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ARE GHETTOS GOOD OR BAD?*

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Spatial separation of racial and ethnic groups may theoretically have positive or negative effects on the economic performance of those groups. We examine the effects of segregation on outcomes for blacks in schooling, employment, and single parenthood. We find that blacks in more segregated areas have significantly worse outcomes than blacks in less segregated areas. We control for the endogeneity of location choice using instruments based on political factors, topographical features, and residence before adulthood. A one standard deviation decrease in segregation would eliminate one-third of the black-white differences in most of our outcomes.

I. INTRODUCTION

Racial segregation is the norm in urban America. In the average American city, 60 percent of blacks would have to change residences to create an even distribution of the races across neighborhoods, and the average black lives in a neighborhood that is 57 percent black.[1] The spatial separation of many blacks from jobs, positive role models, and high quality local public goods has led some to speculate that segregation is a cause of the problems of the black underclass [Massey and Denton 1993]. But economic theory does not suggest that the segregation of a particular group into a ghetto is necessarily bad.[2] Ghettos may have benefits as well as costs, especially if they allow for mixing across income classes within a segregated group and for positive spillovers within that group. Determining whether ghettos are good or bad for their residents is a major issue in forming public policies for urban problems and is the topic of this paper.

Empirical evidence on the effects of segregation on outcomes has typically considered whether blacks who live in predomi-

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1. We refer to census tracts as "neighborhoods." Census tracts are geographic units containing between 3000 and 5000 individuals.

2. We use the term "ghetto" nonpejoratively, to refer to a racially or ethnically segregated community. Indeed, the first use of the word ghetto referred to the legally separate, but not particularly decrepit, Jewish quarter in Venice; see Lestchinsky [1931].

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nantly black areas of cities have worse outcomes than blacks who live outside of those areas.[3] This evidence is difficult to interpret because intracity differences in outcomes may reflect the selection of more successful black families into less black neighborhoods, rather than the effect of neighborhoods on outcomes. We avoid this type of comparison. Instead, our empirical strategy is to examine whether outcomes for minorities *as a whole* are better or worse in cities that are more racially segregated compared with cities that are less racially segregated. By examining segregation and outcomes for all blacks, we avoid issues of within-city sorting by ability.

But conducting our analysis at the city level raises two other difficulties: the potential for reverse causality where poor outcomes lead to increased segregation, and the potential bias from sorting of more and less successful blacks across cities. To address the issue of reverse causation, we use two sets of instruments for segregation across cities: the structure of local government finance and, following Hoxby [1994, 1996], topographical features of the city that affect segregation. We address the issue of cross-metropolitan mobility by examining the effect of location early in life on adult outcomes.

Using a variety of economic and social outcomes, we find strong, consistent evidence that black outcomes are substantially worse (both in absolute terms and relative to whites) in racially segregated cities than they are in more integrated cities. As segregation increases, blacks have lower high school graduation rates, are more likely to be idle (neither in school nor working), earn less income, and are more likely to become single mothers. Further, the quantitative effects of segregation are large. A one standard deviation reduction in segregation eliminates approximately one-third of the difference between blacks and whites in most outcomes. We find some evidence that segregation improves outcomes for whites, but these results are weaker than the results for blacks.

After documenting the relation between segregation and outcomes, we consider why segregation is so harmful to blacks. We differentiate between three explanations: racial segregation is proxying for income segregation and income segregation is harmful to blacks; in more segregated cities, blacks have less contact

3. Kain [1968] pioneered research in this line. Recent contributions include Ellwood [1986], Galster [1987], Kasarda [1989], Ihlanfeldt and Sjoquist [1990], and O'Regan and Quigley [1996a, 1996b]. For recent surveys see Holzer [1991], Jencks and Meyer [1990], and Kain [1992].

with positive role models and this results in worse outcomes; and segregation is harmful because it creates more physical distance between individuals and their jobs. We find evidence for many of these hypotheses. But even accounting for these effects, we still find a substantial effect of racial segregation itself on outcomes. Thus, while we can isolate some of the mechanisms through which segregation harms blacks, we cannot ferret them all out.

The next section of the paper presents a theoretical framework for analyzing the effect of segregation on outcomes. Section III discusses the empirical methodology. Sections IV and V present our results on segregation and outcomes. Section VI looks at why segregation results in worse outcomes for blacks, and the last section concludes.

II. Ghettos and Outcomes

We begin by reviewing the major hypotheses about how segregation affects economic and social outcomes for minority groups.

2.1. The Costs of Ghettos

A growing literature emphasizes the importance of "peer group effects," "social interactions," and "neighborhood effects" [Coleman 1966; Case and Katz 1991; Borjas 1995; or Glaeser, Sacerdote, and Scheinkman 1996], especially for the young. Many argue that these effects are important in the formation of skills and values.[4] Ghettos separate poor blacks from middle-class society, and as a result, ghetto residents may learn few skills and acquire norms that are in conflict with mainstream society. Kain [1968] emphasizes that ghettos create a physical separation or "spatial mismatch" between where blacks live and the location of jobs. This spatial mismatch may hurt outcomes in black areas, both because of physical isolation of blacks and whites and because physical isolation leads to intellectual and social isolation of the two groups. Informational isolation may also hurt blacks if it means that whites end up relying more on stereotypes of blacks rather than actual experience.

Another disadvantage of spatial segregation could result if

4. The costs of peer effects, however, occur only when peers are themselves unsuccessful; among certain ethnicities, Borjas [1995] shows that neighborhood effects help ghetto residents. Lazear [1995] also suggests that ghettos can be good for immigrants because these immigrants are thereby spared the costs of learning English.

there are neighborhood-specific public goods, and these goods are paid for or provided locally (for example, schools), then segregation of the races may mean that blacks will be cut off from high quality public goods. Or, if individuals favor redistribution only to those in their immediate area (as in Cutler, Elmendorf, and Zeckhauser [1993]), then separation of races may result in less desire among whites to direct spending to blacks.

2.2. The Benefits of Ghettos

In the literature on the benefits of segregation, a crucial issue is the alternative to race-based segregation. If the alternative to ghettos is complete integration of society, ghettos may indeed be costly to their inhabitants. But if ghettos keep rich and poor blacks together when otherwise they would live apart, then ghettos may help poor blacks. Wilson [1987] stresses the adverse effects of the outflow of middle-class blacks for the residents of inner city ghettos, echoing arguments of Handlin [1959] and Glazer and Moynihan [1963].[5]

Glazer and Moynihan [1963] also suggest that segregation may help minorities by protecting budding black-owned businesses from white competition—an infant industry argument.[6] Wirth [1956] argues that Jewish ghettos enforced good behavior because of the ability of community leaders to punish misbehavior by expelling people into the hostile outside world.

2.3. A Model

We now demonstrate the costs and benefits of ghettos in a stylized framework meant to formalize some of the preceding discussion; the empirical results of this paper do not rely on the specific assumptions of this model. Consider a city with three groups of people: whites, skilled blacks, and unskilled blacks. For simplicity, we assume that all whites are skilled. Children's human capital is a function $H = H(H_{Parent}, H_{Community})$, where H_{Parent} is parental human capital and $H_{Community}$ is the average human capital

5. Glazer and Moynihan [1963] write: "Segregation helped make Harlem alive. . . . Because of the unbroken pattern of segregation, Harlem included everyone in the Negro community—the old tiny 'upper class,' the new professionals and white-collar workers, the political leaders just beginning to take over the old political clubs, the artists and entertainers and writers, as well of course as the domestic workers, the laborers and shady characters" [p. 27].

6. Douglas [1995] argues that the increased success and integration of black performers in the 1920s hurt black playwrights and songwriters, because performers who in 1920 were using black-written material, were by 1930 more successful and chose to employ white writers.

level in the community where they live. The theory and evidence of the neighborhood effects literature described above suggests that $H_2(.,.) > 0$ (we adopt the usual convention of using $H_i(.,.)$ to denote the derivative of $H(.,.)$ with respect to its ith argument). We assume that parental and community human capital are complements; i.e., $H_{12}(.,.) > 0$.[7]

Two issues are the size of neighborhoods and the process by which these neighborhoods are formed. We divide the city into three geographic areas, which are fixed in size. Human capital spillovers are assumed to matter within a neighborhood but not across neighborhoods. The cost of housing differs by neighborhood. We specify housing costs as a function $C_j(P_j)$, where P_j is the population of neighborhood j. While we think of the $C(.)$ function as reflecting housing costs, in principle the function could also reflect any location-specific public goods where quality declines with population (perhaps local public schools). We assume that $C'_j(P_j) > 0$ to reflect competition for housing and crowding costs. We assume further that blacks must pay a cost, denoted $\delta_b > 0$, to move into areas where whites are a majority, and whites must pay a cost, denoted $\delta_w > 0$, to move into areas where blacks are in the majority. These costs are meant to capture both racially based barriers to mobility (e.g., restrictive covenants) and tastes for living near similar people. There are no other mobility costs. The utility function for a family is therefore

$$(1) \qquad U_k = H(H_{Parent}, H_{Community}) - C_j(P_j) - \delta_k I(Community),$$

for $k = w, b$, where $I(Community)$ is an indicator function that equals one if the individual lives in an area where the other race is in the majority.

There are several possible equilibria in this model, depending on the cost and human capital functions. We assume a stable equilibrium where

Characterization of Equilibrium. All of the whites live in one area (termed the white neighborhood). Some of the skilled blacks live in the white neighborhood, and some live in a second neigh-

7. We assume that neighborhood spillovers are important because of their effects on childhood development (as the discussion above suggests), and we implicitly assume that all individuals work in a common central business district. Alternatively, individuals could work primarily in their own neighborhood and the neighborhood spillovers could influence workplace productivity. One advantage of assuming that spillovers work in the accumulation of human capital is that, unlike in the spatial mismatch hypothesis, firms would not benefit financially from locating in the ghetto. The complementarity assumptions ($H_{12}(.,.) > 0$) follows Assumption A2 in Bénabou [1993].

borhood (the skilled black neighborhood). Some unskilled blacks live in the skilled black neighborhood, and some live in the third neighborhood (the unskilled black neighborhood).

We can now consider how outcomes respond to changes in segregation, or more precisely, changes in the discrimination cost δ_b. In particular, we compare a less segregated city with a more segregated city. The most obvious effect of increased segregation (i.e., increased δ_b) is that fewer skilled blacks live in the white area. This change benefits whites, since crowding costs in the white neighborhood are reduced by the outflow of skilled blacks. Since we assume that all whites are skilled, the outflow of skilled blacks will not lower the average skill level in the white community.

The effect of this change on blacks is more complex, because the movement of skilled blacks will also cause a change in the locational distribution of unskilled blacks. On the one hand, the movement of skilled blacks from the white neighborhood into the skilled black neighborhood raises house prices in the skilled black neighborhood. The increase in housing prices then induces unskilled blacks to move from the skilled black neighborhood into the unskilled black neighborhood. On the other hand, the increased number of skilled blacks in the skilled black neighborhood raises the spillover benefits from being in the skilled black neighborhood, which encourages more unskilled blacks to move into that neighborhood. The net effect on the location of unskilled blacks is indeterminate.

