The Lasting Impact of Historical Residential Security Maps on Experienced Segregation*

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Abstract

We study the impact of the 1930s HOLC residential security maps on experienced segregation based on cell phone records which track visits out of and into home neighborhoods. We compare adjacent neighborhoods, one of which was assigned a lower grade for creditworthiness than the other. We use a sample of neighborhood borders which, based on estimated propensity scores, are likely to have been drawn for idiosyncratic reasons. Neighborhoods on the lower graded side of the border are associated with more visits to other historically lower graded destination neighborhoods. Today, these destination neighborhoods tend to have lower household income and, in some cases, lower educational attainment. We find that these disparities in visits are not driven by work commutes, very local visits, or differences in income. We also find similar disparities for incoming visits. Finally, we study the impact of the maps on non-residential segregation at the city level, based on a comparison of cities around a population cutoff that determined whether a city was included in the HOLC program. Using transition matrices, we describe visit probabilities across the distribution of home and destination neighborhood incomes. In cities with HOLC maps, visits across neighborhood income lines are less common, but this effect is less pronounced for the richest home neighborhoods. These findings suggest that these historical "redlining" maps affect non-residential segregation and the social interactions of urban residents in the present day.

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1 Introduction

With the rise of economic inequality and other societal changes, there is growing concern among some social scientists and policymakers about the fragmentation and polarization of American society along class and other lines (e.g. Fischer and Mattson, 2009, Smith et al., 2014, Doob, 2019). While there is an enormous literature on residential segregation (e.g. Boustan, 2011, Cutler and Glaeser, 1997, Jargowsky, 2016), social scientists are only beginning to study broader daily interactions among individuals from different socioeconomic backgrounds. These studies use innovative data to measure and identify the impact that non-residential forms of segregation, including face-to-face interactions (Moro et al., 2021), news consumption (Gentzkow and Shapiro, 2011), and urban amenities consumption (Davis et al., 2019), have on social connections, and the extent to which these forms of segregation may exacerbate prejudice and polarization (Pettigrew, 1998). Understanding the extent of intergroup exposure - sometimes referred to as "experienced segregation" - requires accounting for where individuals spend their time and resources outside the home (Athey et al., 2020). We contribute to this growing literature by examining a prominent historical housing policy, the Residential Security Maps drawn in the 1930s, that can speak to a potential causal contribution to present-day disparities in the interactions of urban residents.

The Residential Security maps, otherwise known as "redlining maps," were originally drawn in mid-sized and large cities with the intent of advising private lenders and other parties involved in local housing and finance markets on the security of their investments. However, the grades underlying this advice included criteria unrelated to creditworthiness, most infamously a neighborhood's racial and ethnic composition. Our previous work (Aaronson et al. (2021)) argues that the maps causally increased residential racial and economic segregation for decades thereafter. Experienced segregation may exacerbate the pathologies associated with segregation by further limiting interactions beyond a household's residential neighborhood.

Our measures of experienced segregation are based on foot traffic from cellphone location pings. We calculate the number of visits between a cellphone user's home census block group and all destination census block groups within the core-based statistical area (CBSA) in which they live. Destination neighborhoods are categorized in two ways: a) from the grade - ranked from A (least risky) to D (most risky) - received on the 1930s map and b) from a variety of destination neighborhood characteristics drawn from the 2010 census.

Of course, modern cellphone data have only come into existence in the last few years and certainly have no analogue in the pre-map era, complicating tests of pre-trends. We borrow an identification strategy from our earlier papers that allows us to weigh causal claims of the maps while using only present day outcomes. To do so, we start by comparing adjacent census block groups located on different sides of a redlining map boundary. A simple comparison of the lower-graded and higher-graded sides is insufficient, however, since the mapmakers likely drew the outlines of neighborhoods precisely where differences in community characteristics existed. To address this issue, we restrict our estimation sample to a subset of boundaries that appear to have been idiosyncratically drawn with respect to observable pre-1930's neighborhood characteristics. Such boundaries could have been drawn for reasons of convenience, such as to close a polygon neatly. To identify these idiosyncratic boundaries, we use a rich propensity score model to find boundaries that were unlikely to have been drawn given observable contemporaneous characteristics. These idiosyncratic boundaries allow us to eliminate observable differences in pre-trends between lower and higher graded sides of map boundaries. To further limit pre-trends, we primarily focus on neighborhoods along C-B boundaries, where differences in one key characteristic – racial composition – were negligible when the maps were drawn.

We find that residents on the C-graded side of a C-B boundary are much more likely to visit lower graded (C and D) neighborhoods and destinations with lower income and college graduation rates even today. Interestingly, there is no difference in the racial composition of destination visits between residents on the B and C side of the boundary. For economic context, we compare the size of our estimated effects to a measure of "typical" visit patterns between adjacent urban neighborhoods. We find that the C-B boundary disparities are considerable relative to this benchmark. Depending on the outcome in question, the C-B gaps are between 30 percent and 200 percent larger than the typical gap in visits between adjoining neighborhoods. While we highlight the findings at C-B boundaries to stress that the segregating impact of redlining maps is not limited to the neighborhoods that were actually redlined (that is, assigned the lowest D grade), we find economically meaningful, albeit smaller effects at D-C boundaries as well.

We consider three plausible explanations for our findings. First, neighborhoods tend to be spatially clustered by income, and therefore it is possible that residents of lower-graded neighborhoods visit lower income neighborhoods simply because they are physically closer. We assess this explanation by omitting visits to nearby neighborhoods, where 'nearby" is defined as census tracts located along the C-B boundary, as well as any adjoining tracts. Our estimates are reduced in size but remain economically meaningful and, with few exceptions, are also statistically significant. Second, we consider explanations based on differing work locations. Work-related visits are imputed based on length of stay and day of the week. While we find some evidence of differences in work-related travel, we generally find larger effects for non-work related travel, suggesting that our results are not driven by problems of employer-employee spatial mismatch.[1] Third, we use the counterfactual approach developed in Heckman et al. (2013) and Heckman and Pinto (2015) and find that a relatively small portion of the differences in visit patterns can be explained by differences in household income.

Similar results appear among incoming visitors to neighborhoods along the C-B boundary. This outgoing-incoming visit symmetry suggests that the maps may have affected experienced segregation through the creation of what became a persistent difference in neighborhood amenities or reputation. It also implies that any experienced segregation from commonly-visited destinations is amplified by the composition of visitors arriving in the home neighborhood.

Finally, we study experienced segregation at the city level using transition matrices that characterize the

[1] See Gobillon et al. (2007) and McLafferty (2015) on the spatial mismatch between work and residential locations.

probability that, conditional on a home neighborhood's income quantile, a visit leads to a destination neighborhood of the same or different income quantile. This approach allows us to estimate mobility effects across the distribution of home and destination neighborhood incomes in a city, rather than just narrowly along map grade boundaries. Our research design exploits the HOLC's decision to only map cities with a contemporaneous population of at least 40,000 (Aaronson et al. (2021) and Anders (2023)). Comparing cities just above and just below the population cutoff, we find that low and moderate income residents are more likely to visit neighborhoods of similar income levels in redlined cities. No such pattern arises among higher income residents. This result suggests that the presence of a redlining map leads to a higher degree of experienced income segregation for low-income but not high-income neighborhoods.

1.1 Background and Related Literature

The Home Owner's Loan Corporation (HOLC) was created in 1933 with the objective of promoting homeownership in the middle of the Great Depression. Initially tasked with refinancing mortgages for homes at risk of default, the HOLC was subsequently selected to develop a data-intensive credit appraisal process that included drawing urban neighborhood boundaries and assigning community credit risk grades from "A" (least risky) to "D" (most risky) based on the aggregate likelihood of mortgage default. The grading criteria included many community characteristics that might be associated with default risk, including the quality of housing, occupancy levels, recent price trends, and resident income levels. But racial and ethnic criteria, such as the "infiltration" of minority residents were also considered as important factors, and were explicitly stated in neighborhood descriptions as reasons for rating neighborhoods poorly. The Residential Security Maps became known as the redlining maps and we use these terms interchangeably.

There is uncertainty surrounding how and even whether the redlining maps were transmitted into lending practices. Direct sharing of the maps with private lenders may not have been common due to confidentiality restrictions (Hillier, 2003), and private lenders often drew their own maps of mortgage credit risk. However, the HOLC was in communication with local real estate and banking specialists in the private sector, often relying on their advice as the maps were created (Woods, 2012). It is probable that this process influenced local industry sentiment through the information shared during the review of draft maps, resulting in more expensive credit in lower-graded neighborhoods. The maps may also have influenced other government lending practices, most notably those of the Federal Housing Administration (FHA). Light (2010) and Woods (2012) describe instances of data sharing between the HOLC and the FHA. Fishback et al. (2022) argue that the discriminatory role of the HOLC may be limited: they document methodological differences in assessment procedures between the two agencies which may limit the usefulness of shared information, and they document only small changes in FHA lending after the official release of HOLC redlining maps. In response, Aaronson et al. (forthcoming) note that there was a 5 year period before the FHA implemented its distinct methodology, during which the HOLC maps may have been influential, and the time-series data on FHA lending shows patterns that are consistent with this. In this paper, we are agnostic regarding the channels of transmission for the impacts of the HOLC maps, which, except for a small handful of FHA maps, are the only credit rating appraisal outcomes of this era to survive. Our findings reflect all of the channels mentioned above. To the extent that the HOLC maps are a badly measured version of the FHA maps drawn shortly after, our results should be an attenuated estimate of the true effect.

