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Some of the data used in this analysis are derived from sensitive data files of the Panel Study of Income Dynamics, obtained under special contractual arrangements designed to protect the anonymity of respondents. The data are not available from the author. Persons interested in obtaining PSID sensitive data files should contact Camille Ward, The Panel Study of Income Dynamics, Institute for Social Research, P.O. Box 1248, Ann Arbor, MI 48106-1248, phone (734) 763-5166.

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BLACK-WHITE SEGREGATION, DISCRIMINATION, AND HOME ONWNERSHIP

Abstract

The effect of discrimination on black-white racial segregation is studied using a confidential supplement of the Panel Study of Income Dynamics (PSID). Audit studies reveal that the rate of discrimination in rental housing is substantially higher than in owner-occupied housing. Thus, a variable indicating home ownership is used to proxy for the discrimination rate faced by blacks. The fixed-effects estimates of segregation imply that home ownership is associated with a decline in black-white segregation. This effect decreases slightly at higher income levels but increases substantially with the education of the head of household. Evidence is presented that the effect of discrimination on segregation disappears in cross-sectional data but reappears when using a panel and controlling for fixed-effects. The findings of this study suggest that increased government enforcement of fair housing laws may have a quantitatively different effect on different segments of society and that future research on racial segregation should emphasize the use of panel, as opposed to cross-sectional, data.

I. INTRODUCTION

This paper uses information from a series of audit studies conducted by the U.S. Department of Housing and Urban Development (HUD) and local housing authorities to estimate the empirical relationship between racial discrimination in housing and racial segregation. These audit studies, which send pairs of black and white testers to the same realtor or apartment complex to determine the presence of discrimination, not only establish that discrimination in housing markets exists, but also quantify how the discrimination rate varies with housing tenure (Galster [1990] and Yinger [1992]). These results are used in this paper to identify a proxy for racial discrimination, a dummy variable indicating whether a person owns a home or rents, that can be applied to the Panel Study of Income Dynamics (PSID). This proxy is then used to estimate how variation in the discrimination rate faced by blacks is associated with variation in black-white racial segregation while holding constant other relevant variables.

Homeownership is a good proxy for the discrimination rate for two reasons. First, audit studies report higher rates of discrimination in rental markets than in owner-occupied housing. Second, rental and sales housing markets are well integrated. Rental units and owner-occupied homes are found in essentially all of the neighborhoods—defined as a census tract—where PSID respondents reside regardless of whether the individual rents or owns a home. To the extent that higher rates of discrimination in housing availability associated with rental housing increase housing

segregation, blacks and whites with the same socio-economic characteristics should live in more integrated neighborhoods when they own a home compared to when they rent. In fact this is the primary finding reported in this paper. The other major finding of this paper is that the degree to which racial segregation decreases with homeownership varies substantially with other socioeconomic characteristics, particularly educational attainment. These two results suggest that discrimination is an important factor contributing to black-white segregation and that the effect of discrimination on segregation varies considerably across individuals.

This fusion of information found in audit studies to a nationally representative data set permits for the first time the construction of direct evidence on the relationship between the incidence of racial discrimination in housing and housing segregation. In contrast, previous research on discrimination as a cause of segregation (Taeuber [1968]; Kain [1976, 1986]; and Massey and Denton [1993]) relies on indirect evidence derived from the residual method. The residual method models segregation by explicitly controlling for factors (typically income) other than discrimination. It then assigns the portion of segregation not explained by the model, or the residual, to discrimination. A more detailed critique of the residual method is presented in Section II.

An empirical estimate of how discrimination affects segregation may be of interest to policymakers and social scientists who are concerned with the causes of racial inequalities. Recent empirical studies suggest that racial segregation in housing may be partially responsible for the relatively poor social and economic outcomes of minorities compared to whites. Cutler and Glaeser [1997] estimate that a one standard deviation decline black-white segregation would narrow the black-white gap in schooling (high school and college graduation rates), employment (labor force participation rates and earnings) and single parenthood by about one-third. Furthermore, recent reviews (Kain [1992]; Holzer [1991]) of the spatial mismatch literature indicate that the employment prospects of central city residents, especially young and unskilled laborers, have been adversely affected by a geographic shift in the location of entry level jobs away from traditionally black and Hispanic central cities and toward typically white suburban areas. Thus by isolating minorities to low job growth areas, racial segregation increases spatial mismatch and contributes to poor labor market outcomes. Other studies have linked high levels of racial segregation to poor educational attainment (Orfield [1993, 1997]), increased infant and adult mortality rates (La Viest [1989, 1993];

Polednak [1991, 1993]; and Collins and Williams [1999]), increased homicide rates (Peterson and Krivo [1993, 1999]) and even decreases in voter turnout (Cohen and Dawson [1983]).

A unique aspect of this paper is its use of panel data to measure segregation. While previous studies of segregation have used cross-sectional data, mostly from the decennial census, this study uses a confidential supplement to the PSID. Using the PSID instead of data from the decennial census has several advantages. First, most nationally representative panel data sets, such as the PSID, have more detailed information on individual characteristics compared to data derived from the decennial census. For confidentiality reasons, data from the decennial census are limited to race, census tract, and one socioeconomic variable of interest, typically income. The advantage of using the confidential supplement of the PSID is that researchers are able to control for a wider range of factors when modeling racial segregation. Second, panel data allow the use of individual specific (fixed) effects. Using fixed-effects in a model of segregation is appealing because under a reasonable assumption they can be interpreted as preferences over the racial composition of one's neighborhood. The necessary assumption is that these preferences are stable over time. Controlling for these preferences is important because presumably voluntary segregation is an important aspect of black-white segregation.[1] Thus, the researcher who uses panel data can simultaneously control for very detailed individual-level socioeconomic characteristics, the preferences of the individual and the discrimination rate by using homeownership as a proxy.

Furthermore, evidence is presented here that the omission of fixed-effects leads to a large bias in the model's parameter estimates. This bias is severe enough that the estimated effect of discrimination on segregation is reduced to, on average, zero when fixed-effects are ignored but is quantitatively large and statistically significant when fixed-effects are added.

Previous research on racial segregation has relied exclusively on cross-sectional data, such as the decennial census, most likely because current measures of segregation are defined over the population of a geographic region, such as a metropolitan area, and therefore can only be applied to data sets from censuses. For this reason, prior research on racial segregation measures has ignored nationally representative survey data such as the PSID that includes samples from many different geographic regions but no population counts. In order to overcome this limitation, a new measure of

[1]See Schelling [1971] for a discussion of how black and white preferences over the racial composition of one=s

racial segregation is developed in this paper, which is defined over a nationally representative sample taken across several metropolitan areas.

The essay is organized as follows. Section II provides a critique of the residual method. Section III reviews evidence from audit studies and shows that the rate of discrimination in rental housing is substantially higher than in owner-occupied housing. Section IV develops a segregation measure that can be applied to data sampled from many different geographic regions. Section V discusses the data and sample restrictions. A simple method for calculating the new segregation measure is found in section VI. Section VII lays out the estimation strategy and presents the fixed-effects results. Section VIII compares the fixed-effects results to results obtained when the PSID is treated as a cross-section. Section IX concludes.

II. A CRITIQUE OF THE RESIDUAL METHOD

Several researchers have speculated that housing segregation could be reduced through policies that lower the discrimination rate in housing. Yet this claim relies on indirect evidence from studies that utilize the residual method (for example, Taeuber [1968]; Kain [1976, 1986]; and Massey and Denton [1993]), an empirical strategy that estimates the effect of inter-racial economic differences on racial segregation and then assigns the remaining unexplained component to housing discrimination.

