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NATURAL AMENITIES, NEIGHBORHOOD DYNAMICS, AND PERSISTENCE IN THE SPATIAL DISTRIBUTION OF INCOME

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Natural Amenities, Neighborhood Dynamics, and Persistence in the Spatial Distribution of Income

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Abstract

We present theory and evidence highlighting the role of natural amenities in neighborhood dynamics, suburbanization, and variation across cities in the persistence of the spatial distribution of income. Our model generates three predictions that we confirm using a novel database of consistent-boundary neighborhoods in U.S. metropolitan areas, 1880–2010, and spatial data for natural features such as coastlines and hills. First, persistent natural amenities anchor neighborhoods to high incomes over time. Second, naturally heterogeneous cities exhibit persistent spatial distributions of income. Third, downtown neighborhoods in coastal cities were less susceptible to the widespread decentralization of income in the mid-20th century and experienced an increase in income more quickly after 1980.

Keywords: suburbanization, gentrification, locational fundamentals, multiple equilibria
JEL Classification: R23, N90, O18

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1 Introduction

Neighborhood change is common and contentious. Two-thirds of neighborhoods in the 35 U.S. metropolitan areas studied by Rosenthal (2008) transitioned from one income quartile to another between 1950 and 2000. In declining areas, homeowners fear deteriorating values even as entrants enjoy new opportunities; in gentrifying areas, rising prices cause anxiety for longtime renters. And in response to shifting neighborhood demands, policymakers often act to preserve neighborhood quality or quicken the pace of change.

Although changes in neighborhood status are widespread, it is less well known that neighborhood change varies across cities. While in some cities neighborhoods seem immune from change—leading to overall persistence in the internal structure of the city—other cities experienced quickly changing neighborhoods and spatial patterns of income. For example, Los Angeles has long had a stable arrangement of high incomes and prices along its beaches and in its foothills; between 1970 and 1980, the average neighborhood in the Los Angeles metropolitan area moved just 9 percentile points across the city's income distribution. In contrast, over the same period, the average metropolitan Dallas neighborhood moved 21 percentile points.[1]

In this paper, we examine why the geographic distribution of income is persistent for some neighborhoods and cities but turns over frequently elsewhere. Our explanation highlights the role of natural geographic features that have persistent amenity value—for example, oceans, mountains, and lakes. We begin with the idea that persistent natural amenities can "anchor" neighborhoods to high incomes even as they experience various shocks over time. A key implication is that for cities as a whole, greater *natural* variation among neighborhoods can hold back neighborhood change, leading to overall stability in the spatial distribution of income. Thus, in naturally heterogeneous Los Angeles, the spatial distribution of income is persistent, but in flat Dallas, the spatial distribution of income churns quickly.

We present a dynamic model of household neighborhood choice to formalize our thinking. Neighborhoods derive amenity value from both natural features and endogenous characteristics such as safety, school quality, or shopping. High-income households outbid low-income households for neighborhoods with greater overall amenity value. Neighborhoods are also subject to idiosyncratic shocks to amenity value over time. We characterize conditions when these shocks can potentially reverse the historical spatial pattern of income.

We test and confirm several implications of our theory using a new database of consistent-boundary neighborhoods in many U.S. metropolitan areas, spanning the census years from 1880 to 2010. We match these data to spatial information on the location of many persistent natural features, including shorelines, mountains, lakes, rivers, temperate climates, and floodplains. We also develop a hedonic weighting method to aggregate amenity values from many natural features into a single index.

[1] Appendix Figure B1 illustrates these differences between Dallas and Los Angeles.

Our first main result is that persistent natural amenities anchor neighborhoods to high incomes over time. More precisely, conditioned on initial income, neighborhoods with superior natural amenities are more likely to become or remain high-income neighborhoods. In short, this result is a nuanced version of the folk wisdom among realtors that a beachfront home will better retain its value versus one with a mundane view.

Our second and key result is that cities with dominant natural features (e.g., a coastline or mountain range) exhibit internal spatial distributions of income that are dynamically stable. In other words, neighborhood incomes tend to fluctuate less over time in a city such as Los Angeles, with its beaches, hills, and valleys, than in a city such as Dallas, which more closely resembles a flat, featureless plain. Intuitively, a shock to a neighborhood's amenity value—because of idiosyncratic migration, effects of policy, natural disasters, etc.—has the potential to change the historical distribution of income across neighborhoods. But in cities in which some neighborhoods have overwhelming natural advantages, small shocks or interventions are unlikely to undo history.

Our final main result is that the anchoring effect of superior natural amenities is stronger in a city with dominant natural features. Relying on the fact that many U.S. central cities were founded near superior natural amenities, we show that downtowns in coastal cities, compared with downtowns in interior cities, declined less in income in the early and mid-20th century and improved more in income after 1980. In short, coastal city downtowns have been better anchored to high incomes during periods of both nationwide suburbanization and gentrification.

This result relates to a central debate in economics about the roles of natural fundamentals versus endogenous amenities in the spatial distribution of income. In our model, as in others featuring endogenous amenities, multiple equilibria are possible.[2] If households care only about being near other households, then they might crowd together in any neighborhood (e.g., downtown or in the suburbs). However, this multiplicity is limited by locational fundamentals: As emphasized by our third result, fundamentals are more likely to determine outcomes when there is greater natural heterogeneity across locations.

We address several identification issues in evaluating our evidence. One important empirical challenge is that we do not directly observe the amenity value of natural features. For example, a natural feature can be either an amenity or a disamenity: A river used for industrial purposes can detract from surrounding neighborhoods. To address this concern, we first focus on high-value natural features, such as proximity to the ocean, for which we believe the benefits are obvious and significant relative to the value of other amenities. A second strategy is to condition natural features on other observables, such as historical incomes or place names, that are more likely in the presence of superior amenities. Intuitively, stretches of coastline that have historically attracted high-income households are probably quite amenable. We also discuss the related identification challenge of changes in the value of natural features over time. We find that the anchoring effect

[2]This feature is shared with models in which endogenous benefits come instead from agglomeration economies (cf. Krugman, 1991; Rauch, 1993; and Arthur, 1994).

of natural amenities on neighborhood income *ranks* has not increased significantly nor has the tendency for the highest-income households to locate near natural features.

Neighborhoods in growing cities tend to experience greater fluctuations in income. Our view is that city growth acts as a shock to the relative value of extant neighborhoods. In our empirical work, we adjust for changes in city size by examining only changes in *relative* percentile rankings within a fixed group of neighborhoods in each 10-year period. In addition, we show that our results are robust to controlling for city growth and the age of housing.

1.1 Related work

First, our work is related to a broad body of literature examining the geographic sorting of different types of households (e.g., Tiebout, 1956). Of course, the cross-sectional implications of variation in natural value are well known. Neighborhoods near large water features or at elevation have long attracted affluent households. For example, patricians preferred ancient Rome's hills:

> Certain districts [were] favored more than others; some, because they are accessible; and others, because they are beautiful in themselves or command a fine view. The Aventine, Caelian, Palatine, even the Sacred Way and the Subura, the Carinae, the Esquiline, Quirinal, Viminal, Pincian, the Campus Martius, the Capitoline and the district beyond the Tiber—all these furnish sites for private homes. (Witherstine, 1926, p. 566)

Similarly, in New York City, "[b]efore the American Revolution, the wealthiest residents of Manhattan lived on the waterfront lanes—especially Dock Street—at the southeastern tip of the island, where they could enjoy proximity to business and the beauty of the upper bay" (Jackson, 1985, p. 18). What is less frequently observed is how natural features might restrain neighborhood dynamics. In Brueckner, Thisse, and Zenou's (1999) static monocentric city model of household location choice, there may be multiple equilibria—with the rich living in either the city center or the suburbs—if the exogenous amenity advantage of the center is small. Our contribution is to extend this intuition to a dynamic setting. In addition, our work departs from theirs in testing these implications empirically.[3]

Second, previous work has highlighted various factors in neighborhood change: aging homes filtering from high- to low-income households (Brueckner and Rosenthal, 2009), spillovers among neighborhoods (Aaronson, 2001; Guerrieri, Hartley, and Hurst, 2013), transportation technology or infrastructure (LeRoy and Sonstelie, 1983; Baum-Snow, 2007; Glaeser, Kahn, and Rappaport, 2008), African American migration to cities (Boustan, 2010), or a combination of factors (Kolko,

[3]Other theoretical papers share a similar intuition about transitions between static equilibria, with Fujita and Ogawa (1982) being an early example. Krumm (1980) provides and tests a static model with endogenous amenities and location choices. Bond and Coulson (1989) note that neighborhoods are more likely to "tip" from high- to low-income (or vice versa) when housing quality is more homogeneous across neighborhoods.

2007).[4] In contrast to much of this literature, we highlight the role of natural features and emphasize variation across cities in neighborhood dynamics. Moreover, our results suggest that evidence for alternative theoretical channels is often stronger when considering cities in which churning is more salient (i.e., cities that more closely resemble flat, featureless plains).

Third, there is a large body of literature on changes in the internal structure of cities, especially examining the widespread decentralization of U.S. cities in the early and mid-20th century and the more recent gentrification of many central cities (Jackson, 1985; Mieszkowski and Mills, 1993). We extend this literature by documenting income gradients for a wide section of cities as early as 1880. In addition, our paper is one of the few to document and relate variation across cities to differences in natural heterogeneity. Another exception is Burchfield et al. (2006), who show that sprawl—the amount of undeveloped land surrounding an average dwelling—varies across cities according to the presence of available aquifers on the suburban fringe and hilly terrain. Our paper differs in that we examine neighborhood dynamics rather than land use.

Finally, our work is related to the literature in development and geography concerned with persistence in the spatial distribution of income and population. In theory, such persistence might be caused by geographic features, durable sunk factors, or amenities that are endogenous to location decisions (cf. Davis and Weinstein, 2002; Rappaport and Sachs, 2003; Redding, Sturm, and Wolf, 2011; Bleakley and Lin, 2012; Lin, 2015). Our work departs from this literature by focusing on the within-city distribution of income versus the distribution of income or population across cities or other subnational regions.[5] However, our results hint that natural variation may be an important explanation for differences in locational persistence on other spatial scales.

2 Theory

The following stylized model highlights the role of natural amenities in neighborhood dynamics. To clearly illustrate our key economic mechanism and its implications, we present a simple two-neighborhood version. This model abstracts from other important theoretical channels emphasized elsewhere in the literature, but we discuss and control for these omitted channels in our empirical analysis. In Appendix A.3, we relax assumptions and show that our theoretical predictions are robust to settings with more than two neighborhoods and correlated amenity shocks over time.

[4] Carlino and Saiz (2008) find that amenable neighborhoods, defined as those including tourist information offices or sites on the National Register of Historic Places, experienced greater increases in incomes and prices in the 1990s. In contrast to their study, we avoid the endogeneity of neighborhood amenities by examining only natural features. In addition, while their evidence exploits only within-city comparisons, we examine heterogeneous neighborhood dynamics across cities.

[5] Some recent studies examine persistence within a single city: Villarreal (2014) finds persisting effects of historical marshes in Manhattan, and Brooks and Lutz (2014) document persistent influences of historical streetcar lines in Los Angeles. In contrast, our paper compares persistence across cities.

2.1 Model

Consider a city with two neighborhoods, a beach and a desert, indexed by $j = b, d$. Each neighborhood has one unit measure of land, owned by absentee landlords. The beach offers an exogenous, persistently superior natural amenity level, $\alpha_b > \alpha_d$.

Neighborhoods vary in their *endogenous*, *aggregate* amenity level A, which consists of four parts:

$$A_{j,t} \equiv \alpha_j + E(\theta|j,t) + m_t + \varepsilon_{j,t}. \tag{1}$$

First, α_j is the persistent natural amenity value offered by neighborhood j in all periods. Second, $E(\theta|j,t)$ is the average income of neighborhood j residents in period t. With this term, we intend to capture the value of endogenous amenities that tend to be correlated with neighborhood income, such as safety, school quality, or shopping. Note that we normalize units of $A_{j,t}$ so that $E(\theta|j,t)$ has a unit coefficient in utility. Third, m_t captures city-level amenity trends common to all neighborhoods, such as citywide improvements in transportation infrastructure.[6] Fourth, $\varepsilon_{j,t}$ captures idiosyncratic amenity shocks, such as natural disasters or unexpected changes to the quality of local governance. We assume that $\varepsilon_{j,t}$ is independent and identically distributed with a cumulative distribution function $G(-\infty, \infty)$.

The city has a two-unit measure of workers, heterogeneous in income θ. In each period t, a worker chooses a neighborhood j, consumes one unit of land, pays rent $R_{j,t}$, spends the rest of her income on numeraire $c_{j,t}$, and receives utility $A_{j,t} \cdot c_{j,t}$. There are no moving costs or savings, so a worker need not solve a dynamic problem. Instead, each worker chooses the neighborhood that provides the best utility in each period. In sum, a type θ worker solves the following problem in each period:

$$\begin{aligned} &\max_j A_{j,t} \cdot c_{j,t} \text{ subject to } c_{j,t} + R_{j,t} = \theta \\ &= \max_j A_{j,t} \cdot (\theta - R_{j,t}). \end{aligned}$$

2.2 Equilibria within a period

Next, we characterize equilibria within a period. Note in the utility function $A_{j,t} \cdot c_{j,t}$ that aggregate amenities $A_{j,t}$ and numeraire consumption $c_{j,t}$ are complements. This complementarity implies that high-income workers are willing to pay more for aggregate amenities.[7] Therefore, high-income workers sort into superior aggregate amenity neighborhoods by outbidding low-income workers,

[6] Any citywide trends m_t cancel out when workers make neighborhood choices within the city and thus do not affect our theoretical results. We include m_t in equation (1) only to account for the components of aggregate amenities that affect (or do not affect) our theoretical results.

[7] Formally, the single crossing property holds between aggregate amenities and rents: $\frac{\partial}{\partial\theta}(-\frac{\partial V/\partial A}{\partial V/\partial R}) > 0$, where $V \equiv A \cdot (\theta - R)$ is the utility a type θ worker receives in a neighborhood with aggregate amenity A and rent R. Our results are robust to alternative specifications that preserve this property.

who are then priced out by equilibrium rents.

Since each neighborhood has one unit of land and each worker consumes one unit of land, each neighborhood accommodates one unit measure of workers in equilibrium. Therefore, the top 50% of workers by income will live in the superior aggregate amenity neighborhood, and the bottom 50% will live in the other neighborhood. Let Θ_H be the set of θ in the top 50% and Θ_L be the set of θ in the bottom 50%. Then, the average income of the superior aggregate amenity neighborhood is $\bar{\theta}_H \equiv E(\theta|\theta \in \Theta_H)$ and that of the inferior neighborhood is $\bar{\theta}_L \equiv E(\theta|\theta \in \Theta_L)$.

Lemma 1 *(Sorting) In each period, high-income Θ_H workers live in the superior aggregate amenity neighborhood, and low-income Θ_L workers live in the inferior aggregate amenity neighborhood.*

This perfect sorting implies that there are only two possible equilibrium states in each period: S_1 and S_2. In S_1, high-income workers live at the beach, and low-income workers live in the desert. This state is an equilibrium if and only if the aggregate amenity of the beach is greater than that of the desert:

$$S_1 : A_{b,t} = \alpha_b + \bar{\theta}_H + m_t + \epsilon_{b,t} \geq A_{d,t} = \alpha_d + \bar{\theta}_L + m_t + \epsilon_{d,t}. \tag{2}$$

Analogously, in S_2, high-income workers live in the desert and low-income workers live at the beach. This state is an equilibrium if and only if the aggregate amenity of the desert is greater than that of the beach:

$$S_2 : A_{b,t} = \alpha_b + \bar{\theta}_L + m_t + \epsilon_{b,t} \leq A_{d,t} = \alpha_d + \bar{\theta}_H + m_t + \epsilon_{d,t}. \tag{3}$$

Note that S_2 can be supported as an equilibrium by superior endogenous amenities, a large idiosyncratic shock, or both. Intuitively, good schools and high-income agglomerations can rationalize each other in naturally mundane locations.

These two conditions jointly imply that an equilibrium always exists; if one condition is not satisfied, the other one is always satisfied. They also imply that there can be multiple equilibria. For example, both conditions are satisfied if endogenous amenity differences $(\bar{\theta}_H - \bar{\theta}_L)$ are sufficiently large—if, say, households care a lot about school quality.

Proposition 2 *(i) There exists an equilibrium in each period. (ii) There can be multiple equilibria in each period.*

Finally, rents are determined so that the marginal worker (i.e., the median worker on the Θ_H–Θ_L boundary) is indifferent between the two neighborhoods. We set rent for the inferior aggregate amenity neighborhood to be 0.[8]

[8] There are multiple equilibria in rents because demand and supply of land are both perfectly inelastic. Our theoretical implications do not depend on which equilibrium rents are selected.

2.3 Equilibrium selection and history dependence

Amenity shocks to neighborhoods determine which one of the three possible equilibrium configurations is realized. Two possibilities are that the within-period equilibrium is unique: S_1 is the only equilibrium, or S_2 is the only equilibrium. A third possibility is that both S_1 and S_2 are equilibria in that period. When both S_1 and S_2 are equilibria, we select the state chosen in the previous period.[9] Thus, the selected equilibrium state switches back and forth over time between S_1 and S_2, and a selected equilibrium state persists until amenity shocks *rule out* the state as no longer an equilibrium. Note that with multiple equilibria and history dependence, the model can rationalize observations of persistent poverty in superior natural amenity neighborhoods (e.g., an inner-city slum next to the beach).

Since the selected outcome of period t depends on that of $t-1$, the selected equilibrium path follows a Markov chain. We obtain transition probabilities between states from conditions (2) and (3). Following our equilibrium selection rule, the state changes from S_1 to S_2 if and only if S_1 is no longer an equilibrium. Thus, the transition probability is

$$\Pr(S_2|S_1) = \Pr(\varepsilon_{d,t+1} - \varepsilon_{b,t+1} > a_b - a_d + \bar{\theta}_H - \bar{\theta}_L). \quad (4)$$

Analogously, the probability of transitioning from S_2 to S_1 is

$$\Pr(S_1|S_2) = \Pr(\varepsilon_{b,t+1} - \varepsilon_{d,t+1} > a_d - a_b + \bar{\theta}_H - \bar{\theta}_L). \quad (5)$$

Note that $\Pr(S_1|S_2)$ is greater than $\Pr(S_2|S_1)$ because $\alpha_b - \alpha_d > 0 > \alpha_d - \alpha_b$ and both $\varepsilon_{d,t+1} - \varepsilon_{b,t+1}$ and $\varepsilon_{b,t+1} - \varepsilon_{d,t+1}$ follow the same probability distribution.

Lemma 3 $\Pr(S_1|S_2) > \Pr(S_2|S_1)$.

Intuitively, the economy tends to return to S_1, the more "natural" state in which high-income workers live in the superior natural amenity neighborhood.

2.4 Theoretical implications

This section derives three implications we test in Sections 4, 5, and 6. Because our empirical analysis focuses on the relative rank of neighborhoods within a city, we cast theoretical implications in terms of income percentile ranks (i.e., the percentage of neighborhoods in the same city that have the

[9] A large number of papers in economic geography use the idea that equilibrium selection might be determined by history; see, e.g., Krugman (1991). A common way to motivate the role of history is to assume a myopic adjustment process, with migration frictions, from an initial endowment or equilibrium. This approach simplifies analysis of equilibrium selection, and, depending on certain parameter values, may be consistent with fully rational, forward-looking households (Ottaviano, Tabuchi, and Thisse, 2002). Redding, Sturm, and Wolf (2011), Bleakley and Lin (2012), and Hanlon (forthcoming) provide evidence of history dependence in the location of economic activity.

same or lower average income). In our model of two neighborhoods, the income percentile rank r of the low-income neighborhood is $r_L \equiv 0.5$ and that of the high-income neighborhood is $r_H \equiv 1$.