Welfare for the black community depends on house prices and spillover effects in the skilled black neighborhood. Defining segregation by skill within the black community as one minus the share of unskilled blacks living in the skilled black neighborhood,[8] in the Theory Appendix we prove Proposition 1.

PROPOSITION 1. If increased racial segregation (δ_b) results in increased segregation by skill within the black community, then increased segregation by race reduces welfare for unskilled blacks. The effect on skilled blacks is ambiguous.

When increased segregation by race leads to increased segregation by skill within the black community, then the house price

8. Simple algebra shows that black income segregation, as defined by equation (4) and where we define skilled blacks as rich and unskilled blacks as nonrich, equals one minus the proportion of unskilled blacks living in the skilled minority neighborhood.

effect must dominate the spillover effect. As more unskilled blacks move into the unskilled black community, then crowding causes housing prices to rise in the unskilled black neighborhood, and unskilled blacks in that area are worse off. Unskilled blacks in the skilled black area are equally hurt by increased segregation because in equilibrium utility levels for this group must be equal across the two areas.

Skilled blacks are hurt less than unskilled blacks, and may even benefit from the increase in discrimination. With more skilled blacks in the skilled black neighborhood, the spillover effects are greater. Since $H_{12}(.,.) > 0$, spillover effects are always more important for skilled blacks than for unskilled blacks. On net, skilled blacks could be better off.

Increased segregation by race does not necessarily lead to increased segregation by skill within the black community. In the Theory Appendix we also prove Proposition 2.

PROPOSITION 2. If increased racial segregation (δ_b) results in less segregation by skill within the black community, then increased segregation by race raises welfare for all blacks, with the greatest effect on skilled blacks.

If racial segregation is associated with less segregation by skill, then the spillover effect is greater than the crowding effect for unskilled blacks. This means that unskilled blacks in the skilled black area benefit, and since the out-migration of unskilled blacks from their own area leads to a reduction of housing costs in that area, the unskilled blacks in the unskilled black area benefit as well. Skilled blacks will benefit even more than unskilled blacks because the positive spillover effect is more important to the more skilled group. Of course, a different model (see Cutler and Glaeser [1995]) could suggest that skilled minorities would benefit more from integration than unskilled minorities, especially since it is skilled minorities who actually come into contact with whites and unskilled minorities are left behind in the segregated area.

It may seem paradoxical that skilled blacks can be better off when they face more discrimination in the housing market. The intuition for this result is that there is a market failure coming from the fact that the skilled blacks do not internalize the positive externality they create when they move into the skilled black neighborhood and raise the average human capital level in that area. The discrimination cost acts like a tax and helps them inter-

nalize the costs that they impose on their own community by moving to the white area.

Whether or not changes in housing costs, $C(.)$, are reflected in better outcomes for black or white children depends on whether the increase in income from reductions in $C(.)$ goes to improve children's human capital. Of course, if we interpret $C(.)$ as reflecting disamenities of crowding, such as worse schools and crime, then reductions in $C(.)$ are more likely to be reflected in higher achievement of children. We implicitly assume that this is the case.

We think of Propositions 1 and 2 as a formalization of the theories discussed in subsections 2.1 and 2.2. Proposition 1 follows the reasoning of the theorists who argue that ghettos are bad. In this case, increased segregation by race leads to increased segregation by skill within the black community and a general reduction in the quality of the neighborhood of the average black. In Proposition 2, increased segregation by race leads to decreased segregation by skill within the black community, and some minorities benefit.[9] When a ghetto is a mix of skilled and unskilled blacks, the average outcome for blacks may be greater than when skilled blacks are free to live with whites and unskilled blacks live among themselves.

The model emphasizes that reductions in discrimination do not necessarily lead to perfect integration by skill and race within the city. So long as there is an incentive for individuals of different skill categories to sort by skill, then the elimination of discrimination by race does not necessarily lead to equality across neighborhoods, and may even lead to increased segregation by skill.

Propositions 1 and 2 also make clear that the effects of segregation on outcomes for blacks are theoretically indeterminate. In the remaining sections we examine this relationship empirically.

III. Empirical Framework and Data Description

Most studies of the effects of segregation on outcomes (some of which were cited earlier) examine whether, within a city, minorities in predominantly black areas fare better or worse than minorities in integrated areas. As our model illustrated, there are two major problems with this approach. First, this situation will

9. This point follows Wilson [1987], but the spirit of the model somewhat differs from his work. In particular, Wilson suggests that integration will hurt the least skilled blacks most.

naturally be true when demand for housing or public goods varies with economic status; therefore, more successful blacks will choose to live in richer and whiter neighborhoods. This factor suggests that intracity comparisons will likely overstate the effect of ghettos on outcomes. On the other hand, a spatial equilibrium implies that blacks of the same skill level in different areas receive the same utility. When segregation is harmful, segregation will be harmful to *all* blacks within the city, so there will be no difference between relative outcomes of blacks inside and outside of ghettos (as Ellwood [1986] also argues). This fact suggests that intracity comparisons of outcomes for blacks will understate the true effect of segregation on outcomes. Thus, intracity comparisons of the effects of segregation on outcomes are likely to be biased, but the direction of the bias is not clear. Without a way to correct for these intracity problems, we avoid this type of test.

Instead, we ask the question at the city level: do blacks in more segregated cities on average fare better or worse than blacks in less segregated cities? By examining segregation and outcomes for the average black in a city, we avoid the problems of intracity sorting of the population. This approach still encounters two difficulties. In practice, however, we find it easier to deal with these issues than with the intracity problems. The first concern is that our measure of segregation must be exogenous, rather than a response to poor outcomes. To address this issue, we instrument for segregation across cities.[10] Our instruments, which are discussed below, are designed to capture the fiscal and topographical features of cities that should influence segregation but not be influenced by poor outcomes of blacks. The second concern is that our estimates will be biased if abler minorities disproportionately leave cities that are more segregated. To address this problem, we focus on young people, for whom mobility will be less of an issue than it is for older people.

Econometrically, our analysis is of the form,

(2)
$$Outcome = X'\beta + \beta_1 segregation + \beta_2 segregation * black + \varepsilon,$$

where outcomes are measured at the individual level, and segregation is a citywide measure of the separation of the races. The coefficient β_1 measures the effect of segregation on whites, and β_2 is the differential effect for blacks relative to whites. We focus on

10. Our use of instrumental variables should also minimize problems coming from omitted variables such as the ethnic composition of the city.

the coefficient β_2, which measures the average outcome differential for blacks relative to whites in more segregated cities compared with less segregated cities.[11]

3.1. Measuring Segregation

Precise sources for all of our data are given in the Data Appendix. In this section we focus only on our most important variables, and in particular, our key variable: the level of segregation in a city. We measure segregation at the level of the metropolitan statistical area, not the city, because we are interested in segregation within a meaningful economic unit.[12] We proxy for neighborhoods with census tracts—contiguous groups of roughly 3000 to 5000 people, separated by natural barriers such as streets or rivers. Indexing census tracts by i, we define housing segregation within a metropolitan statistical area as

$$\text{(3)} \qquad \textit{Housing Segregation} = \frac{1}{2}\sum_{i=1}^{N}\left|\frac{\textit{Black}_i}{\textit{Black}} - \frac{\textit{Nonblack}_i}{\textit{Nonblack}}\right|,$$

where $\textit{Black}_i$ is the number of blacks in tract i. *Black* is the number of blacks in the metropolitan statistical area. $\textit{Nonblack}_i$ is the number of nonblacks in the tract, and *Nonblack* is the number of nonblacks in the metropolitan statistical area.[13] If blacks are distributed evenly throughout the metropolitan statistical area, the term in absolute value brackets will be zero for each census

11. Alternatively, readers might be more interested in $\beta_1 + \beta_2$, which can be interpreted as the total effect of segregation on blacks. The choice of whether to focus on β_2 or $\beta_1 + \beta_2$ in part depends on whether β_1 is interpreted as the effect of segregation on whites or as a reflection of omitted city-level characteristics. However, since β_1 is usually small, the question of focusing on β_2 or $\beta_1 + \beta_2$ is usually not very important.

12. A metropolitan statistical area is larger than a city; the Boston metropolitan statistical area, for example, has 2.5 million people, but fewer than 1 million live in the city of Boston. We used primary metropolitan statistical areas, rather than consolidated metropolitan statistical areas, which are large agglomerations of multiple primary metropolitan statistical areas (e.g., the New York-Northern NJ-Long Island consolidated metropolitan statistical area contains seventeen primary metropolitan statistical areas). We use the term city and metropolitan statistical area interchangeably throughout the paper to refer to metropolitan statistical areas.

13. This measure is commonly referred to as dissimilarity index. This measure of housing segregation does not capture the degree to which heavily black census tracts are contiguous nor the extent to which areas in which blacks are overrepresented are exclusively black. Cutler, Glaeser, and Vigdor [1996], Taeuber and Taeuber [1965] and Massey and Denton [1993] discuss a number of related measures. Cutler, Glaeser, and Vigdor report that the correlation of dissimilarity and isolation (an alternative index capturing the percent black of the tract inhabited by the average black) is 76.9 percent. We have also reproduced our results using this alternative index.

tract and zero for the metropolitan statistical area as a whole. If blacks and nonblacks never reside in the same census tracts, the measure of housing segregation will be one. This measure of segregation can be shown to answer the question: what share of the black (or white) population would need to change census tracks so that racial groups are evenly distributed within the metropolitan statistical area?

Two points about the measure of housing segregation are worth noting. First, even though the segregation measure is based on detailed information within metropolitan areas, the measure is only defined for the metropolitan statistical area as a whole. Second, because segregation is measured relative to the overall black population, it should not be correlated with the percent of the metropolitan statistical area that is black. As Table I shows, segregation and the percent black in the metropolitan statistical area are uncorrelated in practice.

We formed measures of housing segregation for the 209 metropolitan statistical areas with at least 100,000 people and at least 10,000 blacks in 1990. Having a large population is important to limit the measurement error in the segregation index. Since the microdata we use do not identify all metropolitan statistical areas uniquely and one city was missing the fiscal variables we discuss below, our regressions are based on 204 metropolitan statistical areas.

The first column of Table I shows summary statistics for housing segregation. The average measure of segregation in 1990 is 59 percent. The level of segregation varies dramatically across metropolitan statistical areas. The least segregated metropolitan statistical area is Jacksonville, North Carolina (21 percent); the most segregated metropolitan statistical area is Detroit, Michigan (87 percent). The standard deviation of segregation is 13 percent.

Our theoretical analysis suggests that it is important to understand the relation between racial segregation and segregation by skill within the black community. To examine the relation between these two types of segregation, we form a measure of segregation of higher income blacks from middle and lower income blacks, analogous to our racial segregation measure. We define black income segregation as

$$(4)\ \textit{Black Income Segregation} = \frac{1}{2}\sum_{i=1}^{N}\left|\frac{Rich_{Black,i}}{Rich_{Black}} - \frac{Nonrich_{Black,i}}{Nonrich_{Black}}\right|.$$

TABLE I

CITY-LEVEL CORRELATES OF SEGREGATION

Variable	Housing segregation	Black income segregation	Number of governments 1962	Intergovernmental revenue share, 1962	ln(MSA population)	Percent black	ln(median income)	Manufacturing share
Number of cities	209	209	208	209	209	209	209	209
Mean	.586	.511	40	.283	13.1	.138	10.3	.172
Standard deviation	.126	.087	55	.078	1.0	.092	0.1	.069
Minimum	.206	.330	1	.123	11.6	.009	9.6	.036
Maximum	.873	.710	339	.494	16.0	.457	10.7	.456
				Correlations				
Housing segregation	1.000							
Black income segregation	.695	1.000						
Number of governments, 1962	.478	.459	1.000					
Intergovernmental revenue, 1962	−.289	−.400	−.229	1.000				
ln(MSA population)	.366	.441	.543	−.137	1.000			
Percent black	.000	−.468	−.121	.401	−.097	1.000		
ln(median income)	.114	.217	.174	−.232	.246	−.288	1.000	
Manufacturing share	.125	.075	.126	.141	−.136	−.065	.105	1.000

Data are for MSAs with at least 100,000 people and at least 10,000 blacks. Median income is for households.

