



Are people willing to pay for less segregation? Evidence from U.S. internal migration[☆]



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ABSTRACT

It is difficult to determine whether racial housing segregation is socially desirable, because segregation has some effects that are hard to measure. To overcome this challenge, we estimate a migration choice model to measure the willingness to pay for reduced segregation. The key idea underlying our empirical approach is that if segregation is undesirable, migrants should be willing to give up some earnings to avoid living in segregated cities. Using decennial census data from 1980 to 2000, we provide evidence that segregation is an urban disamenity. It is shown that both black and white migrants prefer to live in less segregated cities. For example, for a one percentage point reduction in the dissimilarity index, the estimated marginal willingness to pay of blacks is \$436 (in 1999 dollars) in 2000. Among whites, this marginal willingness to pay is \$301.

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1. Introduction

Racial residential segregation is a salient feature of urban America. With the massive migration of blacks from the rural South to the urban North, racial segregation in American cities started in the early twentieth century, expanded substantially after the Second World War, and peaked in the 1970s (Cutler et al., 1999). Although segregation has declined since the 1970s, steps toward widespread integration have been modest (Logan et al., 2004). By 2000, 50% of blacks would have to be relocated in order for whites and blacks to be evenly distributed across neighborhoods in the average U.S. city.²

It has long been argued that the persistence of segregation is the root cause of the “black underclass” in American cities (Wilson, 1987; Massey and Denton, 1993). Numerous empirical studies find that segregation has adverse effects on a variety of social and labor

market outcomes for blacks.³ The standard explanation for such negative effects is that living in highly segregated areas spatially separates blacks from job opportunities, reduces their access to high quality local public goods, and diminishes many of the positive spillovers from skilled whites via neighborhood effects (Kain, 1968; Wilson, 1987; Borjas, 1995).

Existing studies of the effects of segregation have two features in common. First, each study focuses on one or few outcomes. For this reason, despite the negative effects of segregation documented in the literature, one may still hesitate to conclude that segregation is socially undesirable. There is always the possibility that some benefits of segregation, such as facilitation of the supply of ethnic goods and services, are missed by these studies because such benefits are difficult to measure. Second, the existing literature focuses primarily on the social and economic effects of segregation and pays much less attention to its

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² In this paper, the term city refers to a metropolitan statistical area (MSA).

³ Many studies focus on the effects of residential segregation on employment outcomes of blacks (e.g., Kain, 1968; Ellwood, 1986; Kasarda, 1989; Ihlanfeldt and Sjoquist, 1990; and Weinberg, 2000). Some others examine its effects on black educational achievement (e.g., Card and Rothstein, 2007), crime rates (e.g., Shihadeh and Flynn, 1996; Krivo et al., 2009), health status (e.g., Williams and Collins, 2001), and poverty (e.g., Ananat, 2011). Cutler and Glaeser (1997) is a more comprehensive study that estimates the effects of segregation on the outcomes of blacks along several dimensions, including educational achievement, income, employment, and the probability of becoming a single mother.

psychic and cultural effects. Given that segregation has been famously referred to as the American Dilemma and the American Apartheid, it is entirely possible that people dislike it simply because segregation contradicts their ideals of a free society. That is, even if segregation has no direct negative effects on individual outcomes, people may prefer to avoid it due to a sense of distaste toward such a social arrangement. Therefore, any comprehensive welfare analysis must also take this kind of distaste into account.

This paper contributes to the literature by estimating blacks and whites' willingness to pay for segregation, which captures the overall effects of segregation on individual outcomes as well as people's attitudes toward segregation. The logic behind our empirical analysis is simple: In the context of internal migration, individuals choose the city where they derive the highest utilities. Cities are characterized by varying income opportunities and segregation levels. All else equal, migrants face a trade-off between the level of segregation and expected income when they choose a city to live in. If migrants are willing to give up some earnings in order to live in less segregated cities, then segregation must be undesirable; otherwise, if the opposite is true, segregation must be a preferred urban characteristic.

Our empirical approach follows Bayer et al. (2009), who estimate a discrete choice model of internal migration decisions to measure the value of air quality in U.S. cities. Despite the narrow focus of their study, Bayer et al. (2009) provide a general method for quantifying revealed preferences for urban amenities and disamenities at the city level. We apply this method to study migrants' willingness to pay to live in cities with different degrees of segregation. Our innovation is to treat residential segregation as an urban (dis)amenity, which represents a significant deviation from the existing literature on the effects of segregation.

Two related studies, Bayer et al. (2007) and Bajari and Kahn (2005), also estimate discrete-choice models to measure preferences toward neighborhood racial composition in U.S. cities. However, both papers examine within-city residential choices. Using data from the San Francisco Bay Area, Bayer et al. (2007) find that blacks are willing to pay for an increased share of black population in a census block group but whites are not, consistent with the notion that people prefer segregation at the block-group level.⁴ In contrast, using data from Atlanta, Chicago, and Dallas, Bajari and Kahn (2005) find that at a much larger community level (Public Use Microdata Area, or PUMA), whites are willing to pay for integration and blacks prefer whiter communities, suggesting that people want to avoid segregation at the PUMA level.⁵ The discrepancy between these findings raises the question whether segregation at higher geographic levels is undesirable. Our study helps answer this question by examining revealed preferences for segregation at the city level based on cross-city residential choices.

To provide a structural framework for empirical analysis, we present a model of migration destination choices in which the degree of segregation directly enters individual utility. We follow a standard two-step procedure to estimate the model: First, a discrete choice model is used to recover a vector of city-specific utilities that are common to all individuals living in these cities. Then, we regress city-specific utilities on a city-level segregation index, along with other city characteristics, to measure the marginal willingness to pay (MWTP) for segregation. A naive OLS estimate of willingness to pay is likely to be biased if segregated cities have unobserved characteristics that affect utility. To address this

⁴ In an earlier working paper, Bayer et al. (2004) report similar segregating preferences at the block-group level in the San Francisco Bay Area. Following a similar approach to Bayer et al. (2007), Wong (2013) finds that in Singapore all ethnic groups prefer to live with some own-ethnic-group neighbors, but only up to certain levels. That is, people would like to have some, but not complete, segregation.

⁵ A typical PUMA has approximately 100,000 residents. Thus a large metropolitan area consists of many PUMAs. Bajari and Kahn (2005) examine residential choices within a single city (separately for three different cities). Two community characteristics computed at the PUMA-level, share of blacks and share of college graduates, are assumed to affect residential choices.

issue, we instrument for city level segregation. We use two sets of instruments, both drawn from the existing literature: the structure of government finance (Cutler and Glaeser, 1997) and the number of inter- and intra-county rivers (Hoxby, 2000; Rothstein, 2007).

Using decennial census data from 1980 to 2000, we find that utility from segregation is always negative and statistically significant for young blacks in 1990 and 2000 and for young whites in all three census years. The magnitude of disutility is large and appears to vary across races and over time. For a one percentage point reduction in the dissimilarity index, our preferred estimates imply that the MWTP among blacks increases from \$89 to \$436 between 1980 and 2000 (in 1999 dollars). For whites, however, the MWTP decreases from \$675 to \$301 during this same period. In some samples, MWTP falls with age and rises with the presence of children. Educational attainment has no significant effect on MWTP in all samples. Overall, our estimates suggest that segregated cities are undesirable to both young blacks and young whites. To the best of our knowledge, these findings represent the first estimates of people's willingness to pay to avoid racially segregated cities.

The remainder of the paper is organized as follows: Section 2 gives a brief overview of why people may care about racial segregation. Section 3 presents a simple migration choice model for empirical analysis. Section 4 describes the data and identification strategies. Section 5 presents empirical results. And finally, Section 6 concludes this paper.

2. Why people care about segregation

We start with a summary of four theories on why people may care about segregation at the city level and briefly discuss the implication of each theory for people's willingness to pay for segregation.

2.1. Segregation and neighborhood effects

Under housing segregation, people from the same racial group tend to live in the same neighborhood. To the extent that race is correlated with other socio-economic characteristics, segregation produces polarized communities that subject urban residents to various neighborhood effects. Scholars have long emphasized that segregation leads to concentrated poverty in black communities, which tends to have all kinds of negative effects on children as well as adults. For example, Cutler and Glaeser (1997) find that blacks in more segregated cities fare worse than blacks in less segregated cities along many dimensions. As segregation increases, blacks tend to have lower high school graduation rates, lower earnings, higher probability of being out of school and not working, and higher probability of becoming single mothers. They also find that segregation improves outcomes for whites, although such results are weaker. Ananat (2011) finds that segregation leads to higher black poverty and inequality and lower white poverty and inequality. Card and Rothstein (2007) find that the black-white test score gap is higher in more segregated cities. These findings are interpreted, at least partially, as the result of neighborhood effects. Because of segregation, blacks tend to live and go to school with other blacks of similar socio-economic status. They tend to interact with peers of lower income and education, which negatively affect their own social and economic outcomes due to lower expectations, lack of role models, and a self-perpetuating culture of poverty (Wilson, 1987; Massey and Denton, 1993). Given these negative neighborhood effects on blacks, it is expected that blacks are willing to pay to avoid segregated cities. In contrast, whites benefit somewhat from positive neighborhood effects under segregation and thus should be willing to pay for increased segregation.

2.2. Segregation and the spatial mismatch

It was first pointed out by Kain (1968) that segregation may lead to a spatial mismatch between workers and jobs. For example, if blacks are predominantly living in central cities with industries located in suburbs, then it will be more difficult for blacks to find or maintain jobs in those

industries. Extensive literature on this topic provides evidence that a spatial mismatch negatively affects labor market outcomes for blacks.⁶ In principle, a spatial mismatch could hurt any group that lives far away from job opportunities. In practice, the spatial mismatch literature has almost exclusively focused on the disadvantageous position of blacks. It is unclear whether segregation also creates a spatial mismatch between white residents and jobs. Thus in more segregated cities, blacks are expected to have a higher unemployment rate and/or a longer commute to work, which should reduce their willingness to pay to live in the city. Predicted effects on whites are ambiguous.

2.3. Segregation and the “consumer city”

As Glaeser et al. (2001) famously emphasize, urban density plays an important role in facilitating consumption. People of the same racial background tend to have similar preferences for consumption goods, leisure activities, and cultural events. Segregation allows people with similar preferences to live in proximity to one another. Consequently, the provision of both private goods and local public goods can be better tailored to more homogeneous groups of residents, which can improve welfare through a better match between supplied goods and consumer tastes. For example, in urban communities like Japantown and Little Italy, ethnic restaurants and stores not only benefit ethnic groups themselves but also other groups in the city. For newly arrived migrants, ethnic enclaves are also valuable as a port of entry that eases their transition into a new environment (Cutler et al., 1999). Thus a segregated city may provide a larger variety of highly specialized consumer goods and therefore could be more attractive to local residents and migrants.

2.4. Segregation and the changed attitude toward race

Historically, racial prejudice and racism were pervasive in American society (Myrdal, 1944). When institutionalized segregation and racial discrimination were widely accepted by whites, many whites were likely willing to pay to maintain segregation while most blacks were willing to pay to avoid it. Extensive evidence shows that racial attitudes have become more tolerant over time (Schuman et al., 1997; Charles, 2003). As is well known, segregational residential patterns are consistent with a wide variety of racial preferences, including pro-integrationist preferences (Schelling, 1971; Zhang, 2011). Thus there is no reason to believe that segregation in today’s urban America reflects preferences at the individual level. Many whites may view segregation as an unfortunate holdover from the past that hampers social progress, and may believe that racial integration is a good thing based on the principles of equality and justice. Furthermore, many blacks may view segregation as a sign of long-lasting racial discrimination. Both groups may consider segregation itself (rather than its effects) as an undesirable social arrangement and are willing to pay to avoid living with it.