Our paper is related to others that examine redlining's legacy. Most of these papers use a neighborhood research design, studying a variety of outcomes, including home prices (Aaronson et al., 2021; Appel and Nickerson, 2016), housing supply and population density (Krimmel, 2018), residential segregation (Aaronson et al., 2021; Faber, 2020), and health-related outcomes (Lynch et al., 2021; McClure et al., 2019). A few papers (Aaronson et al., 2021; Anders, 2023; Hynsjö and Perdoni, 2022) exploit the 40,000 population discontinuity design we use here as well. Finally, Aaronson et al. (forthcoming) link individual children who grew up in redlined neighborhoods in the 1940s to later life outcomes and study effects on educational attainment, adult income, and residential neighborhood quality. Relative to these papers, the outcomes we study are distinctly related to neighborhoods, but our study is innovative in that it allows for casual claims about inter-neighborhood links and social interactions that go beyond narrow places of residence.

2 Data and Sample

We rely on household travel patterns from the Monthly Neighborhood Patterns database compiled by the data vendor Safegraph. Safegraph computes a monthly count of unique cell phone stops within a census block group, disaggregated by home block group of the cell phone. A stop (henceforth referred to as a "visit") is counted when a cell phone sends a series of spatially clustered location pings over a time period of at least one minute.[2] The home block group of the cell phone is identified by its typical nighttime location.[3] Our sample period covers January 2018 to December 2019. We find little month-to-month variation in the relative popularity of destination neighborhoods for a given home neighborhood over these two years and therefore do not make use of variation in visits over time.

We merge visit data with digitized versions of the redlining maps, which are provided by the "Mapping Inequality" project (Nelson et al., 2022). We use the sample of 149 cities used in Aaronson et al. (2021), representing around 60 percent of the cities originally covered by the HOLC, including 17 of the 20 largest cities in the 1930s. Using GIS software, we overlay Residential Security Maps with modern census block group maps to assign historical mortgage credit risk ratings to the block groups in our sample. We allow for a small level of tolerance for misaligned borders. Despite this flexibility, some block groups fall into multiple redlining map neighborhoods with different credit risk grades, resulting in ambiguity in assigning a grade to the block group. We face an important trade-off between minimizing ambiguity in neighborhood credit risk grade and preserving the size of the sample and its potential representativeness. We address this trade-off differently for home and destination neighborhoods, as explained below.

[2] The clustering requirement is intended to prevent travel through the block group from being interpreted as a stop. Detailed information on the methodology has not been made public.

[3] Because we do not observe individual trips, we cannot determine whether the cell phone visits a destination neighborhood directly from home or via a third location. Consequently, when we refer to visits by home-destination neighborhood pair, this refers to a stop in the destination block group made by a cell phone with a nighttime location in the home block group.

2.1 Sample Restrictions

The analysis of boundary effects uses an estimation sample of adjacent "home" neighborhoods located at C-B boundaries.[4] First, we select all block groups that are located within a quarter mile of a C-B boundary that is at least a quarter-mile long. We allow for repetition if a block group is located close to two or more boundaries. Naturally, modern block groups do not perfectly align with the boundaries drawn on 1930s era maps, so we drop block groups that appear on both sides. Despite this restriction, many block groups in our sample still fall into multiple neighborhoods on the maps. To address the ambiguity in assigning credit risk grades, we further restrict our sample to block groups where at least 80 percent of land area can be assigned the same grade.[5]

The above restrictions result in a sample of 589 home census block groups, much smaller than the 2,811 block groups along C-B boundaries and the 20,343 block groups in a B or C neighborhood (see Table A1 for more details on the C-B boundary sample). In Table 1, we provide additional context about what these neighborhoods look like today by comparing 2010 neighborhood characteristics of block groups located at C-B boundaries (column 3) against the sample of all block groups in cities with redlining maps (column 1) or block groups graded B or C (column 2).[6] In the present day, C-B boundary block groups are clearly more disadvantaged than the average neighborhood: they have lower household income, a greater fraction of non-White/Black residents, a lower college graduation rate, and a lower employment to population ratio. All differences are highly statistically significant. C-B boundary block groups are also more disadvantaged than the average B or C graded block group in all variables except for college graduation rate. Imposing the sample restrictions (column 4) further amplifies the differences with the overall B or C sample in terms of income, racial composition, and employment to population ratio. The additional imposition of the low propensity score restriction (column 5), which we describe in section 3.1, skews somewhat more toward poorer, non-White, and Black residents but reduces to statistical insignificance the difference in the employment to population ratio.

We make several further restrictions to the sample of visits that we study. First, we omit infrequent trips by excluding any home-to-destination block visit that appears fewer than five times in a month. We introduce this restriction to avoid using infrequent visit patterns that are more likely to contain measurement error due to SafeGraph's differential privacy procedures.[7] In practice, allowing for the inclusion of unusual (less than 5 per month) visits has a small economic impact on our inferences, especially since we weight block-to-block visits by frequency. Second, we exclude visits that do not finish within a core-based statistical

[4]Separately, we define an analogous sample of home neighborhoods located at D-C boundaries.

[5]We also impose more stringent restrictions, in the most extreme case, requiring that the entire land area be assigned the same grade. We find similar results but the sample size is further reduced and precision somewhat declines.

[6]To be clear, this table does not report a test of pre-trends as the data is from 2010 (and averages lower-graded and higher-graded block groups together). Pre-trends are discussed in section 3.1.

[7]To ensure privacy, Safegraph adds noise to the data in the following manner. Conditional on observing at least one visit to a block group, SafeGraph adds noise that is distributed Laplace (0,10/9). After adding the noise, SafeGraph rounds the visits down to the nearest integer. If the resulting number is less than 2, they drop the observation. If the resulting number is 2 or 3, they replace it with a visit count of 4 (Sakong and Zentefis, 2023).

Table 1: Block Group Descriptive Statistics for C-B Boundary Neighborhoods in 2010

	All	B/C	C-B Boundary		+ Restrictions		+ Low Propensity	
	(1) mean (s.e.)	(2) mean (s.e.)	(3) mean (s.e.)	(3) = (2)? p	(4) mean (s.e.)	(4) = (2)? p	(5) mean (s.e.)	(5) = (2)? p
Median Household Income	61,080	51,250	46,688	0.000	45,970	0.000	45,388	0.000
	(120)	(203)	(485)		(1,058)		(1,261)	
Fraction Non-white	0.334	0.423	0.455	0.000	0.495	0.000	0.519	0.000
	(0.001)	(0.002)	(0.006)		(0.014)		(0.020)	
Fraction Black	0.189	0.269	0.319	0.000	0.357	0.000	0.373	0.000
	(0.001)	(0.002)	(0.007)		(0.015)		(0.022)	
College Graduation Rate	0.314	0.289	0.288	0.844	0.289	0.980	0.282	0.520
	(0.001)	(0.002)	(0.004)		(0.009)		(0.011)	
Employed/Population	0.777	0.757	0.751	0.008	0.744	0.013	0.747	0.191
	(0.000)	(0.001)	(0.002)		(0.005)		(0.007)	
Population	1,343	1,127	1,091	0.000	1,081	0.019	1,083	0.106
	(2)	(4)	(9)		(20)		(27)	
Observations	81,730	20,343	2,811		589		301	

Notes: The table shows means (standard errors) of 2010 Census block group variables, for a sample of: (1) all block groups in cities with redlining maps, (2) all block groups with credit risk grades of B or C, (3) all block groups located at C-B boundaries, (4) all C-B boundary block groups meeting sample restrictions, and (5) all low propensity C-B boundary block groups meeting sample restrictions. See sections 2.1 and 3.1 for sample restrictions and the definition of the low propensity score sample. Additional columns report p-values from t-tests for equality between block groups in a named smaller sample and the sample of all block groups graded B or C (column 2). T-tests for equality between columns 2-5 and column 1 yield very small p-values. For brevity, these p-values are not reported.

area (CBSA).[8] Third, in the spirit of measuring non-residential experienced segregation, we ignore all visits where the home and destination neighborhood are identical.

2.2 Dependent Variables

Our main outcome is based on visits from a home census block group to a destination census block group within a CBSA. Destinations are described by either their historical credit risk grade or their 2010 neighborhood characteristics. For the former, we compute the fraction of visits to destination neighborhoods graded C and/or D in the 1930s.[9] In defining the historical grade of destination neighborhoods, we apply a more stringent restriction than our sample restriction for home neighborhoods: here, we require the entire land area to be assigned either a C or a D grade for a destination neighborhood to be defined as graded C and/or D. For the latter, we compute a frequency-based destination block group mean of several key socioeconomic indicators available in the 2010 census, including fraction minority residents, fraction Black residents, fraction college graduates, and log median household income.[10]

While our baseline estimates aggregate across all types of visits, we also look separately at work and non-work visits. If work tends to be concentrated in a small number of urban locations (e.g a downtown commercial core or retail district), any disparities in where residents travel for recreation or socializing will

[8] The CBSA covers areas far wider than the redlining maps so blocks outside of CBSA's are dropped for lack of credit grades, regardless.