We define blacks as rich if they are in the top 25 percent of the black income distribution in their city and nonrich if they are in the bottom 75 percent of that income distribution.[14] The correlation of segregation by race and segregation by income is positive and large (.70). If the model is correct, this finding suggests that racial segregation is unlikely to be beneficial for poor blacks.

Segregation might be correlated with other features of cities. Table I examines several of these features: the logarithm of city population, the percentage of the city's population that is black, the logarithm of median household income in the city,[15] and the share of city employment in manufacturing industries. Segregation is positively related to city size, income, and the manufacturing share, although only the city size correlation is substantively large ($\rho = .37$).

3.2. Measures of Outcomes

We relate segregation to measures of outcomes for young people: people aged 20–24 and 25–30. We use data from the 1990 1% Census Public Use Micro Sample. We focus on young people because the theories of segregation noted above apply most readily to young people, where peer influences should be strongest. Also, the problems from cross-metropolitan statistical area mobility should be least severe when we are looking at people who have had a short period of adult life in which to chose their place of residence. For the same reasons, we eliminate people born in a foreign country. Our basic sample contains 97,976 people aged 20–24 and 139,715 people aged 25–30, currently residing in metropolitan statistical areas with at least 100,000 people and 10,000 blacks.

Our outcome measures are of three types. The first is educational attainment—the probability that a person has graduated from high school and college. Table II shows means of these variables separately for blacks and whites in our two age groups. About 85 percent of people have graduated from high school. This rate is substantially greater for whites than for blacks; indeed, white outcomes are better than black outcomes for each of our variables. College graduation rates are 12 percent for the entire

14. The Census reports household income in different ranges. We added up ranges within the city from the richest to the poorest until we reached 25 percent of the city's black households.

15. All of our income and earnings data are adjusted for cross-city price differences, as discussed in the Data Appendix.

TABLE II
SUMMARY STATISTICS FOR MICRO DATA

	Age 20–24		Age 25–30	
Variable	White	Black	White	Black
Education				
High school graduate	87.1%	75.4%	88.9%	77.9%
College graduate	13.4%	4.7%	27.2%	11.7%
Work and income				
Idle	6.8%	20.0%	9.5%	19.9%
ln(earnings)	9.1	8.7	9.6	9.1
Social				
Unmarried mother	9.9%	39.2%	11.8%	44.2%
Demographic variables				
Black	15.0%		13.4%	
Asian	1.2		1.0%	
Other nonwhite	0.7		0.6%	
Hispanic	7.6		6.0%	
Female	50.1		51.2%	
N	97,976		139,715	

The data are from the 1 percent Public Use Micro Sample of the 1990 Census. Idleness is defined as not working and not enrolled in school. Earnings are the sum of wage, salary, and self-employment income in 1989. Observations are for native-born people living in one of 204 MSAs where segregation and public finance variables are available and can be matched to the microdata. Earnings data are restricted to 56,627 (people aged 20–24) and 105,997 people aged 25–30 who are working, not enrolled in school, and have nonnegative earnings. Unmarried mother data are restricted to 49,038 women aged 20–24 and 71,531 women aged 25–30.

younger age group and 25 percent for the older age group. Because college graduation is increasing so rapidly over this age range, we focus less on the probability of college graduation in the younger age group than in the older age group.

We also measure outcomes with work status and income. We use an indicator for whether the person is "idle" or not. We define idle as being neither employed nor in school. Empirically, most of the variation in idleness across cities occurs because of differences in the rates of employment rather than the rates of school enrollment. Roughly 10 percent of the sample is idle. Earnings is defined as the sum of wages, salaries, and self-employment income in 1989. We use the logarithm of earnings, conditional on the individual not being in school and having positive earnings.[16]

16. We omit people in school from the earnings regression, since these people are expected to have low income. Since some of our estimates are for people aged 20–24, there is a selection problem that occurs because the ablest people may still be in school. In unreported regressions analogous to those in Table V, we did not find that enrollment was higher for 20–24 year-olds in more segregated cities, so we believe that this problem is not a significant issue.

The third measure of outcomes is particular to women—whether the woman is an unmarried mother. On average, about 15 percent of women are unmarried mothers.

As control variables in our equations explaining outcomes, we include racial dummy variables for blacks, Asians, and other nonwhites, and a dummy variable for Hispanics. We make Hispanic origin and race mutually exclusive; anyone who reports being Hispanic is included in that group alone. About 15 percent of the sample is black, 1 percent is Asian, 0.7 percent is other nonwhite, and 7 percent is Hispanic. Furthermore, we include gender and single year age dummy variables. We also control for the metropolitan statistical area characteristics discussed above: the logarithm of metropolitan statistical area population, the percent black, median household income, and the percent of the labor force employed in manufacturing. Because these variables may have different effects on blacks than on nonblacks, we interact each of these variables with a dummy variable for blacks.

There are several variables that are notably absent from our controls. We do not include variables that indicate whether a person lives in the central city or that reflect the demographic composition of the neighborhood within the metropolitan statistical area where the individual lives, since these may be endogenous with respect to outcomes. One set of variables that is not included in our basic equations that we would like to include is controls for family background—principally education and income of the parents. We return to this issue below.

IV. Preliminary Evidence on Segregation and Outcomes

To examine the unadjusted relation between segregation and outcomes, Table III divides our sample into cities with high and low levels of segregation, based on whether segregation is above or below the mean.[17] We then compare outcomes for blacks and nonblacks in these two groups of cities. More precisely, our estimate of the effects of segregation is

$$(5) \qquad \begin{bmatrix} \left(Outcome^{Black}_{HighSegration} - Outcome^{Black}_{LowSegration}\right) \\ -\left(Outcome^{White}_{HighSegration} - Outcome^{White}_{LowSegration}\right) \end{bmatrix},$$

17. The mean difference in segregation levels between highly segregated and less segregated cities is 0.21.

TABLE III

PRELIMINARY EVIDENCE ON THE RELATION BETWEEN SEGREGATION AND OUTCOMES

	Age 20–24					Age 25–30				
	Education		Income		Social	Education		Income		Social
	High school graduate	College graduate	Idle	ln(earn)	Single mother	High school graduate	College graduate	Idle	ln(earn)	Single mother
Black										
Low segregation	79.5%	4.4%	15.4%	8.77	36.7%	80.0%	10.7%	15.8%	9.18	40.4%
High segregation	74.0	4.9	21.6	8.61	39.9	77.2	12.0	21.3	9.13	45.4
Difference	−5.5	0.5	6.2	−0.16	3.2	−2.8	1.3	5.5	−0.05	5.0
Nonblack										
Low segregation	86.7%	10.6%	7.0%	9.03	10.8%	88.1%	23.9%	9.9%	9.53	13.2%
High segregation	87.3	14.7	6.6	9.05	9.4	89.3	28.7	9.4	9.57	11.2
Difference	0.6	4.1	−0.4	0.02	−1.4	1.2	4.8	−0.5	0.04	−2.0
Difference-in-difference (B−W)	−6.1% (0.7%)	−3.7% (0.7%)	6.6% (0.6%)	−0.17 (0.03)	4.6% (0.9%)	−4.0% (0.6%)	−3.6% (0.8%)	6.0% (0.6%)	−0.09 (0.02)	6.9% (0.9%)

High segregation MSAs are MSAs with housing segregation above the mean. Idleness is defined as not working and not enrolled in school. Earnings are the sum of wage, salary, and self-employment income in 1989. The sample for earnings is people who are working, not enrolled in school, and have nonnegative earnings. Standard errors for the difference-in-differences estimates are in parentheses.

where $Outcome^{Black}_{HighSegregation}$ refers to the mean outcome for blacks in highly segregated cities, and the rest of the notation is defined similarly. If segregation has an adverse effect on blacks compared with whites, then the difference-in-difference estimate (5) will capture this effect.

The first column of Table III shows that 20–24 year old blacks in more segregated cities have a 5.5 percentage point lower high school graduation rate than 20–24 year old blacks in less segregated cities. Nonblacks have an insignificant 0.6 percentage point higher in graduation rates. Therefore, the total difference-in-difference estimate of the effect of segregation is −6.1 percentage points, which is statistically significant. This effect is large; the mean high school dropout rate for blacks is approximately 25 percent, so this is one-quarter of that baseline rate. This finding is one of our basic results that will reappear in ordinary least squares and instrumental variables regressions: blacks in segregated cities graduate less often from high school than blacks in less segregated cities.

The second column repeats the exercise for college graduation rates. There is an insignificant effect of segregation on the college graduation rate for blacks, but whites in segregated cities are more likely to have graduated from college. The net difference-in-difference estimate is −3.7 percentage points. This again shows a negative effect of segregation on black outcomes.

The third and fourth columns examine idleness and earnings. Segregation increases the share of blacks who are idle by 6.2 percentage points, and decreases the share of whites who are idle by 0.4 percentage points. The difference-in-difference estimate (6.6 percentage points) is 25 percent of the average black idleness rate. Segregation also depresses black earnings relative to nonblack earnings. Both of these differences are statistically significant. The fifth column shows a significant positive effect of segregation on single motherhood (4.6 percentage points) that is more than 10 percent of the average rate of black single motherhood.

Thus, all five differences-in-difference estimates show that segregation significantly hurts black outcomes relative to nonblack outcomes. And with the exception of the college graduation rate, essentially all of the effects of segregation on outcomes occur because segregation influences the outcomes of blacks in more segregated cities relative to blacks in less segregated cities, not because segregation improves outcomes for whites.

Columns 6 through 10 show the same results for 25–30 year olds. Again, all the estimates show an adverse effect of segregation on black outcomes. In addition, all the estimates are statistically significant, and all, except for the college graduation effect, are driven primarily by differences in black outcomes between more segregated and less segregated cities rather than by differences in nonblack outcomes. Further, the magnitude of the differential is about the same for the different age groups. These results will be explored in the subsequent tables, but the results in Table III are extremely robust to a variety of specifications and estimation techniques.

4.1. Ordinary Least Squares Estimates

Table IV reports our basic ordinary least squares estimates of equation (2). The columns in Table IV are structured in a manner similar to those in Table III, with the first five columns reporting results for 20–24 year olds followed by five columns for 25–30 year olds. We estimate linear probability models because of the difficulties in performing instrumental variables for probit models.[18] The standard errors in all of our regressions are corrected for heteroskedasticity and for correlation between observations within the same metropolitan statistical area.