Although we discussed these theories one by one, it is important to note that they are not mutually exclusive. For blacks, theories 1, 2, and 4 imply a willingness to pay for less segregation whereas theory 3 implies a willingness to pay for more segregation. For whites, theories 1 and 3 imply a willingness to pay for more segregation, theory 4 implies a willingness to pay for less segregation, and theory 2 has ambiguous predictions. Therefore, the overall willingness to pay for more or less segregation, whether by blacks or whites, is an empirical question.

3. A migration choice model

We next present a model of individual location choices among a set of J cities. In any city j , an individual i chooses the quantity of numeraire

good C_{ij} and housing H_{ij} to maximize a city-specific Cobb–Douglas utility subject to a budget constraint:

$$\begin{aligned} \text{Max } U_{ij} &= C_{ij}^{\beta_c} H_{ij}^{\beta_h} S_j^{\beta_s} X_j^{\beta_x} e^{M_{ij} + \xi_j + \varepsilon_{ij}} \\ \text{s.t. } C_{ij} + p_j H_{ij} &= W_{ij}, \end{aligned} \tag{1}$$

where S_j is the level of segregation in city j ; X_j and ξ_j are other observed and unobserved city characteristics in city j , respectively; M_{ij} captures the psychic costs of moving from birth place to city j ; and ε_{ij} represents the idiosyncratic component of utility, which is independent of city characteristics and migration costs. W_{ij} is individual i 's wage in city j ; p_j is the price of housing in city j ; and the price of C_{ij} is normalized to 1. Note that except for segregation, we assume homogeneous preferences and therefore β_c , β_h , and β_x are not indexed by i . We allow the preference for segregation to vary across individuals, because it would be informative to know whether segregation has any differential effects on different groups of people. It is also worth noting that in our empirical analysis below, we will estimate the model separately for blacks and whites in different census years. That is, we allow all taste parameters to vary by race and cohort. To keep notation clean, we suppress race and year indexes.

The utility maximization problem yields the following demand functions:

$$C_{ij}^* = \frac{\beta_c}{\beta_c + \beta_h} W_{ij} \quad \text{and} \quad H_{ij}^* = \frac{\beta_h}{\beta_c + \beta_h} \frac{W_{ij}}{p_j}. \tag{2}$$

After plugging C_{ij}^* and H_{ij}^* into the utility function, letting $\beta_w = \beta_c + \beta_h$, and rescaling utility by $\left(\frac{\beta_c}{\beta_c + \beta_h}\right)^{-\beta_c} \left(\frac{\beta_h}{\beta_c + \beta_h}\right)^{-\beta_h}$, we obtain the indirect utility function

$$V_{ij} = W_{ij}^{\beta_w} p_j^{-\beta_h} S_j^{\beta_s} X_j^{\beta_x} e^{M_{ij} + \xi_j + \varepsilon_{ij}}. \tag{3}$$

For empirical analysis, the psychic migration cost M_{ij} is specified as

$$M_{ij} = m_1 d_{1ij} + m_2 d_{2ij} + m_3 d_{3ij}, \tag{4}$$

where d_{1ij} is 1 if city j is outside individual i 's birth state and 0 otherwise; d_{2ij} is 1 if city j is outside individual i 's birth division and 0 otherwise; d_{3ij} is 1 if city j is outside individual i 's birth region and 0 otherwise.⁷

Since we can only observe each person's actual wage in his chosen city, we need to estimate how much they would earn in the cities they do not choose. Following Bayer et al. (2009) and Timmins (2007), we construct the following decomposition:

$$\ln W_{ij} = \ln \hat{W}_{ij} + v_{ij}, \tag{5}$$

where $\ln W_{ij}$ is the logarithm of the actual wage that individual i would earn in city j ; $\ln \hat{W}_{ij}$ is the logarithm of individual i 's estimated wage in city j ; and v_{ij} is an idiosyncratic error term. We will describe the method of obtaining $\ln \hat{W}_{ij}$ at length in the data section.

To allow for heterogeneous preferences for segregation, we assume that β_{is} is a function of observed individual characteristics:

$$\beta_{is} = \beta_s + \sum_c \beta_{sc} Z_{ic}, \tag{6}$$

where Z_{ic} is individual i 's observed characteristic c (e.g., educational attainment) and β_{sc} represents heterogeneous preferences for segregation that vary with Z_{ic} . Each Z_{ic} is demeaned (i.e., subtracted by its sample average) so that β_s represents the mean preference for segregation.

⁶ See Kain (1992) and Ihlanfeldt and Sjoquist (1998) for two comprehensive reviews of the literature.

⁷ There are nine U.S. census divisions (New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific) and four U.S. census regions (Northeast, Midwest, South, and West).

Substituting Eqs. (4)–(6) into (3) and taking natural logs yield

$$\ln V_{ij} = \Theta_j + \lambda_{ij} + \eta_{ij}, \quad (7)$$

where

$$\Theta_j = \beta_s \ln S_j + \beta_x \ln X_j - \beta_h \ln p_j + \xi_j, \quad (8)$$

$$\lambda_{ij} = \beta_w \ln \hat{W}_{ij} + m_1 d_{1ij} + m_2 d_{2ij} + m_3 d_{3ij} + \sum_c \beta_{sc} Z_{ic} \ln S_j, \quad (9)$$

and

$$\eta_{ij} = \beta_w v_{ij} + \varepsilon_{ij}. \quad (10)$$

In Eq. (7), Θ_j represents a city-specific constant that extracts the portion of utility provided by city j that is common to all individuals. From this point on, we refer to it as city-specific mean utilities. λ_{ij} captures the portion of utility provided by city j that varies by individual wage, birth place, and observed individual characteristics. η_{ij} is the error term that combines individual i 's idiosyncratic preferences for city j .

To estimate the model parameters of interest, we follow a two-step estimation procedure developed by Berry (1994) and Berry et al. (1995). With the assumption that η_{ij} is an independently and identically distributed type I extreme value, the probability that individual i chooses city j has the following standard logit form:

$$P_{ij}(\ln V_{ij} \geq \ln V_{ik} \forall k \neq j) = \frac{\exp(\Theta_j + \lambda_{ij})}{\sum_{s=1}^J \exp(\Theta_s + \lambda_{is})}.$$

Given the assumption of independent individual decisions, the probability that each person in the sample makes the actually observed choice is

$$L = \prod_i \prod_{j=1}^J P_{ij}^{\kappa_{ij}},$$

where κ_{ij} is 1 if city j is chosen by individual i and 0 otherwise. The first step of our estimation procedure is to maximize L over a vector of city-specific constants (Θ_j) and parameters in λ_{ij} .

Since there is a large number of cities, we use a contraction mapping algorithm developed by Berry (1994) and Berry et al. (1995) to facilitate the estimation of city-specific constants (Θ_j). Let s_j be the share of migrants in the sample who actually choose city j , and $\hat{s}_j(\Theta_j) = \frac{1}{N} \sum_{i=1}^N P_{ij}$ be the predicted share of individuals who choose city j , where N is the total number of individuals. The maximum likelihood estimation with contraction mapping proceeds as follows: (1) Starting with any trial value of parameters in λ_{ij} , city-specific constants are obtained by iteratively adjusting the formula $\Theta_j^{t+1} = \Theta_j^t + \ln s_j - \ln \hat{s}_j(\Theta_j^t)$, where t indexes the iterations.⁸ (2) Plug the estimated city-specific constants into the likelihood function and use a gradient-based method to generate new estimated parameters in λ_{ij} , which will be used as the new trial value. (3) Repeat (1) and (2) until the likelihood function is maximized.

Once the estimates of Θ_j are obtained, the second step of the procedure is to estimate the mean preference for segregation (β_s) via a simple linear Eq. (8). However, a naive OLS estimate of β_s may be biased due to potential endogeneity issues. First, it is very likely that any improvement in unobserved city amenities in ξ_j will lead to an increase in housing price (p_j). That is, $\ln p_j$ is correlated with ξ_j . This endogeneity of housing price will generally bias the estimate of β_s ,

even if $\ln S_j$ itself is exogenous.⁹ To cope with this issue, following Bayer et al. (2009), we rewrite Eq. (8) by moving $\beta_h \ln p_j$ to the left-hand side to obtain

$$\Theta_j + \beta_h \ln p_j = \beta_s \ln S_j + \beta_x \ln X_j + \xi_j, \quad (8')$$

where the new dependent variable ($\Theta_j + \beta_h \ln p_j$) can be thought of as housing-price-adjusted city-specific mean utilities. Notice that if the true value of β_h is known, then the endogeneity of $\ln p_j$ would not cause any problem for the estimation of β_s after this rearrangement. Of course, β_h is not known, but theory suggests an alternative way to estimate it. It follows from Eq. (2) that $\beta_h = \beta_w \frac{H_{ij} p_j}{W_{ij}}$, where β_w is the marginal utility of wage that can be obtained from the first step of the estimation procedure; $\frac{H_{ij} p_j}{W_{ij}}$ is the share of income spent on housing that can be calculated from data. Given that we have to rely on this estimated β_h to construct the left-hand side variable in Eq. (8'), we will explore whether our main results are sensitive to different values of β_h .¹⁰

The second concern is that more racially segregated cities may have unobserved city amenities and disamenities in ξ_j that affect individual utility. That is, $\ln S_j$ itself may be endogenous. For example, cities with ghettos may have better ethnic restaurants and a unique culture; such cities may also suffer from a declining industrial base and be ill-equipped to provide a variety of local public goods. To address this source of potential bias, we take the standard instrumental variable approach. Specifically, we use two sets of instruments for segregation—both drawn from the existing literature—that exploit variations in segregation caused by fiscal factors and topographic features.

4. Data and identification

This section briefly describes the data we assembled for empirical analysis. Further details are available in the data Appendix A.

4.1. Data on household heads

Following standard practice, we estimate the migration choice model using data on household heads. Thus we assume that household heads are the decision makers when choosing which city to live in. Household data are drawn from the 1980 1%, 1990 1%, and 2000 5% Integrated Public Use Microdata Series or IPUMS (Ruggles et al., 2010). For each household head, the census data provide rich information on demographic and economic variables such as race, age, gender, educational attainment, marital status, household composition, and income. The migration distance dummies are generated using information on an individual's birth state and the metropolitan area where she or he currently lives.

Given two races and three census years, we construct six separate study samples to estimate migration choices. Cities are included in a study sample if they are identifiable in IPUMS,¹¹ are not in Alaska or Hawaii, and have at least 10,000 blacks (for black samples) or 1000 blacks (for white samples). For black samples, we exclude cities with less than 10,000 blacks to make sure that any city in the sample is reasonably well represented in the sample of household heads. For white samples, we exclude cities with less than 1000 blacks so that ghettos

⁹ In general, one endogenous explanatory variable will bias the estimated coefficient of another explanatory variable unless the two are uncorrelated. See Wooldridge (2013, p.92) for this point.