[9] When reporting results along the D-C boundary, we focus on D grade destinations.

[10] The visit frequency-weighted means are computed over the full two years of our Safegraph data.

be attenuated when work and non-work visits are aggregated. One reason for studying work and non-work travel separately is that the consequences of intergroup contact could differ between work and non-work contexts. For example, Pettigrew et al. (2011) find that intergroup contact in stressful or competitive situations, which frequently occur in work environments, can increase prejudice instead of reducing it. Practically, work-related visits are defined by Safegraph as any stay lasting at least 6 hours.[11] Conversely, a visit lasting less than six hours is defined as non-work-related. Additionally, we use weekend visits as an alternative, albeit imperfect, measure of non-work travel.

Finally, a plausible explanation for why visit patterns might appear different between the C-graded and B-graded sides of the C-B boundary is that the lower C graded side is typically closer to lower income neighborhoods. These proximity effects are partly mitigated by our empirical strategy, which only compares small, physically adjacent neighborhoods. However, modest differences in distance may still be impactful if preferences for proximity are strong and neighborhoods are spatially clustered. Therefore, to study this potential explanation, we report a set of estimates that omit visits to nearby destinations, where "nearby" is defined in two ways: first, any census tract located at the boundary and second, any census tract along the boundary or adjacent to a boundary census tract.

3 Empirical Strategy and Results

3.1 Empirical Specification

We study the impact of a lower grade assignment at a redlining map boundary on experienced segregation using the specification in Equation (1):

$$Y_i = \beta \cdot \mathbf{1}\left(Grade_i = g_{low}\right) + \phi_{b(i)} + \varepsilon_i. \tag{1}$$

For each block group i, we regress mean characteristics of outgoing visits, Y_i, on an indicator for living on the lower-graded side of the border, $\mathbf{1}\left(Grade_i = g_{low}\right)$, controlling for fixed effects at the boundary level, $\phi_{b(i)}$. The coefficient β reflects the impact of a residing in a lower-graded side of a border on visit patterns, relative to the higher-graded neighborhood(s) on the other side. ε_i is an error term.

β only describes a causal effect under the strong assumption that, in the absence of the maps, current residents of higher graded and lower graded neighborhoods living along the same border would exhibit the same visit patterns. The boundary design addresses some empirical concerns about the non-comparability of neighborhoods since many factors that influence visit patterns (e.g. accessibility to public transportation or roads) are likely to be very similar, if not identical, in adjacent neighborhoods. However, there is ample evidence that redlining map boundaries were not drawn randomly with respect to neighborhood characteristics. In drawing the maps, HOLC planners likely relied on pre-existing patterns of neighborhood

[11] This definition will undercount part-time employment and jobs where work location changes frequently within the day. It may also misrepresent students at school as employment.

disparities, which would suggest neighborhood differences in visit patterns even in the absence of the maps and consequently upwardly biased estimates of β.

For this reason, we rely on a subsample of boundaries that may have been drawn for idiosyncratic reasons, such as closing a polygon that outlines the shape of a neighborhood. These "low propensity" boundaries have differences in characteristics across boundary lines such that a probit model predicts that they are in the bottom half of the distribution for the likelihood of boundary placement. Estimation of the propensity score model is described in Aaronson et al. (2021).[12] We select all C-B boundaries that have propensity scores below 0.3, the sample median value. Since these boundaries are poorly predicted by historical neighborhood conditions, it is plausible that neighborhoods on either side of such low propensity borders were comparable at the time the maps were drawn.[13]

To provide a sense of scale for the regression estimates, a typical benchmark statistic is the standard deviation of the dependent variable. However, since two adjacent neighborhoods tend to have more similar visit patterns than two randomly chosen neighborhoods in the same city, the full sample of a city's block groups would yield a standard deviation that is too large. Therefore, we derive a measure of the typical variation between adjacent neighborhoods located at boundaries. We begin with the sample of all boundary block groups for the full set of boundaries (i.e. A-A, B-A, C-A, D-A, B-B, C-B, D-B, C-C, D-C, AND D-D boundaries). For each dependent variable we analyze, we regress the variable on boundary fixed effects without other controls. This procedure absorbs any variation in the dependent variable that is attributed to non-proximate neighborhoods. We collect the regression residuals and calculate the standard deviation, referring to this as the "within-boundary standard deviation" in the tables to follow.

3.2 Baseline Results

Our baseline C-B boundary results are reported in Table 2. We find neighborhoods on the C-graded side are associated with a greater fraction of visits to neighborhoods that were historically graded as risky by the HOLC. Column 1 shows that, across all C-B boundaries, C grade assignment leads to a statistically significant 8.9 percentage point (standard error of 1.4 p.p.) increase in the fraction of visits to neighborhoods with grades of C or D, with impacts on both C (column 3) and D (column 5) neighborhoods separately.[14] Conversely, there is a significant and large decrease in the fraction of visits to A and B-graded neighborhoods

[12] The 1910, 1920 and 1930 (when available) neighborhood characteristics used to estimate the propensity score model are share Black residents, Black resident population density, White resident population density, share foreign born, the home ownership rate, the share of homeowner households that have a mortgage, log house value, and log rent.

[13] In support of this strategy, Aaronson et al. (2021) find that these boundaries have much smaller differences (less than half) in home ownership rates, rents, and home values than the average C-B boundary. Since there were almost no Black residents in C or B areas in 1930, on average, there is no difference in the Black population share across the C-B boundaries as a whole and across the low propensity C-B boundaries.

[14] The point estimate in column 1 (or 2) does not equal the sum of the point estimates in columns 3 and 5 (or 4 and 6) because of the way we define a destination neighborhood. That is, a neighborhood that encompasses both a C-graded and a D-graded area is counted as a C or D grade destination in column 1 (and 2) but would not be included as a strictly C grade (columns 5 and 6) or strictly D grade (columns 3 and 4) destination.

collectively (column 7).[15] When we restrict the sample to our preferred low propensity score boundaries (even columns), the point estimates are usually slightly larger, albeit indistinguishable from the estimates using all C-B boundaries.

To judge the magnitude of these estimates, the bottom of Table 2 reports the standard deviation of visits to different destinations for all urban block groups and block groups along redlining map boundaries. We also report the ratio of the point estimate to these standard deviations. The estimated effect on C or D neighborhood visits is roughly 40 percent of the 22.5 percentage point standard deviation of all urban block group visits to C or D neighborhoods and almost twice the size of the 5.3 percentage point within-boundary standard deviation of adjacent boundary block groups that we consider a more appropriate benchmark.

Table 2: C-B Boundary Effects on Outgoing Visit Patterns, Historical Credit Risk Grades

Fraction of Outgoing Visits to:	C,D		D		C		A,B	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lower Graded (C) Side	0.089***	0.098***	0.018***	0.016***	0.058***	0.069***	-0.070***	-0.075***
	(0.014)	(0.024)	(0.003)	(0.004)	(0.012)	(0.020)	(0.012)	(0.021)
Observations	443	217	443	217	443	217	443	217
Standard Deviation of Dependent Variable								
• Overall	0.225	0.225	0.109	0.109	0.136	0.136	0.087	0.087
• Within-Boundary	0.053	0.053	0.042	0.042	0.046	0.046	0.035	0.035
Ratio of Point Estimate to Std Dev								
• Overall Std Dev	0.396	0.437	0.168	0.149	0.427	0.511	-0.802	-0.863
• Within-Boundary Std Dev	1.676	1.849	0.439	0.392	1.271	1.519	-2.004	-2.158
Sample: Low Propensity Boundaries		Y		Y		Y		Y

Statistical significance: $^{+}\ p < 0.1$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$.
Notes: An observation is a census block group located at C-B boundaries that follows the sample restrictions and the definition of the low propensity score sample described in the text. Block groups are added to the sample with repetition if they are part of multiple boundaries. The dependent variable is the fraction of visits by residents of boundary block groups to destinations in neighborhoods with historical credit risk grades given in the column header. All specifications control for boundary fixed effects. The overall standard deviation is estimated using all block groups in cities with Residential Security Maps. The within-boundary standard deviation is estimated from boundary block groups using the residual variance from a secondary regression of the dependent variable on boundary fixed effects. Standard errors are clustered by boundary.

Table 3 reports an analogous exercise where destinations are categorized by four 2010 neighborhood characteristics – household income, share college graduates, share minority residents, and share Black residents – rather than redlining map credit grades. Being on the C graded side of the C-B boundary results in statistically fewer visits to higher-income (columns 1 and 2) and more college educated destinations (columns 3 and 4). The point estimates are economically significant, representing roughly 60 and 34 percent of the within-boundary standard deviation of our income and college visit measures for the low propensity score sample. However, we find no evidence of boundary effects on the racial composition of destination neighborhoods (columns 5 through 8).