These studies assume that the effect of discrimination on segregation can be measured by examining the segregation of blacks and whites that have similar social or economic characteristics. The assumption used in these studies is that discrimination is the only cause of segregation that remains after controlling for inter-racial economic differences. Typically, such studies construct a segregation measure[2] conditional on a single dimension of economic status, usually income. For each income group, this segregation measure is computed for all black and white individuals who fall within a given range of income. For instance, for 30 different MSAs Massey and Denton [1993]

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neighborhood may interact to produce voluntary segregation in housing.

[2]The most commonly used segregation measure is the dissimilarity index, a measure that is bounded between zero and one and represents the percentage of blacks that would have to move in order to achieve full integration. See Cortese, Falk and Cohen (1976) for a fuller discussion of segregation indices.

calculate a segregation measure for blacks and whites who have annual incomes less than $2,500, between $25,000 and $27,500, and more than $50,000. On average the segregation measure is equal to 74.4 for blacks and whites in the low-income category, 66.7 for those with incomes in the middle range and 72.8 for those with high incomes. The finding that higher levels of economic status do not result in substantially lower levels of racial segregation then forms the basis for the claim that discrimination is the principal cause of segregation.

There are three important limitations to this methodology. First, housing segregation is caused not only by economic inequality and discrimination but also by other important factors, such as voluntary segregation. One would expect such factors to be in the "residual" segregation not explained by inter-racial income differences. Thus, the effect of discrimination on segregation is not identified.

Second, even if the residual method could identify the total effect of discrimination on segregation, the effect of a discrete reduction in discrimination on segregation would not be known. Causation and responsiveness are logically separate concepts. For instance, if a zero price implies infinite demand for a good, it does not follow that all price reductions for that good will result in an elastic change in demand. Likewise, while it might be the case that discrimination is largely responsible for black-white segregation, it does not follow that a policy that effectively reduces, but does not eliminate, discrimination would substantially decrease housing segregation. The function that maps discrimination into segregation may have a few flat spots, even if it passes through the origin. Thus, even if the residual method accurately identifies the aggregate effect of discrimination on segregation, it would not identify the effect of a policy that reduces but does not eliminate discrimination.

Third, the effect of discrimination on segregation may vary across different groups within minority communities. By construction, the residual method cannot be sensitive to this issue. According to this technique, if the segregation levels of high and low income blacks are similar (different) then discrimination is said to be an important (unimportant) determinant of segregation. Yet this comparison requires that high and low income blacks be equally responsive to discrimination. To the extent that this responsiveness varies by economic status, the residual method mis-measures the aggregate effect of discrimination on segregation.

The methodology presented here addresses these shortcomings. First, because the PSID contains detailed information on individuals, the estimates of segregation in this paper are able to control for a richer set of socioeconomic variables compared to previous studies. Second, the proxy used in this case corresponds to a discrete change in the discrimination rate that may be similar to a change in enforcement policy that produces a moderate decline in the discrimination rate. If so, the estimates found in this paper might be used by policymakers to predict the possible impact of increased enforcement of fair housing laws on segregation. Third, by interacting a proxy for the discrimination rate with socioeconomic characteristics, it is possible to identify how the impact of discrimination on segregation might vary with these variables of interest.

III. THE AUDIT EVIDENCE

The only direct evidence on the rate of discrimination in housing comes from audit studies. A fair housing audit pairs a white tester with a black or Hispanic tester. Each tester in a pair is given a similar false identity and visits the same rental unit or realtor. At the end of their visit, each tester independently logs information about their visit, including information presented to them regarding the availability of housing. In the HUD supervised audits, discrimination is said to occur if the black or Hispanic tester is treated less favorably than the white tester in at least one category while the white tester is always treated at least as well as the black or Hispanic tester. The audit is termed ambiguous and no discrimination is said to occur if both testers were treated less favorably than the other in at least one category.

HUD has conducted two national studies of discrimination in housing, one in 1977 (Wienk et al. [1979]) and one in the summer of 1989 (Yinger [1991]). The 1977 HUD study inspired a number of local housing authorities to conduct their own audit studies during the 1980s. Galster [1990] reviews these audit studies. These three studies comprise the only nationwide evidence of the pattern of housing discrimination in the United States.

There are several dimensions along which the treatment of auditors is recorded: housing availability, credit assistance and sales effort are three examples. This paper focuses on discrimination in housing availability because it is considered the most severe form of discrimination in housing. According to Wienk et al. [1979]:

> The principal focus of this study is on housing availability for two reasons. First, differential treatment on housing availability is a clear violation of Title VIII of the Civil Rights Act of 1968. Second, differential treatment on housing availability is the most fundamental form of discriminatory practice that a black apartment seeker might encounter. If a rental agent told one auditor that no apartments were available but told the other auditor that something was available, it matters little whether both auditors received the same treatment for each of the other items. Therefore, differential treatment on apartment availability is considered most important.[3] (p. ES-6 – ES-7)

Other types of behavior that qualify as differential treatment in housing availability include differences in the type of housing made available, the number of units inspected, information on waiting lists and the number of units shown. This contrasts with discrimination in housing availability in the sales market. This type of discrimination occurs when a realtor falsely claims that a home is not for sale, offers multiple listing directories to minority customers at a lower rate compared to white customers, or restricts the number of homes offered for inspection or actually inspected to minority customers compared to white customers.

A key issue in the measurement of discrimination is distinguishing random unfavorable treatment from systematic discrimination (Heckman [1992], Yinger [1992]). The former may be the result of random factors such as a realtor's mood or the time of day and likely would have little or no effect on racial segregation in housing since both minority and whites would receive such treatment with equal probability. The latter, however, is likely the result of prejudice against minorities and presumably can have a large effect on racial segregation in housing because it systematically excludes minorities from certain neighborhoods. Random unequal treatment can cause individual instances of unequal treatment to overstate or understate the presence of systematic discrimination. For instance, random factors may cause a discriminating landlord to favor a minority auditor during a particular audit or may cause a non-discriminating landlord to favor the white auditor.

Two methods are used in audit studies to discern the rate of discrimination in data that report instead the rate of unfavorable treatment. The first method assumes that minority auditors receive favorable treatment only as a result of random variation in the behavior of realtors and rental agents.

[3]When discussing discrimination in sales units, Wienk makes essentially the same argument, stating that discrimination in housing availability is the most important form of discrimination faced by black house seekers (ES3–14).

Thus, the discrimination rate is taken to be the difference of the rate of favorable treatment for white auditors and the rate of favorable treatment for minority auditors. Suppose, for example, that white auditors were favored in 30 percent of the audits and minority auditors were favored 10 percent of the time. Then this methodology would imply a discrimination rate equal to 20 percent. This "net incidence" measure is reported as a measure of discrimination in all three major national studies of racial discrimination.[4]

Yinger [1991] points out that the "net incidence" measure may understate the actual rate of discrimination. As previously noted, random factors may lead a discriminating realtor or rental agent to favor a minority auditor, yet using the "net incidence" measure such an audit would not be counted as discriminatory. Yinger proposes constructing a measure of systematic discrimination using the predicted values of a logistic regression. The dependent variable is the probability that a minority auditor is treated less favorably, more favorably or the same as the white tester and the independent variables include characteristics of the testers, realtors and rental agents. If unfavorable treatment of the minority auditor is the most likely of the three outcomes then the audit is classified as discriminatory.

The findings of these studies are presented in table 1. The 1977 HUD study and Galster's review of audits by local housing authorities report only the "net incidence" discrimination rate measure. Both the "net incidence" and logit measure are reported for the 1989 HUD study. Housing audits conducted by HUD in 1977 in 40 different metropolitan areas found that blacks confront racial discrimination 27 percent of the time in rental units and 15 percent of the time in owner-occupied housing (Wienk et al. [1979]). HUD repeated the 1977 study in the summer of 1989 using 20 metropolitan areas. Using the logit measure, discrimination in housing availability against blacks occurred in 46.9 percent of the rental units and 34.2 percent of the sales units in 1989. In that same follow-up study, discrimination occurred 21.3 percent of the time in rental units and 18.0 percent of the time in sales units according to the "net incidence" measure.