The results in Table IV closely resemble the basic difference-in-difference estimates in Table III.[19] In almost all cases, the cross effect between segregation and the race dummy variable, shown in the second row of the table, is statistically significant and shows that segregation hurts black outcomes relative to white outcomes. This is true for both age groups and for all of the variables with the exception of college graduation. Indeed, the coefficients on segregation for the two age groups are also similar, suggesting roughly comparable effects for all of the young. Just as in Table III the first row of Table IV shows that the effect of segregation on outcomes for whites is small and insignificant.

These coefficients are quite large. A one standard deviation

18. The linear probability results do not differ from the probit results (qualitatively) without instrumentation.

19. We have also run these regressions separately for males and females, and the coefficient on segregation for blacks is almost the same for the two genders for idleness, earnings, and college graduation. The effect of segregation on high school graduation rates is almost 50 percent higher for black males than for black females. Including region dummies or city fixed effects has almost no effect on any of our results. Furthermore, none of our results change if we use nominal, unadjusted income or if we use nominal income and allow local price levels to enter as an independent variable.

TABLE IV

ORDINARY LEAST SQUARES ESTIMATES OF THE EFFECTS OF SEGREGATION ON OUTCOMES

	Age 20–24					Age 25–30				
	Education		Income		Social	Education		Income		Social
Independent variable	High school graduate	College graduate	Idle	ln(earn)	Single mother	High school graduate	College graduate	Idle	ln(earn)	Single mother
Segregation										
Segregation	.016	0.67	−.006	−.060	.008	.021	−.014	.000	−.066	−.023
	(.033)	(.040)	(.019)	(.069)	(.030)	(.025)	(.067)	(.025)	(.067)	(.024)
Segregation * black	−.323	−.081	.324	−.740	.355	−.257	−.050	.279	−.515	.471
	(.044)	(.035)	(.044)	(.150)	(.063)	(.046)	(.052)	(.040)	(.118)	(.059)
Demographics										
Black	−.599	.018	.388	−1.682	.650	−.321	.273	.442	−.957	.683
	(.283)	(.327)	(.313)	(.772)	(.356)	(.271)	(.561)	(.218)	(.587)	(.299)
Asian	.042	.064	−.013	−.034	−.007	.025	.137	−.011	.026	−.021
	(.012)	(.027)	(.008)	(.051)	(.019)	(.008)	(.048)	(.009)	(.059)	(.017)
Other nonwhite	−.134	−.088	.092	−.260	.193	−.147	−.192	.060	−.428	.203
	(.018)	(.010)	(.018)	(.045)	(.026)	(.018)	(.012)	(.013)	(.047)	(.021)
Hispanic	−.161	−.090	.086	−.152	.129	−.160	−.182	.062	−.222	.146
	(.013)	(.012)	(.010)	(.022)	(.015)	(.012)	(.019)	(.009)	(.023)	(.015)
Female	.029	.026	.054	−.278	—	.024	.004	.106	−.451	—
	(.002)	(.003)	(.003)	(.015)		(.002)	(.003)	(.003)	(.016)	

TABLE IV
(CONTINUED)

Independent variable	Age 20–24					Age 25–30				
	Education		Income		Social	Education		Income		Social
	High school graduate	College graduate	Idle	ln(earn)	Single mother	High school graduate	College graduate	Idle	ln(earn)	Single mother
MSA characteristics										
ln(population)	.005	.016	−.003	−.002	−.004	.014	.047	−.007	.030	−.008
	(.003)	(.005)	(.003)	(.008)	(.003)	(.003)	(.007)	(.002)	(.008)	(.003)
ln(population) * black	.007	−.010	−.006	.045	−.032	.003	−.028	.002	.022	−.028
	(.005)	(.004)	(.005)	(.016)	(.006)	(.004)	(.006)	(.004)	(.011)	(.006)
Percent black	−.062	.078	.007	.432	−.098	−.097	.170	−.003	.454	−.096
	(.043)	(.061)	(.021)	(.104)	(.028)	(.032)	(.091)	(.023)	(.083)	(.031)
Percent black * black	.008	−.106	−.004	−.387	−.012	−.072	−.149	−.093	−.365	−.055
	(.071)	(.049)	(.055)	(.177)	(.068)	(.060)	(.078)	(.046)	(.152)	(.072)
ln(median household income)	.028	−.001	−.060	.597	−.008	.035	.053	−.078	.587	−.045
	(.020)	(.042)	(.009)	(.051)	(.013)	(.023)	(.073)	(.013)	(.033)	(.023)
ln(median household income) * black	.054	.008	−.036	.129	−.009	.031	−.003	−.053	.062	−.024
	(.024)	(.033)	(.028)	(.064)	(.030)	(.024)	(.055)	(.018)	(.050)	(.025)
Manufacturing share	−.149	−.152	.049	.251	.087	−.145	−.312	.041	−.120	.067
	(.067)	(.082)	(.035)	(.166)	(.047)	(.048)	(.138)	(.041)	(.140)	(.048)
Manufacturing share * black	.108	.128	.035	−.795	−.085	.130	.212	.093	−.164	.027
	(.103)	(.071)	(.093)	(.308)	(.143)	(.088)	(.120)	(.072)	(.217)	(.120)
Summary statistics										
N	97,976	97,976	97,976	56,627	49,038	139,715	139,715	139,715	105,997	71,531
σ_ε^2	.121	.096	.076	.868	.111	.106	.181	.092	.835	.123
R^2	.034	.093	.050	.090	.108	.031	.040	.049	.092	.109

Idleness is defined as not working and not enrolled in school. Earnings are the sum of wage, salary, and self-employment income in 1989. The sample for earnings is people who are working, not enrolled in school, and have nonnegative earnings. All regressions include single year age dummy variables. Standard errors, reported in parentheses, are corrected for heteroskedasticity and intra-MSA clustering of the residuals.

increase in segregation would reduce earnings of 25–30 year old blacks by 7 percent.[20] Averaging across the different outcomes, a one standard deviation increase in segregation leads to an increase of approximately 10 to 15 percentage points in the probability of a black having an adverse outcome: dropping out of high school, idleness, or single motherhood. This is roughly one-third of the overall difference in adverse outcomes between blacks and whites. Alternatively, if we consider the more extreme experiment of eliminating current levels of segregation entirely, then all of the black-white differences in earnings, high school graduation rates, and idleness would disappear, as would two-thirds of the black-white difference in single motherhood.

V. Correcting for Endogeneity

There are three principal difficulties with the ordinary least squares results. First, segregation may be the result of poor economic outcomes or may reflect omitted city characteristics, rather than be a cause of poor outcomes. Second, individuals who choose to live in more segregated cities may be those who are least successful, while those who move to less segregated cities may be more successful. Third, omitted parental characteristics may be correlated with segregation. All of these factors could result in a spurious correlation between segregation and black outcomes. In this section we address these issues.

5.1. Endogeneity

We deal with the first problem, reverse causality or omitted variables, by instrumenting for segregation with factors that are unlikely to be directly related to black outcomes but that should affect segregation. We use two sets of instruments. The first is public finance characteristics of the metropolitan statistical area that might increase the benefits of segregation or the ability to segregate. We use two such instruments: the number of municipal and township governments encompassed in the metropolitan statistical area and the share of local revenue that comes from

20. In Cutler and Glaeser [1995] we decompose earnings into weeks per year, hours per week, and wages per hour. We found that approximately 75 percent of the relation between earnings and segregation occurs because of a relation between segregation and weeks worked per year and 20 percent of the earnings-segregation relation occurs because segregation depresses hours worked per week. Only 5 percent of this effect occurs because segregation very weakly depresses wages.

intergovernmental sources. The number of local governments could affect segregation through a Tiebout mechanism: when there are more local governments, tax rates and service provision will vary more within an area, and thus the desire for sorting will increase. Similarly, when less money comes from intergovernmental sources, local taxes need to be higher, and the gains from sorting to take advantage of these tax differentials will be greater.

In counting the number of local governments, we use data from the Census of Governments survey (see the Data Appendix). We include only municipal and township governments. Other local governments—school districts and special districts (such as water or fire districts)—vary much more dramatically over time and may be the result of economic differences between the races. The number of municipal and township governments, in contrast, is essentially constant over time; the correlation across metropolitan statistical areas of the number of municipal and township governments in 1962 and 1987 is over .98. To further alleviate concerns about causality, we use the number of municipal and township governments in 1962 as our instrument.[21]

Our second instrument, also from the Census of Governments, is the share of local revenue coming from intergovernmental sources (the state or Federal government). To purge local, endogenous factors from this variable, we measure the share of intergovernmental revenues for the localities in the state as a whole, rather than for each particular metropolitan statistical area. The statewide average of the local tax burden should capture much more of the state-specific political characteristics that we want to include than city-specific factors that may be influenced by outcomes in that city. As with the number of governments, we use the 1962 value of this variable to reduce any

21. The example of Cleveland, Ohio, illustrates why we believe that number of governments is exogenous. The governmental patterns of the area around Cleveland largely reflect the township structure imposed by the Northwest Ordinance of 1787. Through the nineteenth century the city of Cleveland grew by annexing adjoining unincorporated areas and early suburban villages. Economies of scale in the provision of local public goods made a common government structure attractive. As streetcar and automobile transportation technology evolved between 1900 and 1930, the townships of Cuyahoga county were carved into incorporated villages and cities. These cities and villages frequently opposed annexation, in opposition to what they perceived as a corrupt central city government in Cleveland [Shauffler 1941]. By the time the largest waves of black migration arrived during and after World War II, Cuyahoga county's government structure had assumed its modern form. The number of municipal and township governments in Cuyahoga county stood at 60 in both 1930 and 1987.

remaining endogeneity. The correlation between intergovernmental revenue shares in 1962 and 1987 is high ($\rho = .55$).

Table I gives summary statistics for these measures. The average metropolitan statistical area had 40 local governments in 1962, with a range of 1 (Fort Myers, Florida, and Honolulu, Hawaii) to 339 (Philadelphia, Pennsylvania). In the average city in 1962, 28 percent of revenues came from intergovernmental transfers. The range is 12 percent (Nebraska) to 49 percent (New Mexico).

Figures I and II show the relation between segregation and the logarithm of the number of local governments (Figure I) and the intergovernmental transfer share (Figure II), both using the 1962 values.[22] As predicted, segregation is positively related to the number of local governments and negatively related to the intergovernmental transfer share. Indeed, a regression of segregation on these two factors yields (standard errors in parentheses):

$$\begin{aligned}\text{Segregation} = {} & .053 \times \text{Log(Number of Governments)} \\ & (.006) \\ & - .228 \times \text{Transfer share} \quad N = 208 \\ & (.097) \qquad\qquad\qquad R^2 = .312.\end{aligned}$$

Since our basic regression specification (2) includes segregation and its interaction with the black dummy variable, we instrument for these variables with the two fiscal variables and their interactions with the black dummy variable.