[15] Although there are only four grades, the estimate in column 7 for A and B visits is not the negative of column 1 for C and D visits because some neighborhood were ungraded by the HOLC in the 1930s, and some destinations are not assigned letter grades due to misalignment between census and redlining map boundaries.

Table 3: C-B Boundary Effects on Outgoing Visit Patterns, Frequency-Weighted Means of 2010 Destination Neighborhood Characteristics

Destination Neighborhood:	Income		College		Minority		Black	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lower Graded (C) Side	-0.050***	-0.039**	-0.015***	-0.010*	0.014+	0.002	0.010	0.003
	(0.008)	(0.011)	(0.003)	(0.004)	(0.008)	(0.013)	(0.007)	(0.011)
Observations	443	217	443	217	443	217	443	217
Standard Deviation of Dependent Variable								
• Overall	0.317	0.317	0.133	0.133	0.189	0.189	0.170	0.170
• Within-Boundary	0.065	0.065	0.030	0.030	0.040	0.040	0.042	0.042
Ratio of Point Estimate to Std Dev								
• Overall Std Dev	-0.156	-0.122	-0.111	-0.077	0.072	0.010	0.060	0.016
• Within-Boundary Std Dev	-0.757	-0.591	-0.491	-0.342	0.338	0.048	0.242	0.066
Sample: Low Propensity Boundaries		Y		Y		Y		Y

Statistical significance: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Notes: An observation is a census block group located at C-B boundaries that follows the sample restrictions and the definition of the low propensity score sample described in the text.. Block groups are added to the sample with repetition if they are part of multiple boundaries. The dependent variable is the frequency-weighted mean of a destination neighborhood's characteristics, drawn from the 2010 census. These characteristics are: log median income (columns 1 and 2), college graduation rate (columns 3 and 4), fraction non-White residents (columns 5 and 6), and fraction Black residents (columns 7 and 8). All specifications control for boundary fixed effects. The overall standard deviation is estimated using all block groups in cities with Residential Security Maps. The within-boundary standard deviation is estimated from boundary block groups using the residual variance from a secondary regression of the dependent variable on boundary fixed effects. Standard errors are clustered by boundary.

3.3 Robustness, Heterogeneity, and Alternative Outcomes

Table 4 examines whether our results are sensitive to the estimation sample.[16] Column 1 reintroduces rare (less than five per month) home-to-destination visits that we suspect are exacerbated by the noise that is injected into the data for confidentiality purposes. Consistent with this measurement error story, the estimated coefficient is attenuated by half. However, the point estimate remains statistically significant and, because the within-boundary standard deviation is also smaller, the effect size is still economically meaningful at roughly 1.5 times the within-boundary standard deviation. Column 2 drops visits to census tracts along the same C-B boundary, and column 3 also drops tracts adjacent to the boundary. While the point estimates decline, they remain economically sizable relative to the within-boundary standard deviation benchmark. Since we still find sizable boundary effects after excluding these nearby neighborhood visits, we conclude that preferences for convenience and the spatial clustering of low-income neighborhoods are at best a partial explanation for our findings. Similar results arise using mean destination neighborhood income, shown in columns 4 through 6.[17]

[16] We only present results using the more conservative low propensity score sample for the next set of tables. For simplicity, we focus on two dependent variables: fraction of visits to C or D neighborhoods and mean destination neighborhood income. The full set of results are available in Table A2.

[17] Additional robustness checks show little difference to the magnitude of the point estimates relative to the within-boundary standard deviation. These include changing the low propensity cutoff for inclusion in estimation, relaxing (to 50 percent) or tightening (to 100 percent) the restriction on minimum land area covered with a common grade for a block group to be included, and restricting the sample to the high-propensity boundaries (i.e. those where 1930s boundary differences in neighborhoods were most pronounced). See Table A2 for these results.

Table 4: C-B Boundary Effects on Outgoing Visit Patterns, Alternate Restrictions on Visit Counts

Destination Neighborhood:	Pr(C or D Grade)			Income		
	(1)	(2)	(3)	(4)	(5)	(6)
Lower Graded (C) Side	0.044***	0.078***	0.047***	-0.024***	-0.041**	-0.024+
	(0.009)	(0.017)	(0.009)	(0.007)	(0.013)	(0.012)
Observations	217	215	215	217	215	215
Within-Boundary Standard Deviation	0.031	0.051	0.047	0.047	0.067	0.067
Point Estimate / Std Dev	1.422	1.528	1.002	-0.510	-0.607	-0.355
Add Bottom-coded Visits	Y			Y		
Drop Nearby Visits:						
– Boundary Tracts		Y	Y		Y	Y
– Adjacent Tracts			Y			Y

Statistical significance: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Notes: An observation is a census block group located at C-B boundaries that follows the sample restrictions and the definition of the low propensity score sample described in the text. Block groups are added to the sample with repetition if they are part of multiple boundaries. The dependent variable is either: the fraction of outgoing visits to destinations with a historical grade of C or D (columns 1 to 3), or the frequency-weighted mean of destination neighborhood log median income (columns 4 to 6). Columns 1 and 4 include rare (less than 5 per month) home-to-destination visits (see text for more detail). Columns 2, 3, 5, and 6 drop nearby destinations, defined as census tracts containing any boundary block groups (columns 2, 3, 5, and 6) and census tracts adjacent to boundary tracts (columns 3 and 6). All specifications control for boundary fixed effects. The overall standard deviation is estimated using all block groups in cities with Residential Security Maps. The within-boundary standard deviation is estimated from boundary block groups using the residual variance from a secondary regression of the dependent variable on boundary fixed effects. Standard errors are clustered by boundary.

In Table 5, we determine the extent to which differing work locations can explain our results. We divide visits into work-related and non-work-related, using the criteria of a six-hour dwell time. For the fraction of visits to C or D neighborhoods, we find significant positive effects of lower grade assignment for work-related travel (Column 1), with an effect size slightly greater than the within-boundary standard deviation. For non-work-related travel (Column 2), the coefficient and estimated effect size is almost identical to our baseline results. Using weekend travel as an alternative definition for non-work travel (Column 3) results in a coefficient and within-boundary standard deviation that are both slightly larger, and an effect size that is again comparable to our baseline results. In Column 4, we report essentially no impact of lower grade assignment on the mean income of work destinations. The lack of meaningful disparities in a key socioeconomic variable suggests that redlining maps did not contribute to issues of spatial mismatch at C-B boundaries. In contrast, for non-work-related travel (Column 5) and weekend travel (Column 6), we find coefficients and effect sizes that are similar to the baseline results. Taken together, these findings suggest that non-work-related travel makes a larger contribution to boundary disparities in overall outgoing visit patterns.

Table 6 reports the impact of lower grade assignment on incoming visits. If lower grade assignment has led to persistent gaps in neighborhood amenities or reputation, boundary effects on incoming visits are also plausible. Treating boundary block groups as destination neighborhoods, we compute the proportion of visits originating from home neighborhoods with low (C or D) HOLC grades, as well as frequency-weighted means of year 2010 neighborhood characteristics for these home neighborhoods. Lower grade assignment

Table 5: C-B Boundary Effects on Outgoing Visit Patterns, Work and Nonwork Visits

Destination Neighborhood:	Pr(C or D Grade)			Income		
	(1) Work	(2) Nonwork	(3) Weekend	(4) Work	(5) Nonwork	(6) Weekend
Lower Graded (C) Side	0.114***	0.097***	0.111***	0.007	-0.040***	-0.038***
	(0.029)	(0.024)	(0.031)	(0.016)	(0.011)	(0.011)
Observations	217	217	217	217	217	217
Within-boundary Standard Deviation	0.107	0.053	0.065	0.139	0.065	0.076
Point Estimate / Std Dev	1.062	1.843	1.710	0.053	-0.614	-0.503

Statistical significance: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Notes: This table reports results analogous to those in Tables 2 and 3 but split by work and non-work visits. A visit is considered work (non-work) if it lasts longer (shorter) than six hours. Columns 3 and 6 report weekend visits as an alternative definition of non-work. See the notes to Tables 2 and 3 for further details.

is associated with more incoming visits from lower-graded neighborhoods (Column 1), from lower-income home neighborhoods on average (Column 2), and from neighborhoods with lower college graduation rates (Column 3). There is no significant difference in the racial composition of incoming visits (column 4) or the number of incoming visits per neighborhood resident (column 5). The constellation of estimates mirrors outgoing visits, suggesting that the redlining maps had the effect of creating segregated networks of neighborhoods that tend to visit each other, even to the present day.