[4]Yinger [1992] also reports the raw rate of unfavorable treatment as a discrimination measure. This "gross incidence" measure has the disadvantage of ignoring random factors that lead to unfavorable treatment and thus, likely overestimates the actual discrimination rate. Nevertheless, the gross incidence measure is useful because it likely provides an upper bound on the true discrimination rate.

Galster [1990] reviewed the results of 71 housing audits carried out by local housing authorities during the 1980s. The total number of audit studies available to calculate the rate of discrimination against blacks in housing availability is unfortunately lower. A number of the audit studies focus on discrimination against Hispanics and so are not applicable. Furthermore, many of the written reports that Galster received in response to his survey were incomplete. Nevertheless Galster found that the incidence of racial discrimination against blacks is 47 percent in rental markets and 21 percent in owner-occupied housing markets.

Table 2 presents the results of equality of means tests[5] for the discrimination rates in rental and sales units. It seems unlikely that the different discrimination rates across rental and sales markets reported in Galster's study and the 1977 HUD report are the result of chance. The *p*-value for an equality of means tests for both of these studies is 0.00. However, the *p*-value for an equality of means test for the 1989 HUD study is higher (0.07). In the last row of table 2 an equality of means test is conducted in which the data from the three studies are combined. In this case, the *p*-value for the equality of means test is 0.00. Thus, even though the 1989 HUD audits appear to provide the weakest evidence of higher discrimination rates in rental markets, the cumulative evidence from all three audits taken together strongly indicates that black renters face higher rates of discrimination than blacks searching in the sales market.

These results are not surprising given the different financial incentives and the differences in black-white contact after a contract is signed in the two markets. Most homeowners have a relatively large percentage of wealth at risk when they sell their house compared to the percentage of their wealth that apartment owners risk when attempting to rent a single unit (see Eller and Wallace [1995], Table A, page 3). Thus, risk averse, racially biased homeowners should be more willing to sell to blacks than similarly risk averse, racially biased apartment owners would be willing to rent to blacks. Moreover, many rental agents are salaried while realtors work on commission.

[5]The method used here is the standard equality of means of two independent samples. The random variable in question in these three audit studies, whether or not an individual experiences discrimination, follows a Bernoulli process. A Bernoulli process is fully characterized by its mean and thus any assumption that the populations of two Bernoulli processes have the same mean necessarily implies that these two populations have the same variance. For this reason the test used here is an equality of means test in which it is also assumed that the two samples have the same variance (Greene, [1997]).

Furthermore, when a white owner sells a house to a black buyer, black-white contact is limited to negotiations and closing. After closing the deal the buyer and seller may never meet again. In contrast, a landlord and tenant have a relationship after the rental agreement is signed. Therefore, white homeowners and realtors with equal tastes for discrimination have more financial and personal incentives to sell to black buyers than similar rental agents have to rent to black tenants.

IV. MEASURING SEGREGATION IN PANEL DATA

Traditional measures of segregation[6] are defined over geographic regions, such as a metropolitan area. Typically these measures are used with census data to assign an index of segregation to major metropolitan areas in the United States. Since these measures are defined over geographic areas they can be used only with data that provides information on all locales within a geographic area. As such, these measures cannot be applied to nationally representative survey data.

Since this study uses data from the PSID, a nationally representative survey data set, a new segregation measure is needed. Let $\Delta(X)$ be defined as the difference in the average proportion of blacks in the neighborhoods of observationally equivalent black and white individuals or, mathematically,

$$\Delta(X) = E[b|X = x, B = 1] - E[b|X = x, B = 0] \tag{1}$$

where b is the percentage of blacks in a person's neighborhood, X is a vector of socioeconomic variables such as income and education, B is a dummy variable equal to one if a person is black and

[6]The most popular traditional measure of segregation is the dissimilarity index (D) which is defined as

$$D = \frac{1}{2}\sum_{j=1}^{n}\left|\frac{p_j}{P} - \frac{w_j}{W}\right|$$

where the summation is over neighborhoods $j = 1, 2 \ldots, n$ within the metropolitan area, p_j is the number of black people living in neighborhood j, w_j is the number of white people living in neighborhood j, P is the number of black people in the MSA and W is the number of white people in the MSA. This index ranges from zero to one and is commonly interpreted as the proportion of minority individuals who would need to move in order for each neighborhood to have an equal proportion of minorities. This measure cannot be applied to a typical panel data set because these data sets usually do not provide information on the minority and total populations in each neighborhood within a metropolitan area (Taeuber, [1968]).

the expectation is taken over individuals in the sample. $\Delta(X)$ range from –1 to 1 and applies to individuals with the same set of socioeconomic characteristics X, rather than to individuals in the same metropolitan area.[7] Thus of racial segregation is simply an artifact of economic segregation then $\Delta(X)$ should be equal to zero.

$\Delta(X)$ can also be used to measure segregation across different housing markets. Let $\Delta^r(X)$ and $\Delta^h(X)$ denote the $\Delta(X)$ segregation measure taken over rental and owner-occupied housing units, respectively. These two quantities are defined as

$$\Delta^r(X) = E[b|X = x, R = 1, B = 1] - E[b|X = x, R = 1, B = 0] \tag{2}$$

$$\Delta^h(X) = E[b|X = x, R = 0, B = 1] - E[b|X = x, R = 0, B = 0] \tag{3}$$

where R is a dummy variable equal to 1 if an individual rents. Then, provided three assumptions detailed below, the difference $\Delta^r(X) - \Delta^h(X)$ quantifies the effect that the additional discrimination found in rental compared to sales markets has on segregation.

If one assumes that 1) segregation due to sorting is constant across rental and owner-occupied housing markets; 2) the function that maps discrimination into segregation is the same across both rental and owner-occupied housing; and 3) the discrimination and voluntary sorting components of segregation are additive, then $\Delta^r(X) - \Delta^h(X)$ will measure only the increase in

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[7] $\Delta(X)$ is a modification of the Isolation index (E_{bb}) which is given by

$$E_{bb} = \sum_{j=1}^{n} \frac{p_j}{P} b_j = E[b|B = 1]$$

where p_j is the number of blacks in neighborhood j, P is the population of blacks in the MSA, B is a dummy variable equal to one is an individual is black and b_j is the proportion of residents of neighborhood j who are black. Subtracting from this mean the same expectation taken over whites yields a quantity (Δ) equal to the average difference in the proportion of blacks in a black and white person's neighborhood

$$\Delta = E[b|B = 1] - E[b|B = 0]$$

To account for differences in economic status between whites and blacks, convert the above unconditional expectations into conditional expectations,

$$\Delta = E[b|X = x, B = 1] - E[b|X = x, B = 0]$$

where X is a vector of variables describing the economic status of a given individual.

segregation due to the higher discrimination rate found in rental relative to owner-occupied housing. Note that if

(4) $$\Delta^r(X) = \delta(\alpha_r) + s$$

(5) $$\Delta^h(X) = \delta(\alpha_h) + s$$

then

(6) $$\Delta^r(X) - \Delta^h(X) = \delta(\alpha_r) - \delta(\alpha_h)$$

where α_r and α_h are the discrimination rates in the rental and sales markets, s is the component of segregation due to voluntary sorting and $\delta(\alpha_r)$ and $\delta(\alpha_h)$ are the components of segregation due to the rates of discrimination in the rental and sales markets respectively. If $\Delta^r(X)$ and $\Delta^h(X)$ each consist of a discrimination component plus a sorting component and if the two sorting components are equal, then when $\Delta^h(X)$ is subtracted from $\Delta^r(X)$ the two sorting components cancel, leaving only the difference of the two discrimination components.

