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Evidence on Discrimination in Consumer Markets

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In New Orleans in 1996, a black woman arrived at an apartment complex for an appointment and identified herself to the rental agent. After waiting 15 minutes, she was given a tour. When she asked for an application, the agent said that it was locked in her car. The agent then opened her car door, got in the car, and drove away. A short time later, an equally qualified white woman arrived for her appointment and saw the agent speaking with another person. The agent immediately excused herself from the conversation and took the white customer on a tour. After the tour, the agent handed the white customer an application and encouraged her to put down a deposit on the apartment (Fair Housing Action Center, 1996, p. 13). In San Antonio in 1997, an Hispanic female applying for a two-bedroom apartment was quoted a rent of $670 per month with a $200 security deposit. A equally qualified white female applying for the same apartment was quoted a special rent of $616 per month with a $100 security deposit (San Antonio Fair Housing Council, 1997, p. 19).

These true stories illustrate the types of behavior that many black and Hispanic Americans encounter when they search for housing or buy a car, consumption items that together make up one-fifth of personal consumption expenditures (Council of Economic Advisers, 1997, Table B-14). Similar acts of discrimination may occur in other consumer markets, although this topic has not yet been studied. This behavior not only violates the law but also violates the widely held principle of equal treatment. Moreover, it restricts the options of many black and Hispanic households and contributes to continuing intergroup disparities in income, home ownership, wealth, education, and employment (Galster, 1991; Yinger, 1995). This paper reviews economists' contributions to the measurement of discriminatory barriers in consumption and to the identification of their causes.

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Preliminaries

Before turning to the evidence on discrimination, we must take care of some preliminaries, including definitions and issues of methodology.

Race and Ethnicity

Although many people think of "race" as a meaningful biological concept, this view is now rejected by the vast majority of experts on the subject. Genetic differences between "races," however defined, involve, at most, a tiny fraction of human genes and have not been shown to have any significant behavioral consequences (Cavalli-Sforza, 1995; Shanklin, 1994). Moreover, human history involves so much migration and blending of people with different birthplaces that genetic distinctions across groups of people disappear over time. In the United States, for example, the vast majority, probably at least 90 percent, of people who have some African ancestry also have European ancestry, and as many as 25 percent of people who appear to be "white" may have African or Native American ancestors (Simpson and J. Milton Yinger, 1985). Thus, our so-called racial distinctions are literally only skin deep.

This point is important because it reminds us that differences in the treatment of "racial" groups are based on social distinctions, not innate biological ones. The distinction between blacks and whites flows from our history and is maintained by the stereotypes, prejudice, and socioeconomic disparities that are the legacy of this history. Some social distinctions based on culture, instead of on superficial physical characteristics, also become associated with social conflict. In this country, an "ethnic" distinction of this type exists between people whose ancestors came to America directly from Europe and people who can trace their ancestry back to Spain or Portugal through Mexico, Central America, or South America, often called Hispanics. Hispanic people can often be identified by their accent or their surname, and conflicts between Hispanics and non-Hispanics, sometimes associated with immigration, have given social significance to this distinction.

The line between "racial" and "ethnic" distinctions is not clear. In some regions the distinction between Hispanics and non-Hispanics gains additional power because of superficial physical differences between non-Hispanic whites and Hispanics, many of whom have Native American or African ancestors. Moreover, social distinctions based on race often gain a cultural dimension over time, as people who are treated differently because of their superficial physical characteristics, such as skin color or the shape of facial features, develop behavioral patterns that respond to or rationalize their differential treatment. Indeed, some scholars argue that we should drop the misleading notion of a racial distinction and redefine an ethnic group to be any group that has some shared culture and that perceives itself and is perceived by others as separate (J. Milton Yinger, 1993). In this paper we are concerned with cases in which such ethnic distinctions are being used to treat certain people in ways that society deems to be inappropriate.

Definition of Discrimination

Our civil rights laws define discrimination as the unfavorable treatment of a person solely on the basis of that person's membership in a "protected class." This definition codifies the principle of equal treatment; under a wide range of circumstances, people in protected classes and people in the "majority" should be treated equally. In the words of the 1968 Fair Housing Act (Section 804), "it shall be unlawful . . . to discriminate against any person . . . because of race, color, religion, sex, or national origin."[1] This act covers many types of discriminatory behavior, including refusal to sell or rent, differential terms or conditions, differential advertising, and the provision of inaccurate information.

A more precise legal definition of discrimination is based on two standards, and a violation of either standard by an economic agent constitutes discrimination. The first standard involves the "disparate treatment" of customers on the basis of their membership in a protected class. According to the disparate treatment standard, any economic agent who applies different rules to people in protected groups is practicing discrimination.