Table 6: C-B Boundary Effects on Incoming Visits

Home Neighborhood of Incoming Visits:	(1) Pr(C or D Grade)	(2) Income	(3) College	(4) Minority	(5) Visits/Capita
Lower Graded (C) Side	0.129***	-0.043*	-0.018**	0.012	0.058
	(0.026)	(0.018)	(0.006)	(0.015)	(0.055)
Observations	217	217	217	217	217
Within-boundary Standard Deviation	0.089	0.115	0.041	0.062	1.614
Point Estimate / Std Dev	1.446	-0.374	-0.449	0.188	0.036

Statistical significance: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Notes: This table reports results analogous to those in Tables 2 and 3 but for incoming visits. The dependent variable is: the fraction of incoming visits that originate in home neighborhoods with a historical grade of C or D (column 1), the frequency-weighted mean of home neighborhood log median income, fraction non-White, or fraction college graduates (columns 2 to 4), or the number of incoming visits per neighborhood resident (column 5). See the notes to Tables 2 and 3 for further details.

3.4 Are The Results Driven by Income Segregation?

One potential explanation for our results is that the lower-graded side of the boundary also has persistently lower income, and therefore its residents are less likely to visit and interact with higher income neighbors in a city, especially for non-work related reasons. In this subsection, we consider this possibility by measuring the extent to which modern boundary differences in neighborhood household income can explain boundary differences in outgoing visits.

To do so, we adopt the counterfactual approach of Heckman et al. (2013) that is also used in Heckman and Pinto (2015). We suppress the subscript i denoting neighborhoods and instead use the subscripts d to denote the (potentially counterfactual) state in which a neighborhood either receives the "treatment" of lower grade assignment (d = 1) or is assigned the higher grade (d = 0). Thus, in our sample of C-B boundary block groups, 0 denotes the state in which a given block group is assigned a grade of B and 1 denotes the state in which it is assigned a grade of C. We assume a linear model for each counterfactual outcome, which is given by Equation (2):

$$Y_d = \kappa_d + \alpha_d N_d + \gamma_d U_d + \phi_d B + \tilde{\epsilon_d}. \tag{2}$$

This equation introduces three new variables: N denotes neighborhood household income, U denotes other unobserved aspects of the neighborhood which vary within boundaries, and B denotes unobserved aspects of the area which are common across all neighborhoods at a given boundary and therefore are not affected by the treatment.

Equation (2) reveals that lower-grade assignment could affect visit patterns in several ways. It could have a direct impact ($\kappa_1 \neq \kappa_0$). It could change the level of neighborhood household income ($N_1 \neq N_0$) or modify its impact on visit patterns ($\alpha_1 \neq \alpha_0$). It could also change the unobserved aspects of the neighborhood that vary across neighborhoods within boundary areas ($U_1 \neq U_0$) or modify their impact on visit patterns ($\gamma_1 \neq \gamma_0$). Finally, it could also modify the impact of unobserved common boundary-level features on visit patterns ($\phi_1 \neq \phi_0$).

Taking into account these unobserved variables, Equation (2) can be rewritten as:

$$Y_d = \beta_d + \alpha_d N_d + \phi_d B + \epsilon_d, \tag{3}$$

where: $\beta_d = \kappa_d + \gamma_d E[U_d]$ and $\epsilon_d = \tilde{\epsilon}_d + \gamma_d(U_d - E[U_d])$. This implies that, for ϵ_d to be uncorrelated with N_d, neighborhood income must be uncorrelated with unobserved aspects of the neighborhood. This assumption is implausible in our setting. While adding more observable measures of the neighborhood is a potential econometric solution, it is unlikely that existing neighborhood data is sufficiently rich. Recognizing this limitation, we redefine income to encompass other unobserved aspects of the neighborhood projected onto income. Under this interpretation, the above uncorrelatedness assumption holds by construction.

We now simplify the problem further by assuming that lower-grade assignment does not modify the impact of income on visit patterns ($\alpha_1 = \alpha_0$). Under the maintained assumption that neighborhood income is uncorrelated with unobserved aspects of the neighborhood, it is possible to test this hypothesis in a standard regression (Heckman and Pinto, 2015). We do so and, as we show below, find no evidence to reject it. Similarly, we also assume that lower-grade assignment does not modify the impact of unobserved aspects of the area ($\phi_1 = \phi_0$). In principle, it is possible to test this assumption as well. However, because we use a boundary fixed-effects regression, it is impractical to do so.[18]

[18] Testing this assumption would require estimating the interaction between lower-grade assignment and an indicator for each boundary in our sample ($49 \times 2 = 98$ additional parameters on a sample size of 217), which would result in extremely noisy

With the above assumptions in place, realized visit patterns can be expressed as:

$$\begin{aligned}Y &= D(\beta_1 + \alpha N_1 + \phi B + \epsilon_1) + (1-D)(\beta_0 + \alpha N_0 + \phi B + \epsilon_0) \\ &\equiv \beta_0 + \beta D + \alpha N + \phi B + \epsilon,\end{aligned} \tag{4}$$

where $D \in \{0,1\}$ represents realized treatment status. Equation (4) can be estimated by replacing unobserved boundary-level features with boundary fixed effects: this is equivalent to Equation (1) with the addition of neighborhood income as a control. The resulting estimates are consistent as long as the previously mentioned assumptions are maintained. The results can be interpreted as the average treatment effect $E[Y_1 - Y_0]$ and decomposed as:

$$E[Y_1 - Y_0] = \alpha E[N_1 - N_0] + \beta D \tag{5}$$

The first term in Equation (5) represents the component of the average treatment effect that is explained by boundary income segregation and the second term represents the residual component that includes the impact of changes to unobserved aspects of the neighborhood.

We perform the above decomposition in two ways. In the first method, we obtain our estimate of $E[N_1 - N_0]$ by repeating our baseline analysis with neighborhood income as the dependent variable. Then Equation (4) is estimated directly using low propensity C-B boundary block groups. To formally test the assumption that lower-grade assignment does not modify the impact of neighborhood income, we concurrently estimate a version of Equation (4) where the coefficient on neighborhood income is allowed to vary by lower grade status and test for equality of the coefficients.

$$\bar{Y}_1 - \bar{Y}_0 = \alpha(\bar{N}_1 - \bar{N}_0) + \beta + \bar{\epsilon}_1 - \bar{\epsilon}_0 \tag{6}$$

We also generate results using a second method that takes advantage of our boundary design. At the boundary level, the difference in mean dependent variable between the C and the B-graded side can be expressed as Equation (6), where $\bar{Y}_1$ and $\bar{Y}_0$ denote the mean taken over block groups on the C-graded and B-graded side of the boundary, respectively. Collapsing our data to the boundary level, we analyze how boundary differences (C minus B) in visit patterns are associated with boundary differences in 2010 neighborhood income.

Panel (a) of Figure 1 plots the difference in the share of visits to C or D graded neighborhoods against the difference in home neighborhood income. Similarly, Panel (b) plots the difference in visited neighborhood income against the difference in home neighborhood income. We observe a considerable amount of variation in the boundary difference in neighborhood income. While this difference is on average negative, it is fairly

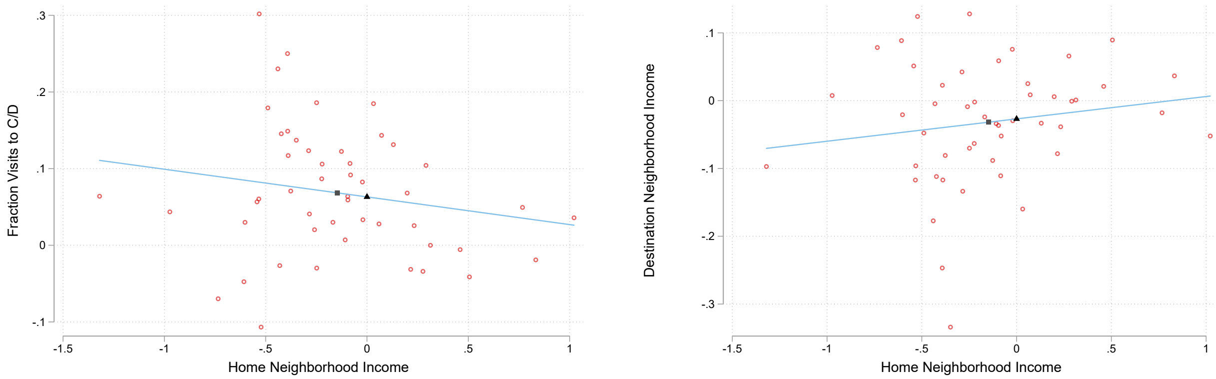
estimates. Another effect of this would be that β_0 and β_1 would not longer be identified, since we would effectively be estimating a boundary-specific effect of lower grade assignment.

common to observe individual boundaries where the difference is reversed (that is, the lower graded side exhibits higher present-day household income). As expected, we observe that boundaries with a more negative difference in neighborhood income also tend to have visit destinations that are more disadvantaged in terms of historical grade and present-day neighborhood income. We formally estimate α as the slope coefficient from a univariate regression of boundary differences in visit patterns against boundary differences in neighborhood income.