Provided that $\Delta^r(X) - \Delta^h(X)$ is positive, this difference can also be interpreted as a lower bound on the decrease in segregation that would occur from eliminating discrimination in the rental housing market. Let the hypothetical change in segregation in the rental market due to an elimination of racial discrimination be given by $\Delta^r(X) - \Delta^0(X)$, where $\Delta^0(X)$ is the level of segregation corresponding to a discrimination rate equal to zero. Because one would expect a positive relationship between the discrimination rate and segregation, it seems reasonable to assume that $\Delta^h(X)$ is at least as large as $\Delta^0(X)$. If $\Delta^h(X)$ is greater than $\Delta^0(X)$, then it follows that $\Delta^r(X) - \Delta^0(X)$ is greater than or equal to $\Delta^r(X) - \Delta^h(X)$.

V. DATA AND SAMPLE RESTRICTIONS

The data used in this study comes from the Panel Study of Income Dynamics (PSID). The PSID contains information on a sample of approximately 37,500 U.S. individuals (men, women and children) and their families beginning in 1968. Because the original intent of the study was to

facilitate the study of poverty, the PSID over-samples low-income individuals. The core data is collected annually and contains economic and demographic information. Special attention is given to income amounts and sources, employment history, family composition and residential location. However, the only geographic data found in the core data set is aggregated at the state level.

A confidential supplemental data set, the PSID Geocode Match Files, has been acquired with permission from the Institute for Social Research (ISR) at the University of Michigan. This data set provides a detailed portrait of the neighborhood environment of the PSID respondents from 1968–1985. The data set is unique because of the level of disaggregation of the neighborhood variables. The PSID Geocode Match Files is the only United State data set that provides geographic information at the census tract/block numbering area level. A census tract/block numbering area is a small, relatively permanent area containing approximately 5,000 people. Usually census tracts are found in urban areas, while block-numbering areas are usually found in more rural regions.

For every year of the survey, individuals are assigned several geocodes. The geocodes designate the country, state, county, zip code, and census tract/block numbering area of residence for each respondent. Two sets of geocodes are provided: codes which correspond to the coding scheme used by the Census Bureau for the 1970 census and codes which correspond to the coding scheme used by the Census Bureau for the 1980 census. In addition to the raw geocodes, ISR provides two extract files from the 1970 and 1980 population censuses. These files contain geocodes as well as descriptive statistics. Information on the geocodes includes empirical income distributions, racial composition, welfare participation, labor force participation and occupational mix. The census files can then be merged with the individual and family files by the selected geocodes to create a data set that contains detailed individual, familial and neighborhood characteristics.

The only neighborhood characteristic extracted from the Geocode Match Files is the percentage of blacks in a person's census tract/block numbering area (hereafter called "percent black"). If no data were available from a person's census tract/block numbering area then the data from the person's enumeration district were used. An enumeration district is the work area of a census enumerator and is approximately the same size as a census tract.

For 1968–1970 only data from the 1970 census were used to calculate percent black. For 1980–1985 only data from the 1980 census were used to calculate percent black. For 1971–1979 a weighted average of percent black from the 1980 census and percent black from the 1970 census was

used to calculate percent black for each year. The weights given to each census data point sum to one and diminish as the absolute difference between the year in question and the year of the census increase. For example, in 1974 and 1977 the data from the 1970 census were given a weight of 0.6 and 0.3 respectively and the data from the 1980 census were given a weight of 0.4 and 0.7 respectively.

For every year that a head of household is in his prime earning years, 25 to 65 years old, the following variables were extracted from the PSID individual, family and Geocode Match Files:

- the percentage of blacks in the person's census tract or enumeration district,
- the MSA of residence,
- the year of the survey,
- the family's income to needs ratio,
- the education of the head of the household,
- the census region of residence,
- whether the family rented, owned its home or neither, and
- the race of the head of household.

The PSID constructs the income to needs ratio by dividing nominal family income by a measure of the poverty level based on the family's food needs expressed in 1968 dollars. The numerator was deflated to 1968 dollars using the January Consumer's Price Index from 1968 and the year in which the income was earned.

The education variable was converted to three dummy variables. High school is equal to one if a person has a high school degree or equivalent and has not received any college training. Some college is equal to one if a person has a high school degree and has received some college training but has not received a four-year degree. College is equal to one if a person has a four-year college degree. The census region of residence variable was converted to a dummy variable (South) which is equal to one if a person lives in the Southern Census region (DE, MD, DC, VA, WV, KY, TN, NC, SC, GA, FL, AL, MS, AR, LA, TX, and OK). The variable describing housing status was converted to a dummy variable (Rent) equal to one if the family was renting, equal to zero if it owned its home and was coded as missing otherwise. The variable describing the race of the head of

household was converted to a dummy variable (Black) equal to one if the individual was black and equal to zero otherwise.

Since the audit data reviewed applies only to discrimination against blacks, the sample is restricted to black and white head of households. A small number of individuals classified themselves as black and white in different survey years. These individuals were excluded from the sample.

Inference is restricted to heads of households who are observed both renting and owning a home in the same MSA while in the panel in order to avoid confounding the independent effect of homeownership on segregation with the effect of migration across MSAs.[8] For example, suppose an individual rented an apartment in Chicago from 1968–1969, lived in a Chicago area home from 1970–1980, then lived in a Milwaukee home from 1981–1985. This individual would be in the sample from 1968–1980. The years in Milwaukee would be excluded since the individual did not both rent and own a home while living in this metropolitan area.

Table 3 provides some summary statistics for the 25,297 annual observations included in the sample used for analysis. From 1968–1985, 48 percent of the sample is black, 42 percent live in the South, and 43 percent rent. The head of households typically have 1.6 children living with them, have a family income 2.8 times the poverty rate and live in a neighborhood composed of 37 percent blacks. Sixty percent (0.3053 + 0.1491 + 0.1453) of these heads of household have a high school degree or more while only 15 percent have a four-year college degree.

The difference between blacks and whites, renters and homeowners are striking. Blacks are much poorer than non-blacks, have a lower probability of receiving a high school diploma and a much lower probability of receiving a four-year college degree. Blacks are more likely to live in the South, are more likely to rent and have more children. Blacks typically live in a neighborhood composed of 70 percent blacks while whites typically live in a neighborhood consisting of five percent blacks. Similarly, renters are poorer and less educated (lower rates of high school and college graduation) than homeowners, are more likely to live in the South and typically live in a

[8]This sample restriction *decreases* the measured effect of homeownership on segregation. When the fixed-effects model detailed in Section VII is estimated using the unrestricted sample, the implied decrease in segregation associated with homeownership is substantially larger compared to the decline in segregation that is implied by the coefficient estimates obtained from the same model using the restricted sample.

neighborhood with a higher percentage of blacks. However, unlike the black to non-black comparison, renters have fewer children than homeowners. Renters are also more likely to be black than are homeowners.

The data also reveal that most people who rent live in neighborhoods with a substantial proportion of people who own and vice-versa. Renters live in census tracts in which 46 percent of the people are homeowners. Homeowners live in neighborhoods in which 31percent of the people are renters. Of the 25,297 person-year observations, there are only six in which people live in census tracts containing either all renters or all homeowners. The fact that these markets overlap suggests that people choose over a reasonably similar set of census tracts when they own versus when they rent holding constant income, education and other relevant variables.

VI. A SIMPLE APPROACH

Some simple calculations of the effect of homeownership on segregation using the data set described in the previous section are presented in table 4. Row A gives the mean proportion of blacks in a black renter's neighborhood by educational attainment of the head of household. Row B gives the mean proportion of blacks in a white renter's neighborhood by educational attainment of the head of household. Row C is Row A minus Row B, the segregation measure proposed in this paper, $\Delta(X)$, for renters. Rows D, E, and F are analogous to Rows A, B, and C but refer to homeowners. Row G, which is equal to Row C minus Row F, gives the change in segregation associated with homeownership for each educational group. The only conditioning variable (X) used to construct this table is the education of the head of household. As will be seen in Section VII, educational attainment of the head of household is quantitatively the most important determinant of segregation. Other factors, most significantly income, are ignored in this simple analysis. These factors are incorporated into a fuller model in Section VII.