The second standard involves the use of practices with a "disparate" or "adverse" impact on the members of a protected class. Under the Fair Housing Act, along with civil rights legislation that applies to employment and public accommodations, economic agents are discriminating whenever they use practices that do not explicitly consider a person's group membership but instead have an adverse impact on a protected class without any "business necessity" (Schwemm, 1992). Under current law, if a particular practice can be shown to have an adverse impact on a protected class, then the burden of proof shifts to the business, which must then support a business necessity claim. This so-called "effects test" rules out a potential loophole in an anti-discrimination law, because it prevents a business from disguising its mistreatment of a racial or ethnic group as an apparently neutral policy based on a characteristic that is highly correlated with race or ethnicity but is not necessary for business success. Moreover, this test requires all firms to eliminate out-moded rules of thumb and other unnecessary business practices that have a disproportionate impact on a protected class.

Some types of behavior are illegal according to both standards, but the two standards are logically separate. Disparate treatment of a protected class is illegal in and of itself, regardless of whether it can be shown to have an adverse impact. Moreover, business rules that have an adverse impact on a protected class, and cannot be shown to be a business necessity, are illegal even if applied equally to all groups.

Following the lead of most scholars, this paper uses an analytical definition of discrimination that coincides with the legal one. However, some economists have argued that unfavorable treatment by an economic agent is discrimination only if it is motivated by that agent's prejudice. For example, Becker (1993, p. 18) writes

[1] In 1988, amendments to the Fair Housing Act defined new protected classes based on age, familial status, and handicap. For a detailed overview of this act, as amended, see Schwemm (1992).

that "discrimination in the marketplace consists of voluntarily relinquishing profits, wages, or income in order to cater to prejudice."[2] This definition implies that profit-based actions against a particular group should not be called discrimination. Some people might argue that the law should follow this narrower definition, as well.

This alternative approach is consistent with the adverse impact standard; business practices that are profitable, and for which there is no equally profitable substitute without an adverse impact, would generally satisfy the business necessity test. However, legitimizing profit-based discrimination would be a dangerous and inappropriate step, in my view, because it undermines the disparate treatment standard.[3] To be specific, this narrower definition would allow a business to have explicit rules denying opportunities to members of certain protected classes, as long as the rules could be shown to enhance profits. Allowing such rules would be a dramatic rejection of the principle of equal treatment. It may prove helpful to investigate the extent to which people discriminate in order to increase their profits, but such actions violate the disparate treatment standard, regardless of their motivation. Indeed, it is quite plausible that discrimination (by the legal definition) sometimes is profitable. As discussed below, economic incentives that lead to the unfavorable treatment of certain groups today reflect the socioeconomic disparities and prejudicial attitudes that are the product of past discrimination. To some degree, therefore, legitimizing profit-based discrimination would validate and institutionalize our discriminatory past.

Testing for Discrimination

Scholars have used two main techniques to search for the existence of discrimination in various consumption markets: regression analysis and audits.

The regression methodology employs some consumption outcome, typically a price, as the dependent variable and group membership indicators, along with relevant controls, as the explanatory variables. The test for discrimination is whether the coefficient for the relevant group membership variable is significant. The regression methodology, originally developed for labor markets (Cain, 1986), also has been applied to housing (Chambers, 1992; Wachter and Megbolugbe, 1992; Keil and Zabel, 1996) and car sales (Goldberg, 1996).

The regression approach is subject to several potential biases. The three most important are omitted variable bias, "included variable" bias (Killingsworth, 1993), and, to coin a phrase, diverting variable bias. Omitted variable bias arises when

[2] Ironically, this is not the definition used by Becker in his classic treatise on discrimination (1971), which includes examples of discrimination based on economic incentives, such as catering to the preferences of customers. Another way to look at this issue is proposed by Cain (1986), who distinguishes between "individual" and "group" discrimination. The latter is defined as behavior that puts one group at a disadvantage, on average, which is similar to the notion of "disparate impact." Cain shows how some types of profit-based individual discrimination involve group discrimination whereas others do not.

[3] There are also efficiency arguments for rejecting certain kinds of profit-based discrimination. See Schwab (1986).

relevant control variables correlated with group membership are omitted from the regression. This bias can work in either direction. One well-known example arises in a study of housing prices when neighborhood variables, such as the quality of the surrounding housing, are excluded from the regression (Keil and Zabel, 1996). Because neighborhood quality is poorer, on average, for black households, leaving out these variables biases the coefficient of the variable indicating black households toward zero—that is, away from a finding of discrimination.

The included variable problem, a kind of endogeneity bias, arises when some of the explanatory variables are influenced by the economic actors whose discriminatory behavior is being studied. If car dealers refuse to give loans to people in protected classes, for example, then a variable indicating which purchasers received loans could bias downward an estimate of discrimination in car prices. A similar bias also could arise from a control for a trade-in allowance whenever dealers offer a higher allowance to whites than to minorities relative to the market price of the vehicle that is traded in (Goldberg, 1996).