Figure 1: Boundary Differences in Visit Patterns against Boundary Differences in Resident Incomes

(a) C-B Boundaries: Fraction Visits to C/D

(b) C-B Boundaries: Destination Neighborhood Income



Notes: Each circle describes a low propensity C-B boundary. The y-axis is the boundary difference (C-graded minus B-graded) in visits, measured in Panel a by C or D neighborhoods and Panel B by household income in 2010. The x-axis is the boundary difference in home neighborhood income (C minus B). The blue line represents the linear best fit line. The square (triangle) represents the linear prediction evaluated at the sample mean (zero) for the home neighborhood income boundary difference.

Table 7 displays the results. The first row shows our estimates of the average effect of living on the lower-graded side of a boundary on visit patterns. The second row of estimates are the coefficients on income of home census block group. The third row shows the share of the treatment effect that is explained by home census block group income. The last row, after the number of observations, displays the p-value from a test of whether the coefficient on income on the lower graded and higher graded side are equal. For the fraction of visits to C or D neighborhoods (Columns 1 and 2), we find that home neighborhood income differences explain about 10 percent of the overall baseline boundary effect. For destination neighborhood income, in Columns 3 and 4 we find that boundary income differences can explain between 15 and 22 percent of the overall boundary effect. As mentioned, for the block-group-level analysis (Columns 1 and 3), we also test and are unable to reject the hypothesis that the effect of neighborhood income does not vary with lower grade status. We conclude that residential income segregation effects at C-B boundaries do not appear to be the primary driver of our baseline results.

3.5 D-C Boundary Effects

Our results for D-C boundaries are overall similar to the results for C-B boundaries, albeit with slightly smaller effect sizes and some loss of statistical significance at times. In Appendix Tables A3 through A7 we present versions of the results tables for the boundary effect of D grade assignment at D-C boundaries. We

Table 7: Explaining Boundary Effects Using Home Neighborhood Income

Destination Neighborhood:	Pr(C/D Grade)		Income	
	(1) Block Group	(2) Boundary	(3) Block Group	(4) Boundary
Average Treatment Effect (ATE)	0.098***	0.068***	-0.039**	-0.032*
	(0.026)	(0.012)	(0.013)	(0.014)
Explained by Home Neighborhood Income:				
Explained Component	0.011+	0.005	-0.008	-0.005
	(0.006)	(0.004)	(0.006)	(0.004)
as Fraction of ATE	0.108*	0.078	0.219	0.153
	(0.044)	(0.053)	(0.145)	(0.177)
N	217	49	217	49
p-value: Treatment x Income = 0?	0.143		0.630	

Statistical significance: $^{+}\ p < 0.1$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$.
Notes: This table decomposes the estimated outgoing visit effects along the C-B boundaries estimated in previous tables using the low propensity sample but split into a component that is explained by home neighborhood income differences and an unexplained residual component. The explained component is the mean boundary difference in home neighborhood income, multiplied by a coefficient that describes its impact on visit patterns. Columns 1 and 3 estimate this coefficient by controlling for home neighborhood income in the boundary fixed effects regression. Separately, we estimate a specification that allows for the interaction between C grade assignment and home neighborhood income, and report p-values for a test of its significance. In Columns 2 and 4, the sample is collapsed to the boundary level and boundary differences (lower-graded side minus higher-graded side) in dependent variables are regressed on boundary differences in neighborhood income. Standard errors are clustered by bootstrap resampling of boundaries.

highlight only differences with the C-B boundary results here.

We find smaller positive effects on the fraction of visits to C or D neighborhoods, although this is due to a large increase in the fraction of visits to D neighborhoods and a decrease in the fraction of visits to C neighborhoods. Therefore, the impact of assigning the lowest grade appears to be to increase the fraction of visits to other D neighborhoods at the expense of visits to all other credit risk grades. Considering the 2010 mean characteristics of destination neighborhoods, we only find significant negative impacts on neighborhood income but not the college graduation rate nor (same as with C-B boundaries) racial composition. Our findings regarding the fraction of visits to D neighborhoods are robust to dropping short-distance visits but the coefficients for mean destination neighborhood income are not precisely estimated and become statistically insignificant. Boundary effects on work and non-work travel are similar to those for total visits, but once again the results for destination neighborhood income are noisily estimated and not statistically significant. Finally, we find significant effects on incoming visits in terms of historical D grade and income, but not for the college graduation rate.

4 City-level Impacts of Redlining Maps

The boundary design identifies disparities in visit patterns between adjacent neighborhoods along map boundaries. However, the redlining maps likely had broader effects, potentially impacting patterns of experienced segregation throughout a city. For example, if the presence of a redlining map caused disinvestment that increased awareness of a city's income disparities or changed public sentiment towards public transport investment, there may be fewer visits between high-income and low-income neighborhoods.

4.1 Empirical Strategy

To identify aggregate city effects, we take advantage of the population discontinuity that determined whether cities were mapped in the 1930s. In particular, the HOLC created maps for cities with a population of at least 40,000 people. Similar to Aaronson et al. (2021) and Anders (2023), we restrict the sample to 51 cities with a population within 10,000 persons above (and therefore mapped) or below (and therefore not mapped) this cutoff. Because these cities were comparable in size at the time of map creation, differences within this sub-sample plausibly reflect the causal effect of the redlining maps on experienced segregation at the city level.[19]

Our city-level analysis is based on transition matrices. We categorize census block groups into city-specific neighborhood income quintiles and then estimate a quantile transition matrix of home-to-destination visits.[20] The (i, j) cell of a matrix describes the percentage probability that, conditional on residing in a home neighborhood of income quantile i, a visit has a destination in a neighborhood of income quantile j. The appendix includes the transition matrices for redlined and non-redlined cities separately but we focus on the difference between redlined and non-redlined cities in Table 8. A positive (negative) cell indicates relatively higher (lower) mobility between the home and destination neighborhoods of the corresponding income quantile among redlined cities, which when describing mobility between different quantiles translates into a lower (higher) degree of experienced segregation. Positive numbers along the diagonal indicate a higher degree of experienced segregation in redlined cities.

This transition matrix approach allows us to assess mobility at different points of the neighborhood income distribution. The segregation of low-income neighborhoods may be of particular interest for understanding how experienced segregation may further isolate households already facing difficult economic circumstances. Furthermore, the particular salience of the C or D neighborhood grades is likely to have led to larger mobility impacts at lower income quintiles, where the majority of neighborhoods receiving such grades are found.[21]

[19] Aaronson et al. (2021) report no difference in 1910-1930 characteristics between mapped and non-mapped cities within 10,000 people of the population cutoff.

[20] Since the income quintiles are computed from each city's income distribution, our results characterize patterns in relative mobility, such as the probability of visits from a city's poorest neighborhoods to a city's richest neighborhoods. Estimating transition matrices on the national distribution is complicated by large cross-city income differences, meaning that many cities have no neighborhoods in one or more quintiles. While it is possible to estimate transition matrices with a smaller number of quantiles, this would result in a loss of detail and (as we explain later) we expect to see the greatest impacts at the bottom of the distribution.

[21] In the sample of redlined cities just above the population cutoff, 41 percent of C-B boundary block groups and 50 percent of D-C boundary block groups lie in the lowest income quintile.

4.2 Results

Table 8 presents the main results.[22] Looking across the first row, visits originating from bottom-quintile home neighborhoods are 2.4 percentage points more likely to lead to a bottom-quintile destination neighborhood in redlined cities. For economic context, this effect is 7 percent of the base 34 percent visit rate in non-redlined cities. Conversely, visits from the poorest neighborhoods in redlined cities are less likely to conclude in neighborhoods in any other income quintile. Moreover, redlined cities experience similar reductions in incoming visits from other income quintiles into bottom-quintile neighborhoods; this result can be seen in the first column of the transition matrix. Overall, these patterns provide clear evidence that redlining maps increased experienced segregation near the bottom of the income distribution. We find similar results for the second poorest quintile of neighborhoods (row 2), especially for incoming visits. The story is mixed at the top of the neighborhood income distribution – outgoing visits originating from the top quintile neighborhoods in redlined cities are less likely to be destined for top or bottom quintile neighbrhoods and more likely to be to third and fourth quintile neighborhoods (row 5), suggesting by at least one metric that experienced segregation may be higher among the wealthier households in redlined cities. But fewer residents are visiting top quintile neighborhoods (column 5), regardless of their home income quintile.

Table 8: Difference in Neighborhood Income Quintile Mobility, Redlined Minus Non-redlined Cities

Neighborhood Income of Destination:	1	2	3	4	5
Origin:					
1	2.4	*-0.4*	*-0.4*	*-0.5*	*-1.2*
	(0.07)	(0.07)	(0.07)	(0.06)	(0.06)
2	*-0.7*	0.5	0.7	0.6	*-1.2*
	(0.07)	(0.07)	(0.06)	(0.06)	(0.05)
3	*-3.4*	*-0.6*	2.5	1.5	*-0.1*
	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)
4	*-2.7*	*-1.1*	2.2	2.6	*-1*
	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)
5	*-1.6*	*-0.8*	2.9	2	*-2.5*
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)

Notes: This table shows the difference (redlined cities minus non-redlined cities) in the percent of visits from home census block groups (column) to destination census block groups (row), where home and destination block groups are determined by their income quintile ranking within a city. The sample of redlined cities had a population between 40,000 and 50,000 in 1930. The sample of non-redlined cities had a population between 30,000 and 40,000 in 1930. Standard errors are generated from 100 bootstrap replications, each sampling 10 million visits.