¹⁰ An alternative approach to dealing with the endogeneity of housing price is to find valid instrumental variables for housing price. For example, to instrument for the price of a given house, Bayer et al. (2007) use the exogenous attributes of houses and neighborhoods that are located a certain distance away, Ferreira (2010) uses variation from transaction costs generated by California's Proposition 13 property tax law, and Wong (2013) exploits a unique ethnic housing quota policy implemented in Singapore. In our case, since we have so many cities in our sample, a valid instrument is difficult to come by.

¹¹ IPUMS assigns an identifier to an MSA only if its population is higher than 100,000.

⁸ Berry (1994) shows that, for any given parameters in λ_{ij} , there exists a unique set of Θ_j that equates the predicted with the actual shares. Berry et al. (1995) further prove that the adjustment process is a contraction mapping and will converge to the exact solution.

Table 1
Summary statistics for city-level variables.

Variable	Description	1980		1990		2000	
		Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
ln(Housing price)	ln(Housing price)	11.380	0.282	11.445	0.430	11.557	0.338
Dissimilarity index	Dissimilarity index	0.624	0.122	0.568	0.127	0.502	0.134
Population size	Populations in millions	0.624	1.051	0.659	1.046	0.763	1.206
Black share	The percent of population that is black	0.109	0.094	0.108	0.094	0.114	0.104
Population density	Population per square kilometer of land area	180	359	190	357	185	379
College share	The share of adults aged 25 + with a college or higher degree	0.161	0.055	0.201	0.066	0.239	0.074
Gini	Gini coefficients	0.376	0.021	0.406	0.025	0.435	0.026
Median household income	Median household income in thousands	34.716	5.068	38.213	8.182	40.297	7.672
Manufacturing share	The share of labor employed in manufacturing industry	0.234	0.096	0.179	0.072	0.143	0.066

Notes: This table reports the summary statistics of city characteristics for white samples, which include 258, 281, and 288 cities in 1980, 1990, and 2000, respectively. The numbers of cities in black samples are smaller, but there are no statistically significant differences in the mean city characteristics between black and white samples for each year. All money values are in 1999 U.S. dollars.

Three variables (dissimilarity index, population size, and black share) are from Cutler et al. (1999) and Glaeser and Vigdor (2001). Three variables (college share, median household income, and manufacturing share) are from the 1980 1% IPUMS and the 1990 and 2000 U.S. decennial census summary tape files. Population density is calculated by dividing metropolitan area population by land area, where the land area data is from the 1990 U.S. Gazetteer Files. Housing prices and Gini coefficients are from the 1980 1%, 1990 1%, and 2000 5% IPUMS. See the data Appendix A for details.

and segregation are empirically meaningful in these cities. In total, the number of cities included in black samples is 186 in 1980, 203 in 1990, and 211 in 2000; and in white samples is 258 in 1980, 281 in 1990, and 288 in 2000.

For each race in each census year, we randomly draw 20,000 household heads aged 25–35 who are born in one of the 48 contiguous states plus the District of Columbia, are full-time workers, and live in one of the cities included in the study sample.¹² To better match the scenario of the migration choice model, we focus on the younger age groups because the attributes of residential locations more likely reflect the tastes of these household heads. Also, the location choices of young people are more likely to be driven by current local amenity differences. Using these six samples, we estimate the migration choice model separately for blacks and whites for each of the three census years.

4.2. Measure of residential segregation and city-level controls

There are different measures of residential segregation (Massey and Denton, 1988). In our analysis, we focus on the widely used dissimilarity index. The dissimilarity index is defined as

$$S_j = \frac{1}{2} \sum_{i=1}^N \left| \frac{Blacks_{ij}}{Blacks_j} - \frac{Nonblacks_{ij}}{Nonblacks_j} \right|,$$

where $Blacks_{ij}$ is the number of blacks in neighborhood (i.e., census tract) i in city j ; $Blacks_j$ is the total number of blacks in city j ; $Nonblacks_{ij}$ is the number of nonblacks in neighborhood i in city j ; and $Nonblacks_j$ is the total number of nonblacks in city j .¹³ This index ranges from 0 to 1, with 0 indicating that every neighborhood has an equal share of blacks and nonblacks in the city, and 1 indicating that blacks and nonblacks never live in the same neighborhood.

We obtain the dissimilarity index data from Cutler et al. (1999) and Glaeser and Vigdor (2001); these data are summarized in the second row of Table 1. The mean dissimilarity index declines by approximately six percentage points every decade, from 62% in 1980 to 50% in 2000. The standard deviation of the dissimilarity index is roughly 13% for each year, indicating that segregation varies substantially across cities. For example, the most segregated city in the 1980 sample is Fort Myers–Cape Coral, FL, with a dissimilarity index of 0.88, while the least segregated city is Danville, VA, with a dissimilarity index of 0.31.

¹² Table A1 illustrates the key attributes separately for black and white household heads in each year.

¹³ Although our discussion focuses on blacks and whites only, there are other minority groups living in each city. Here we lump other minority groups with whites and call them nonblacks. An alternative way to construct the index is to drop all other groups and only use the data on blacks and whites, which seems to be uncommon in the literature.

Table 1 also presents the summary statistics of other city characteristics used in the second-step regressions. The first set of controls includes city population, the share of population that is black, population density, and median household income, all of which represent standard controls in the existing literature. Since more educated people may have a higher degree of tolerance toward unlike neighbors, we control for local human capital using the share of people aged 25 or over with a college or higher degree. Another control is the city-level Gini coefficient, which measures the level of income inequality. Since blacks on average are poorer than whites, higher income inequality may reflect a greater economic distance between blacks and whites, which tends to cause higher levels of segregation. We also control for the share of labor employed in manufacturing, since labor shortages in America's older industrial cities spurred the first wave of black migration to urban areas.

4.3. Identification

4.3.1. Predicting individual wages

To predict \hat{W}_{ij} , we run a separate wage regression for each city to estimate city-specific returns to individual characteristics and then substitute each person's characteristics into the estimated city-specific wage equation. It should be noted, however, that simple OLS estimates of returns to individual characteristics may be biased due to nonrandom sorting of individuals across cities. To correct for this selection bias, we follow a semi-parametric estimation method proposed by Dahl (2002) and subsequently used by Bayer et al. (2009).

Consider the following wage equation for city j :

$$\ln W_{ij} = \alpha_j + \Gamma_i \rho_j + \varepsilon_{ij}, \tag{11}$$

where $\ln W_{ij}$ is the logarithm of individual i 's wage in city j ; Γ_i is a vector of individual characteristics; and ε_{ij} is the error term. Since one can only obtain wage information for those who have actually chosen city j , the sample is not random, and in general

$$E(\ln W_{ij} | \text{city } j \text{ is chosen}) = \alpha_j + \Gamma_i \rho_j + E(\varepsilon_{ij} | \text{city } j \text{ is chosen}). \tag{12}$$

The OLS estimate of ρ_j is likely to be biased due to self-selection, i.e., $E(\varepsilon_{ij} | \cdot)$ and Γ_i are correlated. Dahl (2002) shows that $E(\varepsilon_{ij} | \cdot)$ can be written as a function of $(P_{i1}, P_{i2}, \dots, P_{ij})$, where P_{ik} , $k = 1, 2, \dots, J$, is the probability of individual i moving to city k . To reduce the dimension of this function, Dahl invokes a single index sufficiency assumption: If individual i actually moved to city j , then the probability P_{ij} alone would contain sufficient information to predict $E(\varepsilon_{ij} | \cdot)$. It follows that a consistent estimate of ρ_j can be obtained from the following regression

$$\ln W_{ij} = \alpha_j + \Gamma_i \rho_j + \psi_j(P_{ij}) + \pi_{ij}, \tag{13}$$

where $\psi_j(P_{ij})$ is an unknown function of P_{ij} that equals $E(\varepsilon_{ij}|\cdot)$, and π_{ij} is the remaining error term. Dahl proposes that this unknown function can be approximated by a polynomial or Fourier series of P_{ij} . Since individual i has indeed moved to city j , P_{ij} can be estimated nonparametrically.

The data for wage regressions are taken from the 1980, 1990, and 2000 IPUMS. For each year, we select all blacks and whites aged 20 to 65 who are born in one of the 48 contiguous states plus the District of Columbia, are full-time workers, and live in one of the cities included in that year's study samples. As suggested in Dahl (2002), we estimate P_{ij} using the following method: For each year, we first assign all individuals into one of four educational groups: less than high school, high school, some college, and college degree or higher. Within each educational group, we then divide individuals into two racial groups: blacks and whites. Within each education by race group, we further classify individuals into nine census divisions by birth state. In total, we have 72 education-by-race-by-census-division data cells with each individual belonging to one of them. The migration probability P_{ij} is calculated as the fraction of the population in individual i 's cell that has migrated to city j .

We approximate $\psi_j(P_{ij})$ with the second degree polynomial of P_{ij} and estimate the following wage equation:

$$\ln W_{ij} = \alpha_j + \Gamma_i \rho_j + \phi_{j1} P_{ij} + \phi_{j2} P_{ij}^2 + \pi_{ij}, \quad (14)$$

where individual characteristics Γ_i include age, age squared, and dummy variables for white, male, high school, some college, college degree or higher, and marital status.¹⁴ Note that when predicting individual wages, we exclude P_{ij} and P_{ij}^2 from the calculation because they only serve as additional controls to correct for selection bias.

4.3.2. Measuring housing price

Next, we measure housing price at the city level. Recall that housing price will be used to construct the dependent variable in Eq. (8'), which then will be regressed on the level of segregation and other city characteristics to estimate β_s and β_x . One possible way to measure housing price is to use a simple aggregate statistic such as the median housing price in a given city. However, such a measure is likely to be correlated with housing quality. Furthermore, there are reasons to believe that housing quality is correlated with city characteristics. For example, Farley and Frey (1994) find that a city's level of segregation is negatively correlated with the share of new housing construction, perhaps reflecting reduced racial discrimination in newer housing markets as a result of fair housing legislations. This correlation between segregation and housing quality implies that if median housing price is used, it is likely to produce a biased estimate of β_s .

To overcome this bias, following Bayer et al. (2009), we use a version of quality-adjusted housing price generated from the following housing price regression:

$$\ln P_{ij}^H = \ln p_j + \Omega_i \gamma + v_{ij}, \quad (15)$$

where $\ln P_{ij}^H$ is the logarithm of the price for house i in city j ; Ω_i is a vector of housing characteristics; and v_{ij} is the error term. $\ln p_j$ represents a city-specific constant that captures quality-adjusted housing

prices in city j .¹⁵ Again, we utilize IPUMS data to estimate housing price regressions for 1980, 1990, and 2000 separately, and then use the estimated $\ln p_j$ to replace $\ln p_j$ in Eq. (8').¹⁶ For our baseline regressions, we estimate housing prices using data on owner-occupied houses only. In robustness analysis, we check whether including data on rental housing units affects our main results. See the data Appendix A for more details on the housing characteristics included in these regressions.

Note that in Eq. (8'), we only replace the $\ln p_j$ on the left side using the estimated housing prices from Eq. (15). The share of income spent on housing, which is needed for estimating β_h in Eq. (8'), is calculated using actual housing prices. We assume that a house is financed by a fixed rate mortgage and the owner pays back the loan over 30 years (360 months). For each racial group in each census year, we calculate the monthly payment for each homeowner using the prevailing mortgage rate in that year.¹⁷ This monthly payment is then divided by monthly income, and we use the sample average of this fraction as the estimated share of income spent on housing. These estimates are 0.35, 0.24, and 0.20 for whites in 1980, 1990, and 2000, respectively; and 0.27, 0.20, and 0.19 for blacks in 1980, 1990, and 2000, respectively. To be consistent with the estimation of Eq. (15), we only use the data on homeowners here; we incorporate data on renters in robustness analysis.