Our first implication is at the neighborhood level: Conditioned on initial income, the beach tends to increase more (or decrease less) in future income versus the desert. In other words, superior natural amenities "anchor" neighborhoods to high incomes over time. To illustrate this implication, we separately calculate the expected change in income percentile rank for the beach and the desert when they are inhabited by low-income workers.

First, consider the beach inhabited by low-income workers. This happens when the city is in S_2. If the city remains in S_2 in the next period, the income percentile rank of the neighborhood does not change. If the city changes to S_1, its income percentile rank rises from r_L to r_H. Thus,

$$E(\Delta r | j = b, r = r_L) = (r_H - r_L) \cdot Pr(S_1 | S_2). \tag{6}$$

Next, consider the desert inhabited by low-income workers. This happens when the city is in S_1. As above, we compute the expected change in rank as

$$E(\Delta r | j = d, r = r_L) = (r_H - r_L) \cdot Pr(S_2 | S_1). \tag{7}$$

These two equations and Lemma 3 jointly imply

$$E(\Delta r | j = b, r = r_L) > E(\Delta r | j = d, r = r_L).$$

In other words, conditioned on initially containing low-income households, the beach tends to increase in income more than the desert. Similarly, we can also show that, conditioned on initially containing high-income households, the beach tends to decrease in income less than the desert. Combining the two cases, we obtain Proposition 4.

Proposition 4 *(Natural amenities anchor neighborhoods to high income.) Conditioned on initial income percentile rank, a superior natural amenity neighborhood tends to increase more in income than an inferior natural amenity neighborhood.*

Our next implication is at the city level: Cities that feature greater heterogeneity in natural amenities tend to have spatial distributions of income that are more stable over time. Notice that $(\alpha_b - \alpha_d)$ captures heterogeneity in natural amenities across neighborhoods. We use the expected over-time variance of neighborhood income for the city $E[Var(r_{j,t}|j)]$ to capture *instability* in the city's spatial distribution of income.

Proposition 5 *(Naturally heterogeneous cities have persistent spatial distributions of income.) The expected over-time variance of neighborhood income percentile rank,* $E\left[Var(r_{j,t}|j)\right]$*, decreases with across-neighborhood heterogeneity in natural amenity,* $\alpha_b - \alpha_d$*.*

Proof. See Appendix A.1. ■

Proposition 5 is our key theoretical result. To view the intuition, suppose that the two neighborhoods are ex ante identical: $\alpha_b = \alpha_d$. Over time, each neighborhood's income will be r_H or r_L with equal probability. This maximizes the over-time variance in income rank and churning at the city level. Now suppose that natural heterogeneity $\alpha_b - \alpha_d$ is larger. With greater natural heterogeneity, more periods will be observed when the beach is the high-income neighborhood and the desert is the low-income neighorhood. This will reduce the over-time variance of neighborhood income. In the limiting case, if the difference in natural value $\alpha_b - \alpha_d$ is extremely large, then the beach will be the high-income neighborhood nearly all of the time. This implies an over-time variance in income for both beach and desert close to zero.

Our final implication combines elements from the previous two propositions. As in Proposition 4, we compare how superior and inferior natural amenity neighborhoods change, and, as in Proposition 5, we compare cities with varying internal natural heterogeneity.

Proposition 6 *(Natural amenities are stronger anchors in naturally heterogeneous cities.) The difference between superior and inferior natural amenity neighborhoods, in expected income changes conditioned on initial income, increases with across-neighborhood heterogeneity in natural amenities,* $\alpha_b - \alpha_d$.

Proof. See Appendix A.2 ■

In other words, the anchoring effect of natural amenities increases with natural heterogeneity at the city level. In Section 6, we test this implication and extend this test to the context of central city neighborhood dynamics.

2.5 Discussion

Relaxing simplifying assumptions. Our stylized model makes several simplifying assumptions and uses specific functional forms to clearly illustrate the role of natural amenities without unnecessarily complicating our analysis. For example, households' perfectly inelastic demand for a unit measure of land ensures that the income cutoff for sorting between neighborhoods is constant at the median, thus avoiding cutoff changes over time.

However, our key propositions hold for other specifications that preserve the following core elements. First, workers' preferences are such that there is perfect sorting by income on aggregate amenities. Second, aggregate amenities increase with natural amenities, average income, and amenity shocks. Together, these assumptions ensure that there are three possible equilibrium configurations in each period: S_1 only, S_2 only, or both S_1 and S_2. Combined with a history-based equilibrium selection rule, our three main results follow.

Housing demand. A natural concern about simplifying assumptions is whether they are innocuous. In fact, Proposition 4 might be reversed if the income elasticity of demand for land is sufficiently high: High-income households would choose inferior aggregate amenity neighborhoods in exchange for more space. This would violate the core structure described previously. Then, conditioned on initial income, superior natural amenity neighborhoods would tend to *decline* in income.

Fortunately, these contrasting predictions allow us to test whether the income elasticity of demand for land is large enough to reverse our theoretical prediction. Our empirical results consistent with Proposition 4 suggest instead that our simplifying assumption of inelastic demand is indeed innocuous, at least in this context.

More than two neighborhoods and correlated amenity shocks. Other simplifying assumptions in the model are that the city consists of two neighborhoods and that the idiosyncratic shocks are uncorrelated over time. Appendix A.3 presents an extended model that relaxes these assumptions and shows that our theoretical implications are robust. Instead of two neighborhoods, the city has $J \in \mathbb{N}$ neighborhoods, and the aggregate amenity shock $\epsilon_{j,t}$ follows the AR(1) process $\epsilon_{j,t+1} = \rho\epsilon_{j,t} + \nu_t$, where ν_t is independent and identically distributed. We also extend the equilibrium selection rule in Section 2.3: When multiple equilibria are possible, we choose the one that is closest to the selected equilibrium in the previous period in terms of Euclidean distance between neighborhood income vectors.

With the extended model, we analytically prove Lemma 1 and Proposition 2. We use numerical methods to demonstrate that Propositions 4, 5, and 6 hold widely when the aggregate amenity shock follows a stationary process (i.e., $\rho < 1$). Note that the stationarity condition is not very restrictive, since overall trends in amenities are captured by m_t in equation (1).

3 Data

3.1 Census data and geographic normalizations

We confirm several testable implications of our theory using a novel database of consistent-boundary neighborhoods spanning many U.S. metropolitan areas from 1880 to 2010. We use census tracts as neighborhoods because tracts are relatively small geographic units and data are available at the tract level over our sample period, even in historical census years. For each census tract, we collect information about household income, population, and housing from decennial censuses between 1880 and 2000 and the American Community Survey (ACS) between 2006 and 2010.[10]

Since boundaries change from one decade to the next, we normalize historical data to 2010 census tract boundaries. For example, we calculate average household income in 1940 for each 2010 tract by weighting the average household incomes reported for overlapping 1940 census tracts,

[10]Because of small annual sample sizes and privacy concerns, the ACS data represent five-year averages of residents and houses located in each tract. For convenience, we refer to these data as coming from the year 2010, although they actually represent an average from 2006 to 2010.

where the weights are determined by overlapping land area.[11]

Our panel is unbalanced. Growing cities that add neighborhoods and expanding census coverage both contribute to increases in the number of tracts over time. In addition, our ability to match households to neighborhoods is limited by the availability of maps showing the spatial location of historical census tracts or enumeration districts. Table 1 shows the number of metropolitan areas and consistent-boundary neighborhoods available in each year. Overall, we observe over 60,000 neighborhoods across 308 metropolitan areas and 12 census years from 1880 to 2010. However, the number of observations used in our empirical analysis varies across tests with data availability.[12] The data are most complete for later census years, especially after 1960, and we do not have any data for census years 1890 and 1900.

[Table 1 about here.]

We assign each neighborhood to a single metropolitan area, using the Office of Management and Budget's definitions of core-based statistical areas (CBSAs) from December 2009. We refer to each metropolitan area as a "city." (We address changes in metropolitan area boundaries over time by dropping nonurbanized areas in each period, as described in Appendix B.3. Thus, neighborhoods do not appear in our panel until they are urbanized and part of the metropolitan economy.) When relevant, we aggregate CBSAs to consolidated statistical areas. For example, we combine the Los Angeles-Long Beach-Santa Ana CBSA with the Oxnard-Thousand Oaks-Ventura and Riverside-San Bernardino-Ontario CBSAs.

Finally, we spatially match neighborhoods to a variety of persistent natural features. We collect information on a large number of highly visible and important physical attributes. For each neighborhood, we separately calculate the distance from the tract centroid to (i) the nearest coastline (i.e., the Atlantic or Pacific Ocean, the Gulf of Mexico, or a Great Lake), (ii) the nearest (non-Great) lake, and (iii) the nearest major river. We also calculate (iv) the average slope, (v) the flood-hazard risk, (vi) the average 1971–2000 annual precipitation, (vii) July maximum temperature, and (viii) January minimum temperature. In addition, we match neighborhoods to other factors, including distances to the nearest seaport and the city center or central business district (CBD). Appendixes B.4 and B.5 describe these data.

3.2 Neighborhood percentile ranks

Because we are interested in neighborhood income relative to other neighborhoods within the same city, we rank tracts within each metropolitan area and census year. We use neighborhoods'

[11] For census data from 1970 and later, we use the population of overlapping census blocks as weights, instead of overlapping land area. Appendix B.2 describes the census data and geographic normalization in detail. Appendix Table B1 reports summary statistics.

[12] Small boundary normalization errors account for the small number of tracts in 2000 that do not appear in 2010, but these tract fragments are ultimately dropped in our regressions.

percentile rank $r_{i,t}$, a variable bounded by 0 and 1. For example, in 2010, Malibu (within the Los Angeles metropolitan area) and the Upper East Side (within the New York metropolitan area) have $r_{i,2010} = 0.979$ and 0.990, respectively. By using ranks, we also control for differences in wage levels across cities and years, and we accommodate alternative measures of neighborhood status in historical years when income measures are unavailable.

We use average household income to rank tracts within each metropolitan area, except in historical census years 1880–1940 when income data are not available. For 1930 and 1940, we use average housing rents to rank tracts. In 1880–1920, lacking data on both income and prices, we use an imputed occupational income score or the literacy rate. The assumption behind these substitutions is that the ordering of average income among neighborhoods is the same as that of housing rent, occupational income score, or the literacy rate. We have verified empirically that our results are robust to using these alternative measures when they are available. For example, a regression of neighborhood ranks by average rent on ranks by average income for the three census years in which both measures are available yields an estimated coefficient of 0.927 (cluster-robust s.e.= 0.002) and an R^2 of 0.857, suggesting that these measures are very closely related.

4 Natural amenities as neighborhood anchors

In this section, we evaluate Proposition 4's prediction that *conditioned on initial income*, neighborhoods with superior natural amenities tend to increase in income more than other neighborhoods. This proposition suggests the following neighborhood-level regression:

$$\Delta r_{i(m),t} = \beta_0 + \beta_1 \mathbf{1}(a_i) + \beta_2 r_{i,t} + \delta_{m,t} + \epsilon_{i,t}, \tag{8}$$

where $\Delta r_{i(m),t}$ is the forward change in neighborhood i's income percentile rank within metropolitan area m between t and $t+1$, $\mathbf{1}(a_i)$ is an indicator for superior natural amenities, $r_{i,t}$ is its initial percentile rank in t, and $\delta_{m,t}$ is a metropolitan area–year fixed effect.[13] We cluster errors $\epsilon_{i,t}$ at the metropolitan area–year level.

Proposition 4 predicts that $\beta_1 > 0$. Including the initial rank $r_{i,t}$ follows from the conditioning statement of the proposition. The metropolitan area–year fixed effect $\delta_{m,t}$ ensures that identification of β_1 comes from variation in natural amenities *within*, not across, metropolitan area–years. As noted previously, we use various persistent features to measure superior neighborhood natural amenities: first separately and then combined into a single index using predicted values from a

[13]Note that we compute the change in percentile rank $\Delta r_{i,t}$ for each neighborhood by subtracting its initial rank $r_{i,t}$ from next period's rank $r_{i,t+1}$. Since this change can only be calculated for neighborhoods that exist in both the initial period and the subsequent period, neighborhoods that are added to the metropolitan area are *not* included in this calculation. This is one way in which our empirical analysis abstracts from differences in city growth rates. The included metropolitan area–year effect also controls for city growth common to all neighborhoods in each metro–year. In addition, using 10-year changes restricts our baseline sample to observations between 1910 and 2010, although later robustness checks with varying time horizons and start years use our 1880 data.

housing-price regression.

4.1 Coastal proximity

We begin with coastal proximity as a measure of natural amenity. In our baseline regressions, we assign $\mathbf{1}(a_i) = 1$ for a neighborhood i if its centroid is within 500 meters of an ocean, the Gulf of Mexico, or a Great Lake.[14] Table 2, column (1) reports estimation results for the starkest specification suggested by the model, including only coastal proximity, initial rank, and metropolitan area–year effects. Conditioned on initial income, the estimated effect of coastal proximity is slightly negative, although it is indistinguishable from zero.

[Table 2 about here.]

At first view, this result appears to challenge our theory. But omitted variables in this parsimonious specification, which abstracts from several important neighborhood factors, bias the result in column (1) downward. In particular, since many U.S. cities and their downtowns were founded near significant natural features such as harbors, there is a strong correlation among neighborhood proximity to coastlines, downtowns, and seaports.[15] Because these factors may also affect subsequent neighborhood change—Brueckner and Rosenthal (2009) show that neighborhoods with older homes tend to decline—estimation of equation (8) without them may lead to a downward omitted-variables bias. In columns (2) through (4), we attempt to correct this bias by including the following control variables: (i) distance to the nearest seaport (interacted with metropolitan coastal status), (ii) distance to the CBD, (iii) initial population density, and (iv) the average age of the initial stock of houses in the neighborhood.[16] By including these regressors, either singly or together, we hope to control for the historical structure of the city.

When controlling for historical factors, the estimated effect of coastal proximity is now positive and precisely estimated, consistent with Proposition 4. The estimate in column (4) suggests that, conditioned on initial income, coastal neighborhoods tend to increase 1.4 percentile points more than interior neighborhoods every 10 years. The comparison of the estimates across columns (2) through (4) confirms that these variables control for omitted factors in similar ways.

A second source of downward bias is measurement error. We do not observe the true natural amenity value of neighborhoods: Some beaches may be extraordinary, while others might be continually socked in by fog or even polluted. To address this downward bias, we condition natural features on other observables that tend to increase with natural value. One, we examine natural features near historically high-income neighborhoods. Because households can observe whether

[14] We use an indicator variable to allow for nonlinear effects of proximity. Our results are robust to alternative distance thresholds; see Appendix B.8. Our results are also robust to considering oceans and Great Lakes separately.

[15] In our sample of coastal cities, the correlation coefficients between distance to the coast and distances to the city center or the nearest seaport are 0.68 and 0.44, respectively.

[16] Note that data availability constraints narrow the sample sizes in columns (3) through (6).

a particular natural feature is an amenity, natural features near top-ranked neighborhoods in an initial year are more likely to be positive amenities. For example, the beach near historically high-income Malibu is likely to be a superior amenity.[17] We assign $\mathbf{1}(a_i) = 1$ if and only if the neighborhood is proximate to a natural feature *and* the neighborhood was initially in the top decile of neighborhoods by average income. (We verify that this strategy mitigates the effects of this type of measurement error and reduces the downward bias of our estimates using Monte Carlo simulations in Appendix C.) Column (5) shows the estimated conditional effect of coastal proximity on neighborhood change increases to 4.5 percentile points.

Two, we examine neighborhoods with names suggesting superior natural amenities. If a neighborhood next to a polluted beach is relatively unlikely to call itself a "beach," we can reduce measurement error by assigning $\mathbf{1}(a_i) = 1$ if and only if the neighborhood is proximate to a natural feature and its name contains words connoting desirable natural amenities. We match neighborhoods to place names from the Geographic Names Information System (GNIS).[18] Column (6) of Table 2 shows that the estimated conditional effect of coastal proximity on neighborhood change increases to 3.1 percentile points when conditioning on amenable names. (Conditioning coastal neighborhoods on names including "beach," "coast," "bay," "cove," "lagoon," "ocean," or "shore" increases the average income of coastal neighborhoods from 0.47 to 0.57. This is one way to see that conditioning on names increases the likelihood that a geographic feature is indeed an amenity.)

A third source of downward bias is unobserved time-invariant neighborhood factors. Suppose that in equation (8), $\epsilon_{i,t} = u_i + v_{i,t}$, where u_i captures unobserved factors.

$$\Delta r_{i(m),t} = \beta_0 + \beta_1 \mathbf{1}(a_i) + \beta_2 r_{i,t} + \delta_{m,t} + u_i + v_{i,t}. \tag{9}$$

The correlation between $\mathbf{1}(a_i)$ and u_i, conditioned on initial income $r_{i,t}$, is negative, leading to downward bias in $\hat{\beta}_1$.[19] This bias makes our estimate of $\hat{\beta}_1$ a lower bound for the true effect of superior natural amenities (see Appendix C). To see why, consider the two neighborhoods i and j with varying *measured* natural amenities $\mathbf{1}(a_i) < \mathbf{1}(a_j)$ but the same initial income $r_{i,t} = r_{j,t}$

[17]While this strategy is consistent with our theory, unobservable factors that favor neighborhoods with both superior natural amenities and high resident income could also generate the same empirical pattern. For example, land use policy might favor coastal high-income neighborhoods compared with interior high-income neighborhoods but not similarly favor coastal low-income neighborhoods compared with interior low-income neighborhoods. We discuss the role of historical housing and land use regulation, two possible factors fitting this description, in Section 4.3.

[18]The GNIS maintains uniform usage of geographic names in the federal government. We use named populated places, which range from rural clustered buildings to metropolitan areas and include housing subdivisions, trailer parks, and neighborhoods. These named populated places exclude natural features. See Appendix B.5.

[19]This issue is related to dynamic panel bias in the cross-country growth convergence literature (c.f., Caselli, Esquivel, and Lefort, 1996). However, it differs in that our interest is in the effect of time-invariant natural factors, while that literature has traditionally focused on consistently estimating the mean reversion parameter β_2. A further issue raised by Caselli et al. is that control variables $\mathbf{X}_{i,t}$ are endogenous. The direction of bias is not clear since we do not model these control variables explicitly. However, the conditions for an endogenous control variable to *overestimate* β_1 are not easy to satisfy. In short, both the unobserved effect and the measured natural amenity must increase neighborhood income, but their correlations with the endogenous control variable must have opposite signs.

in period t. That they have the same initial income suggests that the neighborhood with inferior measured natural amenities is likely to have other unobserved fixed characteristics that are amenable (i.e., $u_i > u_j$). Unfortunately, the common solution to eliminate unobserved fixed factors u_i by time-differencing equation (9) precludes identification of β_1 since time-invariant natural features drop out. Further, few instrumental variables would seem to satisfy the exclusion restriction since any variable related to coastal proximity is likely to be correlated with income change.[20]

It is well known that central-city neighborhoods declined in the mid-20th century: In our data, neighborhoods within 5km of CBDs experienced a relative decline of 2.9 percentile points every 10 years between 1950 and 1980. Interestingly, our estimates of the anchoring effect of coastal proximity are similar in magnitude to the absolute average rate of decline of downtown neighborhoods during this period. Further, the additional downward bias from unobserved fixed factors suggests these estimates are a lower bound.

Finally, consistent with mean reversion in neighborhood status, the coefficient on initial rank is negative and precisely estimated. In Appendix B.7, we note that this mean reversion is (i) robust to using nonlinear techniques; (ii) driven by the middle of the income distribution, not by censored changes at extreme incomes (as might be expected if mean reversion were purely mechanical); and (iii) apparent even in nominal incomes, showing that this pattern is not exclusively driven by our use of percentile ranks. Mean reversion in neighborhood status is also consistent to long-run results for Philadelphia neighborhoods reported by Rosenthal (2008).

