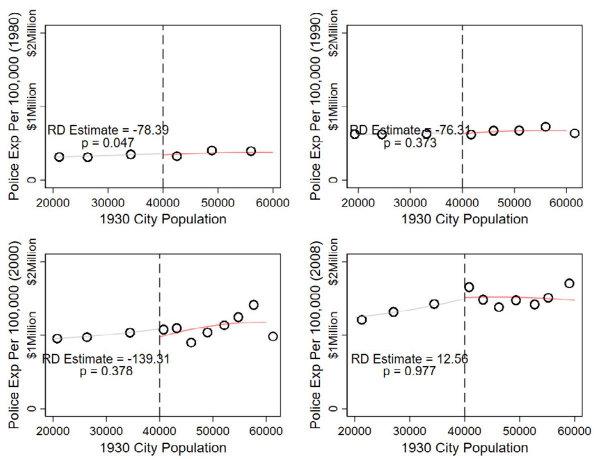


(b) Annual, 1960-2015

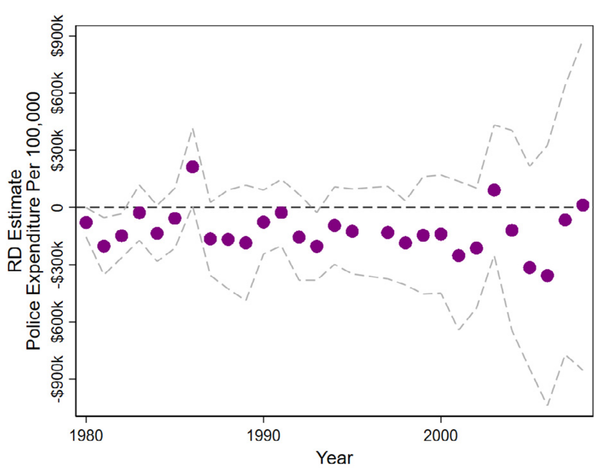
Fig. G1. Between-city estimates: police officer volume. **Note:** Each figure shows results of a regression discontinuity estimation of Eq. (1) where the outcome variable is the number of police officers per 100,000 population in a given city in a given year. Panel a shows 4 regression discontinuity diagrams in which the outcome variable is the police officer measure in 1960, 1980, 2000 and 2020, respectively. Panel b shows a profile of annual RD estimates where the outcome variable is the police officer measure annually from 1960 to 2015. The running variable is always 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following Calonico (2017). Data sources are Kaplan (2021) and Home Owner Loan Corporation (HOLC) archival records.

present in these expenditure data, and for years prior to 1980 the geographic coverage is poor enough to make those years unusable for my design. Fig. G2 shows regression discontinuity estimates separately for selected years (panel a), and annual estimates for each year from 1980 to 2006 (panel b). Overall, I do not find strong evidence of a difference in police expenditures across the mapping threshold. However, annual estimates are rather noisy and, for many years in the profile on Fig. G2, panel b, suggest that mapped cities *may* have spent less on police officers. If mapped cities spent less on police officers and yet hired relatively the same number of officers (as in Fig. G1) then perhaps mapped cities are hiring a lower quality police force on average, and crime is being increased partly through the change in police force quality.

[38] Back of the envelope calculations show that the point estimates in Table F2 can at most explain a third of the city level crime effects



(a) RD Diagrams, 1980(10)2006



(b) Annual, 1980-2006

Fig. G2. Between-city estimates: police expenditures. **Note:** Each figure shows results of a regression discontinuity estimation of Eq. (1) where the outcome variable is the number of police officers per 100,000 population in a given city in a given year. Panel a shows 4 regression discontinuity diagrams in which the outcome variable is the police officer measure in 1960, 1980, 2000 and 2020, respectively. Panel b shows a profile of annual RD estimates where the outcome variable is the police officer measure annually from 1960 to 2015. The running variable is always 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following Calonico (2017). Data sources are Chalfin et al. (2022) and Home Owner Loan Corporation (HOLC) archival records.

Appendix H. Racial animus in HOLC area description files

To better understand the determinants of neighborhood quality assignments on HOLC Security Maps, I use a novel dataset of HOLC administrative data to provide the first quantitative evidence that HOLC assignments were partly driven by racial animus. To show this I focus on the "1.e Shifting or Infiltration" response field on the HOLC area description files. (See Fig. A1.) This response field of the survey sheet is where surveyors were asked to record their expectations concerning future racial composition of the neighborhood they surveyed. In Los Angeles, each of the 416 HOLC delineated neighborhoods received a survey sheet with this field response. As discussed in Section 2.1, Table H1 shows a sample of text responses surveyors in Los Angeles made on this line. It is not difficult to see that the language is racially charged and shows a clear stated preference for white, native-born households.