Row A reveals that, on average, black renters with no high school diploma live in a census tract or enumeration district with 76.9 percent blacks. Black renters with a high school diploma on average live in neighborhoods with 70.3 percent blacks and those with some college training on average live in neighborhoods with 65.8 percent blacks. The mean neighborhood black proportion

for black renters with college degrees is 53.8 percent. Row A indicates the isolation of black renters declines with educational attainments.

Row B indicates that for white renters the average proportion of their neighbors that are black is substantially lower the corresponding figures for black renters. White renters with no high school diploma live in neighborhoods composed of 8.8 percent blacks, on average. The figures for white renters with a high school degree, some college training and a college degree are 5 percent, 5 percent, and 6.5 percent respectively. In contrast to black renters, there is no clear relation between educational attainment and the racial composition of white renter's neighborhoods.

Row C reports the difference of Row A and Row B, which is the $\Delta(X)$ segregation measure. The entries in this row indicate that segregation among renters declines as educational attainments increase. This decline is particularly steep for college educated individuals.

Rows D, E, and F repeat the same analysis as Rows A, B, and C but for homeowners instead of renters. Row D indicates that the isolation of black homeowners sharply declines with education. Black homeowners with no high school diploma on average live in neighborhoods with 73 percent blacks compared to neighborhoods with on average 42.4 percent blacks for college-educated blacks. The mean proportion of blacks in white homeowners' neighborhoods declines with education but this decline is minimal compared to the decline experienced by black homeowners. White homeowners with no high school diploma on average live in neighborhoods with 5.6 percent blacks compared to neighborhoods with on average 3.8 percent blacks for college graduates.

The difference in segregation between homeowners and renters is given in Row G, which is the difference of Row C (segregation among renters by educational attainment) and Row F (segregation among homeowners by educational attainment). In general, homeownership is associated with a decline in segregation but this effect is small or moderate for individuals without a four-year college degree. For individuals with no high school diploma, a high school diploma or some college training, homeownership is associated with a one to five percentage point decline in segregation. However, this effect is more pronounced for college graduates who on average experience a 10.7 percentage point decline in segregation when they move from a rental to a sales unit.

VII. MODEL SPECIFICATION AND FIXED-EFFECTS ESTIMATES

A statistical model of the proportion of blacks in a person's census tract is used to construct the segregation measure ($\Delta(X)$) and the difference in difference ($\Delta^r(X) - \Delta^h(X)$) used to quantify the effect of discrimination on segregation. The model states that the proportion of blacks in a person's neighborhood is a function of a time effect, an MSA specific effect, whether an individual lives in the South, an individual's race, family income, the education of the head of household and the discrimination rate, which is captured by the housing tenure variable. In addition, an individual specific fixed effect is included in the model.[9] The fixed-effects specification was chosen over random effects because a Hausman specification test rejected the null hypothesis that the random effects and fixed-effects coefficients are identical. Under the assumption of constant preferences over time, this fixed effect can be interpreted as the propensity of an individual to live in a neighborhood with a high black proportion. Mathematically, the model is a linear probability[10] model for grouped data with fixed-effects and is given by

$$(7) \qquad b_{it} = \mu_i + \gamma_t + MSA_{it}\beta_0 + X_{it}\beta_1 + R_{it}X_{it}\beta_2 + B_iX_{it}\beta_3 + R_{it}B_iX_{it}\beta_4 + R_{it}\beta_5 + B_i\beta_6 + B_iR_{it}\beta_7 + e_{it}$$

where b_{it} is the proportion of blacks in a person's neighborhood, μ_i is a fixed effect for each individual in the sample, γ_t is a constant term for year t, MSA_{it} is a dummy variable for each metropolitan area in the sample, X_{it} is a 1 x 5 vector of socioeconomic variables including the family's income to needs ratio, the education of the head of household[11] and a dummy variable equal to one if a person resides in the southern census region, R_{it} is a dummy variable equal to one if

[9] The most general way to control for individual heterogeneity is by using fixed-effects. Unlike random effects, the use of fixed-effects does not require an assumption that individual heterogeneity is uncorrelated with the other right hand side variables. The fixed-effects model does have one major disadvantage. As mentioned previously, the coefficients on time invariant variables, such as the race of the head of household, are not identified. Despite this loss of information, the linearity assumption permits the calculation of $\Delta^r(X) - \Delta^h(X)$ as will be shown below.

[10] The rationale for choosing the linear probability model specification is given in Appendix I.

[11] The education variable is parsed into three dummy variables (High school, Some college, and College) as described in Section V.

person i rents at time t, B_i is a dummy variable equal to one if person i is black, and $e_{it} \sim \text{IID}(0, \sigma_e^2)$. It is assumed that e_{it} is uncorrelated with MSA_{it}, γ_t, X_{it}, R_{it}, B_i and μ_i.[12]

Note that R_{it} and B_i have been interacted with X_{it} in such a way as to allow white homeowners, white renters, black homeowners and black renters to have different coefficients on the socio-economic variables included in X_{it}. For example, β_2 and β_5 parameterize the difference between the expected proportion of blacks in a white renter's neighborhood and a white homeowner's neighborhood. β_5 is the effect of renting common to all renters while β_2 is a vector of coefficients which allow the effect of renting to vary by education and income. β_3 and β_6 parameterize the effect of being black on the proportion of blacks in one's neighborhood. β_6 captures the effect common to all blacks while β_3 allows the effect of race to vary by education and income. β_4 and β_7 parameterizes the effect of being both black and a renter. β_7 captures the effect common to all black renters while β_4 allows that effect to vary by education and income. The chart below identifies the coefficients of the model, which belong to the four distinct groups considered in the above model.

Group	Relevant coefficients
White homeowners	B_1
White renters	$\beta_1, \beta_2, \beta_5$
Black homeowners	$\beta_1, \beta_3, \beta_6$
Black renters	$\beta_1, \beta_2, \beta_3, \beta_4, \beta_5, \beta_6, \beta_7$

Since blacks in rental units face a higher rate of discrimination than blacks in sales units the coefficients on the "Rent x Black" variables characterize the "discrimination effect." Notice that in

[12]The grouped nature of the data induces heteroskedasticity in the model which can be corrected by constructing weights equal to

$$\sqrt{\frac{n_i}{p_i(1-p_i)}}$$

and running weighted least squares regression on the equation above. The weights are constructed using predicted values from an unweighted estimation of the same equation. About 120 observations have negative predicted values. These are white individuals with high incomes living in metropolitan areas with small black populations. The weight used to correct for heteroskedasticity is not defined for a negative predicted value. For this reason a truncated predicted value is used instead of the actual predicted value to construct the weight. The truncated predicted value equals 0.0001 if the actual predicted value is less than 0.0001. The actual predicted value is used to construct the weight if it is greater than or equal to 0.0001.

terms of the variables and coefficients of the empirical model above, $\Delta^r(X)$ can be expressed as a difference constructed by using the coefficients that pertain to black and white renters and $\Delta^h(X)$ can be expressed as a difference constructed by using the coefficients that pertain to black and white homeowners:

(8) $$\Delta^r(X) = x(\beta_1 + \beta_2 + \beta_3 + \beta_4) + \beta_5 + \beta_6 + \beta_7 - x(\beta_1 + \beta_2) + \beta_5$$

(9) $$\Delta^r(x) = x(\beta_3 + \beta_4) + \beta_6 + \beta_7$$

(10) $$\Delta^h(X) = X(\beta_1 + \beta_3) + \beta_6 - X(\beta_1)$$

(11) $$\Delta^h(X) = X(\beta_3) + \beta_6$$

Thus,

(12) $$\Delta^r(X) - \Delta^h(X) = X(\beta_4) + \beta_7$$

That is, in order to compute the difference in difference ($\Delta^r(X) - \Delta^h(X)$) which is the effect of the additional discrimination found in rental markets compared to sales markets on segregation, one may confine attention to the coefficients on the "Black x Rent" variables.