4.2 Other natural features and an aggregate natural amenity index

Table 3 shows that our results are robust to other amenity measures. Column (1) reproduces estimated effects of coastal proximity from Table 2. Columns (2) through (6) use indicators for different natural features: lakes, rivers, hills, temperate climates, and low flood risk.[21] Panel A uses the specification from Table 2, column (1), controlling only for initial income and metro-year fixed effects. Panel B adds controls for historical factors, as in column (4). Panel C corrects for measurement error by conditioning natural features on their initial proximity to top-decile neighborhoods, as in column (5). Finally, Panel D corrects for measurement error by conditioning natural features on neighborhood names, as in column (6).

[Table 3 about here.]

In general, the estimated effect of natural features is positive. This is universally true when we correct for measurement error in Panel C. But it is also true for most of our natural amenity

[20]Under certain conditions that appear to be satisfied in our data, it may be that an imperfect instrument can provide a lower bound on the true value of β_1 (Nevo and Rosen, 2012). In our experiments, using place names or an alternative measure of coastal proximity as instruments yields estimates of the anchoring effect of coastal proximity between 4.5 and 10.7 percentile points.

[21]The table notes describe how our indicator variables are defined and which sets of words are used to condition on names in Panel D.

measures in Panel A. This contrasts with the dependence of a positive result for coastal proximity on the inclusion of controls for historical factors, and it highlights the unique effect of the historical development of U.S. cities on our coastal proximity results. As noted previously, within coastal metropolitan areas, neighborhood distance to an ocean or a Great Lake is strongly correlated with distances to the nearest seaport and the CBD, log population density, and average house age (with correlation coefficients of 0.68, 0.44, -0.40, and -0.22, respectively). In contrast, neighborhood distance to a non-Great lake is only weakly related to these factors (the absolute correlation coefficients are all less than 0.06).[22] Thus, it is unsurprising that estimates of the conditional effect of lakes and hills on neighborhood change in Panel A are positive and precisely estimated despite omitted controls for historical factors.[23]

We combine these features together into an index of aggregate *natural* value by predicting rent from our various observed natural features. We regress the logarithm of neighborhood median housing rent, reported in censuses from 1930 to 2010, against a complete vector of dummy variables indicating proximity to all of our natural features (at many thresholds), log population density, log distance to the CBD, log number of housing units, average housing age, log distance to the nearest seaport, and metropolitan area–year effects. Then, we predict values for housing rents based on just the estimated natural feature coefficients.[24]

Table 3, column (7) shows that our results are robust to using this hedonic index to measure aggregate natural value. The estimated effect of aggregate natural value is similar in magnitude to that of coastal proximity. In our preferred estimates controlling for historical factors and correcting for measurement error, the top 5% of neighborhoods by natural value tend to increase 1.3 to 4 percentile points more in rank than other neighborhoods over 10 years. This is about one-half of the sample standard deviation of 0.16 in 10-year changes in neighborhood rank. Again, due to unobserved neighborhood fixed factors, we consider these estimates to be a lower bound on the anchoring effect of natural amenities.

The previous results estimate the average effect of natural amenities on neighborhood income growth across different initial income ranks. As a robustness check, we estimate heterogeneous effects using a nonparametric approach. Figure 1 plots kernel-weighted local polynomial smooths

[22]Hills are negatively correlated with historical factors, explaining the attenuation of their estimated effect from Panel A to B.

[23]Note that the regressor of interest in column (5) is an indicator for moderate temperatures and little precipitation. Nearly all of the within-metro variation in this variable comes from coastal California metropolitan areas. Thus, this indicator is closely related to coastal proximity. Similarly, the indicator for low flood risk in column (6) is also strongly correlated with coastal proximity. These correlations with coastal proximity explain why the estimates in Panel A are negative.

[24]This hedonic regression omits endogenous factors such as school quality. However, the resulting predicted values may be unbiased estimates of the *natural* amenity value of neighborhoods if omitted factors are related to the observed factors in the same way. For example, if school quality is related to coastal proximity but not hills, then the estimated coefficients on coastal proximity and hilliness will be biased, relative to each other. However, if school quality is related to the overall natural advantage of neighborhoods, then the estimated coefficients on coastal proximity and hilliness will be biased in the same way, but the relative weights will be unbiased. In this case, predicted rents may be a good indicator for the aggregate natural value of neighborhoods.

of sample changes in neighborhood income percentile rank, $\Delta r_{i,t}$ versus initial ranks $r_{i,t}$ separately for neighborhoods in the top 5% and bottom 95% in aggregate natural value. Note that for a given initial rank, superior natural amenity neighborhoods tend to improve more in income versus other neighborhoods. The average vertical distance between the two lines corresponds to the estimate in Table 3, Panel A, column (7).[25]

[Figure 1 about here.]

4.3 Discussion

Next we discuss various factors that may be correlated with natural features. As noted earlier, endogenous factors excluded from both our stylized model and equation (8) may be important channels driving the anchoring of superior natural amenity neighborhoods to high incomes. We also discuss the alternative explanation that our anchoring results may be driven by an increasing valuation of natural amenities.

Historical housing and land use regulation. Historical buildings may attract high-income workers, as emphasized by Brueckner, Thisse, and Zenou (1999), and we have just seen that they tend to be correlated with coastal proximity. Similarly, coastal areas may attract households that care about certain amenities and act to preserve them through restrictive land use zoning (Kahn and Walsh, 2015). Ideally, these factors might be controls in our anchoring regression, except for poor data availability in historical census years. Instead, here we perform cross-sectional regressions of changes in income from 2000 to 2010 on coastal proximity, including controls for the pre-1940 housing stock in 2000 (as a proxy for historical buildings) and the Wharton Residential Land Use Regulation Index (Gyourko, Saiz, and Summers, 2008). Other details of the estimation are identical to equation (8). Conditioned on the metropolitan area fixed effects, identification comes from variation across neighborhoods (or municipalities, in the case of the Wharton index) within metropolitan areas.

[Table 4 about here.]

Table 4 shows the result. Columns (1) and (2) show that the cross-sectional results are similar to the pooled results in Table 2.[26] In column (2), the estimated effect of house age on neighborhood change is negative, suggesting that old homes are a disamenity. However, when we add a control for the share of houses built before 1940 in column (3), its estimated effect becomes more negative, while the estimated effect of pre-1940 homes is positive and precisely estimated. This is consistent

[25] Figure 1 may give the mistaken impression that unconditional changes in rank $E(\Delta r)$ are greater for superior natural amenity neighborhoods. This is not necessarily the case since superior natural amenity neighborhoods tend to be of high initial rank.

[26] One exception is that the unconditional estimate in column (1) is positive and precisely estimated, even without controls for historical factors. Although on average over our sample period coastal neighborhoods have tended to decline, the recent gentrification of central cities shows up here as a positive estimated coefficient.

with pre-1940 homes being an amenity, especially if they have been positively selected for survival.[27] Notably, there is hardly any attenuation of coastal proximity effect from column (2) to (3).

Estimates controlling for the Wharton index show similar results. For comparison, column (4) restricts our sample to metropolitan areas where the Wharton index is available. Notably, controlling for land use regulation in column (5) has little effect on the main estimated effect of coastal proximity. Stricter regulation appears to act as an amenity, as seen by the positive estimated coefficient on the Wharton index. But the insensitivity of the estimate on coastal proximity again suggests that the anchoring of incomes is not significantly reinforced by stricter regulations.

Other factors. There may be other factors correlated with natural amenities. For example, higher-quality houses might be built in coastal neighborhoods, further reinforcing the persistence of income there. Or there may be substantial moving frictions in superior natural amenity neighborhoods. However, a feature shared by many of these factors is that they diminish or depreciate over time. If these frictions or other endogenous factors are responsible for generating our anchoring result over 10-year horizons, then we would expect the estimated effects of natural amenities to decline as these frictions dissipate over many decades or even a century.[28]

Instead, we find the opposite. Figure 2 shows the conditional effects of natural amenities on changes in neighborhood income over the (very) long run. Each point is an estimated effect from a separate regression that varies the base year t and the time horizon Δt. The dependent variable is the change in percentile rank from t, the beginning year of its corresponding line segment, to $t + \Delta t$, the year corresponding to the horizontal coordinate of the point. For example, in Panel A, the point at the coordinate (1930, -0.009) indicates that the estimated conditional effect of coastal proximity on the 50-year change (1880 to 1930) in neighborhood percentile rank is -0.009 ($p = 0.916$).[29] (Estimates significant at $p < 0.10$ are circled.) Following the dashed line to the right, the point at the coordinate (2010, 0.089) indicates that the estimated conditional effect of coastal proximity on the 130-year change (1880 to 2010) in neighborhood percentile rank is 0.089 ($p = 0.064$). This figure shows that, across starting years and natural amenities considered, the estimated conditional effects tend to be larger as the time horizon lengthens. We view this evidence as inconsistent with the hypothesis that the anchoring effect is driven by other historical factors,

[27] One caution is that these are not causal estimates of housing age since preservation is an endogenous decision that may depend on expectations of future neighborhood quality. We have experimented with instrumental variables estimates using neighborhood age, historical lags of the stock of housing, and National Register sites as excluded instruments. The results are similar to the OLS estimates reported here. Neighborhood age may be conditionally independent of neighborhood change once distance to the city center is controlled for. Since National Register buildings are self-nominated, they are not great candidates for an instrument. Instead, we use a separate list of National Register sites noted for their historical, as opposed to aesthetic, significance.

[28] One may wonder about endogenous factors caused by natural amenity differences between years t and $t + 1$. Since these natural amenity differences are predetermined, these factors are part of the overall causal effect of natural amenity differences in the initial year and thus should not be controlled for. See Angrist and Pischke (2009, pp. 64–68) on bad control variables.

[29] In all regressions, we include controls for log neighborhood distance to the nearest port (interacted with metropolitan coastal status), log neighborhood population density, and log neighborhood distance to the CBD. We omit the control for log neighborhood average house age because of inconsistent data availability.

endogenous to natural amenities, that exist in the initial year t of our 10-year regressions.

[Figure 2 about here.]

Increasing valuation of natural amenities. A final concern related to the estimation of equation (8) is that the value of natural features may change over time. For example, the valuation of natural features may have increased because of growing income, particularly in the right tail of the income distribution. One way that this hypothesis might be tested is by estimating the conditional effect of natural amenities on 10-year changes in neighborhood percentile rank. These estimates can also be seen in Figure 2: A thick shaded line connects estimates of the 10-year effect of natural amenities ending in each labeled census year.

According to Figure 2, there is little evidence that the conditional effect of natural amenities has increased over time, at least for changes in the percentile ranking (as opposed to changes in nominal prices or incomes) of neighborhoods. Instead, the conditional effect of natural amenities looks stable, or even declining, over time. One potential explanation for these results is that our theory implies that increases in the valuation of aggregate amenities do not affect the relative ranking of neighborhoods. Since we focus on the relative ranking of neighborhoods, the effects of national increases in income or income inequality are obscured. Instead, such income effects will show up in housing or land prices in high-amenity areas, consistent with the results of Gyourko, Mayer, and Sinai (2013) and Diamond (2016).

A second source of changes in the valuation of natural features is shifts in preferences among amenities. Conditioned on tastes for aggregate neighborhood amenities, perhaps high-income households place greater weight today on natural amenities compared with endogenous amenities such as schools, shopping, or safety. One prediction of such a shift in preferences is that natural amenities will better predict high-income neighborhoods today compared with the past. However, we find little evidence in support of this hypothesis. Figure 3 shows that the relative likelihood that a high-income neighborhood is coastal has remained roughly constant over 130 years.

[Figure 3 about here.]

5 Persistence in the spatial distribution of income

Next, we test Proposition 5's prediction that naturally heterogeneous cities have more persistent spatial distributions of income. We begin with the following hierarchical linear model:

$$\begin{aligned} \sigma(r_{i(m)}) &= \psi_m + \varepsilon_i \\ \hat{\psi}_m &= \gamma_0 + \gamma_1 \mathbf{\Gamma}_m + \mathbf{Z}'_m \gamma_3 + \mu_m \end{aligned} \tag{10}$$

Here, $\sigma(r_i)$ is the *over-time* standard deviation of neighborhood i's percentile ranking within city m between some base year t and 2010, and ψ_m is the city mean of $\sigma(r_i)$ estimated in the first level and used as the dependent variable in the second level. $\mathbf{\Gamma}_m$ is a city-level measure of variation in natural value among neighborhoods within city m. Thus, γ_1 is identified by cross-sectional variation across cities in base year t in (within-city) natural heterogeneity. Proposition 5 predicts that $\gamma_1 < 0$. Following Wooldridge (2003), the minimum distance estimator is equivalent to estimating the second step using weighted least squares, where the weights are $1/\widehat{Avar}(\hat{\psi}_m)$. First-stage errors ε_i are clustered at the city level.

Using 1960 as our base year, we compute $\sigma(r_i)$ using the six observations from 1960 to 2010.[30] Earlier historical years have fewer neighborhood and city observations, as seen in Table 1. Later census years have fewer periods over which to compute the over-time volatility in neighborhood income. We show later that our results are robust to the choice of base year. For all choices of base year, we fully balance our panel; thus, for a base year of 1960, our regressions and computations of ranks over time exclude any neighborhoods or cities that do not appear in our sample in any census year between 1960 and 2010.

To measure $\mathbf{\Gamma}_m$, we take two approaches. First, we use a metropolitan indicator for coastal status. We expect coastal cities to have a higher internal variance in natural amenities than non-coastal cities because the ocean is such a dominant natural amenity. Second, we use the within-city standard deviation in log neighborhood distance to the coast. This continuous measure is larger in cities where some neighborhoods are coastal and others are not, and the curvature of the logarithm function ensures that this measure is small in interior cities where all neighborhoods have equally poor access to the coast.[31]

Finally, to control for factors excluded from the model, we add other city-level covariates related to over-time volatility in neighborhood income in $\mathbf{Z}_m$. For example, we control for the within-metro standard deviation of neighborhood income to account for variation in nominal income dispersion across cities. In some specifications, we control for the within-metro standard deviation of neighborhood house age, since neighborhoods in cities with greater heterogeneity in endogenous factors such as housing may be more resistant to turnover.

Table 5 displays results using 1960 as our base year. Each column shows a separate regression. We multiply the dependent variable by 100 for presentation purposes, so the units are percentile points. Column 1 shows that, on average, neighborhoods in coastal metropolitan areas experience smaller fluctuations in income over time. The coefficient on a metropolitan indicator for proximity to the ocean is negative and precisely estimated. The magnitude of the effect is approximately 23% of one standard deviation in volatility across neighborhoods and almost two-thirds of one standard deviation across cities; coastal status alone explains about 12% of the variation in neighborhood volatility across cities.

[30] Our results are robust to using the variance $Var(r_i) = [\sigma(r_i)]^2$, but our linear model using the standard deviation results in a better fit, as measured by R^2.

[31] Results are virtually identical when we interact this measure with a metropolitan coastal indicator, which mechanically sets the internal variation of interior cities to zero.

[Table 5 about here.]

An alternative explanation of our result in column 1 is that heterogeneous cities are also land-supply constrained. This view suggests that, in flat cities, the supply of housing is more elastic, and therefore the causal link between geography and neighborhood stability is mediated by city growth, not the value of natural amenities (cf. Saiz, 2010). To address this concern, we control for log metropolitan area growth in population and area. Column 2 shows the result. The estimates do suggest that growing cities are less stable, consistent with the alternative view. However, even conditioned on city growth, coastal cities are more stable. Thus, we do not view our results as being spuriously caused by differences in land-supply elasticity across cities.

Note that city population growth is associated with greater volatility in neighborhood incomes. This is consistent with the idea that shocks to extant neighborhoods are greater in such cities. Holding land area fixed, neighborhoods in cities experiencing more rapid population growth seem likely to have experienced more rapid infrastructure investment, greater subdivision of older homes, greater influx of immigrants, and so on, that might correspond to larger shocks in our model. But conditioned on population growth, spreading these shocks over a larger area means more modest shocks at the neighborhood level, and, hence, the negative estimated coefficient on metro change in land area. Figure 4 further illustrates this relationship between metropolitan population and area growth and neighborhood volatility.

[Figure 4 about here.]

In column (3), the coefficient on within-city income inequality is negative, too, but it is less precisely estimated. This result suggests that cities with greater income dispersion across neighborhoods are more stable. The coefficient on dispersion in house ages is imprecisely estimated. This regression also includes additional controls for the top and bottom deciles of initial income in the first level of the estimation. One concern is that because we are using percentile ranks to measure income, the over-time volatility in neighborhood income may be censored for those neighborhoods at the very top or very bottom of the income distribution. (Note, however, that such censoring is likely to affect all cities equally. In addition, because Proposition 5 is derived using percentile ranks, it already accounts for such censoring.) The insensitivity of the estimated effect of natural variation to these first-level controls suggests that our results are robust to possible censoring issues in the tails of the income distribution.

In columns (4) and (5), we use alternative measures of within-city natural heterogeneity $\mathbf{\Gamma}_m$. Column (4) uses the within-city standard deviation in the logarithm of neighborhood distance to an ocean or Great Lake. Column (5) uses the within-city standard deviation in our aggregate natural value index described earlier. In both regressions, the estimated coefficients are negative and precisely estimated. Standardizing the effect sizes implies similar magnitudes across measures.

Table 6 shows that these results are robust when we vary base years. Each cell reports the estimated coefficient on the within-metropolitan area standard deviation in natural value from a separate regression, with a specification identical to Table 5, column 2. Thus, that estimate is repeated in the first row, column 5 of Table 6.

[Table 6 about here.]

Each panel shows results where the measure of within-city natural heterogeneity is noted by the panel title. Each column displays results for regressions using the base year indicated. For example, in column 1, we rely on cross-sectional variation in 1880 across 29 cities, and we use the seven census years available for a balanced panel (1880, 1960, 1970, ..., 2010) to compute the over-time volatility in income.

Neighborhoods in coastal cities (Panels A and B) or naturally heterogeneous cities (Panel C) tended to experience smaller fluctuations in income over 1960–2010, echoing results in Table 5. These estimates are negative and precisely estimated. Overall, all of the estimated effects in 1940 or later are negative, and the results are especially strong and precisely estimated when considering base years from 1950 to 2000.

Some of the earlier estimates for 1930 are positive and precisely estimated. In part, we find that this is due to an unrepresentative sample of cities. The 10 metropolitan areas used in the 1930 regression are all in the Northeast and Midwest region.[32] Estimates from regressions in later base years that are restricted to these cities tend to feature reduced magnitudes and precision, suggesting that differences in the sample composition of cities may play a role in these historical estimates.

Finally, Figure 5 illustrates our main result that neighborhoods in naturally heterogeneous cities tended to experience smaller over-time fluctuations in income over 1960–2010. Each point represents a metropolitan area. The vertical axis measures the metropolitan-level residual from a regression of mean variance in percentile rank over time on controls as in Table 5, column 4. The horizontal axis measures the within-city standard deviation in our predicted rent index; Los Angeles and the San Francisco Bay Area (labeled San Jose) are the two most naturally heterogeneous metropolitan areas by this index. The slope of the fitted line corresponds to the estimate reported in Table 6, panel C, column 5.[33] Thus, naturally heterogeneous cities exhibit more persistent spatial distributions of income over time.

[Figure 5 about here.]

[32] In 1930, our metropolitan areas are Boston, Buffalo, Chicago, Cleveland, Columbus, Indianapolis, Nashville, Pittsburgh, St. Louis, and Syracuse.

[33] Note that two outliers, Las Vegas and Tucson, are desert cities that have low volatility but also low measured natural heterogeneity. Intriguingly, there may be unmeasured natural amenities (such as access to aquifers) in these cities that lead us to underestimate the true degree of natural heterogeneity.