4.3.3. Instrumenting for residential segregation

To address the endogeneity of segregation, we draw upon two sets of instruments for segregation from the previous literature. The first set includes two fiscal instruments originally constructed and used by Cutler and Glaeser (1997): the number of municipal and township government units in 1962 and the share of local revenue that results from intergovernmental sources (state or federal) in 1962. The rationale for employing these variables is that households sort themselves across jurisdictions in a Tiebout fashion to take advantage of local differences in tax rates and service provision. More local governments and less money from intergovernmental transfers lead to larger variations in tax rates and service provision, facilitate residential sorting among households, and tend to result in higher levels of segregation.

The second set of instruments includes two topographic variables developed by Hoxby (2000) and revised by Rothstein (2007): the number of inter- and intra-county rivers through one city.¹⁸ Rivers serve as natural boundaries that divide a city into subunits. Hence more rivers may increase segregation in a city by increasing the number of local governments or by providing natural barriers that impede the movement of blacks into white neighborhoods.

¹⁵ Conceptually, housing price is determined by city, neighborhood, and building characteristics. Thus, ideally one would want to control for neighborhood characteristics in this regression. However, the smallest geographic identifier in the IPUMS data is a Public Use Microdata Area (PUMA), with an average of about 100,000 residents, much larger than a typical neighborhood that is relevant for housing price. Note that the observed average neighborhood characteristics in a city can be thought of as city-level characteristics and are controlled for in the second step regression. Only the deviation from average neighborhood characteristics could cause a problem in this regression. But since some missing neighborhood characteristics are below and others are above average, their effects tend to cancel out and are unlikely to cause serious bias in the estimated city-level housing price.

¹⁶ In housing price regressions for 1980, 1990, and 2000, we demean all housing characteristics variables in Ω_i so that $\ln p_j$ measures the price of the average house in that census year in city j .

¹⁷ The 30-year fixed mortgage rate we use are 13.74% for 1980, 10.13% for 1990, and 8.05% for 2000.

¹⁸ Hoxby (2000) analyzes the effect of Tiebout choice on school performance among districts using the number of large and small rivers as exogenous instrumental variables. Rothstein (2007) revisits Hoxby's analysis and argues that the results in Hoxby (2000) are sensitive to some reasonable modifications such as the redefinition of large and small rivers. Rothstein provides five sets of measures for large and small rivers, under different definitions. We use inter- and intra-county rivers through one metropolitan area as our topographic instruments because they appear to be more strongly correlated with the level of segregation.

¹⁴ This empirical specification is rather parsimonious due to data constraints, thus one might be concerned with potential omitted-variable bias. As in typical Mincer wage regressions, we do not have a measure for ability, which could bias the estimated education coefficient. However, this kind of ability bias is not a concern because our goal is not to measure the true return to education, but rather to predict individual earnings. Missing ability becomes a serious problem only if it is rewarded differently across cities and thus causes a selection bias. We use Dahl's method precisely for dealing with this problem. Also missing is information on industry and occupation. One could think of industry and occupation mixes as unobserved city characteristics that affect the matching quality between a city and different individuals. This source of selection bias is also corrected using Dahl's method.

Table 2
Dissimilarity index and instruments.

	Log of the dissimilarity index		Mean	Std. dev.	Min	Max
	(1)	(2)				
ln(number of governments)	0.094*** (0.013)		41	55	1	339
Intergovernmental revenue share	−0.551*** (0.201)		0.238	0.078	0.123	0.494
Inter-county rivers		0.0024*** (0.0005)	44	35	0	215
Intra-county rivers		−0.00028* (0.00015)	116	128	0	812
Constant	−0.682*** (0.078)	−0.664*** (0.022)				
R ²	0.314	0.069				
Observations	203	281				

Notes: Columns 1–2 report the results of regressing the logarithm of the dissimilarity index on each set of instruments using the 1990 white sample. Robust standard errors adjusted for heterogeneity are in parentheses. Columns 3–6 give the summary statistics of the corresponding instruments.

The dissimilarity index is from Cutler et al. (1999). Fiscal instruments (the number of governments and intergovernmental revenue share) are from Cutler and Glaeser (1997). Topographical instruments (inter-county and intra-county rivers) are from Rothstein (2007). See the data Appendix A for details.

* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

To illustrate the effects of these instruments on segregation, we separately regress the logarithm of the dissimilarity index on the two sets of instruments using the 1990 white sample. Columns 1–2 of Table 2 report the regression results and columns 3–6 give the summary statistics of instruments. All the coefficients for these instruments are statistically significant, suggesting that they satisfy the relevance condition of instrumental variables. All but one coefficient have the expected signs. The only exception is the negative coefficient on intra-county rivers, which shows that more intra-county rivers, conditional on the number of inter-county rivers, may slightly facilitate racial mixing.

5. Empirical results

5.1. Wage and housing price regressions

Table A2 summarizes the estimated coefficients from the city-specific wage regressions for 1980, 1990, and 2000. In the average city, from 1980 to 2000, the wage gap between blacks and whites

increases slightly, whereas the gender wage gap diminishes significantly. Wages increase with educational attainment. Since 1980, there has been a substantial increase in the premium on college degrees. The coefficients on age and age squared imply that wages increase with age, but at a decreasing rate. The positive coefficients on marital status suggest that married people tend to earn more, and this effect increases slightly over time.

Table A4 presents estimates from the housing price regression in each year, except that the estimates of quality-adjusted housing prices ($\ln p_j$) are summarized in the first row of Table 1. As shown, almost all the estimates are statistically significant and have the expected signs. Newer houses, houses with more rooms, and houses with complete kitchen and plumbing facilities have higher prices. Housing value is lower if the housing unit is part of a larger multi-family building.

5.2. The first-step maximum likelihood estimates

Panels A and B of Table 3 present the first-step maximum likelihood estimates for blacks and whites in 1980, 1990, and 2000,

Table 3
Maximum likelihood estimates from first-step discrete choice model of residential location decision.

Variable (parameter)	1980		1990		2000	
	Estimates	Standard errors	Estimates	Standard errors	Estimates	Standard errors
<i>Panel A. Blacks</i>						
Mean utilities from city j (θ_j)	−1.231	1.197	−1.048	1.321	−0.901	1.213
Predicted wage (β_w)	3.156***	0.042	2.511***	0.042	2.747***	0.043
Out of birth state dummy (m_1)	−3.013***	0.028	−3.165***	0.027	−3.208***	0.027
Out of birth division dummy (m_2)	−1.591***	0.042	−1.301***	0.040	−1.182***	0.040
Out of birth region dummy (m_3)	0.599***	0.042	0.292***	0.037	0.025	0.037
Age * $\ln S_j$	0.093***	0.015	0.118***	0.013	0.019	0.013
High school or less dummy * $\ln S_j$	−0.091	0.096	−0.064	0.084	0.066	0.082
Presence of children dummy * $\ln S_j$	0.016	0.098	−0.023	0.087	−0.138	0.086
<i>Panel B. Whites</i>						
Mean utilities from city j (θ_j)	−1.885	1.036	−0.987	1.080	−1.123	0.978
Predicted wage (β_w)	3.509***	0.046	4.284***	0.045	3.531***	0.044
Out of birth state dummy (m_1)	−2.800***	0.026	−2.815***	0.027	−2.885***	0.026
Out of birth division dummy (m_2)	−0.895***	0.037	−0.762***	0.036	−0.705***	0.036
Out of birth region dummy (m_3)	−0.478***	0.033	−0.631***	0.033	−0.637***	0.032
Age * $\ln S_j$	0.031**	0.014	−0.010	0.013	0.010	0.010
High school or less dummy * $\ln S_j$	−0.016	0.087	0.095	0.080	0.023	0.066
Presence of children dummy * $\ln S_j$	−0.123	0.088	−0.212***	0.078	−0.258***	0.065

Notes: The first row of Panels A and B present the mean and standard deviation of estimated θ_j in 1980, 1990, and 2000.

All data are from the 1980 1%, 1990 1%, and 2000 5% IPUMS.

* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

Table 4
Results from second-step OLS regressions on city-specific utilities (dependent variable: $\Theta_j + \beta_h \ln p_j$, housing price adjusted mean utilities from city j).

	Blacks			Whites		
	1980	1990	2000	1980	1990	2000
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Baseline: all cities</i>						
ln(dissimilarity index)	−0.232 (0.289)	−1.006*** (0.193)	−0.769*** (0.189)	−0.578** (0.268)	−0.852*** (0.188)	−0.472*** (0.168)
ln(population size)	1.090*** (0.064)	1.137*** (0.058)	1.046*** (0.044)	1.069*** (0.061)	1.045*** (0.052)	0.997*** (0.051)
ln(black share)	0.430*** (0.058)	0.837*** (0.074)	0.901*** (0.058)	−0.244*** (0.062)	−0.133** (0.057)	0.041 (0.060)
ln(population density)	0.018 (0.076)	−0.157* (0.080)	−0.201*** (0.057)	−0.072 (0.063)	−0.161** (0.074)	−0.211*** (0.063)
ln(college share)	0.330* (0.193)	0.413 (0.296)	0.783*** (0.217)	0.476*** (0.179)	0.499** (0.243)	0.918*** (0.262)
ln(Gini coefficient)	1.086 (1.375)	0.826 (1.143)	−1.486* (0.777)	−0.718 (1.100)	−0.171 (0.919)	−3.224*** (0.841)
ln(median household income)	0.147 (0.594)	−0.219 (0.532)	−0.839** (0.413)	−1.124** (0.460)	−1.248*** (0.404)	−2.081*** (0.394)
ln(manufacturing share)	−0.526*** (0.147)	−0.398*** (0.116)	0.061 (0.077)	−0.588*** (0.124)	−0.478*** (0.148)	−0.041 (0.087)
Constant	−4.675 (5.825)	−4.370 (5.269)	3.322 (4.253)	10.975** (4.736)	11.638*** (4.114)	16.831*** (4.142)
R ²	0.813	0.774	0.823	0.799	0.747	0.753
Observations	176	203	184	185	203	185
<i>Panel B. A subset of cities available for both race and all decades</i>						
ln(dissimilarity index)	−0.289 (0.329)	−1.093*** (0.221)	−0.802*** (0.200)	−0.485 (0.342)	−0.861*** (0.208)	−0.518*** (0.179)
R ²	0.822	0.805	0.827	0.789	0.761	0.759
Observations	161	161	161	161	161	161

Notes: Robust standard errors adjusted for heterogeneity are in parentheses. Although not presented, each regression in Panel B includes the same set of controls as in Panel A. Housing prices in dependent variables are estimated using data from the 1980 1%, 1990 1%, and 2000 5% IPUMS. Mean utilities in dependent variables are estimated from the first-step discrete choice model presented in Table 3. See the notes under Table 1 for the sources of independent variables.

* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

respectively.¹⁹ Since it is not feasible to list all the estimated city-level mean utilities (Θ_j), the first row in each panel presents the mean and standard deviation of Θ_j . For each sample, the mean utilities of cities are measured relative to that of Boston, MA, which is set equal to zero.