Diverting variable bias arises when a variable that is not a legitimate control variable, but that is correlated with race or ethnicity, is included in the regression. The key issue, of course, is how to define what variables are "legitimate." Under most circumstances, economists are taught to err on the side of including too many variables. In this case, however, illegitimate controls may pick up some of the effect of race or ethnicity and lead one to conclude that there is no discrimination when in fact there is. According to the definition of discrimination used here, legitimate controls are those associated with a person's qualifications to rent or buy a house, buy a car, and so on—or, to use the legal term, those associated with business necessity. The problem is that economic agents may base their decisions on variables that are not legitimate in this sense. Suppose, for example, that landlords use a rule of thumb, no longer related to their profits, that people in certain occupations make better tenants and should be charged lower rents. If blacks are underrepresented in these occupations, then controlling for occupation in a regression to explain rents will lead to an underestimate of discrimination against blacks. This leaves researchers in a bind. The standard test is whether a control variable is statistically significant, but in this context this tests whether agents rely on a variable, not whether it is a business necessity. The information needed for an independent confirmation that a certain practice is a business necessity is generally not available. As a result, researchers tend to retain all significant controls. This approach is more conservative than the adverse impact standard, under which a practice constitutes discrimination unless it can be shown to be a business necessity.

The regression approach is indirect: it attempts to isolate the impacts of discrimination on prices or other outcomes without directly observing discriminatory behavior. In contrast, an audit can literally catch economic agents in the act of discriminating (Yinger, 1986).

An audit is a matched-pair survey technique that allows researchers to observe how economic agents behave. In an audit study, people from two different groups, one of which is a protected class, are selected, trained, and assigned to two-group pairs such that teammates are equally qualified to buy a house or a car. A sample

of the agents whose behavior is being studied, landlords or car dealers, for example, is then drawn. Audit teammates successively visit each agent to inquire about an advertised housing unit or a type of vehicle. After the visit, each teammate independently records how he or she was treated. Discrimination is said to exist if auditors in the protected class are systematically treated worse than their teammates.

An audit avoids many of the pitfalls facing regression studies. In particular, researchers can match similar individuals, give them the same training, assign them similar or identical characteristics, such as income, for the purposes of the audit, and send them to visit economic agents within a short time of each other. These procedures are designed to ensure that audit teammates do not differ significantly on any characteristic, other than race, ethnicity, or sex, that is relevant to their treatment in the market place. In effect, therefore, a careful application of these procedures can minimize, if not eliminate, the possibility of omitted, included, and diverting variable biases.

However, audit studies face challenges of both design and management (Heckman and Siegelman, 1993a, 1993b; Yinger, 1993b, 1995). To take one example, researchers must decide whether to make audits "blind," in the sense that teammates are unaware of the purpose of the study or of the fact that they have a teammate. The auto sales audits of Ayres (1995) and Ayres and Siegelman (1996) are blind in this sense, and Heckman and Siegelman (1993b) argue that this step is important to protect the integrity of audit results. In the case of housing audits, however, Yinger (1993b, 1995) points out that some discriminatory treatment is so egregious (refer to the example that begins this paper) that it can upset black or Hispanic auditors and compromise their ability to fill out audit survey forms accurately. Under these circumstances, telling auditors the purpose of the study and training them to fill in the forms as objectively as possible helps preserve the accuracy of the audit information. No evidence yet exists, however, for comparing audits with and without a "blind" design.

When these challenges are handled thoughtfully, I believe that audits can provide powerful evidence about discrimination, both in narrative form and in the form of statistical tests. Economists have developed a variety of methodologies to test the hypothesis that membership in a protected class influences an auditor's treatment.[4] These tests recognize unique features of audit design, such as the fact that audit teammates share some kinds of unobserved treatment (Yinger, 1986).

Regardless of the statistical tests employed, audit results must be interpreted with care. An audit study indicates the discrimination that occurs during certain phases of a market transaction when members of a protected class visit a random sample of firms and are qualified to buy what the firm is offering, not the discrim-

[4] Audit studies use many different statistical tests. Wienk et al. (1979) use a sign test; Yinger (1986), Ayres (1995), and Ayres and Siegelman (1996) use a paired difference-of-means test (or equivalent regression procedure); Heckman and Siegelman (1993b) provide exact small-sample tests and tests that recognize the possibility of heterogeneity across audit teams; Page (1995) uses a likelihood ratio test based on a conditional maximum likelihood procedure; and Ondrich, Stricker and Yinger (forthcoming) use a fixed-effects logit.

ination faced by an average member of that protected class. An audit study also may make it possible to estimate whether discrimination varies with auditor characteristics or audit circumstances and even, perhaps, to estimate what discrimination would be for a person with average characteristics for a particular group (Yinger, 1995). However, audits may not be able to measure discrimination in every phase of a market transaction; for example, housing audits cannot observe discrimination in negotiations with a seller over the final sale price of a house. Moreover, audits may not detect business practices that have an adverse impact but do not involve disparate treatment.

Measuring Discrimination

Estimating the magnitude of discrimination turns out to be turns out to be far more complicated than it may at first appear. Following the literature on discrimination in consumption, I will concentrate on measurement with audit data.