6 Decentralization and gentrification in coastal and interior cities

In this section, we test Proposition 6's prediction that natural amenities are stronger anchors in naturally heterogeneous cities. Intriguingly, this prediction also has implications for variation across cities in the widespread decentralization of income in the early and mid-20th century and the regentrification of downtowns since 1980. We show that coastal city downtowns, compared with downtowns in river or interior cities, have been better anchored to high incomes during both periods of nationwide suburbanization and gentrification.

First, we test Proposition 6 by adding to equation (8) an interaction between a neighborhood indicator for superior natural amenities $\mathbf{1}(a_i)$ and metropolitan natural heterogeneity $\mathbf{\Gamma}_m$:

$$\Delta r_{i(m),t} = \beta_0 + \beta_2 r_{i,t} + \beta_1 \mathbf{1}(a_i) + \beta_3 \mathbf{1}(a_i) \times \mathbf{\Gamma}_{m,t} + \delta_{m,t} + \epsilon_{i,t}. \tag{11}$$

Proposition 6 predicts that $\beta_3 > 0$. In Section 4, we introduced various measures for superior natural amenities $\mathbf{1}(a_i)$, and, in Section 5, we introduced various measures for metropolitan natural heterogeneity $\mathbf{\Gamma}_{m,t}$. With $\mathbf{1}(a_i)$ defined as an aggregate natural index value in the top 5%, $\mathbf{\Gamma}_{m,t}$ measured with the (initial) within-city deviation in aggregate natural value, and controls following Table 2, column (4), we find that superior natural amenity neighborhoods increase in income $\hat{\beta}_1 = 1.9$ percentile points (c.r.s.e.= 0.6) in the average metropolitan area by natural heterogeneity, and this effect increases $\hat{\beta}_3 = 3.3$ percentile points (c.r.s.e=0.6) for every one-standard-deviation increase in metropolitan natural heterogeneity. This result is robust to other measures of $\mathbf{1}(a_i)$ and $\mathbf{\Gamma}_{m,t}$.

[Table 7 about here.]

Proposition 6 also has implications for variation across cities in the nationwide decentralization of income in the early and mid-20th century and the regentrification of downtowns since 1980. Owing to historical development patterns, many cities have their central business districts near superior natural amenities; coastal cities tended to develop from harbors and beaches, and interior cities grew from rivers. Thus, intriguingly, since coastal cities are more naturally heterogeneous than interior cities, Proposition 6 predicts that coastal city downtowns will have been better anchored to high incomes compared with interior city downtowns during both the period of widespread suburbanization and the more recent period of downtown gentrification. To test this prediction, we replace $\mathbf{1}(a_i)$ with $\mathbf{1}(CBD_i)$ in equation (11).

$$\Delta r_{i(m),t} = \beta_0 + \beta_2 r_{i,t} + \beta_1 \mathbf{1}(CBD_i) + \beta_3 \mathbf{1}(CBD_i) \times \mathbf{\Gamma}_{m,t} + \delta_{m,t} + \epsilon_{i,t}. \tag{12}$$

The regression results reported in Table 7 confirm that coastal downtowns have been better anchored to high incomes. Column (1) confirms the long-run decline in the income of interior downtowns, by an average of 4.0 percentile points every 10 years, but as indicated by the positive estimated effect of the interaction term, downtown neighborhoods in coastal metros declined by an average of just 0.8 percentile points every 10 years. We have verified that this result is robust to using a continuous treatment and interaction instead of an indicator for downtown proximity.[34] This result is also robust to alternative measures of metropolitan natural heterogeneity versus a simple metro coastal indicator.[35]

Column (2) shows that conditioned on initial income, coastal downtowns tended to decrease less in income versus interior downtowns. Controlling for initial income corresponds to the conditioning statement of Proposition 6. Further, this result is robust in our 1910 to 2000 sample to controlling for neighborhood distance to the nearest seaport and population density, as seen in column (3).

Columns (4) and (5) compare two distinct periods: the decentralization of income before 1980 and the gentrification of city centers after that, respectively. In addition, these regressions restrict the sample to years in which we have information on neighborhood average house age, an additional control. In the first period, the negative coefficient estimate on city center proximity confirms the suburbanization of income. In the second period, the positive estimate reflects the gentrification of downtowns experienced in the past few decades. In columns (3) and (4), the attenuation of the estimated coefficient on downtown proximity compared with columns (1) and (2) suggests that some of the decline of downtowns can be attributed to observable characteristics such as housing age, proximity to ports, or population density.

Notably, the coefficient of interest on the interaction between neighborhood city center proximity and metropolitan coastal proximity remains stable across specifications. Our estimates suggest that coastal downtowns have had a 3-percentile-point advantage over interior downtowns in terms of conditional changes in income rank.

Finally, these results do not appear to be driven by less job decentralization in coastal metropolitan areas. An alternative explanation for our results might be that naturally heterogeneous cities also keep employment more centralized, due perhaps to greater transportation costs associated with industrial or commercial activities. In columns (6) and (7), we show that our results are robust to controls for the degree of job decentralization in a metropolitan area. Kneebone (2009) uses ZIP Code Business Patterns data to estimate the changes in metropolitan employment shares over 1998–2006 in 3-, 3- to 10-, and greater than 10-mile rings around the CBDs of many metropolitan areas, of which 86 match our metropolitan area definitions. Column (6) repeats regressions (4) and (5) on the Kneebone-restricted sample. In column (7), we include the first two measures of job decentralization (as the three shares sum to 1). The estimated effect of downtown proximity and coastal proximity is identical across columns (6) and (7), suggesting that limited job decentralization in coastal cities does not contribute to our results. Interestingly, increases in the job share

[34] For example, the estimated coefficient on log distance to city center is 0.028 (c.r.s.e.=0.002), and the coefficient on log distance to city center times coastal metro indicator is −0.010, c.r.s.e.=0.005.

[35] For example, using the standard deviation of our hedonic measure of natural value yields similar results: The estimated coefficient on downtown proximity is −0.038, c.r.s.e. = 0.004, and the estimated coefficient on downtown proximity times the metropolitan standard deviation of hedonic natural value is 0.119, c.r.s.e. = 0.035.

of neighborhoods within 3 miles of the central business district appear to bolster the incomes of central neighborhoods, although the effect is imprecisely estimated. Job-share increases in a 3- to 10-mile ring around the CBD appear to be negatively and significantly related to incomes in the city center, consistent with the importance of job access to the recent gentrification of central cities. In addition, the estimated coefficient on downtown proximity becomes more positive, consistent with intensifying gentrification of central cities starting in 1990.

These differences in patterns of decentralization and gentrification add a cross-city perspective to literature that have documented historical changes in the location of income within U.S. cities. LeRoy and Sonstelie (1983) report that downtown neighborhoods in 18th- and 19th-century Milwaukee, Philadelphia, Pittsburgh, and Toronto tended to feature higher incomes at least until the introduction of the streetcar in the 1850s and 1860s "caused the first major flight of the affluent to the suburbs" (p. 81). This decentralization would be repeated later on a larger scale between central cities and their outlying suburbs, leading to the dominant U.S. pattern today of poor centers and rich suburbs (Brueckner and Rosenthal, 2009). Subsequently, many central cities have experienced rising incomes and gentrification in the past several decades.

Figure 6 confirms these patterns for a fixed sample of 29 metropolitan areas over 10 census years—a broader section of cities than in previous work. Each panel displays the pattern of income and residential location: The horizontal axis measures distance from the city center, up to 15km, and the vertical axis measures average household income on a percentile rank scale.[36] Plotted lines show lowess regressions, fitted separately for coastal versus interior cities.[37]

The first panel shows that in 1880 income declined with distance to the city center in both coastal and interior cities. This pattern is consistent with the fact that many of these cities were still recently founded as of 1880, and the best-developed areas were clustered near downtown.[38]

[Figure 6 about here.]

However, as early as 1930, we see a divergence in the fortunes of downtowns in coastal versus interior cities: While all downtowns declined, those in interior cities tended to decline faster than downtowns in coastal cities, at least until 1960. Further, the second row of Figure 6 shows that, from 1970 onward, coastal city downtowns tended to improve faster in income than interior city downtowns. Thus, the pattern seen in Figure 6 is consistent with Proposition 6. Throughout both the widespread decentralization of income and the more recent gentrification of central cities, coastal city downtowns have been better anchored to high incomes.

[36] Only six cities in our sample had neighborhoods beyond 15km from the city center in 1880. Overall, the median city's maximum extent was 6km from the city center.

[37] See Appendix Table B2 for our classification of cities.

[38] In Appendix Figure B2, we show that the pattern of income varied across metropolitan areas. For example, peripheral incomes in Columbus, Louisville, and Washington, DC, exceeded incomes in the core. And in Philadelphia, Boston, Cleveland, and New York City, incomes were highest not in the core but at some distance from the center. But most sample cities continued to feature the highest incomes closest to the center in 1880.

7 Conclusion

We combine new theory and a novel database of consistent-boundary neighborhoods to study both neighborhood dynamics and differences across cities in patterns of neighborhood change, suburbanization, and persistence. Our theory and results highlight the role of natural amenities in neighborhood dynamics. Persistent natural amenities anchor neighborhoods to high incomes over time, and they affect neighborhood dynamics citywide. Downtown neighborhoods in coastal cities have been both less susceptible to suburbanization and more responsive to gentrification versus interior cities. Finally, cities with greater internal natural heterogeneity tend to exhibit more persistent spatial distributions of income.

Although our stylized model assumes a closed city for simplicity, our insights should hold in an open city setting. Even if migration across cities causes changes in a city's nominal income distribution, the sorting between workers' income rank and neighborhood aggregate amenities still holds. That said, there are many interesting questions to be examined in a model that considers household mobility across cities. For example, cross-city sorting of households on preferences for natural amenities may have important implications for income inequality and the political economy of coastal cities, complementing recent research by Moretti (2013) and Eeckhout, Pinheiro, and Schmidheiny (2014). Further, the push to implement place-based policies, as well as their consequences, may vary with natural heterogeneity and these sorting patterns. These interesting topics are left for future research.

Finally, we have focused on neighborhood sorting by income in this paper. But our insights extend to sorting on other characteristics as well. For example, the strong correlation between race and income in the United States means that many of the patterns we find apply to racial segregation dynamics as well.

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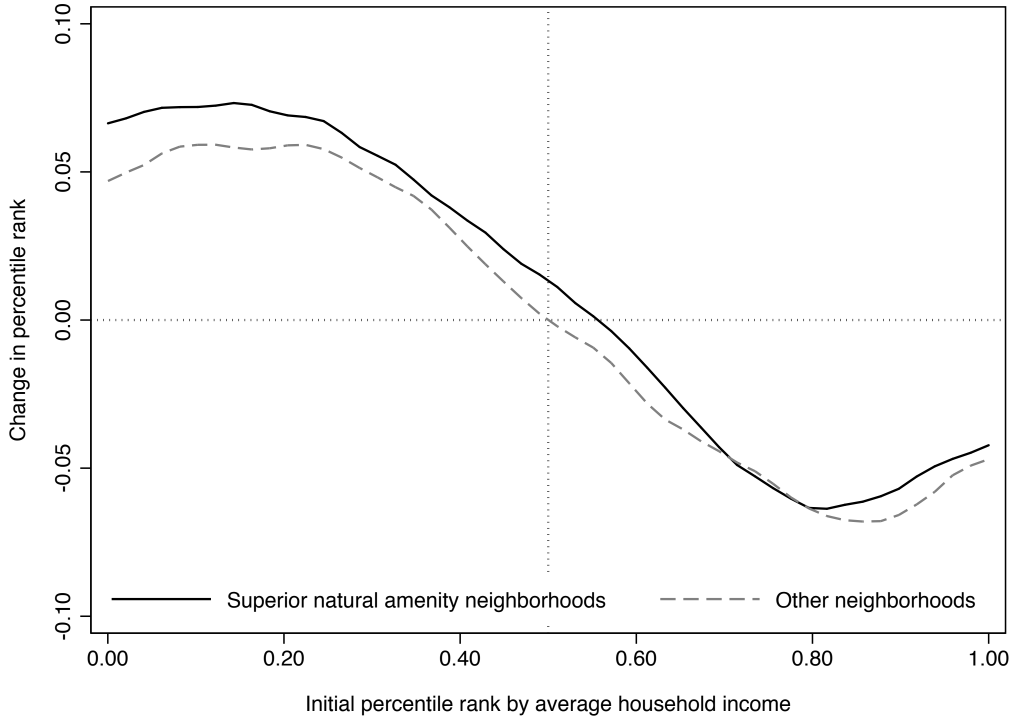
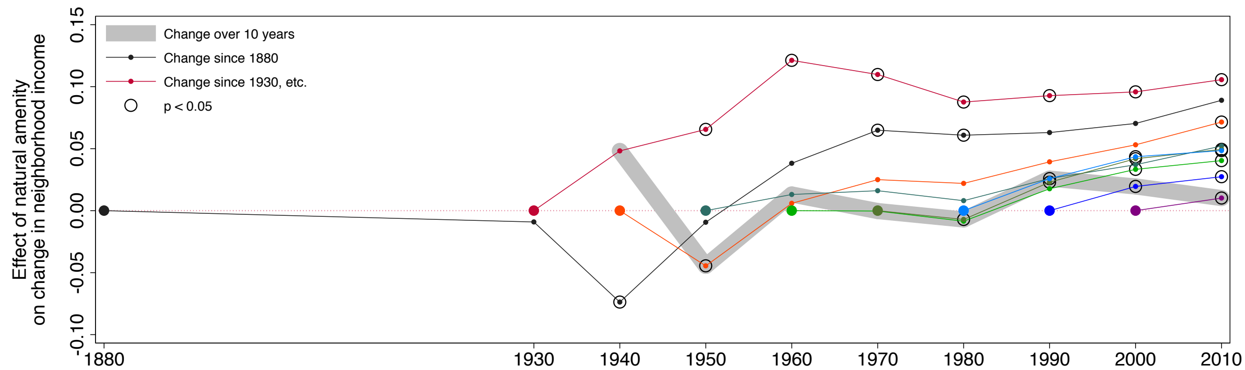


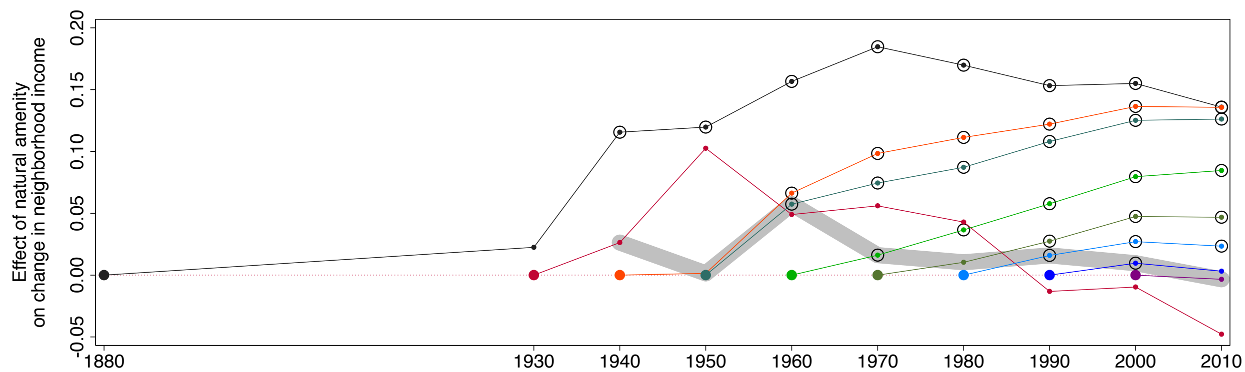
Figure 1: Conditioned on initial income, superior natural amenity neighborhoods increase in income

Kernel-weighted local polynomial smoothing using Stata's lpoly function with Epanechnikov kernel, rule-of-thumb bandwidth, and local-mean smoothing. Superior natural amenity neighborhoods are the top 5% of neighborhoods by our aggregate natural value index.

A. Ocean or Great Lake within 500 meters



B. Average slope greater than 15 degrees



C. Natural aggregate value in top 5%

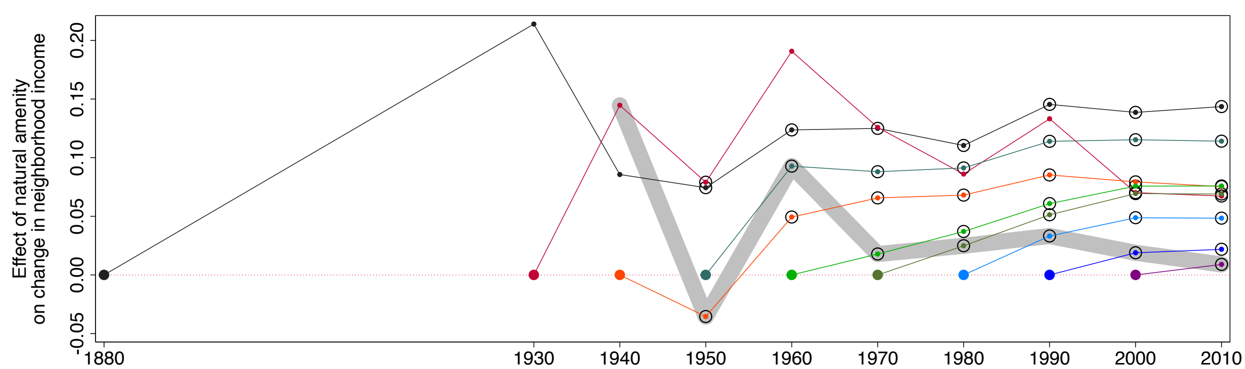


Figure 2: Anchoring: Changes over time in 10-year effects and long-run effects

Each point shows results from a separate regression that varies the base year t and the time horizon Δt. The vertical axis measures the estimated conditional effect of the indicator for the natural amenity noted in the panel title. The dependent variable is the change in percentile rank by income from t (the beginning year of its corresponding line segment) to $t + \Delta t$ (the year corresponding to the horizontal coordinate of the point). Additional control variables are log distance to the nearest seaport, log population density, and log distance to the CBD. Ten-year effects are connected by the thick shaded line. Circled points are significant at $p < 0.10$.

Figure 3: Relative likelihood that a high-income neighborhood is coastal

This figure shows, for each census year, the relative likelihood that a high-income neighborhood (versus a randomly selected neighborhood) is within 500 meters of an ocean, the Gulf of Mexico, or a Great Lake.

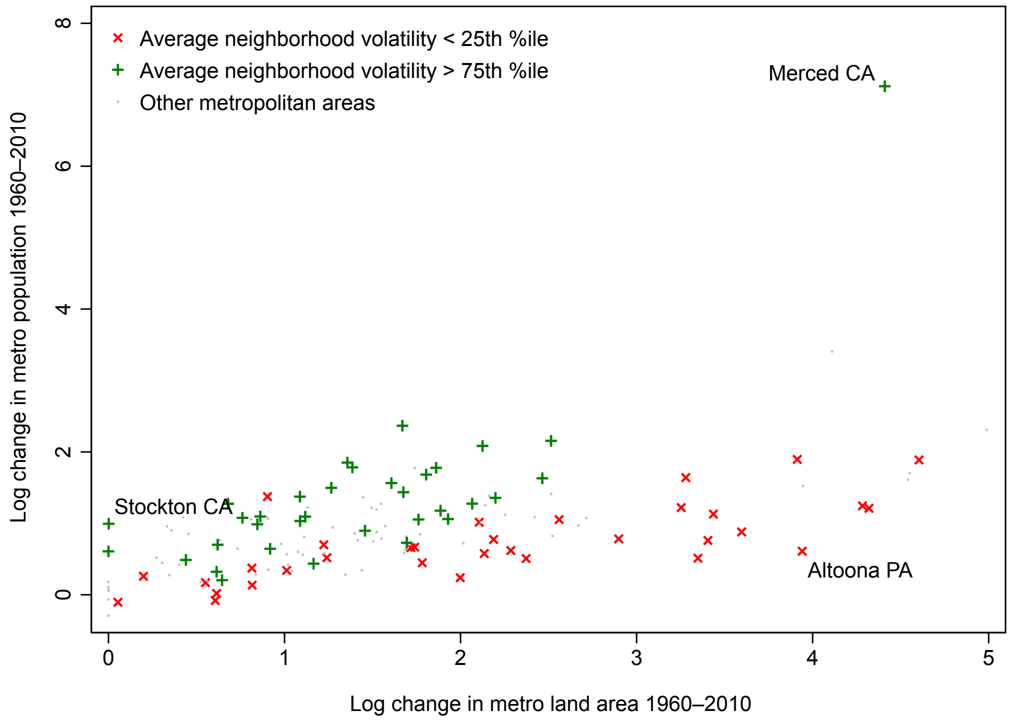


Figure 4: Persistence: Neighborhood volatility and metro growth

This graph shows 50-year changes in population and area for 135 metropolitan areas, 1960–2010. Each point is a metropolitan area. Metropolitan areas with an average over-time variance in neighborhood rank at or above the 75th percentile across metros are indicated by green "+" symbols. Metros with an average over-time variance in neighborhood rank below the 25th percentile are indicated by red "x" symbols.