The estimates of the marginal utility of wage, shown in the second row of each panel, are highly significant with the expected positive sign.²⁰ With few exceptions, the coefficients on migration-distance dummies are also statistically significant, indicating that long-distance migration tends to result in utility losses. Migration costs appear to increase as an individual leaves his or her birth state, birth division, and birth region (for whites only), although at a decreasing rate. For blacks, moving out of birth region has a positive and statistically significant coefficient for the 1980 and 1990 samples. This unexpected result may reflect the fact that a sizable number of young blacks in these two samples were born in the South and migrated to the North with their parents at very early ages, and thus had less emotional attachment to their birth places.

We allow the preference for segregation to vary with three individual attributes: a dummy variable that equals 1 if educational attainment is high school or less and 0 otherwise; a dummy variable that equals 1 if the household has any children and 0 otherwise; and the age of the

household head. All three variables are demeaned by their sample averages so that the coefficient of segregation to be estimated at the second step (β_s) represents the preference of the average household head in the sample.

The last three rows in each panel report the estimated coefficients on individual attributes interacted with the log dissimilarity index. The coefficients on age interacted with segregation are positive in almost every sample and are statistically significant for the 1980 black, 1990 black, and 1980 white samples, suggesting that older migrants tend to tolerate segregation more than younger migrants. For both blacks and whites in each year, there are no significant differences between less and more educated migrants in their preferences for segregation. The results in the last row show that in almost every case, migrants with children dislike segregation more than those without children, but this difference is statistically significant only for the 1990 and 2000 white samples.

5.3. The second-step OLS estimates

Columns 1–6 of Table 4 show the second-step OLS estimates of Eq. (8') for blacks and whites in 1980, 1990, and 2000. We present results from two parallel sets of regressions: one using all cities in each sample (Panel A) and the other using a subset of cities available for both races and all three census years (Panel B) to maintain a uniform sample size. To conserve space, except for our baseline specification in Panel A, we only present the log dissimilarity index coefficients. Since the magnitude of a single estimate in the logit model cannot be directly compared across races and decades, we leave this discussion for the next section, when measuring household marginal willingness to pay.

The OLS estimates of the mean preference for segregation are negative for both blacks and whites in all three census years, and are statistically significant in most samples. One exception is that the negative

¹⁹ Since the estimated parameters in the logit model are scaled by the variance of the unobserved portion of utility (η_{ij} in Eq. (7)) and this variance is different in different samples, the absolute levels of the first- and second-step estimates cannot be directly compared across samples.

²⁰ Since the wage variable is generated from city-specific earnings regressions, the standard error of the wage coefficient is likely to be underestimated. Given that so many earnings regressions are involved and each earnings regression also uses generated regressors (estimated migration probabilities), there is no analytical solution to this problem. Normally, in situations like this, one would consider bootstrapping standard errors. However, since we are estimating so many parameters by maximum likelihood, each round of estimation takes hours, making it infeasible to bootstrap standard errors.

Table 5

Results from second-step 2SLS regressions on city-specific utilities.
(Dependent variable: $\Theta_j + \beta_n \ln p_j$, housing price adjusted mean utilities from city j).

	Blacks			Whites		
	1980 (1)	1990 (2)	2000 (3)	1980 (4)	1990 (5)	2000 (6)
<i>IV: Fiscal instruments</i>						
Panel A. Baseline: all cities						
ln(dissimilarity index)	-0.444 (0.575)	-2.109*** (0.405)	-1.903*** (0.360)	-3.763*** (0.746)	-2.923*** (0.494)	-1.690*** (0.378)
p-Value of Hansens J	0.000	0.066	0.826	0.407	0.326	0.107
F-statistics	11.213	27.443	26.173	15.981	27.443	26.655
R ²	0.812	0.746	0.782	0.597	0.610	0.688
Observations	176	203	184	185	203	185
Panel B. A subset of cities available for both race and all decades						
ln(dissimilarity index)	-0.854 (0.665)	-2.532*** (0.493)	-1.761*** (0.375)	-4.469*** (1.084)	-3.384*** (0.655)	-1.676*** (0.399)
p-Value of Hansens J	0.002	0.055	0.799	0.743	0.930	0.174
F-statistics	10.662	18.410	23.367	10.662	18.410	23.367
R ²	0.818	0.759	0.798	0.526	0.567	0.703
Observations	161	161	161	161	161	161
<i>IV: Number of inter- and intra-county rivers</i>						
Panel C. Baseline: all cities						
ln(dissimilarity index)	-1.735 (1.533)	-1.566* (0.898)	-0.997 (1.056)	-3.035** (1.438)	-1.480 (0.912)	-0.405 (1.142)
p-Value of Hansens J	0.205	0.564	0.182	0.464	0.024	0.014
F-statistics	3.694	5.720	2.651	4.462	5.720	2.723
R ²	0.776	0.767	0.821	0.679	0.734	0.753
Observations	176	203	184	185	203	185
Panel D. A subset of cities available for both race and all decades						
ln(dissimilarity index)	-1.840 (1.770)	-1.617 (1.032)	-0.529 (1.205)	-2.906 (1.809)	-1.342 (1.100)	-0.220 (1.368)
p-Value of Hansens J	0.114	0.813	0.078	0.415	0.036	0.013
F-statistics	2.927	3.676	1.844	2.927	3.676	1.844
R ²	0.789	0.798	0.825	0.692	0.754	0.756
Observations	161	161	161	161	161	161

Notes: Although not presented, each regression in each panel includes a constant and the same set of controls as in Panel A of Table 4. p-Value of Hansens J tests the overidentifying restrictions in the presence of heterogeneity. F-statistics tests the joint significance of excluded instruments in the first stage under the assumption of heterogeneity. Robust standard errors adjusted for heterogeneity are in parentheses. Data sources are the same as in Table 4.

* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

coefficient is always insignificant for blacks in 1980. Negative preferences for segregation suggest that segregation is associated with utility losses for both blacks and whites and that they would be willing to give up some income in exchange for a lower level of segregation in their cities of residence.

The estimated coefficients on other city characteristics shown in Panel A also reveal useful information. From 1980 to 2000, both blacks and whites appear to prefer larger and more educated cities. Blacks prefer to reside in cities with higher black shares. In contrast, whites' preferences for living in a city with a higher share of blacks vary in sign and significance level over time. As expected, high population density is a disamenity. High-income cities also appear to be unattractive for both races, perhaps because it is disproportionately more expensive to live in such cities. Cities with low income inequalities are appealing for whites in each year and for blacks in 2000. Additionally, cities with a larger share of employment in the manufacturing sector appear to be unattractive in all but the 2000 black and white samples.

5.4. The second-step 2SLS estimates

Table 5 presents the second-step 2SLS estimates for Eq. (8') using fiscal and river instruments. Again, we run two sets of parallel regressions, one using all cities in each sample and the other imposing a uniform sample size. Panels A–B use fiscal instruments and C–D use river instruments. In each regression, we include the same set of controls as in Panel A of Table 4. To conserve space we only report the coefficients of log dissimilarity index.

For all 2SLS regressions, we report the F-statistic that tests the joint significance of the instrumental variables in the first stage. As indicated, the fiscal instrumental variables are very strongly correlated with segregation. When used alone as instruments, they always have first-stage F-statistics higher than 10, the rule-of-thumb value for strong instruments. In contrast, when we use the river instrumental variables, the first-stage F-statistics are always lower than 10, suggesting potential weak-instrument problems.

A valid instrumental variable should also be excludable. That is, the instrument (e.g., the number of local governments) has no effect on housing-price-adjusted city-specific mean utilities other than through racial segregation. To assess the excludability of our instruments, we perform the Hansens J test. This test treats one of the excluded instruments as a truly exogenous variable and examines whether the other instruments are exogenous. For most specifications, fiscal and river instruments pass the overidentification test at the five percent level. Fiscal instruments perform poorly for the 1980 black sample and river instruments perform poorly for the 1980 and 1990 white samples.

We also estimate Eq. (8') combining fiscal and river instruments. The coefficients on segregation closely resemble those estimated using fiscal instruments only.²¹ Further exploration reveals that the effect of rivers on segregation diminishes substantially once we control for the number of local governments, suggesting that rivers affect segregation primarily through increases in the number of local governments.

²¹ A similar finding was noted by Cutler and Glaeser (1997), who studied the effects of segregation on the labor market and social outcomes of young blacks using the fiscal and river instruments.

Table 6
Coefficients of log dissimilarity index from alternative specifications.

	Blacks			Whites		
	1980	1990	2000	1980	1990	2000
	(1)	(2)	(3)	(4)	(5)	(6)
1 Baseline	−0.444 (0.575)	−2.109*** (0.405)	−1.903*** (0.360)	−3.763*** (0.746)	−2.923*** (0.494)	−1.690*** (0.378)
Observations	176	203	184	185	203	185
2 Including regional dummies	10.15 (8.061)	−0.985 (1.680)	−2.092** (1.013)	0.275 (1.009)	−3.038 (2.026)	−0.845 (0.906)
Observations	176	203	184	185	203	185
3 Adding climate controls ^a	−0.462 (0.805)	−2.127*** (0.691)	−2.612*** (0.716)	−2.820*** (0.909)	−2.716*** (0.775)	−1.8344*** (0.648)
Observations	166	186	184	175	186	185
4 Dropping Sunbelt-region cities	−0.528 (0.980)	−3.170*** (1.075)	−3.324*** (0.667)	−5.236*** (1.567)	−4.515*** (0.950)	−3.079*** (0.648)
Observations	93	109	96	99	109	96
5 Adding Sunbelt-region dummy	−0.626 (0.647)	−2.551*** (0.646)	−2.726*** (0.581)	−3.551*** (0.963)	−3.294*** (0.767)	−2.208*** (0.590)
Observations	176	203	184	185	203	185
6 Using Ananat's RDI as IV ^a [Control for population size only]	−3.991** (1.886)	−1.923*** (0.829)	−0.270 (0.848)	−3.703*** (1.000)	−2.817*** (0.678)	−0.731 (0.477)
Observations	59	68	61	91	113	99
7 Additional fiscal variables ^a	−0.976 (0.635)	−2.507*** (0.440)	−1.842*** (0.350)	−4.497*** (0.975)	−3.033*** (0.526)	−1.513*** (0.373)
Observations	176	203	184	185	203	185
8 Additional public service variables ^a	−0.895 (0.573)	−1.909*** (0.400)	−1.595*** (0.297)	−4.009*** (0.810)	−2.763*** (0.506)	−1.239*** (0.293)
Observations	176	203	184	185	203	185
9 40% of income in housing	−0.790 (0.610)	−2.504*** (0.430)	−2.146*** (0.367)	−3.893*** (0.766)	−3.462*** (0.536)	−1.999*** (0.386)
Observations	176	203	184	185	203	185
10 15% of income in housing	−0.125 (0.553)	−2.010*** (0.400)	−1.857*** (0.359)	−3.246*** (0.679)	−2.620*** (0.476)	−1.613*** (0.378)
Observations	176	203	184	185	203	185
11 Including rental units	−0.386 (0.580)	−2.060*** (0.406)	−1.879*** (0.356)	−3.577*** (0.721)	−2.659*** (0.477)	−1.610*** (0.371)
Observations	176	203	184	185	203	185
12 1890 black share as instrument ^a	−1.265* (0.658)	−1.946*** (0.468)	−1.691*** (0.389)	−3.547*** (0.728)	−3.135*** (0.502)	−1.612*** (0.432)
Observations	116	125	121	124	125	122

Notes: The first row reproduces the baseline estimates in Panel A of Table 5. In the corresponding sample, unless otherwise noted in the text, the regression specifications of robust analysis are the same as the baseline specifications. Robust standard errors adjusted for heterogeneity are in parentheses.