Consider first the *incidence* of discrimination for some type of agent behavior, such as showing an advertised apartment. The vast majority of audit studies measure this incidence using one of two simple measures. The gross incidence of unfavorable treatment, or the gross measure for short, is the share of audits in which the auditor in the protected class is treated less favorably than his or her teammate. As first pointed out by Wienk et al. (1979), however, discrimination is the systematic unfavorable treatment of a protected class, so a measure of discrimination should not be affected by random differences in treatment. It should not reflect, for example, a case in which an apartment is rented after a white auditor sees it but before her black teammate even inquires about it. Wienk et al. argue that the share of audits in which the teammate from the protected class is favored provides an estimate of the extent to which random factors are at work. Thus, they suggest looking at the net incidence of unfavorable treatment, or the net measure for short, which equals the gross measure minus the share of audits in which the auditor in the protected class is treated more favorably.

The problem, of course, is that the net and gross measures are often far apart. Some economists favor the net measure because it is conservative; by ignoring random factors, the gross measure may overstate the incidence of discrimination, whereas the net measure can be seen as a lower bound. As pointed out by Fix, Galster, and Struyk (1993) and Yinger (1993a), however, this lower bound can be quite inaccurate, because some actions that favor members of a protected class are the result of systematic, not random factors. Consider, for example, the case in which real estate brokers decide not to show houses in largely black neighborhoods to white customers; the net measure implicitly regards this behavior as random, when in fact it is a type of systematic behavior that in no way offsets discrimination that blacks may encounter when they look at houses in white neighborhoods.

An alternative approach taken by Ondrich, Ross, and Yinger (1997) is to use the features of an audit study to estimate the roles played by systematic and random factors and then to remove random factors explicitly when calculating the incidence

of discrimination. For example, they estimate the extent to which unfavorable treatment varies with audit circumstances and then identify cases in which white auditors are favored for systematic reasons—cases that should not be "netted out" in calculating discrimination. This approach reveals that existing hypothesis tests, which are universally based on the net measure, are not really testing the hypothesis that discrimination exists. Instead, they are testing the more conservative hypothesis that members of the protected class are more likely to encounter unfavorable treatment than are whites (or white men), which is not the same thing as discrimination.

For types of agent behavior that are a matter of degree, such as the number of houses shown to a customer, audits have also been used to explore the *severity* of discrimination, defined as the extent to which some people experience less favorable treatment solely because of their membership in a protected class. All existing studies employ a net measure of severity or something analogous to it, defined as the average difference in treatment between white and minority auditors. However, the above analysis of the incidence of discrimination implies that this approach understates discrimination because it nets out cases in which members of a protected class are favored for systematic reasons.[5] Formally, the literature estimates the magnitude of the difference in treatment between whites and members of various protected classes, not of discrimination.

How Much Discrimination Actually Exists?

Many studies have explored the magnitude and statistical significance of discrimination in housing. A few others have examined discrimination in car sales, and one examines related behavior in fast-food purchases.

Housing

Audit studies of housing discrimination were conducted as far back as 1955 (Yinger, 1995). National audit studies sponsored by the U.S. Department of Housing and Urban Development were conducted in 1977 (Wienk et al., 1979) and 1989 (Turner, Struyk and Yinger, 1991; Turner and Mickelsons, 1992; Yinger, 1993a, 1995). Dozens of smaller audit studies, many of which are reviewed in Galster (1990b), also have been conducted during the last 20 years (see also Roychoudhury and Goodman, 1992, 1996). Both national studies and virtually all of the smaller studies find levels of discrimination that are large in magnitude and statistically significant.

[5] The severity and incidence of discrimination are logically connected. Let P_W be the share of audits in which the white auditor is favored and let D_W be the extent to which whites are favored in those audits. Similarly, let P_P be the share of audits in which the auditor in the protected class is favored and let D_P be the extent to which protected-class auditors are favored in those audits. Note that P_W is the gross measure of the incidence of discrimination and $(P_W - P_P)$ is the net measure. Then, as shown by Yinger (1993a), the average difference in treatment between white and minority auditors is $(P_W D_W - P_P D_P)$. Intuitively, this approach starts with a "gross" measure of severity, $P_W D_W$, and subtracts the minority-favored analog, $P_P D_P$, to obtain a "net" measure of severity.

The 1989 national study, called the Housing Discrimination Study (HDS), examined discrimination against both blacks and Hispanics. Black-white audits were conducted in 20 metropolitan areas and Hispanic-white audits were conducted in 13 areas, with both types of audit in eight of these areas. The sites were selected so that the audits would yield nationally representative results. In each site, housing advertisements were randomly selected from the major metropolitan newspaper for several weekends during the spring and summer of 1989. Each audit was based on one of these advertisements. Auditors were all given the same training, audit teammates were assigned virtually identical economic and family characteristics for the purposes of each audit, and the order in which teammates conducted each audit was determined randomly. In total, HDS conducted 1,081 black-white audits in the sales market, 801 black-white audits in the rental market, 1,076 Hispanic-white audits in the sales market, and 787 Hispanic-white audits in the rental market.[6]

The 1989 Housing Discrimination Study examined discrimination in a wide range of housing agent behavior, and Table 1 presents a few of the incidence results.[7] For example, this table reveals that, based on the net measure, black renters faced a 10.7 percent chance of being excluded altogether from housing made available to comparable white renters and a 23.5 percent chance of learning about fewer apartments. For virtually every type of agent behavior in this table, one can reject the hypothesis of no discrimination at conventional levels of significance.