Table H1
HOLC's stated preferences about racial composition.

"Shifting or Infiltration": Sample Text Responses
A threat of subversive racial infiltration from nearby areas.
Area is hopelessly gone and cannot go much further
Being a beach resort, there is always danger of infiltration of lower racial elements.
Continued infiltration of Mexicans and Negroes
Deed restrictions protect against racial hazards.
Definite threat of further infiltration of subversive racial elements
Few Mexicans moving in along Filmore Place - Currier and along Holt. Ave. west of Filmore
Infiltration of Japanese and Negroes is a threat
Infiltration of goats, rabbits, and dark skinned babies indicated.
Infiltration of inharmonious Jewish element predicted. Thought remote.
Mexicans living on border agricultural lands a threat.
Mexicans said to be diminishing
Negroes are moving out but slowly
No further increase of subversive racial groups is anticipated
Possible future infiltration because of lack of restrictions
Said to be slight infiltration of well-to-do immigrant Jews into apartment houses
Serbs and Italians of better class
Said to be considerable infiltration of Jewish families

Note: HOLC surveyors were asked to detail their expectations about future racial composition of a neighborhood on survey forms on the field "1.e. Shifting or Infiltration" (For an example of a survey form see Fig. A1.) A sample of text responses made in field "Shifting or Infiltration" on survey sheets for Los Angeles, California are listed in the table. The data sources are HOLC archival records.

H.1. Were redlining assignments motivated by racial animus?

To test whether or not the racial animus apparent in these stated preferences is associated with differential neighborhood risk grades, I run an ordered logit regression where the ordinal HOLC security grade is the outcome variable and a rich set of indicators drawn from the "Shifting or Infiltration" responses are independent variables. See the section just below for more details about the construction of these variables. Table H2 reports the marginal effects of these variables on the likelihood of a neighborhood being redlined; all results are conditional on HOLC recorded neighborhood income in 1939, median existing home price in 1939, and average new build home price in 1939, as well as expectations about future trends in the foreign born population, about future trends in wealth levels, and about future overall population dynamics. The estimates show that a HOLC surveyor expressing his view that the black population in the neighborhood is likely to increase is associated with a 13% greater probability that the neighborhood would be redlined (graded "red"). The generic declaration that that the surveyor expected an increase in the presence of some "subversive" group in the neighborhood is associated with an 8% increase in the likelihood of being redlined. Lastly, the surveyor noting the existence of a restrictive covenant in the neighborhood (which would prevent racial and ethnic minorities from moving into the neighborhood) decreased the likelihood of a neighborhood being redlined by nearly 3%. Moreover, Table H3 reports the marginal effects for the likelihood of being assigned a green, blue or yellow grade. The story that emerges is consistent: surveyor expectations of an increase in Black, Hispanic or other so-called "subversive" groups raised the risk score, while contrary expectations lowered it.[39] While these results do *not* cleanly disentangle statistical and taste based discrimination, they contribute quantitative evidence supporting the view that HOLC color assign-

[39] An increase in the risk score means that a neighborhood is more likely to be colored red or yellow, while a decrease means that a neighborhood is more likely to be colored blue or green.

Table H2
HOLC's revealed preferences about racial composition.

	Ordered Logit
Pr(Redlined)	
Increasing Black	0.127**
	(0.064)
Increasing Hispanic	0.039
	(0.034)
Increasing Jewish	0.018
	(0.048)
Increasing Japanese	0.103*
	(0.061)
Increasing Subversive	0.082**
	(0.035)
No Inc Subversive	−0.025
	(0.026)
Restrictive Covenant	−0.027
	(0.040)
Test of Joint	χ^2 = 339.4
Significance	p<.001
Observations	416
Mean	.24
Pseudo R^2	.630

Note: Table reports average marginal effects from an ordered logistic regression with standard errors clustered at the neighborhood and reported in parentheses. The outcome variable is the ordinal rank assignment HOLC gave to each neighborhood ("red","yellow", "blue", "green"). The variables of interest are indicator variables constructed by performing text searches through the field on the HOLC Survey form entitled "Shifting or Infiltration" (See Fig. A1 for an example of a HOLC Survey Form, and Table H1 for examples of text responses in the "Shifting or Infiltration" field, and the Appendix for a detailed explanation of the text searches performed.) Results are conditional on HOLC recorded neighborhood income in 1939, median existing home price in 1939, and average new build home price in 1939, as well as expectations about future trends in the foreign born population, future trends in wealth levels (i.e. both increases in wealthy residents and increases in poor residents), and future overall population dynamics (i.e. whether increasing decreasing, staying constant). The regressions control for population using 1920–1930 Census data. Data sources are HOLC archival records. Significance levels indicated by: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table H3
HOLC's revealed preferences about racial composition.