Panel A of Table V presents instrumental variables estimates of the effect of segregation on outcomes. These regressions include the full set of demographic and metropolitan statistical area controls included in Table IV, but for simplicity, we report only the coefficients on segregation and its interaction with the black dummy variable. The instrumental variables results are extremely close to the ordinary least squares results: segregation adversely affects black outcomes for all outcome measures except college graduation. For example, the coefficient in the high school graduation regression for 20–24 year olds (column 1) is $-.323$ in the ordinary least squares regressions and $-.405$ in the instrumental variables estimates. Segregation now has moderately

22. We use the logarithm of the number of governments because it explains segregation better than the level.

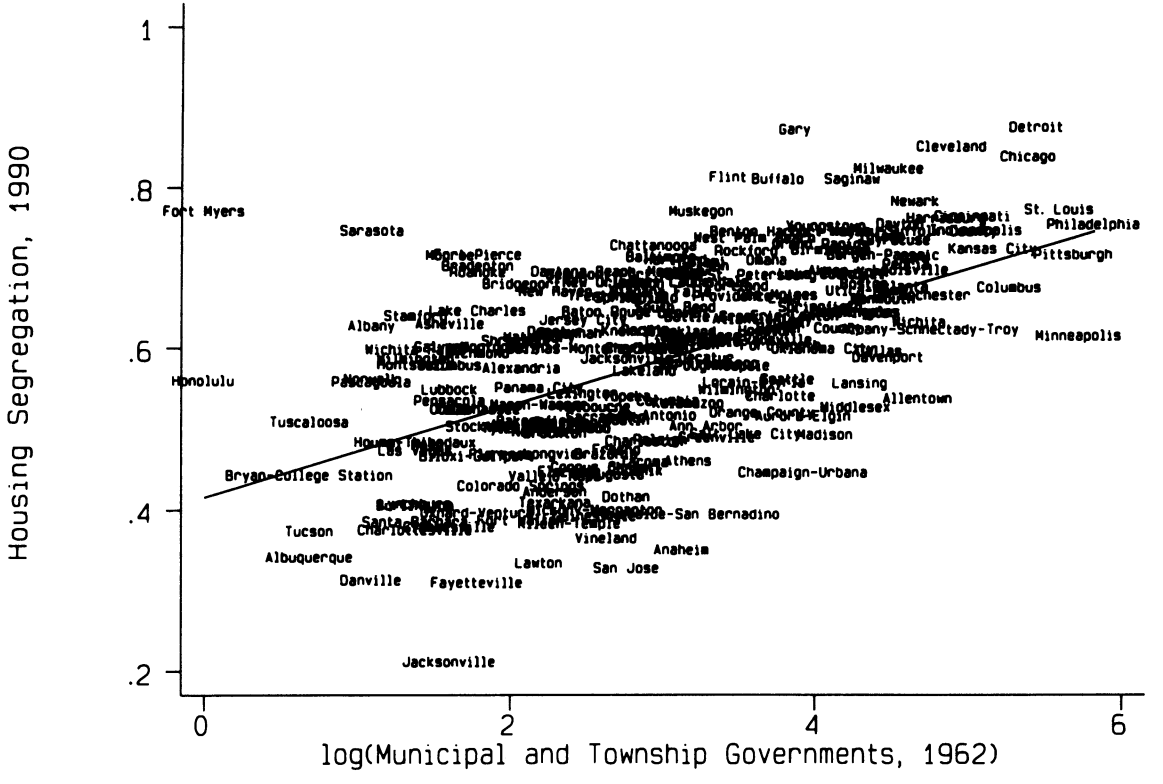

FIGURE I
Segregation and the Number of Municipal and Township Governments

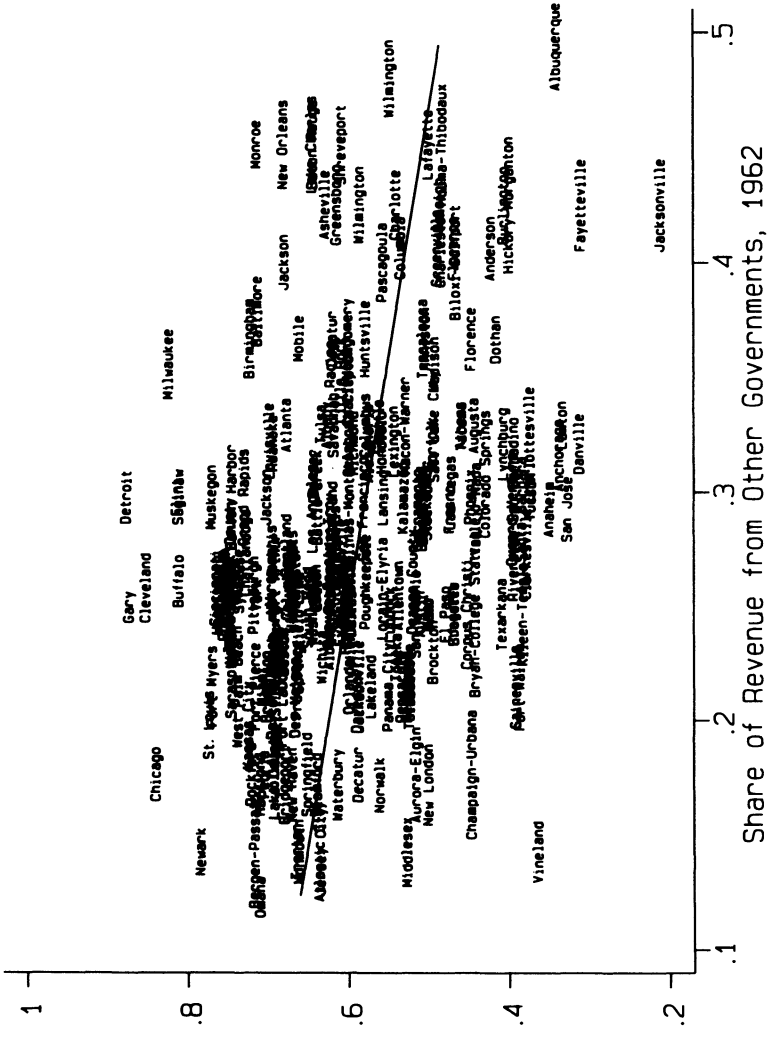

FIGURE II
Segregation and the Share of Intergovernmental Revenue

TABLE V

INSTRUMENTAL VARIABLES ESTIMATES OF THE EFFECT OF SEGREGATION ON OUTCOMES

	Age 20–24					Age 25–30				
	Education		Income		Social	Education		Income		Social
Independent variable	High school graduate	College graduate	Idle	ln(earn)	Single mother	High school graduate	College graduate	Idle	ln(earn)	Single mother
					A. Fiscal variables as instruments					
Segregation	.129	.211	−.046	−.042	−.051	.076	.095	.005	−.005	−.108
	(.044)	(.053)	(.025)	(.095)	(.038)	(.032)	(.077)	(.028)	(.090)	(.035)
Segregation * black	−.405	−.201	.317	−.921	.326	−.231	−.121	.295	−.532	.583
	(.085)	(.056)	(.087)	(.236)	(.101)	(.076)	(.069)	(.062)	(.196)	(.116)
N	97,976	97,976	97,976	56,627	49,038	139,715	139,715	139,715	105,997	71,531
σ_ε^2	.121	.096	.076	.868	.111	.107	.181	.092	.835	.123
					B. Topographical data as instruments					
Segregation	.040	.122	.018	−.208	−.105	.003	.034	−.068	−.126	−.020
	(.078)	(.099)	(.051)	(.217)	(.066)	(.063)	(.150)	(.052)	(.238)	(.065)
Segregation * black	−.579	−.168	.329	−1.100	.261	−.291	−.149	.558	−.719	1.030
	(.199)	(.109)	(.173)	(.602)	(.217)	(.135)	(.140)	(.184)	(.299)	(.242)
N	90,684	90,684	90,684	52,281	45,442	129,324	129,324	129,324	97,973	66,276
σ_ε^2	.122	.096	.076	.873	.112	.107	.180	.093	.838	.124
					C. Fiscal variables from city of residence five years previously					
Segregation	.189	.238	−.060	.082	−.077	.112	.232	−.018	.143	−.137
	(.044)	(.051)	(.022)	(.108)	(.040)	(.036)	(.079)	(.024)	(.094)	(.038)
Segregation * black	−.265	−.229	.197	−.791	.109	−.231	−.255	.311	−.566	.480
	(.083)	(.053)	(.086)	(.266)	(.091)	(.076)	(.073)	(.062)	(.188)	(.107)
N	95,955	95,955	95,955	54,084	47,950	137,496	137,496	137,496	104,078	70,596
σ_ε^2	.124	.092	.078	.895	.112	.110	.177	.095	.869	.126

Idleness is defined as not working and not enrolled in school. Earnings are the sum of wage, salary, and self-employment income in 1989. The sample for earnings is people who are working, not enrolled in school, and have nonnegative earnings. All regressions include single year age dummy variables. Regressions include the same set of controls as in Table IV. Panel A uses as instruments the logarithm of the number of governments in 1962 and its interaction with black, the percent of revenue from intergovernmental transfers in 1962 and its interaction with black. Panel B uses as instruments the number of inter- and intracounty rivers, the square of the number of inter- and intracounty rivers, and each of these variables times the share of the city that is black. Panel C uses as instruments the fiscal variable for the city the individual lived in five years previously. The sample for Panel C is individuals who were living in a city five years previously, whether or not they live in a city in 1990. Standard errors, reported in parentheses, are corrected for heteroskedasticity and intra-MSA clustering of the residuals.

positive effects on outcomes for whites. However, these effects are much smaller than the adverse effects of segregation on blacks.

A second set of instruments is based on the topography of the metropolitan statistical area—the number of inter- and intracounty rivers in the metropolitan statistical area. Since rivers divide metropolitan statistical areas into natural subunits, we expect areas with more rivers to have more segregation; the use of topographical barriers as instruments follows the approach developed by Hoxby [1994, 1996].[23] The river data are only available for 179 metropolitan statistical areas, but these metropolitan statistical areas account for over 90 percent of the population in our sample.[24] We include quadratic terms in the number of rivers to capture potential nonlinearities in the relationship between rivers and segregation. A regression relating segregation to the number of rivers yields

$$\begin{aligned}\text{Segregation} = {} & .033 \times \text{Intercounty Rivers} \\ & (.010) \\ & - .0018 \times \text{Intercounty Rivers}^2 \\ & (.0008) \\ & + .015 \times \text{Intracounty Rivers} \\ & (.004) \\ & - .0005 \times \text{Intracounty Rivers}^2; \\ & (.0002) \\ & N = 179 \\ & R^2 = .198.\end{aligned}$$

The positive and concave relationship between rivers and segregation suggests that natural boundaries do indeed increase the costs for minorities of moving into white neighborhoods.

The number of rivers may affect segregation by increasing the number of local governments, by making it harder for individuals to leave the ghetto and still stay close to their old neighborhood, by increasing differentiation among housing units over space, or by providing obvious boundaries that facilitate the exclusion of a minority group from a neighborhood. Empirically, the first of these effects seems more important, because when we control for governmental fragmentation, the number of rivers has relatively little ability to explain segregation. Thus, while we

23. We are extremely grateful to Caroline Minter Hoxby for the use of these instruments.

24. The rivers data are available for consolidated metropolitan statistical areas, while our segregation and outcome data are grouped by primary metropolitan statistical areas. This only affects a few areas and should not have a large effect on the results.

present instrumental variable results using the rivers data, we place more emphasis on the results using public finance variables.