Table 1 also reveals that the gross measure can be close to the net measure or far above it. To lessen the resulting uncertainty about the true incidence of discrimination, it is possible, as mentioned earlier, to separate discrimination from random differences in treatment using statistical procedures. Ondrich, Ross and Yinger (1997), for example, apply this approach to the HDS data and obtain estimates of discrimination that are always above the net measure, but are closer to the net measure in some cases and closer to the gross measure in others.[8]

The incidence of discrimination does not appear to be abating in recent years. A detailed comparison of the HDS results with those of the 1977 national study finds no clear evidence of a trend in either direction (Yinger, 1995). The preliminary evidence that has become available since HDS shows no sign of a trend since 1989, either. During the last three years, fair housing groups have conducted black-white audit studies in five metropolitan areas (Fresno, Montgomery, New Orleans, San Antonio, and Washington, D.C.) and Hispanic-white audit studies in three areas (Fresno, San Antonio, and Washington, D.C.). These studies, which use standard audit procedures but have not yet received in-depth scrutiny from scholars, yield results that are roughly comparable to those in HDS (Fair Housing Council of Fresno County, 1995; Central Alabama

[6] In this context, Hispanic-white is shorthand for Hispanic compared to non-Hispanic white.

[7] HDS researchers also examined the efforts of real estate brokers to "steer" black and Hispanic customers toward black and Hispanic neighborhoods (Turner and Mickelsons, 1992).

[8] Ondrich, Ross, and Yinger (1997) estimate a probit model (as do Kenney and Wissoker, 1992, and Roychoudhury and Goodman, 1992) with some new twists. For example, they estimate a random effect for audit teams, which accounts for the possibility of heterogeneity across teams, an issue raised by Heckman and Siegelman (1993b).

Table 1
The Incidence of Discrimination in Housing, 1989 Housing Discrimination Study

	Black-White Audits		*Hispanic/Non-Hispanic Audits*	
	Net	*Gross*	*Net*	*Gross*
Sales Audits				
Excluded from available units	6.3*	7.6	4.5+	7.5
Advertised unit inspected	5.6*	13.3	4.2*	13.2
Number of houses made available	19.4*	44.1	16.5*	43.6
Auditor asked to call back	3.3*	25.9	11.5*	30.4
Auditor received follow-up call	7.7*	18.5	5.5*	16.4
Auditor received positive comments on house	12.5*	47.9	7.5*	47.5
Agent offered to help auditor find financing	11.3*	24.4	4.4+	22.1
Rental Audits				
Excluded from available units	10.7*	15.1	6.5*	12.1
Advertised unit inspected	12.5*	23.0	5.1	17.6
Number of apartments made available	23.3*	41.4	9.8	34.6
Auditor asked to call back	15.8*	30.5	8.6*	28.5
Auditor received special renal incentives	5.4*	10.3	5.1*	12.6
Auditor received positive comments on apartment	16.8*	48.4	14.6*	46.4

Note: A * (+) indicates statistical significance at the 5 percent two-tailed (one-tailed) level based on a fixed-effects logit procedure (net measure only).
Source: Yinger (1995, Tables 3.1, 3.3) and calculations by the author.

Fair Housing Center; 1996; Fair Housing Action Center, 1996; The Fair Housing Council of Greater Washington, 1997; San Antonio Fair Housing Council, 1997).

Several audit studies also examine the severity of discrimination in housing (Page, 1995; Roychoudhry and Goodman, 1992, 1996; Yinger, 1986, 1993a, 1995). In the HDS data, for example, black home buyers learn about 23.7 percent fewer houses than do their white teammates, black renters learn about 24.5 percent fewer apartments, Hispanics learn about 25.6 percent fewer houses, and Hispanic renters learn about 10.9 percent fewer apartments (Yinger, 1995, Table 3.2). All of these differences are statistically significant.

Overall, this research demonstrates that black and Hispanic home seekers continue to encounter discrimination in many aspects of a housing transaction. They are told about fewer available units and must put forth considerably more effort to obtain information and to complete a transaction. These barriers are not absolute, but they impose significant costs on black and Hispanic home seekers relative to comparable whites in the form of higher search costs, poorer housing outcomes, or both (Yinger, 1995, ch. 6).

Car Sales

The literature on discrimination in car sales contains both audit studies (Ayres 1991, 1995; Ayres and Siegleman, 1995) and a regression study (Goldberg, 1996)

that examine discriminatory pricing behavior. Car rental companies also have been accused of discrimination, but no studies of this behavior have been conducted (Myerson, 1997).