The estimation results of the linear probability model for grouped data with fixed-effects are presented in table 5. The estimated coefficients on the Income-to-needs ratio, High school, Some college, and College imply that the level of family income and education of the head of household have almost no quantitatively important role in determining the proportion of blacks in a white homeowner's neighborhood. White renters live in neighborhoods that contain a slightly higher fraction of blacks (0.029) than white homeowners. The estimated coefficient on Rent × Income to needs implies that the level of family income does not affect the proportion of blacks in white renter's neighborhoods. In contrast, the coefficients on Rent × High school (–0.034), Rent × Some college (–0.030) and Rent × College (–0.047) imply that the educational achievements of white renters do have a small negative effect on the percentage of blacks in their neighborhoods.

The coefficient estimate on Black × Income to needs (–0.002) implies that the effect of extra income on the racial composition of black homeowners' neighborhoods is practically zero. The coefficient estimates on Black × High school, Black × Some college, and Black × College show that the educational attainments of the black homeowners is associated with a mild increase in the proportion of blacks in a neighborhood. However, the predicted increase for high school graduates is not statistically significant.

The coefficients on Black × Rent and Rent indicate that black renters live in neighborhoods with approximately 8 (0.029 + 0.055) percentage points more blacks than black homeowners. An increase in the income to needs ratio of a black family that rents is associated with a small decrease (–0.014) in the percentage of blacks in one's neighborhood. However, the effect of education on neighborhood composition is quite dramatic for black renters. Black high school graduates who rent live in neighborhoods with 13.7 (0.029 + 0.053 + 0.055) percentage points more blacks than do black homeowners with a high school degree. Black renters with some college training live in neighborhoods with 16.2 (0.029 + 0.078 + 0.055) percentage points more blacks than do their counterparts who own a home. Most dramatically, black renters with a four-year college degree live in neighborhoods with 23.8 (0.029 + 0.154 + 0.055) percentage points more blacks than their counterparts who own a home.

The cumulative effect of these coefficients on changes in racial segregation across rental and sales markets is shown in Table 6. In this table, the predicted increases in racial segregation in rental markets compared to sales markets are presented for the four education groups and for three income to needs ratios for each education category. For instance, a black high school dropout with an income to needs ratio of one would expect to live in a neighborhood with four percentage points fewer blacks when he owns a home compared to when he rents. As can be readily seen in Table 6, the "discrimination effect" decreases slightly with income but increases substantially with educational attainment. For all groups except high school dropouts, the size of the discrimination effect is large: 7–9 percentage points for high school graduates, 9–12 percentage points for those with some college training and 17–20 percentage points for those with four year college degrees.

VIII. COMPARING CROSS-SECTIONAL AND PANEL ESTIMATES

As noted in the introduction, previous research on racial segregation in housing has used cross-sectional data. A novel aspect of this paper is its use of panel data to study segregation. This section of the paper examines whether the panel nature of the PSID provides some benefit in the identification of the "discrimination effect."

In the previous section, a fixed effect model specification was chosen because a Hausman specification test indicated a statistically significant difference between fixed-effects and random effects coefficient estimates. The implication of this finding is that including between individual variation in an estimator of the "discrimination effect"[13] leads to biased coefficient estimates. The only type of variation found in cross-sectional data is variation between individuals and thus, cross-sectional estimates of the impact of discrimination on segregation appear to be problematic. While the Hausman specification test shows that the bias induced by between persons variation is statistically significant, it remains to be seen whether this bias is quantitatively important.

To answer this question, the estimates of the "discrimination effect" derived from the fixed-effects model are compared with two sets of least squares estimates that are derived from essentially the same model minus any individual specific effects. In one case, the comparison model is estimated on the entire sample that was used to estimate the fixed-effects model. The estimates from this model are similar to those derived from a random effects model in that both estimators use weight averages of estimates derived from between and within person variation, although the weights are different for each. However, in contrast to the random effects model, the least squares estimator does not control for individual heterogeneity. The advantage of this comparison model is its large sample size (all observations in the panel are used). Its disadvantage is the presence of a type of variation (within individual variation) not found in cross-sections. In the other comparison, estimates are derived from a cross-section of the data created by restricting the sample to observations from a single year—1980. Since this sample is restricted to a single year, both the individual specific and year dummy variables are removed from the model. Otherwise, the model estimated on this single year cross-section is identical to the fixed-effects model of section VII. The advantage of this comparison model is that, unlike the first comparison model, it truly is a cross-

[13] The random effects estimator uses a weighted mixture of between individual variation and within individual variation while the fixed-effects estimator only uses within individual variation.

section. However, the disadvantage of this comparison is the small sample size used in the model estimation.

These comparisons abstract from the other principle difference between existing cross-sectional data sets, such as the decennial census, and a nationally representative panel such as the PSID—namely the level of detail available in variables that measure socio-economic status. Instead, the comparisons made in this section consider the effect of controlling for individual heterogeneity on the size and sign of estimates of the "discrimination effect" derived from models that contain the same variables as the fixed-effects model but no individual specific effects.

The two sets of estimates use a similar model specification and differ only by the sample used and the presence of time-specific dummy variables. Mathematically, the first model is given by

$$(13) \qquad b_{it} = \gamma_t + MSA_{it}\beta_0 + X_{it}\beta_1 + R_{it}X_{it}\beta_2 + B_iX_{it}\beta_3 + R_{it}B_iX_{it}\beta_4 + R_{it}\beta_5 + B_i\beta_6 + B_iR_{it}\beta_7 + e_{it}$$

where b is the proportion of blacks in a person's neighborhood, MSA is a dummy variable for each metropolitan area in the sample, X is a 1 x 5 vector of socioeconomic variables including the family's income to needs ratio, the education of the head of household and a dummy variable equal to one if a person resided in the southern census region, R is a dummy variable equal to one if a person rents, B is a dummy variable equal to one if a person is black, and $e \sim \text{IID}(0, \sigma_e^2)$. It is assumed that e is uncorrelated with MSA, B, R, and X. Variability in neighborhood size induces heteroskedasticity in the model that can be corrected by constructing weights in the same fashion as before. The second model is the same as above without γ_t and without time subscripts.

The results from these two comparison models are found in table 7. The first three columns of table 7 reproduce table 6, while the next two columns add the percentage point changes in segregation implied by the coefficient estimates from the comparison models. The coefficient estimates of these two models are found in Appendix Tables 1 and 2. As before, a positive number indicates that the extra discrimination found in rental markets is associated with an increase in segregation, while a negative number indicates that the extra discrimination in rental markets is associated with a decrease in segregation. Perhaps the most striking result found in both the full

panel "cross-sectional" estimates and the 1980 cross-sectional estimates of the "discrimination effect" is that *at least half of the estimates have the wrong sign*. In these cases, dropping individual specific effects result in estimates that imply that increased discrimination is associated with lower levels of racial segregation. Furthermore, even in the cases where the discrimination effect has the correct sign, the estimates of the size of the effect appear to suffer from a persistent downward bias. The size of the "discrimination effect" is always smaller in the full panel estimates with no fixed-effects when compared to the full panel estimates with fixed-effects. Similarly, the estimates of the "discrimination effect" derived from the 1980 cross-section are smaller than the fixed-effects estimates in 10 of the 12 cases presented in table 7. The pattern of cross-sectional estimates indicates that when individual heterogeneity is ignored no meaningful "discrimination effect" is found in the data. It is also important to note that on average, the fixed-effects estimates of the "discrimination effect" in table 7 are 9.8 percentage points higher than the two sets of cross-sectional estimates.