We corroborate our transition matrix results with five summary mobility measures from Formby et al. (2004), which are described in Table 9. Each measure is a scalar index of a function of the transition matrix and

[22] The transition matrices for redlined and non-redlined cities are available in Table A8 and A9, respectively. For both sets of cities, we observe high isolation of neighborhoods in the bottom (1st) income quintile: over a third of visits from these home neighborhoods are to bottom quintile destination neighborhoods. Top (5th) income quintile home neighborhoods exhibit a similar pattern of isolation, albeit to a smaller degree. Neighborhoods in the three middle quintiles exhibit visit patterns that are more uniformly distributed.

- in some cases - the marginal distribution of neighborhood incomes. By convention, higher values of each of these measures indicate less mobility across income quantiles, which implies greater experienced segregation.

Table 9: Summary Mobility Measures

$M_1(P) =$	$\frac{1}{m-1}\left(m - \sum_{i=1}^{m} p_{ii}\right)$
$M_2(P) =$	$1 - \lvert\lambda_2\rvert$
$M_3(P) =$	$1 - \left\lvert\Pi_{i=1}^{m}\lambda_i\right\rvert$
$M_4(P) =$	$\frac{1}{m-1}\left(m - m\sum_{i=1}^{m} \pi_i p_{ii}\right)$
$M_5(P) =$	$\frac{1}{m-1}\sum_{i=1}^{m}\sum_{j=1}^{m} \pi_i p_{ij}\lvert i-j\rvert$

Notes:
P is a transition matrix of dimension $m \times m$. In our analysis, $m = 5$. p_{ij} refers to the (i, j)th cell of this matrix, which represents the probability that a visit from home neighborhood income quantile i has a destination in income quantile j. λ_k refers to the kth eigenvalue of P, arranged in descending order by magnitude. π_i is the fraction of home neighborhoods in quantile i. In our analysis, this is uniformly equal to 0.2.

We compute these mobility measures for the 51 cities in our near-40,000 population sample. Next, we run a city-level regression of a mobility measure on an indicator for being above the population cutoff. Table 10 presents the results. We find coefficients that are negative and, in 4 out of 5 cases, statistically significant. Ignoring the large (negative) outlier coefficient, M_3, we find that redlined cities have lower movement across neighborhood income quintiles by roughly 0.6 of a standard deviation, again consistent with the maps increasing experienced segregation by income.

Table 10: Regression of Summary Mobility Measures on HOLC Residential Security Map Eligibility

	(1) M_1	(2) M_2	(3) M_3	(4) M_4	(5) M_5
Above Cutoff	-0.58$^+$	-0.59$^+$	-2.76$^+$	-0.50	-0.67*
	(0.33)	(0.30)	(1.44)	(0.30)	(0.32)
Mean of Dependent variable, Non-redlined Cities	0.964	0.839	1.000	0.967	0.376
S.D. of Dependent variable, Non-redlined Cities	0.0314	0.0792	0.000000985	0.0412	0.0293
N, Non-redlined Cities	25	25	25	25	25
N, Redlined Cities	26	26	26	26	26

Statistical significance: $^+$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.
Notes: The table reports the results of a regression of a summary mobility measure on an indicator for the presence of a HOLC residential security map (and a constant), using a sample of cities within 10,000 residents of the 40,000 population cutoff for map creation. Reported coefficients are standardized: a value of 1 means an effect equal to 1 standard deviation in the below-40,000 population subsample. Summary mobility measures are derived from a city's transition matrix of neighborhood income quintile mobility. Robust standard errors are displayed in parentheses.

5 Conclusion

The creation of the Residential Security Maps in the 1930s played a significant role in the disparate development of urban neighborhoods in major U.S. cities. Beyond the direct impacts on redlined neighborhoods and their residents, this study shows that the maps also affected travel patterns between neighborhoods that in turn, reveal another form of segregation that individuals experience through their daily interactions with others of different backgrounds. In particular, living on the lower graded side of a map boundary, or in a city that was mapped relative to a comparably-sized city that was not, leads to more segregated interactions outside of home neighborhoods. These results are robust to excluding highly localized visits, and we do not find that they are driven by travel to work, or by the income level of the home neighborhood. Together, the results suggest that differing preferences for leisure destinations are the main driver of disparities in visit patterns.

We also find that the presence of a redlining map for a city led to meaningful increases in segregation of visits, particularly among the poorest neighborhoods. This magnifies the significance of previous findings on the impact of redlining: not only does redlining increase income (and other) disparities between home neighborhoods, it reduces the probability that residents of low-income neighborhoods visit medium- and high-income neighborhoods and that residents of medium- and high-income neighborhoods visit low-income neighborhoods.

Segregation remains a feature of contemporary American cities. Beyond residential neighborhoods, segregation also characterizes the pattern of regular trips and the overall exposure of urban residents to different types of neighborhoods. Our study provides evidence that historical government action restricting mortgage credit access has affected the links between neighborhoods in a manner that persists to the present day.

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Appendix

Table A1: Sample Size

Boundary Type:	C-B		D-C	
	(1)	(2)	(3)	(4)
	Block Groups	Low Propensity Block Groups	Block Groups	Low Propensity Block Groups
Full Sample	2811	726	3148	1239
Boundary Has Propensity Score	1410	726	2098	1239
Aligned at Boundary	773	388	1223	712
Uniform Grade	589	301	972	581

Notes: Table shows the number of Census block groups in the sample as restrictions are imposed cumulatively. Full Sample: all block groups intersecting with boundary buffer zones. Propensity scores for location of HOLC neighborhood boundaries are estimated based on 1930s neighborhood differences. Boundary buffer zones and propensity scores used are identical to Aaronson et al. (2021). Low propensity scores are those below 0.3 (the sample median). Aligned at Boundary: Census block group boundary aligns with Residential Security Map boundary. Uniform Grade: At least 80 percent of the block group (by land area) has the same grade.

Table A2: Boundary Effects on Outgoing Visit Patterns, Alternate Sample Restrictions

Destination Neighborhood:	Pr(C/D Grade)					Income				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
C-B Boundaries:										
Lower Graded Side	0.071***	0.086***	0.097***	0.106***	0.079***	-0.040***	-0.035***	-0.055***	-0.043***	-0.061***
	(0.012)	(0.020)	(0.015)	(0.025)	(0.012)	(0.007)	(0.010)	(0.009)	(0.012)	(0.012)
Observations	644	307	280	154	226	644	307	280	154	226
Within-boundary Standard Deviation	0.053	0.053	0.053	0.053	0.053	0.065	0.065	0.065	0.065	0.065
Point Estimate / Std Dev	1.340	1.612	1.831	1.992	1.488	-0.611	-0.532	-0.842	-0.654	-0.936
Destination Neighborhood:	Pr(D Grade)					Income				
D-C Boundaries:										
Lower Graded Side	0.074***	0.078***	0.088***	0.085***	0.084***	-0.015*	-0.016**	-0.013+	-0.011+	-0.019
	(0.007)	(0.008)	(0.008)	(0.009)	(0.015)	(0.006)	(0.006)	(0.007)	(0.007)	(0.014)
Observations	1055	643	545	374	291	1054	642	544	373	291
Within-boundary Standard Deviation	0.042	0.042	0.042	0.042	0.042	0.065	0.065	0.065	0.065	0.065
Point Estimate / Std Dev	1.773	1.870	2.111	2.046	2.018	-0.223	-0.240	-0.193	-0.175	-0.285
Restrict Fraction of Block Group with Grade	0.5	0.5	1	1	0.8	0.5	0.5	1	1	0.8
Restrict Propensity Score		Low		Low	High		Low		Low	High

Statistical significance: $^{+}\ p < 0.1$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$.

Notes: Each observation is a census block group located at C-B (top panel) or D-C (bottom panel) boundaries. Block groups are added to the sample with repetition if they are part of multiple boundaries. Sample restrictions vary by column. Restrict Fraction of Block Group with Grade: Block groups are assigned a grade only if at least this fraction of land area is assigned this grade on a redlining map. Restrict Propensity Score: Low (High) propensity scores are those below (above) 0.3 (the sample median). The dependent variable is either: a) the fraction of outgoing visits that lead to neighborhoods with historical credit risk grades given in the column header or b) the frequency-weighted mean of destination neighborhood income, drawn from the 2010 census. All specifications control for boundary fixed effects. The within-boundary standard deviation is estimated from boundary block groups using the residual variance from a secondary regression of the dependent variable on boundary fixed effects. Standard errors are clustered by boundary.