	Ordered Logit			
	Pr(Redlined)	Pr(Yellow)	Pr(Blue)	Pr(Green)
Increasing Black	0.127**	0.063	−0.109*	−0.081*
	(0.064)	(0.040)	(0.059)	(0.045)
Increasing Hispanic	0.039	0.019	−0.033	−0.025
	(0.034)	(0.018)	(0.029)	(0.022)
Increasing Jewish	0.018	0.009	−0.016	−0.012
	(0.048)	(0.024)	(0.041)	(0.030)
Increasing Japanese	0.103*	0.051*	−0.088*	−0.065*
	(0.061)	(0.031)	(0.052)	(0.039)
Increasing Subversive	0.082**	0.041**	−0.071**	−0.052**
	(0.035)	(0.020)	(0.030)	(0.023)
No Inc Subversive	−0.025	−0.012	0.022	0.016
	(0.026)	(0.013)	(0.022)	(0.017)
Restrictive Covenant	−0.027	−0.013	0.023	0.017
	(0.040)	(0.020)	(0.034)	(0.025)
Observations	416	416	416	416
Mean	.24	.42	.23	.11
Pseudo R^2	.169	.169	.169	.169

Note: Each column reports average marginal effects from an ordered logistic regression with standard errors clustered at the neighborhood and reported in parentheses. The outcome variable is the ordinal rank assignment HOLC gave to each neighborhood ("red","yellow", "blue", "green"). The variables of interest are indicator variables constructed by performing text searches through the field on the HOLC Survey form entitled "Shifting or Infiltration" (See Fig. A1 for an example of a HOLC Survey Form, and Table H1 for examples of text responses in the "Shifting or Infiltration" field, and the Appendix for a detailed explanation of the text searches performed.) Each column reports these marginal effects on the likelihood of being assigned a different ordinal rank, ranging from the lowest rank, "red", to the highest rank, "green". Results are conditional on HOLC recorded neighborhood income in 1939, median existing home price in 1939, and average new build home price in 1939, as well as expectations about future trends in the foreign born population, future trends in wealth levels (i.e. both increases in wealthy residents and increases in poor residents), and future overall population dynamics (i.e. whether increasing decreasing, staying constant). The regressions control for population using 1920–1930 Census data. Data sources are HOLC archival records. Significance levels indicated by: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

ments were at least partly driven by racial preferences for neighborhood composition. This evidence combined with the large body of existing anecdotal evidence (see references in Section 2.1) suggests that neighborhood assignments were driven by racial animus.

H.2. Text analysis of HOLC survey forms

To conduct the analysis reported in Tables H2 and H3, I use text searches within the text responses listed in the item 1.e "Shifting

Table H4
Sample HOLC surveyor text and analysis.

Raw Text Strings	Words that Identify Race Variable	Variable Assignments
Infiltration of Japanese and Negroes is a threat	"infiltration", "Japanese", "Negroes", "threat"	Increasing Black = 1, Increasing Japanese = 1
Both Mexican and Negro population increasing	"Mexican", "Negro"	Increasing Black = 1, Increasing Mexican = 1
Continued infiltration of Mexicans and Negroes	"infiltration", "Mexican", "Negro"	Increasing Black = 1, Increasing Mexican = 1
Japanese and Negroes are increasing	"Japanese", "Negroes"	Increasing Black = 1, Increasing Japanese = 1
Shifting or Infiltration of more Negroes and other subversive racial elements	"shifting", "infiltration", "Negroes", "other subversive", "racial"	Increasing Black = 1, Increasing Subversive = 1
Mexicans living on border agricultural lands a threat.	"Mexican", "threat"	Increasing Hispanic = 1
Shifting or Infiltration of American Jewish families is noticeable	"shifting", "infiltration", "Jewish"	Increasing Jewish = 1
Infiltration of subversive racial elements is a threat	"infiltration", "subversive", "racial", "threat"	Increasing Subversive = 1
Deed restrictions protect against racial hazards	"Deed restrict", "racial hazard"	Restrictive Covenant = 1
Very remote- highly deed restricted	"Deed restrict"	Restrictive Covenant = 1
No evidence at present	"No evidence"	No Increasing Subversive = 1
None apparent	"None"	No Increasing Subversive = 1
Higher income population increasing	–	Increasing Wealthy Population = 1
Infiltration of low income groups	"infiltration"	Increasing Poor Population = 1