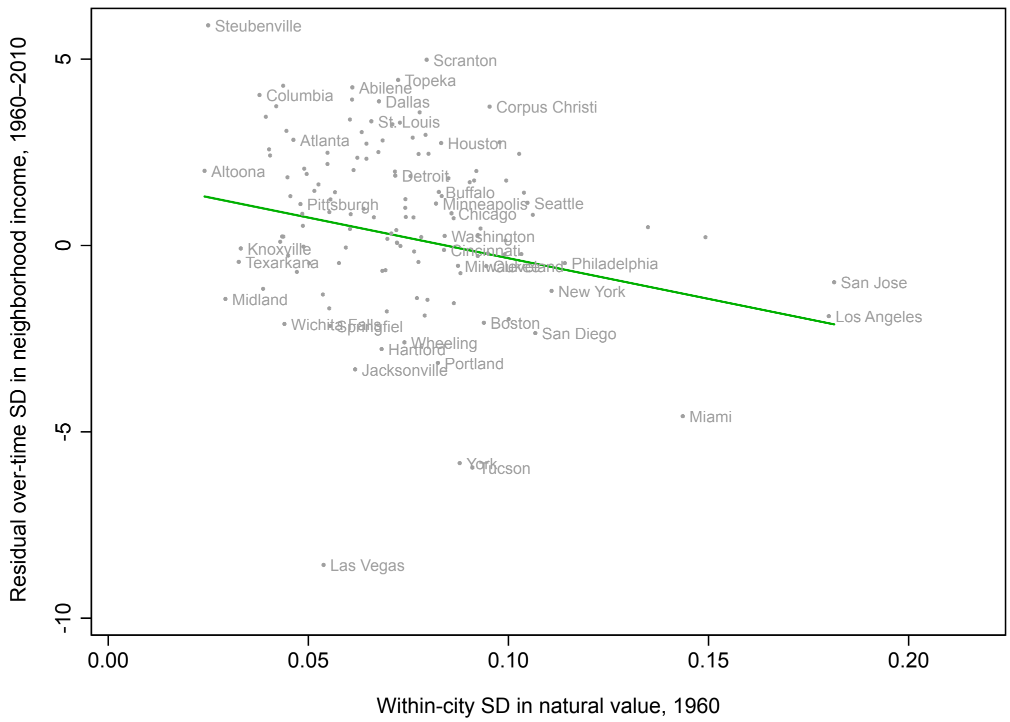


Figure 5: Neighborhoods in naturally heterogeneous cities experience smaller over-time fluctuations in income

The vertical axis measures the metropolitan-level residual from a regression of mean neighborhood 1960 to 2010 standard deviation (SD) in percentile rank by income (×100) on within-metropolitan SD in neighborhood income and log changes in metropolitan population and land area over the same period. The horizontal axis measures the within-metropolitan SD in aggregate natural value using estimated hedonic weights as described in the text. The slope of the fitted line corresponds to the estimate in Table 6, Panel C, column (5).

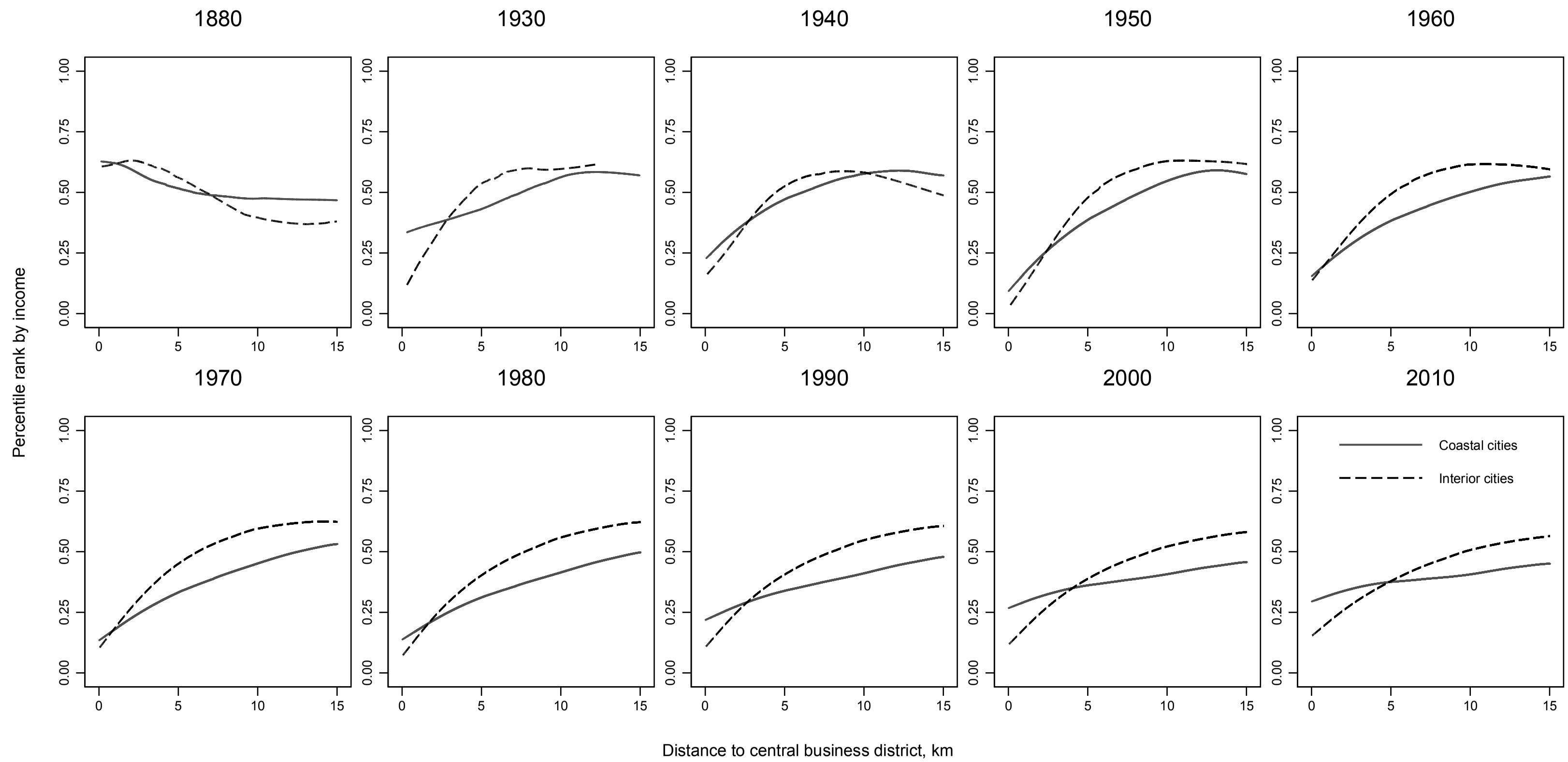


Figure 6: Income and residential location for coastal and interior cities, 1880–2010

Each panel shows, for a different census year, the pattern of neighborhood average household income on the vertical axis versus neighborhood distance to the city center (up to 15km) on the horizontal axis. The smoothed lines are from lowess regressions. Two groups of cities are shown in each panel: Coastal cities are represented by a solid line and interior cities by a dashed line. The sample is a fixed group of 29 metropolitan areas with some missing city observations from 1930 to 1950. Coastal and interior cities are classified in Appendix Table B2.

Table 1: Number of consistent neighborhoods by census year

Year	Metros	Neighborhoods
2010	308	60,757
2000	308	60,766
1990	308	60,299
1980	277	56,176
1970	229	49,888
1960	135	38,669
1950	51	17,681
1940	43	11,527
1930	10	1,962
1920	2	2,505
1910	1	1,748
1880	29	3,071

Table 2: Coastal proximity anchors neighborhoods to high incomes

		(1)	(2)	(3)	(4)	(5)	(6)
	μ $[\sigma]$					$r_{i,t} > 0.9$	Names
1(Coast)*,†,‡	0.05	-0.004	0.013^c	0.007^a	0.014^c	0.045^c	0.031^c
	[0.22]	(0.004)	(0.004)	(0.004)	(0.003)	(0.005)	(0.005)
Initial %ile	0.50	-0.161^c	-0.169^c	-0.184^c	-0.202^c	-0.204^c	-0.203^c
rank by income ($r_{i,t}$)	[0.29]	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)
Log distance to	5.02		0.028^c		-0.004	-0.004^a	-0.004^a
nearest seaport§	[4.83]		(0.004)		(0.002)	(0.002)	(0.002)
Log distance to	7.51			0.035^c	-0.008^c	-0.008^c	-0.008^c
city center	[1.95]			(0.003)	(0.002)	(0.002)	(0.002)
Log population	9.74				-0.036^c	-0.036^c	-0.036^c
density	[1.04]				(0.001)	(0.001)	(0.001)
Log average house	3.00				-0.019^c	-0.019^c	-0.019^c
age	[0.53]				(0.004)	(0.004)	(0.004)
Metro–year f.e.		✓	✓	✓	✓	✓	✓
R^2		0.081	0.090	0.116	0.202	0.202	0.202
Neighborhoods		298,776	298,776	297,518	281,321	281,321	281,321
Metro–years		1,357	1,357	1,313	1,263	1,263	1,263

Each numbered column displays estimates from a separate regression. Column titled "μ $[\sigma]$" shows sample means and standard deviations. Regressions use pooled observations of 60,872 consistent-boundary neighborhoods over 10 census years, 1910–2000. Dependent variable is 10-year forward change in percentile rank by income ($\Delta r_{i,t}$); mean, 0, standard deviation, 0.16. All regressions include metropolitan area–year effects. Standard errors, clustered on metropolitan area–year, in parentheses; ^{a}—p<0.10, ^{b}—p<0.05, ^{c}—p<0.01. *—Neighborhood centroid is within 500m of an ocean, the Gulf of Mexico, or a Great Lake. †—Explanatory variable in column (5) is neighborhood centroid is within 500m of an ocean, the Gulf of Mexico, or a Great Lake, *and* neighborhood initial rank is in the top income decile. ‡—Explanatory variable in column (6) is neighborhood centroid within 500m of an ocean, the Gulf of Mexico, or a Great Lake *and* neighborhood name includes "beach," "coast," "bay," "cove," "lagoon," "ocean," or "shore." §—Log distance to nearest seaport times metropolitan indicator for coastal proximity.

Table 3: Anchoring: Other measures of natural amenity

(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coast	Lakes	Rivers	Hills	Temp. & dry	Pr(flood) < 1%	Nat'l val. > p(95)
A. Indicator for natural feature						
-0.004	0.044^c	0.004^c	0.050^c	-0.033^c	-0.025^c	0.013^b
(0.004)	(0.006)	(0.002)	(0.004)	(0.013)	(0.003)	(0.005)
B. With controls for historical factors						
0.014^c	0.031^c	-0.003^b	0.008^b	0.015	-0.001	0.028^c
(0.003)	(0.005)	(0.001)	(0.003)	(0.010)	(0.002)	(0.004)
C. Indicator for natural feature and $r_{i,t} > 0.9^*$						
0.045^c	0.057^c	0.026^c	0.034^c	0.031^c	0.042^c	0.040^c
(0.005)	(0.014)	(0.005)	(0.004)	(0.009)	(0.003)	(0.005)
D. Place names†						
0.031^c	0.030^c	-0.004	-0.005	0.025^c	0.005^b	0.019^c
(0.005)	(0.007)	(0.003)	(0.003)	(0.007)	(0.002)	(0.005)
E. Sample means of natural amenity indicator, 1910–2000						
0.054	0.006	0.094	0.064	0.072	0.641	0.050

Each cell displays estimates from a separate regression. Dependent variable is the forward 10-year change in percentile rank by income ($\Delta r_{i,t}$); mean, 0, standard deviation, 0.16. All regressions include metropolitan area–year effects. Panel E shows the sample means of natural feature indicators noted in column headings. Standard errors, clustered on metropolitan area, in parentheses; ^{b}—p<0.05, ^{c}—p<0.01. Explanatory variable is an indicator for proximity within 500m in columns (1)–(3), average slope greater than 15 degrees in column (4), mean January minimum temperature between 0 and 18 degrees Celsius and mean July maximum temperature between 10 and 30 degrees Celsius and mean annual precipitation less than 800mm in column (5), mean annual flood probability of less than 1% in column (6), and top 5% in natural value estimated using hedonic weights as described in the text in column (7). Regressions use 282,581 observations of 61,047 neighborhoods in 308 metros, 1910 to 2010, except column (6), which uses 90,987 observations of 27,133 neighborhoods in 177 metros with valid floodplain data for more than 95% of neighborhoods. *—Explanatory variable in Panel C is an indicator for natural amenity *and* neighborhood initial rank is in top income decile. †—Explanatory variable in Panel D, column (1) is an indicator for natural amenity *and* the neighborhood name includes "bay," "beach," "cape," "coast," "cove," "gulf," "lagoon," "ocean," "sea," or "shore." Column (2): "lake," "pond," or "island." Column (3): "brook," "creek," "fall," "rapid," "river," "spring," or "stream." Column (4): "bluff," "butte," "canyon," "cliff," "height," "hill," "knoll," "mount," "ridge," "summit," "terrace," "view," or "vista." Column (5): same as column (1). Column (6): "stream" or "river." Column (7): all of the above.

Table 4: Anchoring: Endogenous factors, 2000–2010

	μ [σ]	(1)	(2)	(3)	(4)	(5)
1(Coast)	0.05	0.007^b	0.011^c	0.010^b	0.014^c	0.015^c
	[0.21]	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Initial %ile rank	0.50	-0.097^c	-0.129^c	-0.125^c	-0.119^c	-0.120^c
by income ($r_{i,t}$)	[0.29]	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)
Log distance	4.73		-0.006^c	-0.004^b	-0.001	-0.002
to seaport	[4.88]		(0.002)	(0.002)	(0.003)	(0.003)
Log distance	9.87		-0.011^c	-0.009^c	-0.011^c	-0.010^c
to city center	[1.04]		(0.002)	(0.002)	(0.002)	(0.002)
Log population	7.53		-0.022^c	-0.022^c	-0.024^c	-0.024^c
density	[1.79]		(0.001)	(0.001)	(0.002)	(0.002)
Log average	3.37		-0.008^c	-0.020^c	-0.017^c	-0.016^c
house age	[0.48]		(0.003)	(0.003)	(0.005)	(0.005)
Share houses	0.15			0.070^c	0.092^c	0.092^c
built before 1940	[0.19]			(0.010)	(0.010)	(0.010)
Wharton Residential	0.13					0.004^b
Land Use Reg. Index	[0.98]					(0.002)
R^2		0.049	0.095	0.100	0.113	0.113
Neighborhoods		60,073	60,073	60,073	22,591	22,591
Metro areas		293	293	293	247	247

Each numbered column displays estimates from a separate regression. Column titled "μ [σ]" shows sample means and standard deviations. Regressions use cross-section of consistent-boundary neighborhoods, 2000–2010. Dependent variable is change in percentile rank by income ($\Delta r_{i,t}$); mean 0, standard deviation 0.13. All regressions include metropolitan area fixed effects. Standard errors, clustered on metropolitan area, in parentheses; ^{a}—p<0.10 ^{b}—p<0.05, ^{c}—p<0.01.

Table 5: Persistence in metros with variation in coastal proximity, 1960–2010

		(1)	(2)	(3)	(4)	(5)
	$\boldsymbol{\Gamma}_m \equiv$		$1(Coast_m)^*$		$\sigma(C_{i\|m})^\dagger$	$\sigma(\hat{a}_{i\|m})^\ddagger$
	μ $[\sigma]$		0.29 [0.45]		0.42 [0.57]	0.07 [0.03]
Metro natural heterogeneity ($\boldsymbol{\Gamma}_m$)		-1.840^b (0.742)	-1.500^c (0.523)	-1.362^b (0.529)	-1.487^c (0.417)	-36.242^c (5.149)
Metro log change in population, 1960–2010	0.94 [0.78]		4.231^c (0.633)	4.213^c (0.658)	4.083^c (0.629)	5.353^c (0.564)
Metro log change in land area, 1960–2010	1.61 [1.15]		-1.606^c (0.363)	-1.572^c (0.356)	-1.817^c (0.378)	-2.457^c (0.296)
Within-metro SD in neighborhood income (thous.)	1.91 [0.43]			-0.902^a (0.504)	-0.746 (0.563)	-0.744^a (0.441)
Within-metro SD in neighborhood avg. house age	3.69 [0.78]			0.191 (0.372)	0.138 (0.340)	0.726^b (0.335)
1st level						
Initial rank decile				✓	✓	✓
R^2		0.117	0.555	0.572	0.602	0.695
Metropolitan areas		135	135	135	135	135

Each column displays estimates from the second level of separate two-level regressions. Row and column titled "μ $[\sigma]$" show sample means and standard deviations (SD) for dependent and explanatory variables, respectively. First-level OLS regressions (unreported) use neighborhood observations in census year 1960 to estimate 135 metropolitan area means and cluster-robust standard errors. Dependent variable is over-time SD in percentile rank ×100, 1960–2010; mean, 12.9, SD, 7.9 in balanced panel of 38,293 neighborhoods over six census years. Second-level weighted least squares (WLS) regressions use 135 metropolitan areas. Dependent variable is estimated metropolitan area means from first level, and weights are inverse estimated variance from first level; mean, 13.1, SD, 2.8. Robust standard errors in parentheses; ^{a}—$p<0.10$, ^{b}—$p<0.05$, ^{c}—$p<0.01$. *—Measure of metropolitan natural heterogeneity $\boldsymbol{\Gamma}_m$ in columns (1)–(3) is a metropolitan indicator for coastal proximity. †—Measure of $\boldsymbol{\Gamma}_m$ in column (4) is the within-metropolitan area SD in log neighborhood distance to an ocean or Great Lake. ‡—Measure of $\boldsymbol{\Gamma}_m$ in column (5) is the within-metropolitan area SD in estimated aggregate natural value.