The three audit studies are based on audits conducted at car dealers in Chicago in the early 1990s. Ayres (1991) examines 165 audits, Ayres and Siegelman (1995) examine 306 paired audits, and Ayres (1995) examines the same 306 paired audits plus 98 unpaired visits. These studies are characterized by careful attention to teammate matching and, as indicated earlier, by the use of "blind" audits. All of the auditors in the study fell in the same age range (aged 24 to 28), had about the same education (three or four years of college), were of average attractiveness, wore the same type of clothing during an audit, were assigned urban professional occupations, were assigned fake addresses in the same upper-class neighborhood, and were told to volunteer that they did not need a loan. The audits were conducted for selected car models at a random sample of car dealers in the Chicago area; auditors were randomly matched with dealers; and audit teammates visited a dealer in a random order. Finally, audit teammates always elicited a starting offer from the dealer, then used the same bargaining strategy, either a "split-the-difference" strategy or a "fixed-concession" strategy, up to the point when the dealer either accepted the auditor's last offer or refused to make any further concessions.

All three studies yield similar results. Profits on the dealer's initial and final offers both were highest for black males, followed by black females, white females, and white males. Moreover, the differences were quite large. Ayres and Siegelman (1995, Table 2) find, for example, that compared to the results from white males, final profits average $1,061 more from black men, $405 more from black women, and $129 more from white women. The first two of these differences are statistically significant. They find similar differences for the profits on the dealers' initial offers.

Goldberg (1996) explores price discrimination in car sales using a careful application of regression analysis. In contrast to the audit studies, Goldberg concludes (p. 652) that there is "no evidence of price discrimination against blacks or women." Her data, a sample of 1,279 households in the Consumer Expenditure Survey who purchased cars between 1983 and 1987, make it possible to control for region, type of vehicle, and several buyer characteristics other than group membership. For a variety of specifications, she finds, with one exception, that white men do not obtain significantly lower discounts below list price than do white women, minority women, or minority men, where "minority" is defined as black, Hispanic, or Native American. The exception is that minorities receive much smaller discounts (the point estimate is $2,439) in the purchase of vans or utility vehicles.

Can the audit and regression results be reconciled? As Goldberg points out, the data she uses have several advantages over the audit data: her data comes from a national sample, not just Chicago; her sample covers all makes of cars, not a smaller number of representative models; and her study observes actual transactions prices, not offers. She also points out, however, that the data have the disadvantages that they are based on households, not individual buyers, and that they may not eliminate all omitted variable bias. For example, the Consumer Expenditure Survey

data make it possible to control for the major options on each car—automatic transmission, air conditioning, sunroof, power steering, and power brakes—but do not include information on other options, such as paint protection, leather seats, sound systems or extended warranties, which might be correlated with gender or minority status. In addition, only 3 percent of her sample is minority males and only 2.2 percent is minority females (Goldberg, 1996, Table 5); with so few minority buyers, it may be impossible to estimate price discrimination with precision.

Goldberg (1996) also explains that her results do not necessarily contradict the audit results because they apply to a random sample of households, not of car dealers. For example, minorities may tend to shop where they do not expect to encounter discrimination. Goldberg dismisses this possibility based on findings in Ayers and Siegelman (1995): discrimination is no lower when the owners or salesman for a dealership are black or when the dealership is in a largely black area. Another possibility is that Goldberg's results are biased because they exclude minority customers who were discouraged from buying because of discrimination. After conducting several selection corrections, Goldberg also rejects this possibility.

Instead, Goldberg (1996) focuses on a bargaining explanation for the difference between her results and those of the audit studies. She argues (p. 643) that the selection and training of auditors in the audit studies eliminated the effects of actual "differences in the reservation price distributions between minorities and whites or the different bargaining strategies the two groups may engage in." Goldberg assumes that blacks have a higher variance in reservation prices than do whites and that car dealers know this. She then derives two results from a bargaining model. First, agents will offer lower initial discounts to blacks in an attempt to flush out those with high reservation prices—a kind of statistical discrimination. This prediction is supported by the large disparities in initial offers found by the audit studies. Second, dealers will respond to the low reservation prices of other blacks (revealed by their willingness to keep bargaining) by raising the discounts in subsequent rounds of the negotiations. This conclusion is not contradicted by the disparities in final offers found by the audit studies because those studies effectively impose the same reservation price on audit teammates.

The problem with this argument, in my view, is that it loses touch with the definition of discrimination. It says, in effect, that it is not discrimination to treat a group less favorably when they use the same bargaining strategy and have the same reservation price as other groups (as in the audits), so long as the resulting differences are eliminated, on average, by the group's willingness to hold out for a better price. In fact, a researcher cannot isolate discriminatory behavior without controlling for buyer characteristics that might influence the outcome, including not only whether the buyer is a first-time buyer or has a trade-in, which are in the Goldberg regressions, but also the buyer's reservation price and bargaining strategy (or the underlying socioeconomic factors that determine them), which are not. Without these controls, regression-based estimates of discrimination may be biased. Goldberg hints at this alternative interpretation by acknowledging (p. 642) that one of the "weaknesses" of the Consumer Expenditure Survey data is that they do "not contain any information about the actual bargaining strategies employed."