^a Climate variables, fiscal variables, and public service variables are from the 1988, 1994, and 2000 editions of the City and County Databook except that the high-school dropout rate is from IPUMS. The RDI instrument from Ananat (2011) is available at the official website of the American Economic Association. Black share in 1890 is from Cutler et al. (1999).

* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

Given these results from diagnostic tests and exploratory analysis, we feel that the 2SLS estimates using the fiscal instruments are more reliable. We therefore consider the estimates in Panels A and B of Table 5 as our preferred estimates. The estimates using the river instruments are presented for comparison purposes.

Panel A of Table 5 shows that the estimated mean preferences for segregation using fiscal instruments are all negative, and are statistically significant for all but one sample (1980 blacks). This again indicates that both blacks and whites dislike segregation. Compared with the corresponding OLS estimates, the 2SLS estimates using fiscal instruments are much larger in magnitude, ranging from two times (for the 1980 black sample) to seven times (for the 1980 white sample) larger. In Panel C of Table 5, we see that the 2SLS coefficients using river instruments are less precisely estimated. The estimated mean preferences for segregation continue to be negative for all samples. However, they are almost always smaller in magnitude than those estimated using fiscal instruments, which seems consistent with the notion that they suffer from weak instrument biases. Results in Panels B and D, estimated using the same subset of cities across different samples, are qualitatively similar to those estimated using the full sample for each year.

Two points regarding the results in Tables 4–5 are worth noting. First, these results generally suggest that both blacks and whites dislike segregation. For blacks, our findings complement previous studies that

document many adverse effects of segregation on black outcomes. While previous studies focus on very specific outcomes, our results can be interpreted as an overall negative effect of segregation on blacks. For whites, earlier studies find that segregation has moderate positive effects (e.g., Cutler and Glaeser, 1997) or mixed effects (e.g., Ananat, 2011) on specific outcomes. Our findings suggest that there are substantial neglected costs of segregation for whites. Referring to our theoretical discussion, the estimated disutility of segregation suggests that for blacks, negative neighborhood effects, the spatial mismatch with jobs, and perhaps a distaste for segregation outweigh the benefit from living close to neighbors of similar cultural backgrounds; for whites, a distaste for segregation appears to exceed the potential benefits of such a residential pattern.

Our results are consistent with the findings of Bajari and Kahn (2005), but inconsistent with those of Bayer et al. (2004) and Bayer et al. (2007), who find segregating preferences at the block-group level. To reconcile this discrepancy, it is important to recognize that the preference for segregation at the city level is conceptually different from the preference for like neighbors at the block-group level. One could think of a nested-logit type of behavioral model: An individual first chooses a city to live in and then, conditional on the choice of a city, chooses a neighborhood. Whereas city-level segregation affects the choice of a city, it becomes less relevant in the choice of a neighborhood. It is conceivable that one dislikes segregation at the city level

Table 7
Estimated Marginal Willingness to Pay for Reduced Residential Segregation.

	Blacks			Whites		
	1980	1990	2000	1980	1990	2000
	(1)	(2)	(3)	(4)	(5)	(6)
Mean MWTP (OLS)	46.285	252.253	176.260	103.712	125.221	84.165
Mean MWTP (baseline 2SLS)	88.579	528.829	436.180	675.206	429.601	301.352
Age * lnS _j	-18.554	-29.588	-4.355	-5.562	1.470	-1.783
High school or less dummy * lnS _j	18.155	16.048	-15.128	2.871	-13.962	-4.101
Presence of children dummy * lnS _j	-3.192	5.767	31.630	22.070	31.158	46.005

Notes: This table reports the marginal willingness to pay for a one-percentage-point reduction in the dissimilarity index for blacks and whites in 1980, 1990, and 2000. The formula of MWTP is $\frac{\beta_w + \sum_{j \neq w} z_{jc} \beta_{3c} W_{ij}}{S_j}$, where W_{ij} and S_j are evaluated by using the 1990 median household income ($W_{ij} = \$35,763$ in 1999 dollars) and the 1990 mean dissimilarity index ($S_j = 0.568$). The first and second rows present mean MWTP by using the baseline OLS and fiscal instrumental variable estimates of mean preferences for segregation (Panels A of Tables 4 and 5). For the heterogeneous MWTP in the last three rows, β_w and β_{3c} are taken from Table 3.

yet prefers to live with like neighbors once one has decided to live in a rather desegregated city.²²

Second, regression results indicate that the OLS estimates of the mean preference for segregation are generally biased downward in magnitude, suggesting that omitted city amenities (for both blacks and whites) are positively correlated with the level of segregation. There are several possible sources of this bias. First, older industrial cities in the North are more racially segregated, and they also tend to have a richer cultural and historical legacy that constitutes an unmeasured city amenity to many people. Second, while segregation at the city level may represent a disamenity, it allows each racial group to build their own community according to their own needs. For example, in predominantly black neighborhoods, more infrastructure can be constructed to facilitate leisure activities preferred by blacks. Similar developments may also occur in predominantly white neighborhoods. These benefits from community-level specialization may help counter some of the negative effects of city-level segregation. Finally, there may be local public policies that benefit both black and white residents but unintentionally intensify racial segregation. For example, increased investment in community development projects will better serve both black and white residents. However, if such projects are tailored to the dominant race in segregated neighborhoods, they could perpetuate segregation.

5.5. Robustness analysis

We next conduct robustness analysis to test the sensitivity of our main results, which is presented in Table 6. The first row reproduces the baseline estimates using fiscal instruments (from Panel A of Table 5). Unless otherwise noted, regression specifications for robustness analysis are the same as the baseline specifications.

5.5.1. Interregional variations and migration to the Sunbelt

It is well known that, on average, cities in the Northeast and Midwest are more racially segregated and are characterized by the presence of more local governments than those in the South and West. This raises the concern whether negative preferences for segregation are driven by omitted regional characteristics. One way to address this issue is to add region dummies to the baseline 2SLS specifications to control for regional differences. However, since within-region variation of both segregation and instrumental variables are limited, adding region dummies yields no (or weak) first-stage correlations between the log dissimilarity index and the fiscal instruments.²³ Row 2 presents the results after adding region dummies. Most of the coefficients are still negative, but are imprecise and only one is statistically significant. That is, despite the concern for omitted regional characteristics, cross-region variations are

crucial for estimating model parameters, and we cannot afford to throw them away and rely solely on region fixed effects regressions.

We also test whether our main results are driven by the migration from the Rustbelt to Sunbelt states.²⁴ Since the 1960s, the Sunbelt region experienced a substantial gain in population due to its warmer climate and growing economic opportunities. Given that cities in the North generally have higher degrees of segregation than those in the South, this trend of moving to the South could be mistakenly taken as revealed preferences for less segregation. To assess this possibility, we add mean January and July temperatures as climate controls to our city-level regression. In our baseline regressions, we include the employment share of the manufacturing sector in a city, which should capture a main feature of Rustbelt cities. Climate controls account for an additional important attribute that distinguishes Sunbelt cities from Rustbelt cities. We also estimate regressions that exclude all Sunbelt cities and regressions that include a dummy variable for all cities in the Sunbelt region. Results from these alternative specifications are in rows 3–5 of Table 6. In all three cases, the estimates are similar to the baseline results.²⁵

Finally, we try an alternative instrumental variable that is unlikely to pick up the trend of migration to the Sunbelt. Using nineteenth-century maps, Ananat (2011) constructs a railroad division index (RDI) for 121 cities to capture the degree of fragmentation created by railroads, which she uses as an instrument for segregation at present. Compared to the two IVs we used above, Ananat’s RDI is less preferable for two reasons: (1) It covers a much smaller sample of cities; and (2) it becomes a weak IV once some control variables are included. Nonetheless, this RDI variable is more likely to be uncorrelated with the recent trend of moving to the Sunbelt region, and is thus useful for this robustness check. To increase the precision of estimation, we only control for population size when using RDI to instrument for segregation. The results (in row 6 of Table 6) show that the estimates are still qualitatively similar to the baseline results despite significant declines in sample sizes.²⁶ Together, these sensitivity analyses suggest that our baseline results are not driven by the trend of moving to the Sunbelt region.

5.5.2. Competition among jurisdictions

Our preferred estimates use fiscal instruments. One possible threat to the validity of these instruments is competition among jurisdictions. In particular, increasing the number of governments may force governments to implement policies that are in accord with the interests of residents (e.g., Epple and Zelenitz, 1981). Such policies could affect a city’s attractiveness and thus influence individual utilities directly. To address this concern, we try adding two sets of controls in the baseline

²² Both Bayer et al. (2004) and Bayer et al. (2007) analyze data in the San Francisco Bay Area. It remains to be seen whether their results hold in other cities.

²³ With region dummies, the first-stage F-statistics for the fiscal instruments range between 0.8 and 3.8 across different samples.

²⁴ The Sunbelt region includes Alabama, Arizona, California, Florida, Georgia, Louisiana, Mississippi, Nevada, New Mexico, North Carolina, South Carolina, and Texas.

²⁵ We also experimented with dropping each of the four census regions. The results are always similar to the baseline estimates.

²⁶ Ananats IV does not contain sufficient within-region variations either. Once we add region dummies in addition to the population control, the first stage correlation becomes very weak.

specifications. The first set is fiscal controls, including government expenditure per capita and property tax rate. The second set is related to local public services, including crime rate and high-school dropout rate.²⁷ These results are presented in rows 7 and 8 of Table 6. It appears that these controls do not qualitatively affect our main results.²⁸

5.5.3. Share of income spent on housing

For our baseline regressions, the share of income spent on housing (used for calculating β_h) was estimated to be between 0.19 and 0.35 across different samples. Here we check whether our main results are sensitive to this share. Note that given the Cobb–Douglas preference, utility maximization requires that each resident's share of income spent on housing is fixed and proportional to housing's exponent parameter in the utility function. In theory, we can directly calculate this share from data without worrying about the endogeneity of income and housing price. However, in practice, calculating this share requires a series of assumptions (e.g., how to calculate housing price, how to treat renters, etc.). It is thus useful to check whether our results will change dramatically if β_h is over or underestimated.

We rerun the regressions using an artificially high share value of 0.40 and an artificially low share value of 0.15. These results, presented in rows 9 and 10 of Table 6, are qualitatively similar to the baseline estimates, although the magnitudes are slightly larger when using a high share of 0.40 and slightly smaller when using a low share of 0.15.