Table 6: Persistence: Robustness to other years and measures of within-metro natural heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Base year:	1880	1930	1940	1950	1960	1970	1980	1990	2000
μ	19.4	17.5	13.8	13.8	12.1	10.2	9.06	8.90	7.37
$[\sigma]$	[6.92]	[9.05]	[8.44]	[8.16]	[8.29]	[7.29]	[6.53]	[6.63]	[7.25]
A. $\Gamma_m = 1(Coast_m)$ (Metro indicator for coastal status)									
0.22	0.013	3.490^{a}	-0.071	-0.210	-1.500^{c}	-1.004^{b}	-0.372	-0.255	-0.089
[0.42]	(0.388)	(1.772)	(0.531)	(0.518)	(0.523)	(0.398)	(0.300)	(0.229)	(0.176)
B. $\Gamma_m = \sigma(Coast_{i\|m})$ (Within-metro standard deviation in log distance to coast)									
0.42	0.357	3.859^{a}	-0.236	-0.280	-1.605^{c}	-0.952^{c}	-0.479^{c}	-0.354^{b}	-0.183
[0.62]	(0.746)	(1.681)	(0.642)	(0.551)	(0.389)	(0.227)	(0.164)	(0.143)	(0.120)
C. $\Gamma_m = \sigma(\hat{a}_{i\|m})$ (Within-metro standard deviation in aggregate natural value)									
0.08	19.701	81.591^{b}	-4.199	-27.573^{c}	-34.661^{c}	-24.080^{c}	-12.212^{c}	-7.346^{c}	-3.837^{a}
[0.03]	(12.217)	(29.894)	(7.598)	(9.574)	(5.085)	(3.411)	(2.435)	(2.820)	(2.220)
*Years**	7	9	8	7	6	5	4	3	2
Metros	29	10	38	51	135	227	277	308	308
First level *Neighborhoods*	3,002	1,935	11,167	17,420	38,293	49,660	55,911	60,063	60,545

Each cell displays estimates from the second level of separate two-level regressions. Column and row titled "μ $[\sigma]$" show sample means and standard deviations for explanatory variables (in 2010) and dependent variables, respectively. Regression specifications are same as Table 5, column (2). First-level OLS regressions (unreported) use neighborhood observations in the base year to estimate metropolitan area means and robust standard errors. Dependent variable is over-time SD in percentile rank $\times$100, between base year and 2010; metropolitan-level means and standard deviations in the first row. Second-level WLS regressions use metropolitan areas. Dependent variable is the estimated metropolitan area means from first level, and weights are inverse estimated variance from first level. Robust standard errors are in parentheses; ^{a}—$p<0.10$, ^{b}—$p<0.05$, ^{c}—$p<0.01$. *—For each base year, we balance our neighborhood panel to calculate over-time variances.

Table 7: Decentralization of income and metropolitan coastal proximity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample years:		1910–2010¶		1950–1980	1980–2010	1990–2010 (Kneebone)	
1(CBD)*	-0.040^c	-0.085^c	-0.015^c	-0.023^c	0.005^b	0.018^c	0.018^b
	(0.004)	(0.004)	(0.003)	(0.005)	(0.003)	(0.004)	(0.008)
1(CBD) × 1(Coastal metro)†	0.032^c	0.034^c	0.020^b	0.030^c	0.029^c	0.025^b	0.025^b
	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)	(0.011)	(0.011)
Initial %ile rank by income ($r_{i,t}$)		-0.181^c	-0.205^c	-0.313^c	-0.124^c	-0.108^c	-0.108^c
		(0.009)	(0.006)	(0.010)	(0.003)	(0.003)	(0.003)
Log distance to seaport			-0.003	-0.003	-0.008^c	-0.007^c	-0.007^c
			(0.003)	(0.005)	(0.002)	(0.002)	(0.002)
Log population density			-0.035^c	-0.048^c	-0.021^c	-0.020^c	-0.020^c
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log average house age‡				-0.075^c	0.005^a	0.008^c	0.008^c
				(0.010)	(0.002)	(0.003)	(0.003)
1(CBD) × ΔCBD job share§							0.034
							(0.242)
1(CBD) × Δ3–10 mi job share‖							-0.378^b
							(0.148)
Metro–year f.e.	✓	✓	✓	✓	✓	✓	✓
R^2	0.004	0.098	0.207	0.338	0.107	0.099	0.099
Neighborhoods	297,522	297,522	297,520	105,529	175,794	98,006	98,006
Metros	293	293	293	224	293	86	86

Each column displays estimates from a separate regression. Dependent variable is 10-year forward change in percentile rank by income ($\Delta r_{i,t}$); mean, 0, standard deviation, 0.16. Standard errors, clustered on metropolitan area–year, in parentheses; ^{a}—p<0.10, ^{b}—p<0.05, ^{c}—p<0.01. Regressions use observations of 60,400 neighborhoods in 293 metropolitan areas, 1910 to 2010. *—Neighborhood is within 5km of principal city CBD. †—Metropolitan area CBD is within 1km of an ocean or a Great Lake. ‡—Available 1950 and later. §, ‖—Changes in share of metro jobs within 3 miles and 3 to 10 miles, respectively, of CBD, 1998 to 2006 (Kneebone, 2009). ¶—Sample for which 10-year forward change in percentile rank by income is defined.

A Theory Appendix

A.1 Proof of Proposition 5

We write the Markov transition matrix as

$$M \equiv \left\{ \begin{array}{cc} \Pr(S_1|S_1) = 1 - \Pr(S_2|S_1) & \Pr(S_1|S_2) \\ \Pr(S_2|S_1) & \Pr(S_2|S_2) = 1 - \Pr(S_1|S_2) \end{array} \right\} \tag{13}$$

and define the steady-state vector π as

$$\pi = M\pi \tag{14}$$

where the elements of π are positive and sum to 1. The steady-state vector π is a time-invariant probability distribution over the two states, which we can also interpret as the long-run probability distribution over the states in the city.

Any Markov chain with a regular transition matrix (defined as a matrix whose elements are all positive for some power of the matrix) is known to converge to a steady state. Since M is a regular Markov matrix, the probability distribution over states converges to the steady-state vector π. By solving equation (14), we obtain π:

$$\pi \equiv \left\{ \begin{array}{c} p^*_{S_1} \\ p^*_{S_2} \end{array} \right\} = \frac{1}{\Pr(S_2|S_1) + \Pr(S_1|S_2)} \left\{ \begin{array}{c} \Pr(S_1|S_2) \\ \Pr(S_2|S_1) \end{array} \right\}. \tag{15}$$

Denote by $Var(r_{j,t})$ the over-time variance in percentile income rank of a neighborhood j. In steady state, the beach's over-time variance in income rank can be written as

$$\begin{aligned} Var(r_{j,t}|j = b) &= \{p^*_{S_1}(r_H)^2 + (1 - p^*_{S_1})(r_L)^2\} - \{p^*_{S_1} r_H + (1 - p^*_{S_1}) r_L\}^2 \\ &= (1 - p^*_{S_1}) \cdot p^*_{S_1} \cdot (r_H - r_L)^2. \end{aligned}$$

Since the average income of the beach takes exactly the opposite value to that of the desert, the over-time variances are equal. Thus, the average rank variance for the city can be written as

$$E(Var(r_{j,t}|j)) = (1 - p^*_{S_1}) p^*_{S_1} \cdot (r_H - r_L)^2.$$

This city average variance is maximized when $p^*_{S_1} = 0.5$ and decreases monotonically as $p^*_{S_1}$ moves away from 0.5. Equations (4), (5), and (15) imply that, conditional on $r_H - r_L$, $p^*_{S_1}$ increases from 0.5 with $|\alpha_b - \alpha_d|$. Therefore, $E(Var(r_{j,t}|j))$ decreases with $|\alpha_b - \alpha_d|$. Intuitively, the city's spatial distribution of income experiences the least persistence over time when there is no natural heterogeneity within the city (i.e., the city is in a flat, featureless plain). As the beach's natural advantage increases, the likelihood of churning between states declines, leading to stability and persistence in the spatial distribution of income.

A.2 Proof of Proposition 6

Just as we derive equations (6) and (7), we can calculate expected income percentile rank change for each neighborhoood and initial income:

[Table A1 about here.]

Equations (4) and (5) imply that, as $|\alpha_b - \alpha_d|$ increases, $Pr(S_2|S_1)$ decreases and $Pr(S_1|S_2)$ increases. It follows from Table A1 that, as $|\alpha_b - \alpha_d|$ increases, the gap between beach and desert in expected income change increases regardless of initial income level. In other words, the anchoring effect of natural amenities is stronger in naturally heterogeneous cities.

A.3 Full model

This section presents the full model that allows the city to have more than two neighborhoods and the amenity shocks $\epsilon_{j,t}$ to be correlated over time. The full model differs from the simple model presented in Section 2.1 in the main text in the following ways. First, the city has $J \in \mathbb{N}$ neighborhoods and J unit measure of workers. Second, the aggregate amenity shock $\epsilon_{j,t}$ follows an AR(1) process: $\epsilon_{j,t+1} = \rho\epsilon_{j,t} + \nu_t$ where ν_t is independent and identically distributed. Third, we extend the equilibrium selection rule in Section 2.3 as follows: When there are multiple equilibria, we choose the one that is closest to the selected equilibrium in the previous period, in terms of the Euclidean distance in the vector of average incomes across neighborhoods.

A.3.1 Equilibrium within a period

Lemma 1, which states that higher-income workers sort into superior aggregate amenity neighborhoods, holds with the full model, because it is driven solely by workers' preferences.

To precisely describe the sorting with J neighborhoods, we introduce new notation. First, we partition the set of worker incomes $[\underline{\theta}, \bar{\theta}]$ into J intervals $\{\Theta_1, \Theta_2, ..., \Theta_J\}$ so that each group has a unit measure of workers; Θ_1 is the top income group, and Θ_J is the bottom group. $\hat{\theta}_{i,i+1}$ denotes workers who divide group i from group $i+1$; i.e., $\Theta_J \equiv [\underline{\theta}, \hat{\theta}_{J-1,J}]$, $\Theta_{J-1} \equiv [\hat{\theta}_{J-1,J}, \hat{\theta}_{J-2,J-1}]$, ..., $\Theta_1 \equiv [\hat{\theta}_{1,2}, \bar{\theta}]$. Second, we define a neighborhood rank function $\tilde{r}_t : J \to J$ such that $\tilde{r}_t(j)$ is the rank of neighborhood j in terms of aggregate amenities in period t. For example, suppose that neighborhood 1 is the third best neighborhood in terms of aggregate amenities in period 2. Then we have $\tilde{r}_2(1) = 3$. Note that its inverse function $\tilde{r}^{-1}$ maps back to neighborhood index numbers, given aggregate amenity rankings. For example, suppose that the second best neighborhood in period 3 is neighborhood 4. Then we have $\tilde{r}_3^{-1}(2) = 4$.[39]

Since each Θ_j group of workers consumes one unit measure of land and each neighborhood has one unit measure of land, each Θ_j group of workers occupies one and only one neighborhood. Further, since higher-skill workers select into better aggregate amenity neighborhoods, each group Θ_j occupies neighborhood $\tilde{r}_t^{-1}(j)$ in each period t.

We characterized workers' location choices as a function of the distribution of aggregate amenities across neighborhoods. In turn, workers' location choices must generate a distribution of aggregate amenities across neighborhoods that is consistent with their location choices. In other words,

$$a_{\tilde{r}_t^{-1}(j)} + E(\theta|\theta \in \Theta_j) + \varepsilon_{\tilde{r}_t^{-1}(j)} \geq a_{\tilde{r}_t^{-1}(j+1)} + E(\theta|\theta \in \Theta_{j+1}) + \varepsilon_{\tilde{r}_t^{-1}(j+1)}. \tag{16}$$

Proposition 2, which states that an equilibrium exists in each period and there can be multiple equilibria, holds with the full model. First, an equilibrium always exists because condition (16) is satisfied if higher-income workers choose to live in neighborhoods with greater exogenous amenities: $a_j + \varepsilon_{j,t}$. Second, there can be multiple equilibria. For example, condition (16) is satisfied for any matching pattern between income groups and neighborhoods if exogenous amenities (i.e., $\alpha_j + \epsilon_{j,t}$) are identical across all neighborhoods.

Now we characterize how rents are determined in each period. We normalize rent for the least favored neighborhood to be 0, that is,

$$R_{\tilde{r}_t^{-1}(J)} = 0.$$

For the other neighborhoods, equilibrium rent $R_{\tilde{r}_t^{-1}(j)}$ is recursively determined so that $\hat{\theta}_{j,j+1}$ workers (i.e., workers who divide Θ_j and Θ_{j+1}) are indifferent between neighborhood $\tilde{r}_t^{-1}(j)$ and $\tilde{r}_t^{-1}(j+1)$:

$$A_{\tilde{r}_t^{-1}(j)} \cdot (\hat{\theta}_{j,j+1} - R_{\tilde{r}^{-1}(j),t}) = A_{\tilde{r}_t^{-1}(j+1)} \cdot (\hat{\theta}_{j,j+1} - R_{\tilde{r}_t^{-1}(j+1)}).$$

[39] Note also that this rank function $\tilde{r}$ differs from the percentile rank income r in three ways. First, $\tilde{r}$ ranks neighborhoods based on their aggregate amenity levels A_j while r on average income (i.e., endogenous amenity level) $E(\theta|j)$. Second, $\tilde{r}$ assigns a lower number for a better aggregate amenity neighborhood, while r assigns a higher number for a better average income neighborhood. Third, $\tilde{r}$ gives an integer rank, while r gives a percentile rank.

This equation recursively pins down rent for each neighborhood. Note that neighborhood rents follow the same order as average incomes, as with the simple model.

A.3.2 Equilibrium selection and history dependence

When there are multiple equilibria, we choose the one that is closest to the selected equilibrium in the previous period, in terms of the Euclidean distance in the vector of average incomes (i.e., endogenous amenities) across neighborhoods.

Partly because the number of possible location-choice patterns increases dramatically with that of neighborhoods (i.e., $J!$ with J neighborhoods), we cannot analytically prove Propositions 4, 5, and 6 with the full model. Instead, we use numerical methods to demonstrate that the results are robust with more than two neighborhoods and serially correlated amenity shocks.

For various combinations of parameters, we calculate the equilibrium path for 100,000 periods and test whether the propositions hold. The following list of parameters are used in the simulations. For the number of neighborhoods J, we use 3, 5, and 7 neighborhoods. For the natural amenity distribution across neighborhoods, we use $\xi \times (1, 2, ..., J)$ and vary ξ to be 1, 3, 5, and 10. Note that the variance in natural amenity levels increases as ξ increases. For the average income distributions across neighborhoods, we use $\psi \times (1, 2, ..., J)$ and vary ψ to be 1, 3, 5, and 10. For amenity shocks, we assume that $\epsilon_{j,t}$ follows an AR(1) process $\epsilon_{j,t} = \rho\epsilon_{j,t-1} + \nu_{j,t}$, where $\nu_{j,t}$ follows a Normal distribution $(0, \sigma^2)$. We vary ρ to be 0, 0.2, 0.6, 0.9, 0.95, 0.98, 0.99, and 1 and vary σ to be 1, 3, 5, and 10. ρ determines how much the amenity shocks are correlated over time. Note that the amenity shocks are stationary if ρ is less than 1. σ determines how volatile the shocks are. This grid of parameters generates 1,536 unique combinations of parameters.

We begin with Proposition 4. For each combination of all parameters with $\rho < 1$ (1,344 combinations in total), we obtain a number $J \times 100,000$ of neighborhood-period level simulated data. For each combination with $\rho < 1$, we regress change in percentile rank income of a neighborhood on its percentile rank natural amenity level and its current period percentile rank income. This is the base specification we use in our empirical analysis.

Proposition 4 implies that the coefficient on natural amenity should be positive. Our simulation results confirm this prediction with stationary amenity shocks. With $\rho \leq 0.98$, the coefficients were weakly positive for all 1,152 combinations. With $\rho = 0.99$, only four parameter combinations out of 192 show small negative values. The small number of negative outcomes seem to be driven by numerical errors, as neighborhood shocks become close to a nonstationary unit-root process. With a unit-root process (i.e., $\rho = 1$), our predictions do not hold: 128 of 192 cases show negative values.

Next, we test Proposition 5. We calculate $E(Var(r_{j,t}|j))$ for each parameter set. The Proposition implies that $E(Var(r_{j,t}|j))$ decreases with ξ, and our simulation results show that $E(Var(r_{j,t}|j))$ indeed decreases with a stationary amenity shock.

Finally, we test Proposition 6. The effect of superior natural amenities is captured by the coefficient on percentile rank natural amenity level in the previous regressions used to test Proposition 4. We test if the coefficients tend to increase with ξ. (Recall that natural amenity values are $\xi \times (1, 2, ..., J)$ across neighborhoods. As ξ increases, heterogeneity in natural amenity values increases.) We calculate the mean value of the coefficient estimates for each ξ=1, 3, 5, 10. Each ξ group has 384 parameter sets. The results show that the mean coefficient increases monotonically with ξ.

Table A1: Expected change in income conditioned on natural amenity and initial income

	Initial Income	
Neighborhood	r_H	r_L
Beach	$(r_L - r_H)Pr(S_2\|S_1)$	$(r_H - r_L)Pr(S_1\|S_2)$
Desert	$(r_L - r_H)Pr(S_1\|S_2)$	$(r_H - r_L)Pr(S_2\|S_1)$
Difference	$(r_H - r_L)(Pr(S_1\|S_2) - Pr(S_2\|S_1))$	$(r_H - r_L)(Pr(S_1\|S_2) - Pr(S_2\|S_1))$

B Data appendix

B.1 Figures and tables referenced in footnotes

[Figure B1 about here.]

[Figure B2 about here.]

[Table B1 about here.]

[Table B2 about here.]

B.2 Census data and boundary normalization

We use 2010 census tract data from the American Community Survey (ACS) five-year summary file via the National Historical Geographic Information System (NHGIS) (Minnesota Population Center, 2011). These data cover the entire geographic extent of the U.S., although we focus on metropolitan (core-based statistical) areas only. The ACS is the annual replacement for the decennial long-form data, and it includes much of the detailed information on population and housing (e.g., income) that is no longer reported in the decennial census. However, the ACS has one important limitation. Because of small annual sample sizes and privacy concerns, these data represent five-year averages of residents and houses located in each tract. Thus, although we refer to these data as coming from the year 2010 throughout the paper, they really represent an average over 2006–2010. Finally, since these data already follow 2010 census tract boundaries, no normalization is required.

Census data for 1970–2000 are from the Geolytics Neighborhood Change Database (NCDB) (Tatian, 2003). These data are already normalized to 2000 (n.b., not 2010) census tract boundaries. The NCDB methodology compares maps of the 2000 census tract and block boundaries with earlier years. Then, 1990 census block information (each tract is composed of many blocks) is used to determine the proportion of people in each historic tract that should be assigned to each overlapping 2000 tract. These proportions are then used as weights to normalize the data to 2000 boundaries.[40]

To normalize the NCDB data to 2010 census tract boundaries, we use the Longitudinal Tract Database (LTDB) (Logan, Xu, and Stults, 2014). The LTDB uses the same block-weighting methodology as the NCDB. Thus, our analysis uses weights defined by 2000 census block populations to normalize all of the Geolytics NCDB data, from 1970 to 2000, to 2010 census tract boundaries. It is important to note that in 1980 and earlier, the entire geographic extent of the U.S. was not completely organized into tracts, and missing data problems are more severe for earlier years. However, since we focus on metropolitan areas, data quality is quite good as early as 1970. (We also drop tract observations in years when their respective metropolitan area is incompletely tracted. See more on sample selection below.)

For census years 1910–1960, we use decennial census information from the NHGIS. The 1940, 1950, and 1960 NHGIS extracts are collectively known as the Bogue files (2000a, 2000b, and 2000c), and they are also available from the Inter-university Consortium for Political and Social Research. These files contain tract information for selected cities and metropolitan areas. The 1910, 1920, and 1930 NHGIS extracts are known as the Beveridge files. Note that data availability is sparse, especially before 1950. Even for cities that are completely tracted, sometimes the data do not contain complete information on population, housing, or income. (For example, in 1910, tract information on household income is only available for New York City; in 1920, such information is only available for New York City and Chicago. Ten metropolitan areas have valid data in 1930, and 43 metropolitan areas have valid data in 1940.) We normalize these data to

[40] We make a small adjustment to the 1980 Geolytics NCDB. The 1980 census prized identification of "places" (e.g., towns, villages, boroughs) over tracts when confidentiality restrictions were binding. The NCDB propagates this censoring in its normalization procedure, even if the proportion of households in the tract with suppressed income data is negligible. We restore this income information from the original 1980 census as long as the proportion of censored households in a census tract is less than 20%.