Note: The leftmost column lists sample text responses from item 1.e on HOLC survey sheets for Los Angeles, CA in 1939. The middle column lists the words relevant to the string searches I perform; these searches form the basis for the value assignment of race indicator variables. The rightmost column lists every non-zero indicator variables assignment given to the text string in its row. For comparison I include examples of some non-zero assignments for variables which aren't about race, e.g., a variable indicating increases in the wealthy or poor populations. Race variables I consider are limited to: Increasing Black, Increasing Hispanic, Increasing Jewish, Increasing Japanese, Increasing Subversive, No Increasing Subversive, Restrictive Covenant.

or Infiltration" for the 416 survey documents constructed through Los Angeles (See Fig. A1 for a sample survey document.) The construction of racial and ethnic categories follows the language in the text in a straightforward way and is assigned informally. The procedure is an intuitive version of the Continuous Bag of Words model used in Atalay et al. (2017); a formal procedure would be necessary to analyze all HOLC survey forms in all 239 mapped cities but is not necessary for an analysis of Los Angeles alone. Table H4 just below offers detailed examples.

Appendix I. Decadal breadkdowns of the impact of redlining on racial segregation

To complement the results displayed in Fig. A18, I also consider specifications which estimate city-decade level measures by decade. Fig. I1 shows a panel of city level regression discontinuity diagrams where the outcome is White-Black racial segregation in a given year as measured by the White-Black Dissimilarity Index (a standard measure of racial segregation in cities). Fig. I1, subfigure (a), shows a placebo test for White-Black segregation in the period just before redline mapping was implemented: I find no significant difference in White-Black racial segregation across the population threshold in 1930.

Fig. I1, subfigures (b)–(c), show estimates of the impact of redline mapping on White-Black segregation in 1980 and 1990, respectively. (By 1980, cities which were redline mapped had been subject to *de jure* discrimination in the credit market for approximately 30 years.) I estimate that in 1980 redline mapping was responsible for an increase of 11 dissimilarity points, approximately a 24% increase off the mean. This estimate is significant at the ten percent level (See Fig. I1, subfigure (b)). I separately estimate that in 1990 redline mapping was responsible for an increase

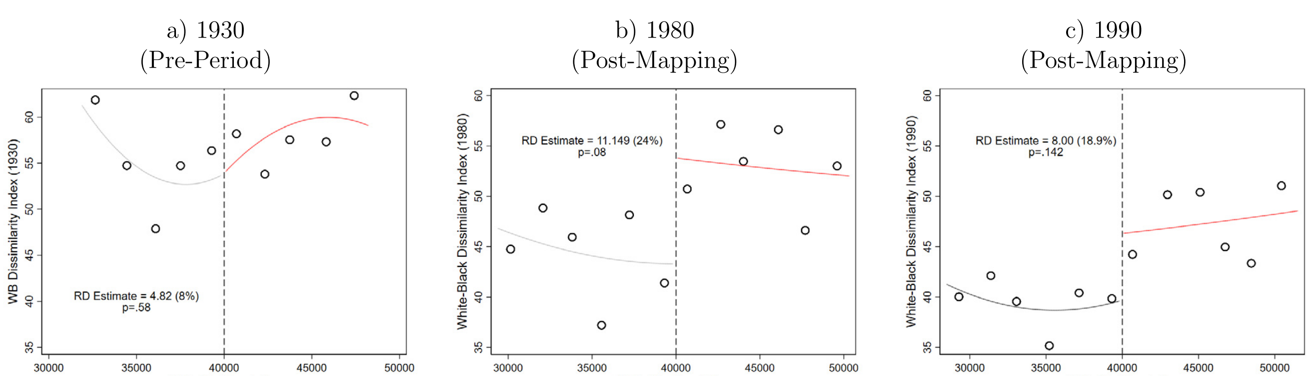


Fig. I1. Impact of redline mapping on racial segregation: between-city estimates over decades. **Note:** Figure shows a regression discontinuity diagram where the outcome variable is White-Black Dissimilarity Index for a given city in a given year The running variable is always 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following Calonico (2017). Subfigure (a) shows a placebo test for White-Black segregation in the pre-period. Subfigures (b)-(c) show the impacts of redline mapping on White-Black segregation in 1980 and 1990, respectively. Data sources are Cutler et al. (1999); Logan (2014), and Home Owner Loan Corporation (HOLC) Archival records.

of 8 dissimilarity points, approximately a 19% increase off the mean. This estimate is significant at the fifteen percent level (See Fig. I1, subfigure (c)).

The estimate for 1980, for example, suggests that, as a result of being redline mapped, in redline mapped cities 11% *more* White households would have to move neighborhoods in order for each neighborhood to have the same racial composition as the city as a whole. Taken together, subfigures (a)–(c) of Fig. I1 suggest that redline mapping caused increases in racial segregation by slowing the rate at which racial segregation was otherwise declining at the national level. In other words, redline mapping seems to have allowed racial segregation to persist longer than it would have in the absence of mapping.

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