Table A3: D-C Boundary Effects on Outgoing Visit Patterns, Historical Credit Risk Grades

Fraction of Outgoing Visits to:	C,D		D		C		A,B	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lower Graded (D) Side	0.016**	0.018*	0.083***	0.082***	-0.060***	-0.060***	-0.008***	-0.010***
	(0.006)	(0.007)	(0.008)	(0.009)	(0.007)	(0.008)	(0.002)	(0.003)
Observations	790	499	790	499	790	499	790	499
Standard Deviation of Dependent Variable								
• Overall	0.225	0.225	0.109	0.109	0.136	0.136	0.087	0.087
• Within-Boundary	0.053	0.053	0.042	0.042	0.046	0.046	0.035	0.035
Ratio of Point Estimate to Std Dev								
• Overall Std Dev	0.072	0.082	0.756	0.749	-0.440	-0.445	-0.097	-0.111
• Within-Boundary Std Dev	0.304	0.346	1.982	1.964	-1.309	-1.322	-0.243	-0.277
Sample: Low Propensity Boundaries		Y		Y		Y		Y

Statistical significance: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Notes: An observation is a census block group located at D-C boundaries that follows the sample restrictions and the definition of the low propensity score sample described in the text. Block groups are added to the sample with repetition if they are part of multiple boundaries. The dependent variable is the fraction of visits by residents of boundary block groups to destinations in neighborhoods with historical credit risk grades given in the column header. All specifications control for boundary fixed effects. The overall standard deviation is estimated using all block groups in cities with Residential Security Maps. The within-boundary standard deviation is estimated from boundary block groups using the residual variance from a secondary regression of the dependent variable on boundary fixed effects. Standard errors are clustered by boundary.

Table A4: D-C Boundary Effects on Outgoing Visit Patterns, Frequency-Weighted Means of 2010 Destination Neighborhood Characteristics

Destination Neighborhood:	Income		College		Minority		Black	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lower Graded (D) Side	-0.015*	-0.013+	-0.010**	-0.005	0.016***	0.006	0.015*	0.007
	(0.006)	(0.006)	(0.004)	(0.003)	(0.005)	(0.005)	(0.006)	(0.007)
Observations	789	498	789	498	789	498	789	498
Standard Deviation of Dependent Variable								
• Overall	0.317	0.317	0.133	0.133	0.189	0.189	0.170	0.170
• Within-Boundary	0.065	0.065	0.030	0.030	0.040	0.040	0.042	0.042
Ratio of Point Estimate to Std Dev								
• Overall Std Dev	-0.046	-0.040	-0.074	-0.035	0.083	0.034	0.091	0.039
• Within-Boundary Std Dev	-0.224	-0.193	-0.330	-0.156	0.391	0.159	0.369	0.159
Sample: Low Propensity Boundaries		Y		Y		Y		Y

Statistical significance: $^{+}\ p < 0.1$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$.
Notes: An observation is a census block group located at D-C boundaries that follows the sample restrictions and the definition of the low propensity score sample described in the text.. Block groups are added to the sample with repetition if they are part of multiple boundaries. The dependent variable is the frequency-weighted mean of a destination neighborhood's characteristics, drawn from the 2010 census. These characteristics are: log median income (columns 1 and 2), college graduation rate (columns 3 and 4), fraction non-White residents (columns 5 and 6), and fraction Black residents (columns 7 and 8). All specifications control for boundary fixed effects. The overall standard deviation is estimated using all block groups in cities with Residential Security Maps. The within-boundary standard deviation is estimated from boundary block groups using the residual variance from a secondary regression of the dependent variable on boundary fixed effects. Standard errors are clustered by boundary.

Table A5: D-C Boundary Effects on Outgoing Visit Patterns, Alternate Restrictions on Visit Counts

Destination Neighborhood:	Pr(D Grade)			Income		
	(1)	(2)	(3)	(4)	(5)	(6)
Lower Graded (D) Side	0.033***	0.062***	0.022**	-0.011*	-0.011	-0.007
	(0.004)	(0.007)	(0.007)	(0.005)	(0.007)	(0.007)
Observations	499	499	499	498	499	499
Within-Boundary Standard Deviation	0.021	0.040	0.034	0.047	0.067	0.067
Coefficient/(Within-boundary S.D.)	1.610	1.565	0.628	-0.224	-0.162	-0.102
Bottom-coded Visits	Y			Y		
Drop Nearby Visits:						
– Boundary Tracts		Y	Y		Y	Y
– Adjacent Tracts			Y			Y

Statistical significance: $^{+}\ p < 0.1$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$.
Notes: An observation is a census block group located at D-C boundaries that follows the sample restrictions and the definition of the low propensity score sample described in the text. Block groups are added to the sample with repetition if they are part of multiple boundaries. The dependent variable is either: the fraction of outgoing visits to destinations with a historical grade of C or D (columns 1 to 3), or the frequency-weighted mean of destination neighborhood log median income (columns 4 to 6). Columns 1 and 4 include rare (less than 5 per month) home-to-destination visits (see text for more detail). Columns 2, 3, 5, and 6 drop nearby destinations, defined as census tracts containing any boundary block groups (columns 2, 3, 5, and 6) and census tracts adjacent to boundary tracts (columns 3 and 6). All specifications control for boundary fixed effects. The overall standard deviation is estimated using all block groups in cities with Residential Security Maps. The within-boundary standard deviation is estimated from boundary block groups using the residual variance from a secondary regression of the dependent variable on boundary fixed effects. Standard errors are clustered by boundary.

Table A6: D-C Boundary Effects on Outgoing Visit Patterns, Work and Nonwork Visits

Destination Neighborhood:	Pr(D Grade)			Income		
	(1) Work	(2) Nonwork	(3) Weekend	(4) Work	(5) Nonwork	(6) Weekend
Lower Graded (D) Side	0.096***	0.081***	0.087***	0.008	-0.013*	-0.012
	(0.015)	(0.009)	(0.011)	(0.013)	(0.006)	(0.008)
Observations	499	499	499	498	498	498
Within-boundary Standard Deviation	0.088	0.041	0.052	0.139	0.065	0.076
Point Estimate / Std Dev	1.090	1.961	1.689	0.060	-0.202	-0.155

Statistical significance: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Notes: This table reports results analogous to those in Tables A3 and A4 but split by work and non-work visits. A visit is considered work (non-work) if it lasts longer (shorter) than six hours. Columns 3 and 6 report weekend visits as an alternative definition of non-work. See the notes to Tables 2 and 3 for further details.

Table A7: D-C Boundary Effects on Incoming Visits

Home Neighborhood of Incoming Visits:	(1) Pr(D Grade)	(2) Income	(3) College	(4) Minority	(5) Visits/Capita
Lower Graded (D) Side	0.128***	-0.024*	-0.006	0.002	-0.520
	(0.012)	(0.012)	(0.004)	(0.007)	(0.702)
Observations	498	498	498	498	498
Within-boundary Standard Deviation	0.064	0.115	0.041	0.062	1.614
Point Estimate / Std Dev	2.005	-0.212	-0.143	0.037	-0.322

Statistical significance: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Notes: This table reports results analogous to those in Tables A3 and A4 but for incoming visits. The dependent variable is: the fraction of incoming visits that originate in home neighborhoods with a historical grade of D (column 1), the frequency-weighted mean of home neighborhood log median income, fraction non-White, or fraction college graduates (columns 2 to 4), or the number of incoming visits per neighborhood resident (column 5). See the notes to Tables 2 and 3 for further details.

Table A8: Neighborhood Income Quintile Transition Matrix, Redlined Cities

Neighborhood Income of Destination:	1	2	3	4	5
Origin:					
1	36.49	23.32	18.17	12.8	9.22
	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)
2	25.57	23.22	21.7	17.03	12.47
	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)
3	19.25	21.89	22.79	20.26	15.81
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
4	16.09	19.23	22.77	22.63	19.28
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
5	13.23	16.23	20.15	22.42	27.97
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)

Notes: Each cell shows the percent of visits by cell phones with nighttime locations in a neighborhood of row income quintile that lead to a neighborhood of column income quintile. Visits are pooled across all cities with year 1930 population just above the 40,000 cutoff for HOLC residential security map creation. There are 27 such cities, with populations ranging from 40,108 to 48,764. Neighborhood income quantiles are determined separately by city. Standard errors are generated from 100 bootstrap replications, each sampling 10 million visits.

Table A9: Neighborhood Income Quintile Transition Matrix, Non-redlined Cities

Neighborhood Income of Destination:	1	2	3	4	5
Origin:					
1	34.05	23.68	18.57	13.26	10.44
	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
2	26.23	22.73	20.98	16.43	13.63
	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)
3	22.61	22.48	20.29	18.74	15.88
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
4	18.8	20.29	20.57	20.02	20.31
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
5	14.8	17.04	17.28	20.45	30.43
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)

Notes: Each cell shows the percent of visits by cell phones with nighttime locations in a neighborhood of row income quintile that lead to a neighborhood of column income quintile. Visits are pooled across all cities with year 1930 population just below the 40,000 cutoff for HOLC residential security map creation. There are 26 such cities, with populations ranging from 30,729 to 39,614. Neighborhood income quantiles are determined separately by city. Standard errors are generated from 100 bootstrap replications, each sampling 10 million visits.