Based on these results, it seems reasonable to conclude that the 1980 cross-section, full panel with no fixed-effects and fixed-effects coefficient estimates imply dramatically different response of segregation to changes in the rate of discrimination. Failing to control for individual heterogeneity appears to result in a substantial downward bias in the estimated relationship between discrimination and racial segregation in housing, at least in the data used in this study.

IX. CONCLUSION

Social scientists commonly assume that a reduction in the housing discrimination rate would be associated with a large decrease in black-white segregation. Yet prior to this essay there has not been any direct evidence to support this belief. There is little doubt that a decrease in the discrimination rate would have some effect on segregation, but given the state of the current literature it would be impossible to deduce whether that effect would be large or small. This study is the first to quantify the relationship between a discrete change in the discrimination rate and a corresponding change in racial segregation in housing.

Audit studies reveal that the rate of discrimination is higher in rental housing than in owner-occupied housing. Thus, the effect of racial discrimination on segregation is quantified by observing

individuals who change housing market types while living in the same metropolitan area while holding constant other relevant factors.

This is also the first study of racial segregation to use panel data. Previous studies have not used panel data because conventional measures of racial segregation are not defined over nationally representative surveys such as the PSID. A new measure of racial segregation is developed which can be applied to survey data. This new measure is the expected proportion of blacks in a black person's neighborhood conditional on a number of socioeconomic variables minus the same conditional expectation for a white person with the identical socioeconomic background.

Racial segregation is estimated controlling for individual heterogeneity in the propensity to live in neighborhoods with a high black proportion through the use of fixed-effects. In this model, the decrease in discrimination from the level observed in rental housing to that observed in owner-occupied housing is associated with a 1–4 percentage point drop in segregation for individuals with no high school diploma, a 7–9 percentage point drop in segregation for individuals with a high school diploma, a 9–12 percentage point drop in segregation for individuals with some college training, and a 17–20 percentage point drop in segregation for individuals with a four-year college degree.

There are two striking features of these estimates. First, the effect of the additional discrimination in rental markets on racial segregation is not only statistically significant but, except for high school dropouts, substantial. Second, the size of this effect varies tremendously with educational status and to a lesser degree with income. At this time it is difficult to know what accounts for this variation in the "discrimination effect." One possible explanation may be that the housing search techniques used by lower education and high education blacks differ. In fact, Farley, Bianchi and Colasanto [1979] have found that blacks with higher educational attainments are more likely to utilize realtors than blacks with lower educational attainments. Nevertheless, this one study is merely suggestive, not conclusive. Given what is little is known about minority housing search techniques (and other possible factors that might interact with the discrimination rate to produce these effects), anything more than speculative explanation of this variation will have to be left to future research efforts.

In the last part of this paper the empirical importance of controlling for individual heterogeneity is assessed by examining the difference in segregation across housing markets using

two sets of cross-sectional estimates. The unobserved variable bias in the cross-sectional coefficient estimates is severe. In most cases examined, the cross-sectional coefficient estimates implied that the *increased* discrimination in rental markets compared to sales markets either *decreased* racial segregation or had no measurable effect. On average, the cross-sectional estimates of the "discrimination effect" were 9.8 percentage points lower than their fixed-effects counterparts. The size and persistence of this bias suggests that future research on the relationship between racial segregation and racial discrimination should emphasize the use of panel, as opposed to cross-sectional, data.

Appendix I: The Choice of a Linear Probability Specification

The linearity assumption used in both these models, as opposed to a generalized linear model such as a logit or probit, is admittedly unrealistic but is maintained for three reasons. First, experimentation has revealed that alternatives such as a probit for grouped data do not produce reasonable predicted values for white respondents. Predicted values for whites from a probit for grouped data are tightly clustered about 0.1 percent, which is far from the sample mean of five percent.

Second, not only are these estimates unrealistically small, they are not responsive to changes in socio-economic variables. The effect of statistically significant coefficients on education and income variables are overwhelmed by the coefficients for whites, which do not vary with economic status. In effect using a probit for grouped data forces the researcher to ignore the variation in the percentage of blacks for whites when computing segregation.

Third, the effect of discrimination on segregation is not identified in a generalized linear model with fixed-effects but is identified in a linear model with fixed-effects. A well known property of fixed-effects model is that parameters on time invariant variables, such as the black dummy variable, are not identified. In the linear model it is not necessary to know the coefficient on the black dummy variable in order to calculate $\Delta^r(X) - \Delta^h(X)$. This difference can be calculated simply by summing and subtracting the appropriate coefficients. However, this is not the case for a generalized linear model with fixed-effects. In a generalized linear model, the difference in two predicted values depends not only on the size of the coefficients on the variables that change but also depends on the level of the constant terms taken as arguments to the non-linear link function.

To see this, consider a generalized linear model with fixed-effects

$$b_{it} = g(\mu_i + B_i\beta_1 + R_{it}\beta_2) + \varepsilon_{it}$$

where g is a non-linear link function such as a logit or normal cdf, μ_i is the fixed effect, B_i is a dummy variable equal to one if person i is black and R_{it} is a dummy variable equal to one if person i rents at time t. For each individual $\mu_i + B_i\beta_1$ is identified but not β_1. One cannot ignore these terms because the difference in predicted values for renters and homeowners will depend on the level of this intercept. For instance assume that $\beta_1 = \beta_2 = 1$ and that g is the standard normal cdf. Then segregation among renters is given by $g(1 + 1) – g(1) = 0.977 – 0.841 = 0.136$. Now suppose $\beta_1 = 0.1$. Then segregation among renters is given by $g(0.1 + 1) – g(1) = 0.844 – 0.841 = 0.003$. As such, defining a segregation measure which can be generalized to the population at large is impossible because one cannot abstract from unique individual effects.

Appendix II: Supplemental Tables

Appendix Table 1. Full Panel Estimates Without Fixed-effects

Variable	Coefficient	Standard error	P Value
Number of observations	25,297		
R^2	0.8239		
Root MSE	42.066		
Chicago	0.0422	0.0049	0.00
1985	0.0045	0.0022	0.05
Income to needs	–0.0013	0.0003	0.00
High school	0.0066	0.0016	0.00
Some college	–0.0027	0.0015	0.07
College	0.0059	0.0017	0.00
South	0.1283	0.0020	0.00
Black	0.7514	0.0085	0.00
Rent	0.0340	0.0048	0.00
Rent × income to needs	–0.0013	0.0007	0.09
Rent × high school	–0.0310	0.0047	0.00
Rent × some college	–0.0111	0.0052	0.03
Rent × college	–0.0091	0.0056	0.10
Rent × south	–0.0058	0.0052	0.26
Black × income to needs	–0.0054	0.0027	0.04
Black × high school	–0.1023	0.0082	0.00

Black × some college	–0.0838	0.0129	0.00
Black × college	–0.2818	0.0210	0.00
Black × south	–0.1745	0.0087	0.00
Black × rent × income to needs	–0.0164	0.0038	0.00
Black × rent × high school	0.0728	0.0117	0.00
Black × rent × some college	0.0054	0.0174	0.76
Black × rent × college	0.0782	0.0293	0.01
Black × rent × south	0.0460	0.0107	0.00
Black × rent	0.0034	0.0112	0.76

Notes: The model includes dummy variables for each year and each metropolitan area found in the sample. The estimates for all years and metropolitan areas are suppressed except for Chicago and 1985. Dependent variable is the percentage of blacks in an individual's census tract or enumeration district of residence.