Panel B of Table V shows regressions of outcomes on segregation instrumented using the number of rivers. We use the number of rivers and their square, and these variables interacted with the black dummy variable as instruments. We again find our results substantially unchanged in these instrumental variables estimates, although the standard errors increase appreciably, because of the poorer first-stage fit.[25] For example, in the high school graduation rate regression for 20–24 year olds, the coefficient on the interaction of segregation and race rises in absolute value from −.405 to −.579. At the same time, however, the standard error of this coefficient more than doubles. Moving across the columns, the magnitudes of the coefficients with this set of instruments are similar to the magnitudes using the fiscal variables as instruments and to the ordinary least squares results. Thus, even accounting for the loss in precision, the topographical data also suggest that segregation adversely affects outcomes for blacks relative to whites. These results, along with the results using the public finance variables as instruments, lead us to reject the view that segregation is a result of poor black outcomes.[26]

Our results offer a somewhat different view of the relationship between topographical barriers and outcomes than Hoxby [1994]. She finds that competition among schools improves the productivity of schooling and student outcomes, using the topographical barriers variable that she developed to predict competition, which we also employ. In part, the difference between Hoxby and us is that Hoxby focuses on the effect of barriers on the entire community, while we focus on the effect of barriers on differences between whites and blacks. Furthermore, we consider only the effects of topography as it is correlated with racial segregation, not any effects that topography might have through increased school quality or other channels. The different effects of barriers within a metropolitan area are quite interesting and merit further research.

25. If we use both the fiscal variables and the natural boundaries variables, our estimates become more precise, and the estimated coefficients are very close to the coefficients using the fiscal variables alone.

26. Our instruments generally fail the standard test of overidentifying restrictions, primarily because of the very large number of observations in our regressions, and this test is based on an assumption of uncorrelated error terms, which is plainly violated in our data. When we performed tests based on city-level

5.2. Cross-Metropolitan Area Mobility

Addressing the potential bias that occurs when abler blacks leave highly segregated cities is more difficult. If we had information on the individual's city of birth, we could instrument with segregation in that city. Even this measure, however, is not immune to the bias that results from parental choice of location before birth. And, in practice, the census does not ask individuals about their city of birth, so we cannot use this as an instrument. However, the census does ask individuals where they were living five years previously. We use that information to address the mobility issue.

As Table VI shows, five-year migration rates are reasonably high, particularly among college graduates; approximately 30 percent of 25-to-30 year old black college graduates are living in a different metropolitan area in 1990 than in 1985. This figure is much smaller for less educated blacks; only 13 percent of black high school dropouts have changed metropolitan areas. While there is a shift of black population from more to less segregated cities, this shift occurs across the education spectrum and also occurs for whites. Since the migration patterns of blacks look similar to those of whites and our estimates look only at the differences between blacks and whites, the results of Table VI do not suggest a selective migration problem.

To address this issue formally, we estimate models based on residence of the individual five years before the census. In particular, we relate current outcomes to segregation in the city of residence five years previously, instrumenting with the fiscal variables in the city of residence five years previously. Our sample for these regressions is everyone living in a metropolitan area five years previously, including individuals who did not live in a metropolitan area in 1990.

Panel C of Table V shows these results. The coefficients on the interaction of segregation and the black dummy variable are very similar to the ordinary least squares and the other instrumental variables results. For example, the coefficient in the high school graduation rate regression for 20–24 year olds is −.265, which is quite close to the previous panels. Similar results are true across the columns. We conclude that selective migration by young adults is not a particularly large problem.

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average regressions, our fiscal variables still generally fail the overidentifying restrictions test, but our topographical variables generally pass this test.

TABLE VI
MOBILITY BY RACE AND EDUCATION

Variable	Age 20–24, by education				Age 25–30, by education			
	High school dropout	High school graduate	Some college	College graduate	High school dropout	High school graduate	Some college	College graduate
Black mobility								
Move—more segregated city	3%	5%	6%	11%	3%	5%	7%	12%
Move—less segregated city	5	9	11	13	4	6	9	14
Move to rural area	5	4	5	5	5	4	4	4
Do not move	87	83	77	71	87	85	80	70
N	3,658	5,186	4,938	650	4,188	6,078	6,003	2,083
White mobility								
Move—more segregated city	5%	5%	8%	14%	5%	6%	9%	16%
Move—less segregated city	6	8	12	16	5	6	9	15
Move to rural area	9	9	12	8	10	9	9	9
Do not move	81	78	69	61	79	78	73	60
N	10,803	24,820	35,709	10,191	13,813	36,097	38,425	30,809

The sample is people who lived in cities in 1985. Each cell entry is the share of people with that education who moved from a city in 1985 to an area with the characteristics indicated in the row. The columns add to 100 percent in each case.

5.3. Family Background Controls

Because the Census does not ask about family background, our results in Tables IV and V were not able to differentiate the effects of segregation from potential parental influences. In an attempt to control for family background, we consider the relation between segregation and outcomes using the National Longitudinal Survey of Youth. We examine outcomes from the National Longitudinal Survey sample in 1990, when the individuals in the survey were between 26 and 33 years old (see the Data Appendix). This is roughly the age range for the older sample from the Census.

Panel A of Table VII reproduces the regressions from Table IV with this survey. The results are very similar to the results found using Census data. Segregation adversely affects high school graduation rates, idleness, earnings, and single motherhood, and is unrelated to college graduation rates. Further, the magnitudes of the coefficients are similar to our other results.

The second panel of the table relates outcomes to segregation in the city where the individual lived in 1979 (the earliest year of survey data), when people in the survey were between 15 and 22 years old. We do this to examine the issue of interurban mobility. These results are similar to those in the first panel. Segregation is strongly related to increases in idleness, decreases in earnings, and an increased rate of single motherhood among blacks. The one difference is that with this measure, segregation is no longer related to high school graduation rates.

The third panel of the table adds controls for family background: a dummy variable for a male householder or spouse of householder; a dummy variable for female householder or spouse of householder; dummy variables for less than high school, high school graduate, some college and college graduate of the male and female householders; and dummy variables for the number of workers in the family (0; 1; 2 or more). Controlling for family background characteristics has little effect on the results. In general, the coefficients in Panel C are within 90 percent of the coefficients in Panel B.

Finally, in the last panel we reestimate the regressions in Panel C, instrumenting for segregation using our 1962 fiscal variables for the city of residence at age 14. These results confirm strong positive effects of segregation on single motherhood, earnings, and, unlike Panels B and C, high school graduation rates.

TABLE VII
LONGITUDINAL ESTIMATES OF THE EFFECT OF SEGREGATION ON 1990 OUTCOMES

	Education		Income		Social
Independent variable	High school graduate	College graduate	Idle	ln(earn)	Single mother
A. 1990 segregation of 1990 residence					
Segregation	.053	−.202	−.107	−.344	−.024
	(.044)	(.120)	(.078)	(.199)	(.076)
Segregation * black	−.241	−.018	.268	−.631	.777
	(.094)	(.142)	(.141)	(.289)	(.162)
N	7175	7175	7175	5843	3778
σ_ε^2	.139	.149	.235	.889	.165
B. 1980 segregation of 1979 residence					
Segregation	.035	−.192	−.092	.166	−.023
	(.086)	(.171)	(.144)	(.317)	(.118)
Segregation * black	−.010	−.032	.470	−1.479	.782
	(.142)	(.228)	(.230)	(.451)	(.203)
N	6623	6623	6623	5221	3453
σ_ε^2	.144	.144	.237	1.008	.171
C. 1980 segregation of 1979 residence, parental variables included					
Segregation	.044	−.078	−.077	.176	−.015
	(.074)	(.140)	(.146)	(.325)	(.112)
Segregation * black	.036	−.035	.426	−1.413	.790
	(.129)	(.200)	(.227)	(.452)	(.194)
N	6623	6623	6623	5221	3453
σ_ε^2	.132	.125	.231	.978	.166
D. 1962 number of governments of 1979 residence as instrument for 1980 segregation, parental variables included					
Segregation	.456	.492	−.369	1.158	−.171
	(.210)	(.246)	(.235)	(.633)	(.146)
Segregation * black	−.496	−.838	.534	−2.593	.918
	(.294)	(.302)	(.392)	(.855)	(.390)
N	6287	6287	6287	4951	3274
σ_ε^2	.131	.123	.229	.986	.164

Regressions include the same set of controls as in Table IV. NLSY sample weights are used in all specifications. Idleness is defined as not working and not enrolled in school. Earnings are the sum of wage, salary, and self-employment income in 1989. The sample for earnings is people who are working, not enrolled in school, and have nonnegative earnings. Parental variables are indicators for whether one or both parents worked at age 14, whether the respondent lived in a single-parent household at age 14, whether a parent graduated from high school, and whether a parent graduated from college. Standard errors, reported in parentheses, are corrected for heteroskedasticity and intra-MSA correlation of the residuals.

The effect of segregation on idleness is large economically, but the standard errors increase significantly, so that the coefficient is not statistically significant.

We have also examined the effects of segregation on 16–18

year olds living at home with their parents, controlling for parental attributes. In these results, we found that controlling for family background characteristics reduces the effect of segregation on idleness but leaves the coefficient on the single motherhood regression largely unchanged. O'Regan and Quigley [1996a, 1996b] also focus on outcomes for teenagers, controlling for parental attributes. They also find that parental attributes do not eliminate the adverse effect of segregation on outcomes. O'Regan and Quigley's [1996b] results suggest that eliminating racial segregation entirely would raise employment among 16-to-19 year old blacks who are living at home by 10 percentage points (a one-third increase in the employment rate for this group).

Thus, using a variety of instruments, and a variety of different data sets, we confirm that segregation harms blacks. The ordinary least squares estimates suggest that segregation has no effect on whites, but instrumental variables results more often suggest that segregation has a small, positive effect on white outcomes. Given the weak nature of the results for whites, we do not feel comfortable drawing strong conclusions about this effect.[27] However, we believe that our estimates of the adverse effects of segregation on blacks are strong and convincing.

5.4. Differential Effects of Segregation

Segregation may have different effects on outcomes for more and less skilled blacks. We examined this issue using Census and National Longitudinal Survey of Youth data. Using both forms of data, segregation appears to be more harmful to the earnings of less educated minorities or minorities from less educated families. However, segregation has statistically the same effect on the idleness, single parenthood, and graduation rates for individuals from more or less educated families. Overall, we believe that segregation may be most harmful for the poorest minorities, but that our evidence is weak, and this issue needs further investigation.

Alternatively, segregation may not be costly for blacks per se, but for all residents of central cities—black or white. The differential effect of segregation on blacks may just be a result of the

27. If whites were helped by segregation, it would be easier to understand the history of white attempts to enforce segregation. However, since we are not including any attempts to measure the extent of discriminatory tastes or housing price effects of integration, this paper cannot give a thorough analysis of why discrimination occurs.

fact that central city residents are disproportionately black. To test this hypothesis, we estimated regressions interacting central city residence and segregation.[28] In essentially all of the equations that we ran (reported in Cutler and Glaeser [1995]), the effect of segregation on outcomes appears to be more an effect of race than a central city effect. But because the number of blacks outside central cities is small, these results are not very precise. This is an issue that also merits future research.