In our baseline regressions, both the share of income in housing expenditure and the quality-adjusted housing price are estimated using information on only homeowners and their houses; data on renters and rental units are ignored. Here we incorporate data on renters. For quality adjusted housing price, we add a rental unit dummy to Eq. (15) and run the regression using data on both owner-occupied houses and rental units. Again, the quality-adjusted housing price is the estimated city-specific constant $\ln p_j$. For each renter, we calculate the share of income spent on housing by dividing monthly rent by monthly income. For homeowners, this share is calculated in the same way as described in the data section. We then average this share over all homeowners and renters, which is then used to calculate β_h in Eq. (8'). Row 11 of Table 6 shows the coefficients of log dissimilarity index from this alternative estimation method, which are more or less the same as the baseline estimates.²⁹

5.5.4. Endogeneity of black population share

Another concern is that a higher share of blacks in a city may reflect unobserved city characteristics that are particularly attractive to blacks. Although the coefficient of log black share is not the focus of this study, the endogeneity of black share—similar to that of housing price—may spill over to the estimate of β_s , the key parameter of interest for this study. In row 12 of Table 6, we use the black share in 1890 to instrument for current black share in 1980, 1990, and 2000. The 1890 black share is unlikely to be correlated with omitted city variables for the 1980–2000 samples because that year predates the first wave of black migration from the rural South to the urban North (Cutler et al., 1999).³⁰ Although

²⁷ The data for government expenditure, property tax rate, and crime rate come from the 1988, 1994, and 2000 editions of the City and County Databook. High-school dropout rate is measured as the share of individuals aged 16–19 who are neither in school nor high school graduates, which is calculated using the decennial census data from IPUMS.

²⁸ Also, interjurisdictional competition seems most likely to be an amenity, thus its correlation with segregation tends to bias our baseline estimates toward zero. In that case, the true preferences for segregation could be even stronger than estimated here.

²⁹ Instead of estimating β_h separately and moving the housing price term to the left-hand side as in Eq. (8'), we also tried to estimate β_h simultaneously with other preference parameters according to Eq. (8), instrumenting for segregation index in the same way. It turns out that this makes a difference only for the 1980 black sample: If not moving the housing price term to the left-hand side, individuals in the 1980 black sample show a willingness to pay for more segregation (statistically significant only at the 0.1 level).

³⁰ The data for black share in 1890 also come from Cutler et al. (1999), where it is measured at the central city level because data at the metropolitan area level were not available at that time.

the sample size drops to roughly 120 cities for all six samples, the estimates are very similar to the baseline estimates.

Overall, these sensitivity analyses suggest that except for the 1980 blacks, the baseline estimates are robust to a variety of alternative specifications. We also learned that cross-region variations are necessary for estimating key model parameters, thus one has to keep this in mind when interpreting the results.

5.6. Marginal willingness to pay

We now use our estimates to calculate the marginal willingness to pay (MWTP) for reduced segregation.³¹ For individual i living in city j , the MWTP is defined as

$$MWTP_{ij} = \frac{\partial V_{ij}/\partial S_j}{\partial V_{ij}/\partial W_{ij}} = \frac{\beta_{is} W_{ij}}{\beta_w S_j} = \frac{\beta_s + \sum_c z_{ic} \beta_{sc}}{\beta_w} \frac{W_{ij}}{S_j}.$$

For simplicity, we suppress race and year subscripts. The first equality represents the standard definition of MWTP in the literature (Rosen, 1974; Roback, 1982), i.e., the implicit price of segregation equals the marginal rate of substitution between segregation and income at a given utility level. Replacing $\partial V_{ij}/\partial S_j$ and $\partial V_{ij}/\partial W_{ij}$ with parameters in the indirect utility function (Eq. (3)) yields the second equality; allowing for heterogeneous preferences for segregation gives the third equality. As this formula indicates, MWTP varies with individual characteristics, increases with income, and decreases with a city's segregation level.

To facilitate comparisons across races and census years, we separately calculate the MWTP of a representative black and a representative white using the 1990 median household income ($W_{ij} = \$35,763$ in 1999 dollars) and the 1990 mean dissimilarity index ($S_j = 0.568$). β_w and β_{sc} are taken from Table 3. Table 7 reports the estimated marginal willingness to pay by blacks and whites for a one percentage point reduction in the dissimilarity index—a change in the residential pattern that would involve relocating 1% of blacks in the city to less segregated neighborhoods. The first and second rows present the mean MWTP calculated using the baseline OLS and 2SLS (with fiscal instruments) estimates of β_s , i.e., coefficients of log dissimilarity index from Panel A in Tables 4 and 5. It is clear that the estimated mean MWTP increases substantially for both blacks and whites after instrumenting for segregation.

Moving across the columns in row 2, we see that black mean MWTP for reduced segregation rises dramatically from \$89 in 1980 to \$529 in 1990 and then drops slightly to \$436 in 2000. The increase from 1980 to 1990 is striking and requires further scrutiny. Our sensitivity analysis shows that this big increase is robust to various specifications. One possible explanation is that to blacks, segregated cities in 1990 and 2000 represent very different living environments from those in 1980. As discussed in the theory section, segregation subjects blacks to various neighborhood effects. One such effect is that neighborhood-based social networks provide an important source of job information and referral (Bayer et al., 2008). As emphasized in the recent literature, benefits from social contacts depend on the human capital levels and employment status of such contacts.³² If blacks in segregated cities are increasingly living with lower-skilled black neighbors over time, then they benefit less from such neighbors and should be willing to pay more to avoid segregated cities. To explore this possibility, we use IPUMS data to construct two variables to measure the skill level of blacks in each city: (1) the share of blacks

³¹ Marginal willingness to pay is an appropriate welfare measure for marginal changes in segregation. However, for non-marginal changes, households may be better off by changing residential locations, and housing suppliers may be better off by adjusting the quality or quantity of housing supply. Hence, the estimated willingness to pay at the originally chosen location will underestimate the total true benefits or costs. See Bartik (1988) for an extended discussion of this topic.

³² See, for example, Montgomery (1994), Calv-Armengol and Jackson (2004), and Damm (2014).

aged 18 or more with a college degree; and (2) the unemployment rate of blacks aged 18 or more. We rerun the city level regression controlling for these variables and then calculate blacks' willingness to pay to avoid segregation. In both cases, we still find a sharp increase in blacks' willingness to pay after 1980.

Since these exercises controlled for the skill level of blacks only at the city level, there remains the possibility that middle- and upper-class blacks moved to suburban areas within the same city, leaving poor blacks in inner city neighborhoods. As emphasized by Wilson (1987), the flight of higher-income black families out of the city made it difficult to sustain basic institutions (e.g., stores, schools, and churches) in traditionally black communities, which triggered the decline of many black neighborhoods into a dangerous, hopeless, and miserable poverty trap. While this is a plausible explanation, the IPUMS data cannot be used to verify it because the data do not have refined geographic identifiers.

An alternative explanation is that this increase in blacks' willingness to pay reflects a large cohort effect. Young blacks in 1980 grew up in the 1950s, when segregation and racial discrimination were institutionalized and tolerated. In contrast, the next two cohorts grew up in the 1960s and 1970s when the Civil Rights Movement greatly altered public attitudes toward segregation. It is possible that blacks in the younger cohorts have much stronger negative feelings against segregation and thus are willing to pay more to avoid it.³³

For whites, the mean MWTP falls from \$675 in 1980 to \$430 in 1990 and then to \$301 in 2000, indicating that whites disutility from segregation decreased over the past few decades. One interpretation may be that whites increasingly viewed the decline of segregation after 1970 as a positive and encouraging trend. Historically, segregation was enforced primarily through collective actions of whites, including the use of segregation policies, overt discrimination, and illegal threats (Myrdal, 1944; Cutler et al., 1999). Following the Civil Rights Movement, many whites might have had a strong distaste toward segregation due to feelings of white guilt. Over time, the steady decline of segregation may have somewhat relieved such feelings, subsequently decreasing their willingness to pay to reduce segregation. In addition, younger generations of whites lack personal connections to past racial oppression, and therefore may be less motivated to dismantle segregation.

Rows 3–5 in Table 7 report the heterogeneity in MWTP for segregation, calculated from the estimated coefficients on individual attributes interacted with segregation (Table 3). Only some of these estimates are economically significant. For example, in the 1990 black sample, blacks who are one year younger are willing to pay \$30 more. In other words, there is a \$300 difference in the MWTP between 25 and 35 year old blacks. In the 1990 white sample, whites with children are willing to pay \$31 more than those without children. In every sample, the effects of education on MWTP are economically insignificant. That is, blacks or whites at different education levels have rather similar negative preferences for segregation.

6. Conclusion

Previous studies examine the effects of segregation on a limited number of observed outcomes. Deviating from this literature, this paper estimates people's willingness to pay for different degrees of racial segregation at the city level, treating segregation as an urban disamenity. This willingness to pay not only incorporates the overall effects of segregation, both observed and unobserved, but also reflects people's tastes

toward segregation. Given the features of our approach, our estimates are particularly suitable for welfare analysis.

Our empirical analysis provides new evidence that over the 1980–2000 period, segregation is bad not only for young blacks, but also for young whites. The estimated mean preferences for less segregation are economically significant. For a one percentage point reduction in the dissimilarity index, the average black is willing to pay \$529 in 1990 and \$436 in 2000; the average white is willing to pay \$675 in 1980, \$430 in 1990, and \$301 in 2000. These findings suggest that reducing segregation at the city level could generate substantial social benefits for both blacks and whites.

We conclude by discussing two limitations of this study. First, our analysis has focused on full-time workers. This allows us to get around the problem of self-selection between working and not working full time, and thus makes the estimation of counterfactual wages more straightforward. However, full-time workers differ from part-time or unemployed individuals. For example, we find that full-time workers tend to have higher levels of education, higher probabilities of being male and being married, lower probabilities of having children, and, not surprisingly, higher earnings. Although full-time workers constitute the majority of the population, one might argue that unemployed individuals are likely to be more affected by segregation and thus have different willingness to pay. Unfortunately, data and methodological constraints do not permit a straightforward and convincing way to incorporate part-time workers and unemployed individuals into our estimation. Indeed the few existing related studies, including Dahl (2002), Bajari and Kahn (2005), and Bayer et al. (2009), all focus on full-time workers.

A second limitation is our use of selective city samples for estimation. Detailed analysis reveals that cities in our white samples are representative of the whole population of metropolitan statistical areas, thus they are unlikely to bias the estimates for whites. However, because we require a minimum of 10,000 black residents, cities in our black samples are not fully representative—they tend to be larger and have a higher share of black population. Perhaps blacks systematically avoid cities with few black residents. However, expected earnings cannot be precisely estimated for blacks in cities with small black populations. Consequently, one cannot know whether segregation or lower earnings have made these cities less attractive to blacks. It is thus unclear whether dropping these cities has introduced any bias to our estimates. We hope that future work will address these limitations.

Appendix A. Data appendix

A.1. Household data

Data on internal migrants are drawn from the 1980 1%, 1990 1%, and 2000 5% Integrated Public Use Microdata Series (IPUMS). For blacks in each census year and whites in each census year, we randomly draw 20,000 household heads aged 25–35 who (i) are born in one of the 48 contiguous states plus the District of Columbia, (ii) work at least an average of 20 hours per week, (iii) work at least 10 weeks for pay in the previous year, (iv) earn at least 2000 dollars (in 1999 dollars), and (v) live in one of the identifiable metropolitan areas in IPUMS. For black samples, we only select the metropolitan areas with at least 10,000 blacks. For white samples, we only select the metropolitan areas with at least 1000 blacks. Some descriptive statistics of these household heads are shown in Table A1.