2010 census tract boundaries ourselves using NHGIS map layers. For each decade, we compare historical tract boundaries with 2010 census tract boundaries. Since subtract or block information on population is unavailable for these historical years, we are unable to exactly follow the NCDB and LTDB methodologies of constructing weights using block populations. Instead, we normalize using a simple apportionment based on land area.

Finally, we draw 1880 census information from the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al., 2010). We use both the 100% census and the 10% population sample; the 10% sample includes information on literacy, while the 100% census does not. The IPUMS includes data on each person's place of residence, via the enumeration district variable. Enumeration districts were areas assigned to census enumerators to gather data, and they are comparable in population size with modern-day census tracts. (In fact, on average, they are slightly smaller than modern-day census tracts.) We use enumeration districts to normalize the historical 1880 data to 2010 census tract boundaries. First, we obtain maps on historical enumeration district boundaries from the Urban Transition Historical GIS Project (UTHGIS) (Logan et al., 2011). Maps are available for 32 present-day metropolitan areas (totaling 29 consolidated metropolitan areas). (Note that our ability to match households to neighborhoods is limited both by availability of household data—the 1890 census was destroyed by fire—and the availability of maps showing the spatial extent of historical census tracts or enumeration districts.) Second, using the same procedure as for 1910–1960, we compare historical enumeration district boundaries with 2010 census tract boundaries. We apportion to 2010 census tract boundaries using land area.

B.3 Sample selection

We exclude a number of tract observations according to the following criteria. We drop tracts in Alaska, Hawaii, and Puerto Rico. We exclude tracts with zero land area (these are typically "at sea" populations, i.e., personnel on ships) or zero population (e.g., airports or zones otherwise reserved for nonresidential uses).

We do not consider tracts outside of metropolitan areas defined in 2009. One problem with nonmetropolitan tracts is that many of them are not available before 1990, the first year that the U.S. was fully organized into census tracts. Another problem with rural tracts is the difficulty in grouping these tracts into units that share common labor, housing, product, and input markets. (Exceptions are the core-based statistical areas called micropolitan areas. However, many of these micropolitan areas feature a very small number of tracts, making them unsuitable for our analysis. The very small number of tracts means that the entry of even one new neighborhood can elicit a volatile response in within-micropolitan area rankings.)

We drop tracts in particular years that are clearly nonurban. This restriction is more salient in historical years, when tracts or enumeration districts on the urban fringe were not subject to urban land uses. We classify tracts as nonurban if (i) the entire tract population is classified by the census as "rural" or (ii) population density is less than 32 people per square mile, or one person per 20 acres. (Lowering this threshold to one person per 40–160 acres affects the number of excluded tracts minimally. Population densities of less than 32 people per square mile are already well short of standard definitions of urban population densities.) We reason that while these tracts are within counties that contain urban uses, at the time of observation, they are likely to be outside of metropolitan areas and urban household location decisions. In this way, we also address concerns about changing metropolitan area boundaries over time.

We exclude tracts where our normalization procedure is likely to be poor. In some cases, especially for early census years and tracts on the urban fringe, historical tracts cover only a portion of 2010 census tract areas. This is more likely to be the case when historical city boundaries are much smaller than present-day extents. When historical tracts cover less than 50% of the land area of the present-day tract, we exclude these data from our analysis.

We also eliminate tract observations that disappear from one year to the next. This problem is partly mechanical; we cannot compute income changes for a tract that does not appear in the next period. It also is mostly limited to the transition between the 1880 UTHGIS data and the subsequent NHGIS data. The reason this problem arises is because the UTHGIS maps, which we use for our normalization procedure, typically cover entire counties, whereas the NHGIS data and maps used in the early 20th century are confined mostly to city boundaries. Thus, many of the UTHGIS tracts outside city boundaries are dropped anyway because they are nonurban (as previously noted), but to avoid the problem of contracting metropolitan boundaries, we exclude the remaining earlier tracts that do not appear in subsequent years.

A consequence of the unbalanced nature of the data is that forward lags vary by metropolitan area and year. For example, after 1880, it is only 30 years until our next observation of New York neighborhoods (in 1910), but it is 70 years until our next observation of Omaha neighborhoods (in 1950). Out of 1,684 metropolitan area–year groups in our data, 1,342 follow the standard 10-year gap between census year observations. As a result, the actual number of neighborhoods used in regressions varies according to whether the specification requires balancing across two subsequent census years or balancing over a large number of years. In addition, some variables, such as flood hazard or average housing unit age, are unavailable in some years, further affecting sample selection.

B.4 Natural amenity data

We spatially match our consistent-boundary neighborhoods to a number of natural and persistent geographic features.

Water features—coastlines, lakes, and rivers. We use data on water features from the National Oceanic and Atmospheric Administration's (2012) Coastal Geospatial Data Project. These data consist of high-resolution maps covering (i) coastlines (including those of the Atlantic, Pacific, Gulf of Mexico, and Great Lakes), (ii) other lakes, and (iii) major rivers. For each 2010 census tract, we separately calculate the distance to each of the nearest water features (ocean, lake, river) from the centroid of the tract.

Elevation and slope. We use the elevation map included in the Esri 8 package. These data have a 90-meter resolution. In ArcGIS, we use the slope geoprocessing tool to generate a slope map. Then we use the zonal statistics tool to calculate average slope in each 2010 census tract.

Floodplains. The Federal Emergency Management Agency (FEMA, 2012) publishes National Flood Hazard Layer (NFHL) maps covering much of the U.S. The NFHL maps show areas subject to FEMA's flood zone designations. We assign to tracts either a high-risk or low-risk indicator. High risk means that an area has at least a 1% annual chance of flooding (a 26% chance of flooding over a 30-year period), as determined by FEMA. Note that flood maps are unavailable for some metropolitan areas. In our data, 261 metropolitan areas have valid flood zone information.

Temperature and precipitation. We match tracts to temperature and rainfall data available from the PRISM Climate Group (2004) at Oregon State University. These data are 1971–2000 averages, collected at thousands of weather monitoring stations and processed at a spatial resolution of 30 arcseconds for the entire spatial extent of the U.S., of annual precipitation, July maximum temperature, and January minimum temperature.

B.5 Other data

City centers. Data on principal city center locations for 293 metropolitan areas were generously provided to us by Dan Hartley. Fee and Hartley (2013) identify the latitude and longitude of city centers by taking the spatial centroid of the group of census tracts listed in the 1982 Census of Retail Trade for the central city of the metropolitan area. Metropolitan areas not in the 1982 Census of Retail Trade use the latitude and longitude for central cities using ArcGIS's 10.0 North American Geocoding Service.

Seaports. Data on seaport locations are from the *World Port Index*, 23rd edition, published by the National Geospatial-Intelligence Agency (2014).

Land use regulation. The Wharton Residential Land Use Regulatory Index is from Gyourko, Saiz, and Summers (2008).

Neighborhood names. Information on neighborhood names comes from the U.S. Geographic Names Information System (GNIS), maintained by the U.S. Geological Survey (USGS), and the U.S. Board on Geographic Names, which maintains uniform usage of geographic names in the federal government. We use named populated places, which represent "named communities with a permanent human population" (USGS, 2014). These communities range from rural clustered buildings to metropolitan areas and include housing subdivisions, trailer parks, and neighborhoods. These names are assigned point coordinates by the USGS. with no defined boundaries. In our database of consistent-boundary neighborhoods of U.S. cities, 8,983 neighborhoods (out of 60,758) have no named populated place. This could be because a neighborhood has no name or (more likely) the neighborhood's name is also associated with a nearby census tract that happens to have been assigned the point coordinate by the USGS. The remaining tracts have one or more associated place names.

Note that the populated place names database excludes names of natural features, and it includes both incorporated and unincorporated place names.

B.6 Neighborhood percentile ranks

Figure B3 shows the evolution of several New York neighborhoods over our sample period. Recall that each neighborhood corresponds to data normalized to 2010 census tract boundaries. The solid lines show the relative rankings of three neighborhoods—tracts corresponding to the Upper East Side, East Harlem, and Tribeca. (Levittown was unpopulated in 1880 so a corresponding solid line does not appear in the figure.) An interesting feature of this graph is variation in income dynamics across neighborhoods. For example, the Upper East Side has remained a high-income neighborhood throughout our sample period. East Harlem, which was a relatively high-income neighborhood in 1880, experienced decline and has been a low-income neighborhood since 1910. Tribeca saw a large increase in average household income in the 1980s.

[Figure B3 about here.]

The dotted lines show the relative rankings of these three neighborhoods after 1960 and the relative ranking of a fourth neighborhood, Levittown, which first appeared in that census year. In comparing the solid with the dotted lines, note that we have changed the universe used to compute neighborhood ranks from 1880 to 1960 neighborhoods, but the dynamic patterns for the extant three neighborhoods remain qualitatively similar.

In our sample, most neighborhoods experience changes in percentile rank that are close to zero—that is, neighborhood income ranks are largely persistent over time, especially over the 10-year changes that are predominant in our sample. Few neighborhoods experience dramatic increases or declines in rank. The distribution of percentile rank changes has a mean zero and standard deviation of 0.164.

B.7 Mean reversion

We begin by noting the overall relationship between neighborhood change and initial income. In short, neighborhood status tends to mean revert. Figure B4 shows local polynomial smoothing of sample 10-year changes in neighborhood income percentile rank, $\Delta r_{i,t}$, versus initial ranks $r_{i,t}$. (The right axis smooths log changes in average neighborhood income, net of the metropolitan area–year mean.) Neighborhoods that are initially highly ranked tend to decline, and low-ranked neighborhoods tend to improve.

[Figure B4 about here.]

Several features of Figure B4 are noteworthy. First, despite using a nonlinear technique, the pattern of mean reversion is close to linear in initial rank, especially in the middle of the income distribution. In our anchoring regressions, we condition linearly on initial rank. (In robustness checks, we allow differences by decile in initial rank.)

Second, by construction, the sum of changes in income percentile ranks is zero, because one neighborhood's improvements in rank are offset by other neighborhoods' declines. An important implication is that changes for a bottom-ranked neighborhood are restricted to the interval $(0, 1)$, and changes for a top-ranked neighborhood are restricted to $(-1, 0)$. Despite this, mean reversion appears to be driven not by neighborhoods at the top and bottom of the income distribution (as would be expected if mean reversion were purely mechanical) but by neighborhoods near the middle of the income distribution (whose changes in rank are less restricted). The fact that the negative correlation between changes in income and initial income is weaker for neighborhoods with initial rank below the 20th percentile and above the 80th percentile suggests that mechanical effects contribute little to the overall pattern.[41]

Finally, there is mean reversion even in nominal incomes, showing that this pattern is not exclusively driven by our use of percentile ranks (Figure B4, right axis). The expected 10-year sample change in average household income for a neighborhood at the bottom of the income distribution, relative to the average neighborhood in the metro–year, is an increase of about 2%. In contrast, the expected relative change for a top neighborhood is about a 6% decline.

B.8 Robustness to proximity thresholds

In Figure B5, we show that the effect of proximity to an ocean or a Great Lake is consistent for varying definitions of proximity. The gray line connects estimates from 100 separate regressions of neighborhood change on proximity, varying the proximity indicator. (As we move to the right along the horizontal axis, our indicator variable classifies more neighborhoods as being "close" to the natural amenity and fewer neighborhoods as being "far" from the natural amenity.) The black line displays regression estimates when we use only natural features near top-income neighborhoods, as in Table 2, column 6 in the main text. (The intersection of these lines with the vertical dotted line are the estimates from our baseline estimates using a 500-meter definition, shown in Table 2.) Recall that we expect these features to more likely be positive, versus negative, amenities. As expected, the results using this variable are always stronger than the results using oceans and Great Lakes unconditioned on initial income.

[Figure B5 about here.]

Figure B5 also shows the same results for lakes, rivers, and hills. These results show consistent patterns. The important feature of this figure is that it shows that conditioning rivers on their proximity to high-income neighborhoods improves the estimated effect of (positive-amenity) rivers on neighborhood change. This is consistent with the view that, on average, rivers in our sample are a disamenity for households.

[41] Weak mean reversion in the tails of the initial income distribution, despite mechanical censoring effects, may lend further credence to the role of natural amenities: The existence of very strong amenities and disamenities may lead to neighborhoods that are persistently very poor or very rich. See Section 5 in the main text.

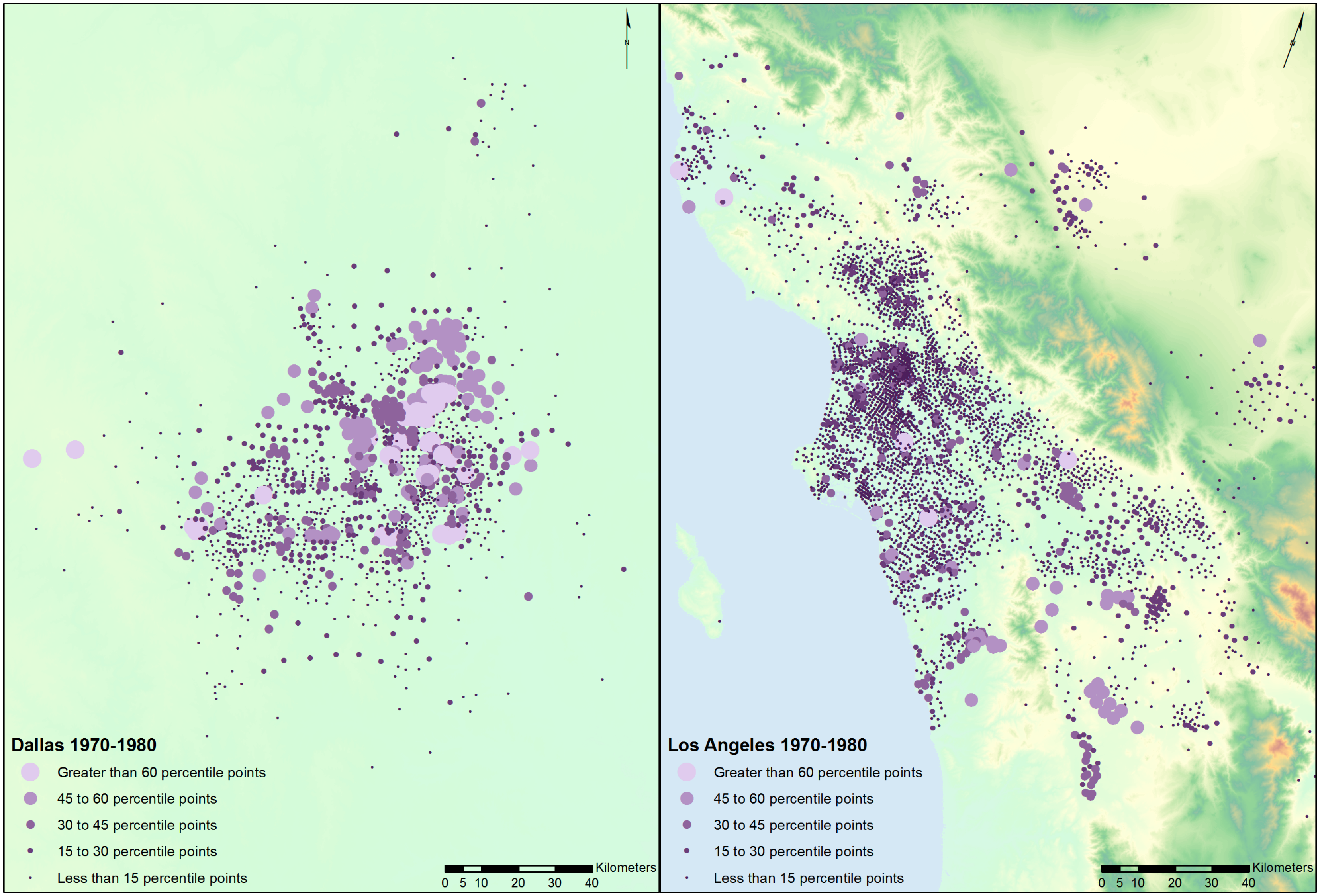


Figure B1: Churning and persistence in the spatial distribution of income: Dallas versus Los Angeles

These maps show 1970 neighborhoods in the Dallas and Los Angeles metropolitan areas as dots. Dots are sized and colored according to the absolute value of change in each neighborhood's percentile ranking within the city by average household income from 1970 to 1980. The average absolute change for neighborhoods in Dallas was 21 percentile points; for neighborhoods in Los Angeles, it was 9 percentile points.

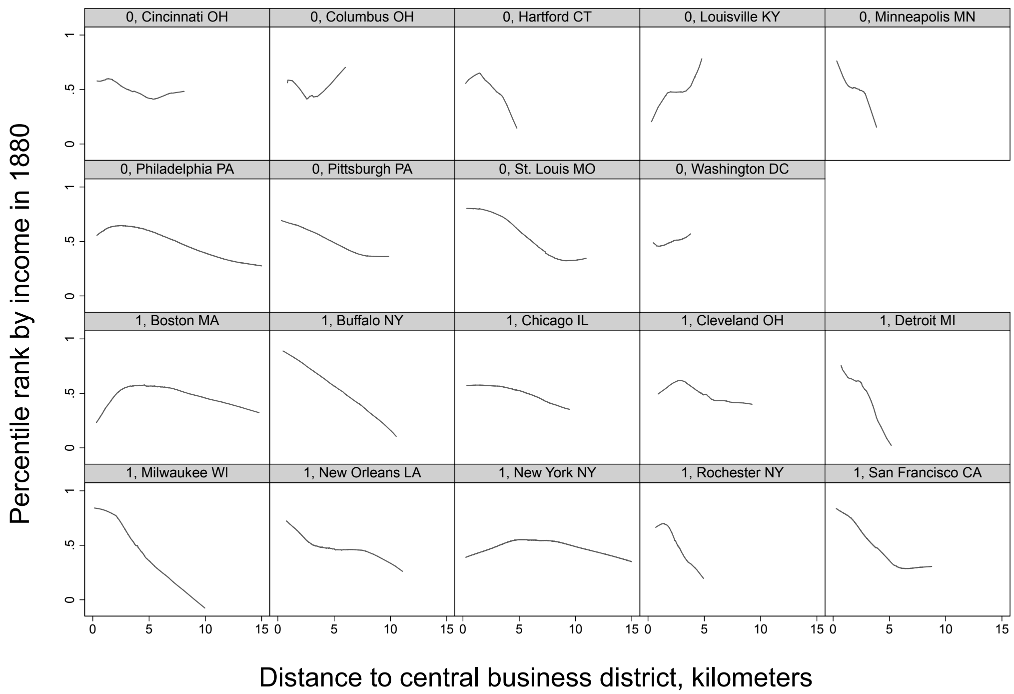


Figure B2: Income and residential location for large cities, 1880

This figure shows the pattern of neighborhood average household income on the vertical axis versus neighborhood distance to the city center (up to 15km) on the horizontal axis for the 19 largest cities in our 1880 sample. Ten metropolitan areas with 20 or fewer neighborhoods in 1880 are not shown. Cities are organized by coastal status (0=interior, 1=coastal) and then alphabetically. The plotted lines are results from lowess smoothing with bandwidth 0.9.