VI. How Does Segregation Affect Outcomes?

Having determined that segregation affects outcomes, in this section we examine why this occurs. We consider three hypotheses. The first hypothesis is that racial segregation proxies for overall segregation by income within the city and that blacks fare worse in cities where people are more segregated by income.[29] To test this hypothesis, we construct income segregation measures for the entire population, measuring the degree to which the top 25 percent of the income distribution is separated from the rest of the population, as in equation (4).

The second column of Table VIII shows summary statistics for income segregation. The average city had income segregation of 22 percent, which is strikingly lower than the level of housing segregation. The maximum level of income segregation is 34 percent (Stamford, Connecticut); the minimum is 11 percent (Hickory-Morganton, North Carolina). Income segregation and housing segregation have a correlation of .24, as the bottom of the table shows.

The second hypothesis is that blacks in more segregated cities fare worse because they have less contact with better role models than blacks in less segregated cities. To test this, we created an exposure measure of the interaction between blacks and those with more education in each city. We start with the percent of the average black's census tract that is well educated, or[30]

28. As central city residence may also be endogenous, we instrumented for central city residence using the share of a metropolitan statistical area's land area that is in the central city.

29. An alternative formulation of this hypothesis is that all poor residents fare worse when there is more income segregation but since blacks are poorer than whites, this shows up as a differential effect for blacks. It is easier to test the hypothesis about black outcomes, however, so we use that formulation.

30. This type of index is often used in the segregation literature; see Massey and Denton [1993].

TABLE VIII

ALTERNATIVE VARIABLES EXPLAINING FOR POOR OUTCOMES

Variable	Housing segregation	Income segregation	Education exposure	Single mothers	Males' education	Time to work
Number of cities	209	209	209	205	205	205
Mean	.586	.218	−.086	.247	−.173	−0.14
Standard deviation	.126	.038	.045	.117	.106	2.64
Minimum	.206	.112	−.206	−.165	−.514	−9.43
Maximum	.873	.342	.081	.587	.143	7.30
			Correlations			
Housing segregation	1.000					
Income segregation	.240	1.000				
Education exposure	−.453	−.302	1.000			
Single mothers	.379	.038	−.206	1.000		
Males' education	−.055	−.176	.411	−.228	1.000	
Time to work	.082	.085	−.019	−.031	.081	1.000

The correlations are for 205 cities with all data series.

$$\sum_{i=1}^{N}\left(\frac{Black_i}{Black}\right)\times\left(\frac{Educ_i}{Persons_i}\right).$$

We define educated people as those who attended college for some time, whether or not they obtained a degree.[31] If all census tracts were racially and educationally equal, this number would be the metropolitan statistical areawide proportion of educated persons. We subtract this metropolitan statistical areawide average from our measure of education exposure, since we do not want to identify our estimates based on average differences in achievement across metropolitan statistical areas. Our measure of educational exposure is therefore

$$(8)\quad Education\ Exposure = \sum_{i=1}^{N}\left(\frac{Black_i}{Black}\right)\times\left(\frac{Educ_i}{Persons_i}\right)-\left(\frac{Educ}{Persons}\right).$$

Our education exposure measure will be greater than zero if blacks differentially live in census tracts with more educated people and less than zero if blacks differentially live in census tracts with less educated people. As Table VIII shows, the mean level of educational exposure is $-.086$, and the standard deviation is .045. Consistent with the theory, blacks have more exposure to educated people in cities with less housing segregation ($\rho = .45$).

The third hypothesis is that blacks fare worse than nonblacks because jobs are more prevalent in white areas, and blacks find the commuting cost to jobs prohibitively high. This is the purest form of the spatial mismatch view, as proposed by Kain [1968]. To test this, we include the time that the average black spends commuting to work (in minutes) relative to the time the average nonblack spends commuting to work. Of course, this variable only includes the jobs where blacks are in fact employed. This number may, therefore, undercount the true distance between blacks and the relevant jobs. We have no way to correct this problem. As Table VIII shows, blacks and whites live virtually the same distance from jobs in our sample.[32]

31. The number of educated persons and the total number of persons include only individuals over the age of 25. Our education exposure measure is highly correlated (approximately 90 percent) with a measure capturing interactions with high school graduates. We also formed income exposure measures and found that these were highly correlated with the education exposure measures.

32. This finding does contradict earlier work on the spatial mismatch hypothesis—see Kain [1992]—which was done on different, and often much older, samples for different time periods.

Finally, we control for parental background factors by including variables for family background of older adults in the metropolitan statistical area: the proportion of women in the 40–60 year old age group who are unmarried but have children, and the proportion of men in the same age group with some amount of college education. In both cases, we have normalized these variables by subtracting the means for nonblacks from the means for blacks. These variables are meant to proxy for the family background characteristics discussed above.

Table IX presents regression results including these additional variables. We report results from regressions like those in Table IV for 25–30 year olds. We use ordinary least squares estimates because we do not have adequate instruments for all the different explanatory variables.

The education, parental, and travel variables are all related to poor outcomes. The share of blacks who get a high school or college education rises with exposure to educated people, and the probability of a black female being a single mother declines with exposure to more educated people. In cities where a greater number of older women are single mothers, children are more likely to become single mothers. Having more educated adult males increases the probability that a black child will receive a college degree. Finally, greater average distance from work for blacks is associated with an increased probability of being idle and lower wages conditional on working.

Still, including these additional variables does not eliminate the effects of segregation on outcomes. For all of the outcomes except college graduation, the coefficient on segregation interacted with blacks is statistically and economically significant. Indeed, as the last row of the table shows, these alternative factors in total explain about 23 to 32 percent of the effects of housing segregation on outcomes. These factors are, thus, important in explaining the link between segregation and outcomes, but even accounting for them, there is a substantial residual relation between housing segregation and adverse outcomes for blacks.

VII. Conclusion

Our examination of segregation and outcomes reaches three conclusions. First, blacks are significantly worse off in segregated communities than they are in nonsegregated communities. If we measure success by high school graduation rates, not being idle,

TABLE IX
ALTERNATIVE EXPLANATIONS FOR THE EFFECT OF SEGREGATION ON OUTCOMES, AGES 25–30

	Education		Income		Social
Independent Variable	High school graduate	College graduate	Idle	ln(earn)	Single mother
Segregation					
Housing segregation	.015	−.096	.031	−.156	−.010
	(.028)	(.063)	(.022)	(.083)	(.027)
Housing segregation * black	−.183	.021	.212	−.351	.362
	(.049)	(.053)	(.043)	(.143)	(.072)
Income segregation					
Income segregation	−.098	−.252	−.053	.311	.238
	(.092)	(.237)	(.068)	(.293)	(.092)
Income segregation * black	.542	.226	−.006	−.276	−.403
	(.191)	(.208)	(.152)	(.502)	(.209)
Education spillovers					
Education exposure	.026	−.376	.117	−.376	.101
	(.086)	(.186)	(.051)	(.227)	(.078)
Education exposure * black	.740	.381	−.120	1.045	−.605
	(.192)	(.184)	(.145)	(.466)	(.310)
Parents' background					
Single mothers	−.023	−.030	−.009	.124	.031
	(.036)	(.069)	(.023)	(.098)	(.032)
Single mothers * black	−.040	.070	.099	−.187	.128
	(.090)	(.073)	(.061)	(.195)	(.098)
Male education	−.086	−.324	.087	−.043	.109
	(.037)	(.083)	(.024)	(.085)	(.032)
Male education * black	−.002	.247	.023	−.441	−.060
	(.080)	(.081)	(.060)	(.218)	(.124)
Time to work					
Mean time	.0009	.0012	−.0002	.0008	.0007
	(.0013)	(.0026)	(.0010)	(.0029)	(.0010)
Mean time * black	−.0017	−.0013	.0055	−.0114	.0023
	(.0024)	(.0024)	(.0020)	(.0058)	(.0031)
N	139,715	139,715	139,715	105,997	71,531
R^2	.032	.045	.051	.096	.110
Reduction in coefficient on segregation * black	29%	—	24%	32%	23%

Regressions include the controls in Table IV. Idleness is defined as not working and not enrolled in school. Earnings are the sum of wage, salary, and self-employment income in 1989. The sample for earnings is people who are working, not enrolled in school, and have nonnegative earnings. Standard errors, reported in parentheses, are corrected for heteroskedasticity and intra-MSA clustering of the residuals.

earnings, or not becoming a single mother, then integration is intimately associated with success. Our estimates suggest that a one-standard-deviation reduction in segregation (13 percent) would eliminate one-third of the gap between whites and blacks in most of our outcomes. Our instrumental variables results suggest that segregation leads to adverse outcomes, not that adverse outcomes result in more segregation.

Second, we find only a small effect of segregation on outcomes for whites. In our ordinary least squares estimates, outcomes for whites are statistically indistinguishable between highly segregated and less segregated cities. Our instrumental variables results suggest that segregation helps whites a little, but it would be valuable to better understand those effects.

Third, some of the effects of segregation on outcomes are associated with other factors such as less exposure to educated people, worse outcomes for parents, and longer times to work. These factors, however, do not explain more than one-third of the effect of segregation on outcomes. We are left with the conclusion that segregation is extremely harmful for blacks, but we do not have an exact understanding of why this is true.

These results may have important public policy implications. Policies addressing racial problems have typically focused on equalizing black and white education, or eliminating discriminatory hiring practices of employers. Our results imply that neighborhood segregation may be a particularly important factor contributing to differences in black-white outcomes. It may be that widespread social changes in attitudes toward minorities and housing choices will be required before equality of outcomes can finally be achieved.

THEORY APPENDIX

In this appendix we will show that it is possible that a stable equilibrium described in the text exists, and conditional upon that equilibrium existing, the propositions in the text are true.

Characterization of Equilibrium. All of the whites live in one area (termed the white neighborhood). Some of the skilled blacks live in the white neighborhood, and some live in a second neighborhood (the skilled black neighborhood). Some unskilled blacks live in the skilled black neighborhood, and some live in the third neighborhood (the unskilled black neighborhood).

Proof of Existence. We begin with some notation. The skill

level of skilled blacks and whites (labeled S and W, respectively) is $\overline{H}$, and the skill level of unskilled minorities (labeled U) is $\underline{H} < \overline{H}$. The population levels of the three groups in the whole city will be denoted N_W, N_S, and N_U, respectively. Since each neighborhood is associated with a particular group, we can also use the subscripts W, S, and U to refer to neighborhoods as well as groups. Thus, the populations of the three neighborhoods will be P_W, P_S, and P_U, respectively, and the average human capital level of each neighborhood will be H_W, H_S, and H_U respectively. We use N_{SW} to refer to the number of skilled minorities in the white area and N_{US} to refer to the number of unskilled minorities in the skilled minority area. In the equilibrium described $H_W = \overline{H}$, $H_U = \underline{H}$, $H_S = [(N_S - N_{SW})\overline{H} + N_{US}\underline{H}]/[N_S - N_{SW} + N_{US}]$, $P_W = N_W + N_{SW}$, $P_S = N_S - N_{SW} + N_{US}$, and $P_U = N_U - N_{US}$.