Ultimately, because of limitations in the Consumer Expenditure Survey data and the difficulties of interpreting the regression results, I find the evidence from the audit studies to be more compelling. But additional research is needed to determine whether the differences in the results are due to differences in the studies' samples or to biases in one or both of these lines of research.

Fast-Food Sales

One recent study, Graddy (forthcoming) uses regression analysis to examine the racial dimension of fast-food pricing.[9] This study is based on a data for 356 fast-food restaurants in New Jersey and eastern Pennsylvania in 1992. These restaurants make up a large share of the restaurants in four major chains. The prices refer to three items at each store: an entree (regular hamburger or two-piece chicken order), a medium soda, and a small order of french fries. This study asks whether, after accounting for differences in income and cost, prices for these items are higher in black than in white neighborhoods (defined as zip-codes). Thus, the study addresses the problem of *redlining*, defined as unjustified differential treatment of people in a particular location, not of discrimination. Indeed, discrimination in fast-food pricing is unlikely since prices are posted in each restaurant. Discrimination in service can occur, however, as demonstrated by the refusal of several Denny's restaurants to serve blacks—behavior that resulted in an $54 million settlement in 1994 (Myerson, 1997).

Graddy controls for cost differences across establishments using starting wage, number of employees, the crime rate, the median value of owner-occupied housing, a state dummy variable, and a dummy variable for each restaurant chain. To account for the role of competition, she distinguishes between franchises and stores that are company-owned, and includes the proportion of the population without a car and the number of stores in the area. She also includes median family income and the poverty rate, and several other control variables. Her principal finding (p. 2) is that "meal prices rise about 5 percent for a 50 percent rise in the black population." This effect is statistically significant and about the same magnitude for all three items.

As Graddy recognizes, this result, like any regression result, could be subject to various biases. Omitted variables, such as unobserved cost factors, might be correlated with the proportion of the racial composition. Moreover, the key variable—namely percent black—could be endogenous since a restaurant selects its location. The coefficients of the cost variables also may not accurately reflect costs, thereby diverting attention from practices with a disparate impact on black neighborhoods. Nevertheless, this careful study pushes the literature on racial differences in access to consumption into some important new territory.

[9] Another interesting study by Graddy (1996) finds that Asian customers pay 7 percent less than white customers, on average, for one kind of fish at a large, wholesale fish market in New York City. She cannot rule out the possibility, however, that this difference reflects differences in search strategies between these two groups.

The Causes of Discrimination

Ever since the seminal work of Becker (1971) and Arrow (1972, 1973), which developed several hypotheses about the causes of discriminatory behavior, economists have been looking for ways to test these hypotheses. Audits provide a unique opportunity to conduct such tests because they illuminate the circumstances under which discrimination occurs. The causes of discrimination in housing have been investigated, using audit data, by Galster (1990c), Newburger (1989), Ondrich, Ross, and Yinger (1997), Ondrich, Stricker, and Yinger (forthcoming), Page (1995), Roychoudhury and Goodman (1992, 1996), and Yinger (1986, 1995). Hypotheses about the causes of discrimination in car sales are tested by Ayres (1995) and Ayres and Siegelman (1995). This literature has focused on several hypotheses.

The first hypotheses is that housing agents and car dealers discriminate because of their own personal prejudice against a particular group. One widely used method for testing this hypothesis is to examine the behavior of housing agents or car dealers who belong to the relevant protected class. The resulting evidence, while mixed, suggests that agent prejudice is not the primary cause of discrimination. Several studies find no evidence that black or Hispanic housing agents discriminate less than white agents (Ondrich, Ross, and Yinger, 1997; Ondrich, Stricker, and Yinger, forthcoming; Yinger, 1986). Moreover, both of the studies of car dealers find that discrimination against blacks does not depend on the racial composition of the car dealer's ownership or of its sales workforce (Ayres, 1995; Ayres and Siegelman, 1995). However, Roychoudhury and Goodman (1992), and Yinger (1995) find evidence that black or Hispanic agents tend to discriminate less than white agents. This effect can be large; for example, Yinger (1995, p. 178) finds that the probability a particular housing unit will be reserved for whites is 25 percent lower if the agent is black or Hispanic.