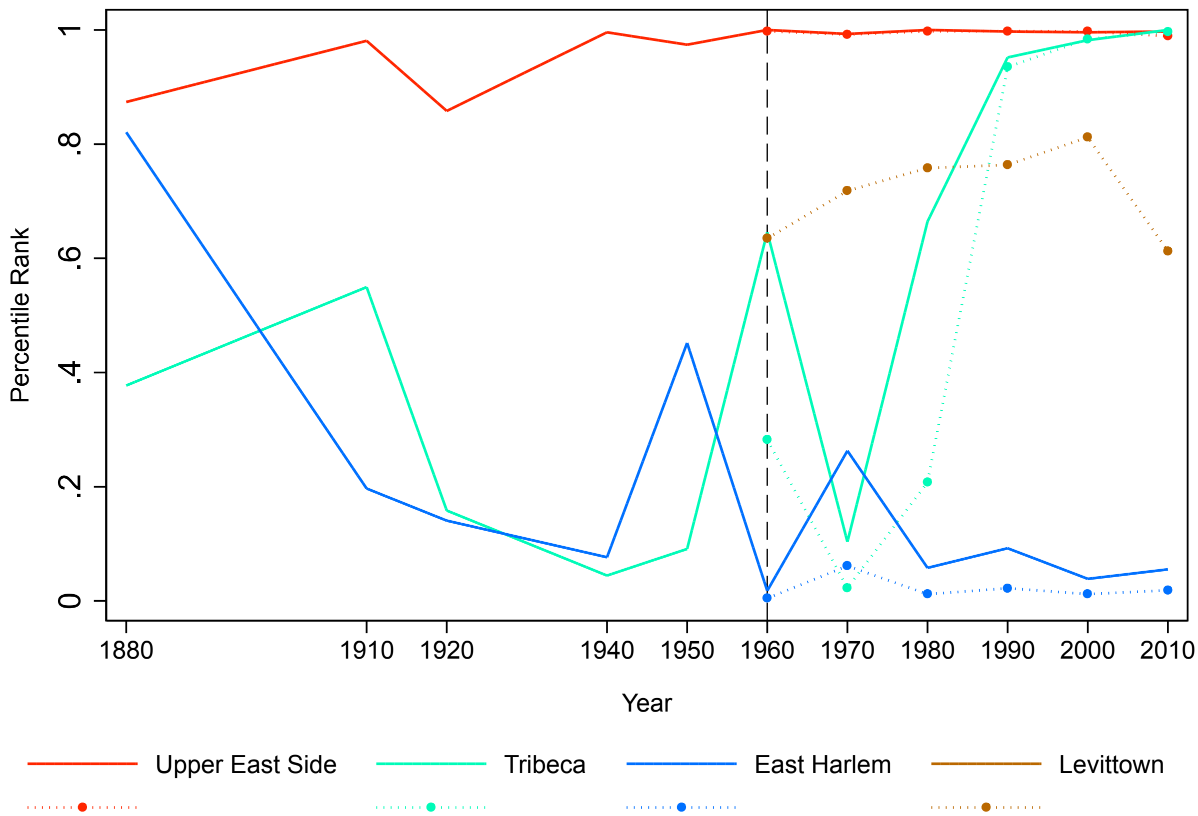


Figure B3: Selected New York neighborhood rankings over time

Solid lines connect neighborhood percentile ranks among 1880 neighborhoods. Dotted lines connect neighborhood percentile ranks among 1960 neighborhoods.

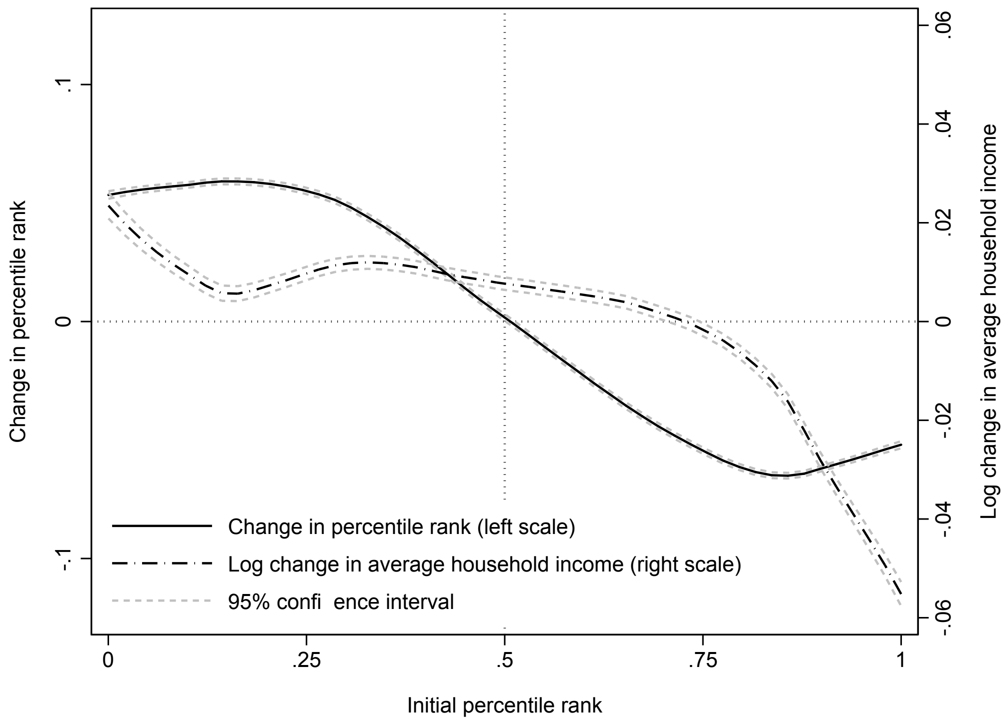


Figure B4: Mean reversion in neighborhood percentile rank by income

Kernel-weighted local polynomial smoothing using Stata's lpoly function with Epanechnikov kernel, rule-of-thumb bandwidth, and local-mean smoothing. Neighborhood log change in average household income (right scale) is normalized by metropolitan area–year mean.

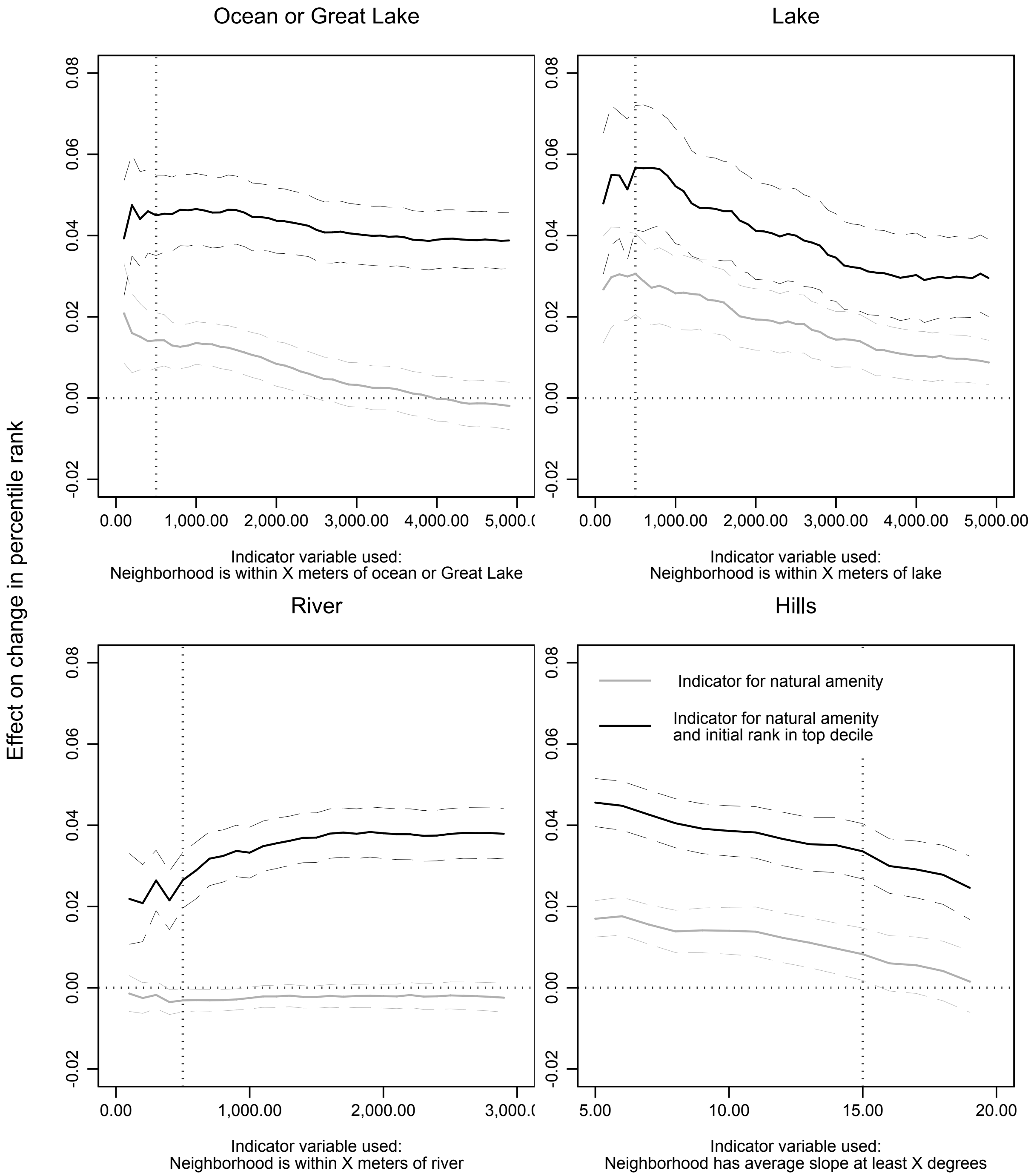


Figure B5: Robustness to indicator variable thresholds

These graphs show the conditional effect of natural features on neighborhood change for varying indicator definitions of proximity to natural features. Each connected point is from a separate regression. The gray solid line connects estimates of the effect of natural amenities, varying the definition of the natural amenity on the horizontal axis, as in Table 3, Panel B. The black solid line connects estimates of the effect of natural amenities conditioned on initial income $r_{i,t} > 0.9$, varying the definition of natural amenity on the horizontal axis, as in Table 3, Panel C. Dashed lines show 95% confidence intervals. Vertical dotted line shows baseline definition used in Table 3.

Table B1: Summary statistics

	μ	(σ)
A. Consistent-boundary neighborhoods		
Δr, 10-year forward change in percentile rank, 1910–2000	0.00	(0.16)
$\sigma(r_{i[m]}) \times 100$, Std. dev. in 1960–2010 percentile rank	12.9	(7.9)
Population, 2010	4,283	(1,912)
Land area (km^2)	27.5	(73.5)
Persons per square km, 1880	5,940	(12,406)
Persons per square km, 1960	2,901	(6,159)
Persons per square km, 2010	2,335	(4,807)
Distance from centroid to nearest seaport (km), 2010	163	(240)
Distance from centroid to city center (km), 2010	29.9	(27.2)
Mean age of housing units (years), 2010	37.3	(14.1)
B. Share of 2010 neighborhoods		
... with centroid within 500m of ocean or Great Lake	0.047	
... with centroid within 500m of lake (ex. Great Lakes)	0.007	
... with centroid within 500m of major river	0.098	
... with average slope greater than 15 degrees	0.069	
... with moderate temperatures*	0.091	
... ... and less than 800mm average annual precipitation	0.063	
... with less than 1% average annual flood risk†	0.630	
C. Metropolitan areas		
$\overline{\sigma(r_m)} \times 100$, Mean std. dev. in 1960–2010 percentile rank	13.1	(2.8)

*–Average January minimum temperatures between 0 and 18 degrees Celsius and average July maximum temperatures between 10 and 30 degrees Celsius. †–Flood information available for 49,517 neighborhoods.

Table B2: Coastal and interior cities

Coastal	Interior
Boston, MA	Albany, NY
Buffalo, NY	Atlanta, GA
Charleston, SC	Cincinnati, OH
Chicago, IL	Columbus, OH
Cleveland, OH	Hartford, CT
Detroit, MI	Indianapolis, IN
Milwaukee, WI	Kansas City, MO
Mobile, AL	Louisville, KY
New Orleans, LA	Memphis, TN
New York, NY	Minneapolis, MN
Rochester, NY	Nashville, TN
San Francisco, CA	Omaha, NE
	Philadelphia, PA
	Richmond, VA
	Pittsburgh, PA
	St. Louis, MO
	Washington, DC

These are the principal cities for consolidated metropolitan areas shown in Figure 6 in the main text.

C Monte Carlo simulations

We analyze the biases in estimates reported in Section 4 of the anchoring effect of natural amenities, using Monte Carlo simulations. There are two identification issues. First, we observe only whether a neighborhood is near natural features (e.g., rivers, oceans), but we do not know whether these natural features are truly amenable. For example, a polluted river can be disamenity. This is a type of measurement error. Second, our anchoring regression includes a lagged dependent variable and may include unobserved neighborhood factors, as in equation (9). Unfortunately, we cannot employ the time-differenced approaches proposed by Arellano and Bond (1991) or Caselli, Esquivel, and Lefort (1996) because differencing would eliminate our time-invariant variable of interest. Therefore, in this Appendix, we sign these biases with Monte Carlo simulations. Our simulations show that the regression estimates reported in the paper are lower bounds for the true anchoring effect of natural amenities.

Section C.1 describes a data-generating process (DGP) for our simulated data. Section C.2 examines the role of measurement error. Section C.3 examines the role of unobserved neighborhood factors. Section C.4 examines both issues together.

C.1 Data-generating process

For exposition, we use a set of parameters chosen to roughly match data. (Our results are robust to a wide range of other parameter values.[42]) We assume 50,000 neighborhoods, of which 10% are "beaches" ($\mathbf{1}^*(a_i) = 1$). Since not all coastal neighborhoods are amenable, we randomly assign only a half of these beach neighborhoods to have positive natural amenity value ($\mathbf{1}(a_i) = 1$).

We generate $r_{i,t}$, neighborhood incomes over time, using equation (9) (repeated here for convenience):

$$\Delta r_{i,t} = \beta_0 + \beta_1 \mathbf{1}(a_i) + \beta_2 r_{i,t} + u_i + \epsilon_{i,t} \tag{9}$$

where we use $\beta_0 = 0$, $\beta_1 = .1$, $\beta_2 = -.1$ and $\epsilon_{i,t} \sim$ i.i.d. $\mathcal{N}(0, .05^2)$. Recall that β_1 is true natural amenity effect. Proposition 4 predicts $\beta_1 > 0$.

The u_i are unobservable neighborhood characteristics, where $u_i \sim$ i.i.d. $\mathcal{N}(0, \sigma_u^2)$. The standard deviation of u_i, σ_u, captures the importance of unobservable characteristics in determining neighborhood income growth $\Delta r_{i,t}$. It turns out that σ_u plays an important role in the bias coming from the lagged endogenous regressors. In Section C.3, we vary σ_u to see the effect of u_i on β_1 estimate.

We generate the data for 20 periods and keep only the last five periods. This is to allow time to arrive at the steady state.[43] Note in equation (9) that we use true natural *amenity* dummy $\mathbf{1}(a_i)$, not natural *feature* dummy $\mathbf{1}^*(a_i)$, in the data-generating process. This is because households in the real world can observe whether a natural feature is an amenity or not.

C.2 Measurement error

This section examines the issue of the measurement error when we estimate equation (9) using natural feature indicator $\mathbf{1}^*(a_i)$ instead of natural amenity $\mathbf{1}(a_i)$. To focus on the measurement error issue, we use the DGP with $\sigma_u = 0$ which, as we show in Section C.3, removes the bias caused by the lagged endogenous regresssor.

Our identification strategy in Table 2, column (5) is to condition observed natural features on historical income. For each set of simulated data, we estimate the following model:

$$\Delta r_{i,t} = \beta_0 + \beta_1 \mathbf{1}^*(a_i) \cdot \mathbf{1}(\text{PRank}(r_{i,t}) \geq \tilde{\theta_H})) + \beta_2 r_{i,t} + \epsilon_{i,t}$$

[42] Replication files are available at the authors' websites. Readers may try other parameter values with this program.

[43] Since we have 50,000 neighborhoods, the simulated data set has 250,000 observations. This is roughly similar to the number of observations used in Table 2 (291,321 to 298,776 across specifications).

where $\mathbf{1}(\text{PRank}(r_{i,t}) \geq \tilde{\theta_H})$ is an indicator variable for a historically high-income neighborhood with a percentile rank of income greater than or equal to $\tilde{\theta_H}$. Note that when we use $\tilde{\theta_H} = 0$ and thus $\mathbf{1}(\text{PRank}(r_{i,t}) \geq \tilde{\theta_H})) = 1$ for all $r \in [0, 1]$, the regression model becomes our base regression in equation (8) of the paper that estimates the natural amenity effect without conditioning on historical income.

We vary the cutoff $\tilde{\theta_H}$ and report the mean β_1 estimates. We also report the mean of $\rho_{\text{AF}}|\text{H}$, the correlation between observed natural amenity $\mathbf{1}(a_i)$ and measured natural features $\mathbf{1}^*(a_i)$ among the top income neighborhoods H whose percentile rank incomes are greater than $\tilde{\theta_H}$. This correlation illustrates how well the indicator for a natural feature measures the true natural amenity. The following table shows the means of $\hat{\beta}_1$ and $\rho_{\text{AF}}|\text{H}$ with their standard errors in parentheses, from 1,000 trials:

$\tilde{\theta_H}$	$\hat{\beta}_1$	$\rho_{\text{AF}}\|\text{H}$
0	0.032 (0.001)	0.698 (0.007)
0.5	0.049 (0.001)	0.809 (0.007)
0.7	0.064 (0.001)	0.872 (0.006)
0.9	0.088 (0.001)	0.951 (0.004)

The results are consistent with our predictions. As the cutoff for historical income $(\hat{\theta_H})$ increases, the estimate $\hat{\beta}_1$ increases toward the true value of 0.1. The correlation $\rho_{\text{AF}}|\text{H}$ also increases with the cutoff, and this suggests that the natural feature is a better indicator for natural amenities after conditioning on historical income.

C.3 Lagged endogenous variables as regressors

We pick σ_u and generate the data as described in Section C.1. With each resulting data set, we estimate the following model by OLS:

$$\Delta r_{i,t} = \beta_0 + \beta_1 \mathbf{1}(a_i) + \beta_2 r_{i,t} + \epsilon_{i,t}.$$

This regression model is the same as the base regression in equation (8), except that it uses true amenity $\mathbf{1}(a_i)$ rather than natural feature $\mathbf{1}^*(a_i)$. This removes the measurement error issue regarding natural amenity and allows us to focus on the roles of the lagged dependent variable and unobserved neighborhood factors.

We vary the standard deviation of u_i, σ_u, from 0 to 0.02, and, for each value, we repeat the entire Monte Carlo exercise 1,000 times and report the mean of the β_1 estimates and its standard error:

σ_u	$\hat{\beta}_1$
0	0.1 (0.001)
0.01	0.069 (0.001)
0.02	0.036 (0.001)

With $\sigma_u = 0$, the β_1 estimate is virtually equal to its true value 0.1. Intuitively, when there are no unobserved time-invariant neighborhood factors, the OLS estimator is consistent even when including the lagged dependent variable. However, as σ_u increases from 0, the estimated $\hat{\beta}_1$ decreases away from the true value.

C.4 Two issues combined

Now we combine the two effects together. The following table shows mean estimates $\hat{\beta}_1$ and standard errors from 1,000 trials when both σ_{u_i} and $\tilde{\theta_H}$ vary.

	$\hat{\beta}_1$		
	$\sigma_{u_i} = 0.00$	0.01	0.02
$\tilde{\theta_H} = 0.5$	0.049 (0.001)	0.037 (0.001)	0.021 (0.001)
0.7	0.064 (0.001)	0.047 (0.001)	0.026 (0.001)
0.9	0.087 (0.001)	0.062 (0.001)	0.033 (0.001)

The two patterns we observed in the previous sections are robust. Either increasing σ_{u_i} with $\hat{\theta_H}$ fixed or decreasing $\hat{\theta_H}$ with σ_{u_i} fixed decreases the estimate $\hat{\beta}_1$ away from its true value of 0.1. Moreover, the estimates are always lower than the true effect, and this suggests that the true anchoring effect of natural amenities is bounded from below by estimates from the regressions we report in the paper.