For skilled minorities to be willing to live both in their own area and the white area, it must be true that

(A1)
$$H(\overline{H},\overline{H}) - \delta_b - C_W(N_W + N_{SW}) = H(\overline{H},H_S) - C_S(N_S - N_{SW} + N_{US}).$$

For unskilled minorities to live in their own area and in the skilled minority area, it must be true that

(A2)
$$H(\underline{H},H_S) - C_S(N_S - N_{SW} + N_{US}) = H(\underline{H},\underline{H}) - C_U(N_U - N_{US}).$$

For large ranges of parameter values and different $C(.)$ and $H(.,.)$ functions, both of these equations can hold, and we assume the parameter values and functions necessary for both of these equations to hold. Furthermore since $H_S > \underline{H}$ and $H_{12}(.,.) > 0$, then $H(\overline{H},H_S) - C(P_S) > H(\overline{H},\underline{H}) - C(P_U)$, so skilled minorities will never live in the unskilled minority area. Using equation (A1), $H_{12}(.,.) > 0$, and $\overline{H} > H_S$, it follows that $H(\underline{H},H_W) - \delta_b - C(P_W) < H(\underline{H},H_S) - C(P_S)$, so no unskilled minorities would want to live in the white area. Finally, using (A1), $\delta_b + \delta_w > 0$, and $H(\overline{H},H_S) - C(P_S) > H(\overline{H},\underline{H}) - C(P_U)$, it immediately follows that no whites would live in either minority area. Together these conditions imply that neither whites nor skilled minorities will ever live in the unskilled minority neighborhood, and no unskilled minorities will live in a white area. Thus, the equilibrium exists. We now turn to prove that it can be stable.

Equation (A2) can be used to define a function $N_{US}(N_{SW})$,

which refers to the number of unskilled minorities who will live in the skilled minority area depending upon the number of skilled minorities who live in the white area. Differentiating (A2) reveals that

(A3)
$$\frac{\partial N_{US}}{\partial N_{SW}} = \frac{C_S'(P_S)P_S^2 - H_2(\underline{H},H_S)(\overline{H} - H_S)P_S}{P_S^2(C_S'(P_S) + C_U'(P_U)) + H_2(\underline{H},H_S)(N_S - N_{SW})(\overline{H} - \underline{H})},$$

where $H_2(.,.)$ refers to the derivative of $H(.,.)$ with respect to its second argument. The term described in (A3) can be either positive or negative depending on whether the effect of increased N_{SW} creating lower housing costs in the skilled neighborhoods is more or less important than the effect of increased N_{SW} creating lower human capital spillovers in the skilled neighborhoods. Using the function, $N_{US}(N_{SW})$, we can also define, $H_S(N_{SW}) \equiv [(N_S - N_{SW})\overline{H} + N_{US}(N_{SW})\underline{H}]/[N_S - N_{SW} + N_{US}(N_{SW})]$, $P_W(N_{SW}) \equiv N_W + N_{SW}$, and $P_S(N_{SW}) \equiv N_S - N_{SW} + N_{US}(N_{SW})$.

The intuitive notion of neighborhood stability that we use requires that

(A4)
$$\Phi \equiv \frac{\partial\left[\begin{matrix}(H(\overline{H},\overline{H}) - \delta_b - C_W(P_W(N_{SW})))\\ -(H(\overline{H},H_S(N_{SW})) - C_S(P_S(N_{SW})))\end{matrix}\right]}{\partial N_{SW}} < 0,$$

where the numerator of the expression refers to the net benefits for a skilled minority of moving into the white neighborhood. If this net benefit rose with N_{SW}, then a slight movement of skilled minorities into the white neighborhood would increase the gains to moving into that neighborhood and in an explicit dynamic setting, create a stampede of skilled minorities into the white neighborhood. This would make the equilibrium unstable. As long as $C_W'(P_W) + C_S'(P_S) > H_2(\overline{H},H_S)dH_S/dN_{SW}$, which it will as long as housing costs rise sharply enough as population rises (which we assume—this assumption is completely compatible with earlier assumptions), then the equilibrium is stable.

Total differentiation of the indifference condition for skilled minorities tells us that $\partial N_{SW}/\partial\delta = 1/\Phi < 0$. Thus, we have shown that a decrease in discrimination, around a stable equilibrium, will cause an increase in the influx of skilled minorities into the white neighborhood. We now turn to proving the two propositions.

PROPOSITION 1. If increased racial segregation (δ_b) results in increased segregation by skill within the black community, then increased segregation by race reduces welfare for unskilled blacks. The effect on skilled blacks is ambiguous.

PROPOSITION 2. If increased racial segregation (δ_b) results in less segregation by skill within the black community, then increased segregation by race raises welfare for all blacks, with the greatest effect on skilled blacks.

Proof. Unskilled minority utility is always equal to $H(\underline{H},\underline{H}) - C(N_U - N_{US})$, which is strictly increasing in N_{US}. Thus, if N_{US} rises with racial segregation (which is by definition a decrease in segregation by skill), then unskilled black utility rises with racial segregation. If N_{US} falls with racial segregation (which is by definition an increase in segregation by skill), then unskilled black utility falls with racial segregation. Differentiation of the unskilled minority's indifference condition tells us that $\partial N_{US}/\partial\delta = (\partial N_{US}/\partial N_{SW}) * (\partial N_{SW}/\partial\delta)$. The sign of this derivative depends on the sign of equation (A3), and equation (A3) is positive if and only if $C_S'(P_S)P_S > H_2(\underline{H},H_S)(\overline{H} - H_S)$, which will hold and fail to hold for different parameter values and function forms. Thus, we have proved the first of the two propositions.

Unskilled minority utility always equals $H(\underline{H},H_S) - C_S(P_S)$, and skilled minority utility always equals $H(\overline{H},H_S) - C_S(P_S)$. Differentiating these terms with respect to δ_b reveals that skilled minorities will benefit from increases in discrimination more than unskilled minorities as long as

$$(A5)\quad H_2(\overline{H},H_S)\frac{dH_S}{d\delta} - C_S'(P_S)\frac{dP_S}{d\delta} > H_2(\underline{H},H_S)\frac{dH_S}{d\delta} - C_S'(P_S)\frac{dP}{d\delta}.$$

The complementarity assumptions implies that $H_2(\overline{H},H_S) > H_2(\underline{H},H_S)$. If rising discrimination causes a decrease of unskilled minorities and an increase in skilled minorities in the skilled minority neighborhood, then obviously, $dH_S/d\delta > 0$. If rising discrimination causes an increase in skilled minorities and an increase in unskilled minorities, then the rental cost of living in the skilled minority neighborhood has risen, the rental cost of living in the unskilled minority neighborhood has fallen, and the only way that the unskilled minorities can still be indifferent between the two neighborhoods is for H_S to rise. Thus, whenever segregation falls, the exodus of skilled blacks to the white neigh-

borhood causes the human capital level in the skilled minority neighborhood to fall; i.e., $dH_S/d\delta > 0$. Given these two conditions, it must be true that an increase in segregation must be more beneficial for skilled minorities than it is for unskilled minorities. As such, it follows that when segregation is good for unskilled minorities, it must be even better for skilled minorities (as in Proposition 2).

In the region where unskilled minorities are hurt by segregation, it must be true when this damage is arbitrarily close to zero that the skilled minorities benefit from segregation. Alternatively, skilled minorities are hurt by segregation if $H_2(\overline{H},H_S)dH_S/d\delta - C'(P_S)dP_S/d\delta < 0$. If the mobility response of the unskilled workers is arbitrarily small, so $\partial N_{US}/\partial N_{SW} \approx 0$, this condition becomes $C'(P_S)P_S > H_2(\overline{H},H_S)(\overline{H} - H_S)$, which must both hold and fail to hold for some parameter values, so it must also be true that for some parameter values skilled minorities are hurt by segregation, and the second sentence of Proposition 1 holds.

Data Appendix

This appendix describes the sources of our data. All of the data are available through online data at http://www.nber.org.

Segregation. Our data on segregation are computed at the census tract level. A census tract is an area of about 4000 people separated by major streets or other landmarks. We use Primary Metropolitan Statistical Areas to proxy for a city. A typical Primary Metropolitan Statistical Area has about 200 census tracts, with some cities having over 10,000. The census tract information are in Summary Tape File 3A of the 1990 Census, which were read from CD-ROMs.

The definition of segregation is

$$\text{(3)} \qquad \textit{Housing Segregation} = \frac{1}{2}\sum_{i=1}^{N}\left|\frac{Black_i}{Black} - \frac{Nonblack_i}{Nonblack}\right|.$$

We define income segregation analogously to housing segregation, substituting "Rich" for "Black." Empirically, we consider households in the top 25 percent of the income distribution in each city to be rich.

Census Microdata. The census data are from the 1990 Census 1 % Public Use Micro Sample. Earnings are total wages, salary, and self-employment income in 1989. We estimate our

earnings models for people who worked at all in the previous year and were not enrolled in school. We deflate earnings by city-specific price indices, taken from the American Chamber of Commerce Research Associates. The American Chamber of Commerce Research Associates data give relative price levels across cities. For each city we found the closest time period to the first quarter of 1990 where data were available. Some cities (50 in total) did not have published price information. For these cities we imputed price indices based on the fitted values of a regression of price indices on the logarithm of city median household income, the logarithm of city population, and region dummy variables.

We note that Hispanics can be either white or black. We do not differentiate between segregation of blacks and whites separately by Hispanic origin. Cutler, Glaeser, and Vigdor [1996] examine how Hispanic origin affects segregation of blacks and whites and concludes that the effects are minor.

Primary Metropolitan Statistical Area Data. We use a number of metropolitan statistical area-level controls in our analysis. The first two variables are the logarithm of metropolitan statistical area population and the percent of metropolitan statistical area population that is black, both from the Summary Tape Files noted above. The third variable is the logarithm of median household income in 1989, from the Census. The final variable is the share of the metropolitan statistical area that was employed in manufacturing in the 1990 Census. For this variable the census reported information at the consolidated metropolitan statistical area, not primary metropolitan statistical area level. We use the value for the consolidated metropolitan statistical area as a whole in each of the primary metropolitan statistical areas.

Our data on the number of local governments are from the Census of Governments, available on data tape. We use the number of municipal and township governments in 1962 as our primary instrument. We include all local governments in the boundary of the metropolitan statistical area as it was defined in 1990. The data on intergovernmental revenue sharing are also from the Census of Governments and were obtained from the published volumes.

National Longitudinal Survey of Youth Data. We obtain longitudinal data from the geocoded version of the National Longitudinal Survey of Youth. This data set contains annual observations for approximately 12,000 individuals nationwide, beginning in 1979 and continuing to 1993. Respondents were between 15 and

22 years old in 1979. Our subsample contains individuals not in the military oversample residing in a metropolitan statistical area in either 1979 (Table VII, Panels B, C, D), or 1990 (Table VII, Panel A). Our race variable separates the populations into blacks, Hispanics, and individuals of neither group; this variable does not separately identify Asians or other nonwhites. Again, Hispanics can be either black or white. The 1990 outcome variables are coded as follows: High school graduates reported completing twelve or more years of school. College graduates reported completing four or more years of college. Idle individuals reported neither having a job, attending school, nor serving in the military in the survey week. The earnings variable is the log of wage and salary income in the previous calendar year. Single mothers report having their own children in their household and not being married. Parental variables, specifically whether one or both parents worked, whether the respondent lived in a single-parent household, and parent educational attainment are derived from data items describing the respondent's household structure at age fourteen.

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