A second well-known hypothesis is that housing agents and car dealers discriminate to protect their actual and potential business with prejudiced white households. According to this hypothesis, car dealers should discriminate more when they serve a neighborhood with a higher concentration of whites. Ayres (1995) and Ayres and Siegelman (1995) find no support for this hypothesis. In the case of housing, agents have an interest in preserving their reputation with whites by excluding blacks from all-white neighborhoods and in discouraging "tipping," defined as rapid transition from largely white to largely black, by excluding blacks from relatively stable integrated areas. Once tipping has begun, white customers tend to look elsewhere, so this incentive to discriminate diminishes. Several studies do find evidence to support this hypothesis. In an analysis of black-white audit data from Boston in 1981, for example, Yinger (1986) finds that discrimination is particularly high in two all-white neighborhoods and in two integrated neighborhoods that are not experiencing a large influx of blacks and particularly low or nonexistent in one neighborhood undergoing rapid racial transition. Using data from the 1989 national Housing Discrimination Study, Ondrich, Stricker and Yinger (forthcoming), Page (1995) and Yinger (1995) also find relatively high discrimination in white

and/or integrated neighborhoods under some circumstances. In addition, Yinger (1986, 1995), Roychoudhury and Goodman (1992), and Page (1995) all find evidence that black or Hispanic couples face more discrimination when they have children. Because prejudiced white households may be particularly concerned about integration in schools, this evidence supports the view that a housing agent is more likely to discriminate against black and Hispanic families with characteristics likely to upset his potential white clients.

A third hypothesis is that agents maximize the return to their effort and therefore do not pursue transactions that are unlikely to be finalized. In an audit setting, an agent's anticipated probability of completing a transaction is based on the agent's perceptions and forecasts about the customer. If these perceptions or guesses, and hence the agent's behavior, differ between audit teammates, then the agent is using membership in a protected class as a forecasting device, a type of behavior called "statistical discrimination" (Arrow, 1972; Phelps, 1972).

One example of this behavior in the housing market involves agents' perceptions about the neighborhood preferences of their customers. If agents believe that whites prefer white neighborhoods and blacks or Hispanics prefer integrated ones, they will withhold units in white neighborhoods from blacks and Hispanics and units in integrated neighborhoods from whites. Evidence of high discrimination in integrated neighborhoods is not consistent with this hypothesis. Thus, this hypothesis is contradicted by Yinger's (1986) finding of high discrimination in two relatively stable integrated neighborhoods in Boston, but it is supported by the Roychoudhury and Goodman (1992) finding that discrimination against blacks is relatively low in both integrated and largely black neighborhoods in Detroit.

In the case of automobile sales, different forms of statistical discrimination may exist. Ayres (1995) uses the audit information on the sequence of dealer actions, from initial to final offer, in the context of a particular bargaining model to obtain (nonstatistical) estimates of car dealers' perceptions concerning each group of customers. Ayres finds that dealers believe black men have the highest reservation prices, followed, in order, by black women, white women, and white men. Because a higher perceived reservation price leads a seller to charge a higher price, Ayres concludes that dealers practice "revenue-based" statistical discrimination against black men and against women. In addition, Ayres estimates that dealers perceive black women to have higher bargaining costs than any other group. Because dealers offer higher prices to groups with higher search costs, this finding suggests that "cost-based" statistical discrimination may partially explain the higher offers to black women.

Exploring the causes of discrimination helps to explain how racial and ethnic prejudice and socioeconomic disparities that flow from our nation's history of discrimination contribute to the incentives that produce discrimination today. Moreover, the available evidence indicates that discrimination in housing and car sales has a variety of causes, that these causes are multifaceted, and that they need not be the same for every type of behavior or for every group. More research on this important topic clearly is needed.

Conclusion

Some economists believe that discrimination is primarily caused by firm prejudice and results in reduced profits, so that unprejudiced firms will drive prejudiced, and hence discriminating, firms out of business. The research reviewed here provides two strong types of evidence against this position. First, discriminatory barriers to consumption show no signs of diminishing over the last 20 years—a direct contradiction of the predicted disappearance of discrimination. Black and Hispanic households still face a significant chance of encountering discrimination when they inquire about housing or visit a car dealer. In some cases, such as withholding of information about available housing units or offering much higher prices for cars, the discriminatory behavior severely restricts the choices of these households.

Second, several studies find that, under some circumstances, housing agents and car dealers may discriminate to boost their profits. For example, housing agents sometimes discriminate to protect their business with prejudiced white clients, and car dealers sometimes discriminate to take advantage of perceived weaknesses in the bargaining position of blacks. Although the incentives that lead to discrimination are complex and may vary from one type of behavior to another, the available evidence indicates that the stereotypes, prejudices, and disparities that arise from past discrimination sometimes give economic agents an incentive to discriminate today. Competition will not eliminate this type of profit-based discrimination.

This ongoing discrimination violates principles that are central to our democracy and imposes high costs both on black and Hispanic households, whose choices it restricts, and on all Americans, who must deal with the hostility and lost opportunities that are its offspring. Continued anti-discrimination efforts are very much in our nation's best interest (Galster, 1990a; Yinger, 1995). Regression analysis has long been admissible in court (Ashenfelter and Oaxaca, 1987), and, in the 1982 *Havens* case, the U.S. Supreme Court unanimously ruled that fair housing audits are a legitimate enforcement tool (Havens v. Coleman, 445 U.S. 363, 1982). Thus, economists can contribute to enforcement efforts through further research on discrimination.

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