A.2. Data samples for wage and housing price regressions

The data for wage and housing price regressions also come from the 1980 1%, 1990 1%, and 2000 5% IPUMS. The wage variable is the reported annual pre-tax earnings from the previous year. Housing price is the value of a housing unit estimated by the respondent at the survey time. In all three census years, the respondents were

³³ Our analysis focuses on full-time workers only and the composition of full-time workers changed over time (as shown in Table A1). This may result in some artificial cohort effect due to non-random sample selection. However, this kind of bias is unlikely to produce significant changes in estimated willingness to pay across samples, especially that some of the individual characteristics are controlled in both the estimation of model parameters and the calculation of the willingness to pay.

Table A1
Descriptive statistics of household heads.

	Blacks			Whites		
	1980	1990	2000	1980	1990	2000
Less than high school	0.176	0.077	0.064	0.101	0.063	0.047
High school	0.434	0.372	0.464	0.339	0.305	0.329
Some college	0.240	0.382	0.284	0.234	0.309	0.234
College or more	0.150	0.169	0.188	0.326	0.323	0.389
Male	0.632	0.542	0.459	0.824	0.764	0.716
Marriage status	0.740	0.602	0.503	0.818	0.740	0.678
Have a child	0.641	0.624	0.649	0.562	0.517	0.488
Income	26,163 (15,059)	26,183 (16,939)	28,124 (22,555)	35,794 (20,410)	39,189 (29,340)	42,321 (38,617)

Notes: All cell entries are sample means except in the last row the standard deviations of income are presented in parentheses. Data come from the 1980 1%, 1990 1%, and 2000 5% IPUMS.

Table A2
Summary of wage regression estimates.

	1980		1990		2000	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Constant	7.670	0.366	7.505	0.481	7.724	0.260
White	0.140	0.176	0.153	0.251	0.150	0.111
High school graduates	0.230	0.097	0.255	0.132	0.251	0.073
Some college	0.338	0.122	0.438	0.141	0.437	0.089
College degree or more	0.584	0.135	0.783	0.157	0.810	0.111
Male	0.644	0.095	0.528	0.090	0.452	0.067
Age	0.065	0.015	0.071	0.018	0.066	0.011
Age square	-0.0006	0.0002	-0.0007	0.0002	-0.0006	0.0001
Marriage status	0.148	0.073	0.175	0.078	0.175	0.043
Observations (cities)	258		281		288	

Notes: This table summarizes the estimates from city-specific wage regressions for 1980, 1990, and 2000. Data come from the 1980 1%, 1990 1%, and 2000 5% IPUMS.

Table A3
Descriptive statistics of housing characteristics.

Variables	Data description	1980	1990	2000
Rooms2	2 rooms in dwelling	0.002	0.008	0.011
Rooms3	3 rooms in dwelling	0.014	0.032	0.040
Rooms4	4 rooms in dwelling	0.079	0.103	0.085
Rooms5	5 rooms in dwelling	0.230	0.220	0.206
Rooms6	6 rooms in dwelling	0.284	0.245	0.232
Rooms7	7 rooms in dwelling	0.189	0.176	0.175
Rooms8	8 rooms in dwelling	0.114	0.114	0.128
Rooms9	9 rooms in dwelling	0.088	0.104	0.124
Bedrooms1	1 bedrooms in dwelling	0.024	0.033	0.036
Bedrooms2	2 bedrooms in dwelling	0.218	0.215	0.190
Bedrooms3	3 bedrooms in dwelling	0.531	0.516	0.503
Bedrooms4	4 bedrooms in dwelling	0.191	0.199	0.226
Bedrooms5	5+ bedrooms in dwelling	0.037	0.038	0.045
Acreprop1-	Acreage of property less than 1 acre	0.893	0.829	0.825
Acreprop1-9	Acreage of property 1-9 acre	0.107	0.136	0.152
Acreprop10+	Acreage of property 10+ acre		0.035	0.024
Kitchen	With kitchen in the dwelling	0.993	0.998	0.998
Plumbing	With plumbing in the dwelling	0.996	0.997	0.997
Year1	Years of dwelling 0-1	0.030	0.023	0.030
Year2	Years of dwelling 2-5	0.096	0.105	0.101
Year3	Years of dwelling 6-10	0.106	0.102	0.100
Year4	Years of dwelling 11-20	0.214	0.244	0.196
Year5	Years of dwelling 21-30	0.235	0.205	0.213
Year6	Years of dwelling 31-40	0.114	0.216	0.174
Year7	Years of dwelling 41-50	0.205	0.105	0.186
Unit1	Mobile home or trailer		0.068	0.074
Unit2	Boat, tent, van, other		0.006	0.001
Unit3	1-family house, detached	0.946	0.819	0.806

Table A3 (continued)

Variables	Data description	1980	1990	2000
Unit4	1-family house, attached	0.054	0.056	0.064
Unit5	2-family building		0.012	0.009
Unit6	3–4 family building		0.007	0.009
Unit7	5–9 family building		0.008	0.009
Unit8	10–19 family building		0.007	0.007
Unit9	20–49 family building		0.007	0.008
Unit10	50+ family building		0.011	0.013
Observations		288,938	346,026	1,898,960

Notes: This table summarizes the characteristics of owner-occupied houses used in baseline housing price regressions. All cell entries are sample means. Data come from the 1980 1%, 1990 1%, and 2000 5% IPUMS.

asked to choose a housing value from a set of intervals (e.g., \$60,000 to \$69,999) in the questionnaires. The housing price variable published by IPUMS is the midpoint of the interval answered by each respondent.

For wage regressions, we select all blacks and whites aged 20–65 who satisfy the same sample selection criteria (i)–(v) as just described above. For baseline housing price regressions, we only select owner-occupied houses that are located in one of the identified metropolitan areas in IPUMS. Housing characteristics include the number of rooms, number of units in a structure, number of bedrooms, acreage of property, age of structure, available kitchen and plumbing facilities. We exclude houses with one room and those with a missing price. Mean housing characteristics are presented in Table A3. In our robustness analysis, we also make use of data on rental units.

A.3. Segregation data and instrumental variables

From Cutler et al. (1999) and Glaeser and Vigdor (2001), we obtain the dissimilarity index, city population, and the percent of city

population that is black in 1980, 1990, and 2000, which they originally calculated or drew from the U.S. decennial census data. Our fiscal instruments, number of governments and government revenue sharing in 1962, come from Cutler and Glaeser (1997), who constructed the variables using data from Census of Governments. To count the number of governments, they include all municipal and township governments in the boundary of an MSA as defined in 1990. To measure the share of local government revenue that comes from intergovernmental transfers, they first calculate this share for all local governments and then take the average over all local governments in each state. They use the same state average share for all the cities in the state. All these data from Cutler and Glaeser (1997), Cutler et al. (1999), and Glaeser and Vigdor (2001) are available at the following website: <http://web.archive.org/web/20031005171713/trinity.aas.duke.edu/jvigdor/segregation>. Our topographic instruments come from Rothstein (2007) and are available at his web page: <http://gsppi.berkeley.edu/faculty/jrothstein>. We use the numbers of inter- and intra-county rivers through a metropolitan area, as per Rothsteins (2007) definition.

Table A4
Estimates from housing price regressions.

	1980		1990		2000	
Rooms3	-0.0380	(0.0283)	0.127***	(0.0167)	0.126***	(0.00661)
Rooms4	0.0213	(0.0277)	0.153***	(0.0176)	0.0891***	(0.00695)
Rooms5	0.209***	(0.0277)	0.331***	(0.0178)	0.242***	(0.00696)
Rooms6	0.376***	(0.0278)	0.494***	(0.0179)	0.383***	(0.00699)
Rooms7	0.549***	(0.0278)	0.652***	(0.0180)	0.520***	(0.00703)
Rooms8	0.697***	(0.0279)	0.791***	(0.0181)	0.647***	(0.00708)
Rooms9	0.914***	(0.0280)	0.989***	(0.0182)	0.852***	(0.00714)
Bedrooms2	0.0729***	(0.00949)	0.134***	(0.00975)	0.144***	(0.00404)
Bedrooms3	0.116***	(0.00965)	0.168***	(0.0103)	0.210***	(0.00426)
Bedrooms4	0.116***	(0.00995)	0.214***	(0.0106)	0.291***	(0.00438)
Bedrooms5	0.131***	(0.0114)	0.235***	(0.0115)	0.382***	(0.00484)
Kitchen	0.137***	(0.0141)	0.201***	(0.0297)	0.194***	(0.0144)
Plumbing	0.783***	(0.0251)	0.401***	(0.0277)	0.145***	(0.0110)
Acreprop1–9	0.161***	(0.00322)	0.136***	(0.00274)	0.231***	(0.00119)
Acreprop10+			0.318***	(0.00637)	0.458***	(0.00346)
Year1	0.681***	(0.00511)	0.519***	(0.00598)	0.476***	(0.00234)
Year2	0.619***	(0.00354)	0.472***	(0.00392)	0.422***	(0.00153)
Year3	0.497***	(0.00356)	0.368***	(0.00405)	0.331***	(0.00151)
Year4	0.418***	(0.00309)	0.234***	(0.00349)	0.212***	(0.00131)
Year5	0.318***	(0.00303)	0.150***	(0.00348)	0.0678***	(0.00126)
Year6	0.164***	(0.00379)	0.0991***	(0.00339)	0.0263***	(0.00128)
Unit1			-1.263***	(0.00542)	-1.291***	(0.00245)
Unit2			-0.0550***	(0.0136)	-1.200***	(0.0273)
Unit4	-0.207***	(0.00482)	-0.132***	(0.00384)	-0.108***	(0.00158)
Unit5			0.0929***	(0.00937)	0.0743***	(0.00463)
Unit6			-0.00823	(0.0115)	-0.0813***	(0.00451)
Unit7			-0.116***	(0.00956)	-0.169***	(0.00411)
Unit8			-0.128***	(0.0110)	-0.177***	(0.00504)
Unit9			-0.109***	(0.0119)	-0.102***	(0.00571)
Unit10			0.0898***	(0.0126)	0.104***	(0.00527)
R ²	0.562		0.674		0.611	
Observations	288,938		346,026		1,898,960	

Notes: Data come from the 1980 1%, 1990 1%, and 2000 5% IPUMS. Robust standard errors adjusted for heterogeneity are in parentheses. City-specific constants are included in every regression, but not reported here.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

A.4. Other metropolitan area controls

We include a number of other metropolitan area controls. The first three control variables are median household income, the share of workers employed in manufacturing, and the share of adults aged 25 or older with a college or higher degree. These three variables are either calculated using the 1% IPUMS data (for 1980) or directly drawn from the U.S. decennial census summary tape files (for 1990 and 2000). The fourth control is population density, which is calculated by dividing metropolitan area population by land area. The data for land area is taken from the 1990 U.S. Gazetteer Files. For metropolitan areas in 1980, 1990, and 2000, we aggregate county-level land area for non-New England metropolitan areas, and aggregate city- and town-level land area for New England metropolitan areas.

The final control is the Gini coefficient. Following Glaeser et al. (2009), the Gini coefficient for one city is defined as $1 - \frac{1}{\bar{y}} \int_0^{\bar{y}} (1 - F(y))^2 dy$, where \bar{y} is mean income and $F(y)$ is the share of population with income less than y . The value of the Gini coefficient ranges between 0 and 1, with 0 indicating complete equality and 1 indicating extreme inequality. We measure Gini coefficients using total household income from the 1980 1%, 1990 1%, and 2000 5% IPUMS. The census income is top coded at \$75,000 in 1980, \$400,000 in 1990, and \$999,998 in 2000. We use top-coded income, which will underestimate Gini coefficients.

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