Appendix Table 2. Cross Sectional Estimates for 1980

Variable	Coefficient	Standard error	P Value
Number of observations	2,057		
R^2	0.5453		
Income to needs	–0.001	0.000	0.001
High school	0.004	0.003	0.273
Some college	0.008	0.004	0.035
College	0.015	0.005	0.001
South	0.072	0.008	0.000
Black	0.764	0.079	0.000
Rent	0.002	0.010	0.813
Rent × income to needs	0.000	0.001	0.848
Rent × high school	0.004	0.011	0.693
Rent × some college	0.000	0.011	0.984
Rent × college	–0.026	0.012	0.035
Rent × south	0.061	0.010	0.000
Black × income to needs	–0.025	0.024	0.293
Black × high school	–0.093	0.071	0.191
Black × some college	–0.082	0.101	0.419
Black × college	–0.234	0.180	0.195
Black × south	–0.133	0.076	0.081
Black × rent × income to needs	–0.037	0.037	0.319
Black × rent × high school	0.131	0.099	0.187
Black × rent × some college	–0.017	0.143	0.904
Black × rent × college	0.063	0.244	0.797
Black × rent × south	–0.056	0.088	0.525
Black × rent	0.021	0.101	0.837

Note: Dependent variable is the percentage of blacks in an individual's census tract or enumeration district of residence.

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Table 1. Nationwide Studies of Racial Discrimination in Housing

	HUD, 1977 (Wienk et al.[1979])	HUD, 1989 (Yinger [1992])	Local housing authorities, 1980s (Galster [1990])
Number of metropolitan areas	40	20	5 (sales), 19 (rental)
Rental housing			
Number of audits	1577	801	142
% White favored	48	40.1	—
% Black favored	21	18.8	—
Net difference	27	21.3	47
Logit	—	46.9	—
Owner-occupied housing			
Number of audits	1641	1081	1529
% White favored	39	35.7	—
% Black favored	24	17.7	—
Net difference	15	18.0	21
Logit	—	34.2	—

Note: Measures of discrimination refer to discrimination in housing availability.

Table 2. Equality of Means Tests for Discrimination Rates in Sales and Rental Markets

	P-value of hypothesis that discrimination in rental and sales market is equal
HUD, 1977 (Wienk, et al. [1979])	0.00
HUD, 1989 (Yinger, [1991])	0.07
Local housing authorities, 1980s (Galster, [1990])	0.00
Above studies considered jointly	0.00

Table 3. Summary Statistics

Variable	Full data Mean (Std. dev.)	Rent = 0 Mean (Std. dev.)	Rent = 1 Mean (Std. dev.)	Black = 0 Mean (Std. dev.)	Black = 1 Mean (Std. dev.)
Number of Observations	25,297	14,459	10,838	13,064	12,233
Number of individuals in panel	3,837				
Average number of years in panel	6.59				
Percent black	0.3666 (0.3973)	0.2958 (0.3734)	0.4610 (0.4084)	0.0518 (0.1258)	0.7027 (0.3011)
Income to needs	2.831 (2.058)	3.258 (2.210)	2.262 (1.676)	3.630 (2.247)	1.978 (1.399)
High school	0.3053 (0.4606)	0.2905 (0.4540)	0.3252 (0.4685)	0.2907 (0.4541)	0.3209 (0.4669)
Some college	0.1491 (0.3562)	0.1495 (0.3566)	0.1486 (0.3557)	0.1849 (0.3882)	0.1109 (0.3141)
College	0.1453 (0.3524)	0.1729 (0.3782)	0.1084 (0.3109)	0.2446 (0.4298)	0.0392 (0.1942)
South	0.4245 (0.4943)	0.4042 (0.4908)	0.4515 (0.4977)	0.2850 (0.4514)	0.5734 (0.4946)
Black	0.4836 (0.4997)	0.3980 (0.4895)	0.5978 (0.4904)	0 (0)	1 (0)
Rent	0.4284 (0.4949)	0 (0)	1 (0)	0.3337 (0.4715)	0.5296 (0.4991)
Percentage of renters in census tract	0.3703 (0.2001)	0.3114 (0.1738)	0.4575 (0.2046)	0.3160 (0.1854)	0.4444 (0.1957)
Number of individuals in census tract or ED	5,326 (2,800)	5,388 (2,860)	5,244 (2,715)	5,403 (2,955)	5,244 (2,622)

Table 4. The Effect of Homeownership on Segregation: A Simple Approach

	Row	No high school diploma	High school diploma	Some college	College degree
Black renters	A	0.769	0.703	0.658	0.538
White renters	B	0.088	0.050	0.050	0.065
Segregation, Δ(X)	C = A – B	0.681	0.653	0.608	0.493
Black homeowners	D	0.730	0.632	0.626	0.424
White homeowners	E	0.056	0.027	0.030	0.038
Segregation, Δ(X)	F = D – E	0.674	0.605	0.596	0.386
Change in segregation	G = C – F	0.007	0.048	0.012	0.107

Note: Table entries are the average percentage of blacks in a person's neighborhood by race and educational attainment. Segregation is measured as the difference in the mean neighborhood black proportion for blacks and whites with the same educational attainment. Row C = Row A – Row B gives segregation for renters. Row F = Row D – Row E gives segregation for homeowners. The means reported are unweighted. The change in segregation associated with homeownership is the difference in difference, Row C – Row F.

Table 5. Fixed-effects Estimates

Variable	Coefficient	Standard error	P Value
Number of observations	25,297		
R^2	0.9083		
Income to needs	–0.001	0.001	0.104
High school	–0.003	0.008	0.705
Some college	–0.006	0.009	0.530
College	–0.017	0.012	0.136
South	–0.113	0.038	0.003
Rent	0.029	0.008	0.000
Rent × income to needs	0.001	0.001	0.385
Rent × high school	–0.034	0.008	0.000
Rent × some college	–0.030	0.008	0.000
Rent × college	–0.047	0.009	0.000
Rent × south	0.040	0.007	0.000
Black × income to needs	–0.002	0.002	0.297
Black × high school	0.012	0.011	0.279
Black × some college	0.068	0.014	0.000
Black × college	0.038	0.023	0.102
Black × south	–0.156	0.069	0.023
Black × rent × income to needs	–0.014	0.002	0.000
Black × rent × high school	0.053	0.011	0.000
Black × rent × some college	0.078	0.013	0.000
Black × rent × college	0.154	0.018	0.000
Black × rent × south	–0.061	0.009	0.000
Black × rent	0.055	0.010	0.000

Note: Dependent variable is the percentage of blacks in an individual=s census tract or enumeration district of residence.

Table 6. Implied Percentage Point Changes in Segregation from Fixed-effects Regression, $\Delta^r(X) - \Delta^h(X)$

Education	Income	Fixed-effects
No high school diploma	1	0.04
	2	0.03
	3	0.01
High school diploma	1	0.09
	2	0.08
	3	0.07
Some college	1	0.12
	2	0.11
	3	0.09
College	1	0.20
	2	0.18
	3	0.17

Note: Predicted values are calculated for individuals not living in the south.

Table 7. Implied Percentage Point Changes in Segregation, $\Delta^r(X) - \Delta^h(X)$

Education	Income to needs ratio	Fixed effects	Full panel, no fixed-effects	1980 Cross-section
No high school diploma	1	0.04	–0.01	–0.02
	2	0.03	–0.03	–0.05
	3	0.01	–0.05	–0.09
High school diploma	1	0.09	0.06	0.12
	2	0.08	0.04	0.08
	3	0.07	0.03	0.04
Some college	1	0.12	–0.01	–0.03
	2	0.11	–0.02	–0.07
	3	0.09	–0.04	–0.11
College	1	0.20	0.07	0.05
	2	0.18	0.05	0.01
	3	0.17	0.03	-0.03

Note: Predicted values are calculated for individuals not living in the south.