Racial Discrimination in ‘Everyday’ Commercial Transactions: What Do We Know, What Do We Need to Know, and How Can We Find Out?

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by

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Introduction

In 1992, ABC TV took two young testers, one black and one white, and covertly filmed what happened to them as they spent a week (pretending to be) going about the business of everyday life—looking for a job, negotiating to buy a car, going shopping, trying to find an apartment, and so on (Prime Time Live, 1992). The 20-minute segment vividly documented many instances of discriminatory treatment: the black tester was ignored in a shoe store, while the white tester received instant and friendly service; the black tester was followed as a suspected shoplifter in a record store, while his partner was accorded normal, courteous treatment; the black tester was quoted a much higher price than his partner for the identical car.

But while the documentary provided incontrovertible evidence of the existence of discriminatory treatment in many kinds of commercial transactions, it did not address the important question of how common such discrimination is. We still do not have a good answer to this question. Although there is now a large body of research on the frequency and amount of discrimination in what are arguably the two most important markets in which most of us participate—employment and housing—we know very little about discrimination in other kinds of transactions.\(^1\)

It is hard to claim that discrimination in restaurants or shopping is as important as discrimination in employment or housing. The latter two activities are central to a person’s life-chances in ways that the former two clearly are not. But it also seems clear that discrimination in everyday transactions imposes significant psychological costs on its victims and is a clear violation of our civil rights laws. There is thus ample justification both for wanting to know more about it and for doing something to prevent it.

This paper has two goals: to summarize what little we know about racial discrimination in
everyday commercial transactions (which I will loosely refer to as “public accommodations”) such as buying a car, hailing a taxicab, or eating a restaurant meal; and to make some practical suggestions for how we should go about testing for discrimination in these markets. I consider questions of technique, but also discuss some broader issues such as the justifications for testing and some of the policy and enforcement implications of conducting testing in these areas.

The second section discusses discrimination in the market for new cars, one of the few areas in which we actually have data from an audit study. As the study (of which I was a co-author) has been subject to criticism for being “unrealistic,” I devote some attention to defending its results. The third section briefly discusses another audit study that measured discrimination in the market for taxicab service in Washington, D.C.. The fourth section considers discrimination in public accommodations more broadly (restaurants, shopping, etc). There have been no systematic studies of this topic, so one can rely—cautiously—only on survey evidence, isolated journalistic narratives, and judicial opinions in public accommodations cases.

Perhaps the most important conclusion of this paper is that discrimination is likely to differ in form, motive, intensity, and effects across the various markets that comprise the category “public accommodations” or “everyday commercial transactions.” No single theory is likely to explain the wide variety of observed discrimination(s), and no single method is appropriate for studying such a heterogenous phenomenon.

Table 1 sketches a crude taxonomy that may be useful in analyzing the prevalence and severity of discrimination. It classifies markets along two dimensions: whether discrimination is “visible” or not, and whether discrimination takes the form of higher prices as opposed to denial or degradation of some service or opportunity.
Cell 1 of the table consists of those markets in which: (a) discrimination takes the form of higher prices charged to minorities, and (b), an individual buyer finds it difficult or impossible to ascertain whether the price paid was high or low relative to other buyers. Interbuyer price comparisons are ruled out because the good or service being sold is essentially heterogenous across buyers (e.g., cars of different models, or with different options), or because consumers bargain individually with sellers over the price or other important aspects of the sale. The best-studied example is the market for new cars, but cell 1.B suggests that there are other markets with similar structural characteristics, such as television or home repairs.

Because it is virtually impossible for an individual consumer to detect discrimination in such markets, and because higher markups presumably raise sellers’ profits, discrimination is likely to be relatively more frequent and severe in the markets described by cell 1. And indeed, there is substantial evidence—from an audit study, from a conventional statistical analysis of transactions data, and from pending litigation—of racial price discrimination in the sale of new cars: blacks are asked, on average, to pay anywhere from $400 to $1000 more than white males for identical vehicles.

Cell 2 describes markets in which there is open or overt discrimination on the basis of price. It is blank because—as far as I know—there are no examples of posted prices that are higher for blacks than for whites. Cell 3 contains markets in which discrimination takes the form of a covert denial of access to an opportunity or service. Preeminent examples are housing and employment, where victims who are not offered a job or not shown an apartment because of their race are usually
unaware of what has happened to them, or why. As discrimination in these markets has generated substantial literatures that are discussed by other papers at this conference, I will ignore them here.

Finally, cell 4 encompasses markets where discrimination is overt and takes the form of a denial or degradation of service. It includes restaurants that refuse to seat black customers, stores that harass black shoppers, taxicabs that refuse to pick up black customers, and so on.⁴ We know very little about the prevalence or severity of discrimination in these markets, but it seems plausible a priori that it is less widespread and less serious than in cell 1. Because the type of discrimination in cell 4 is easier to detect, customers are in a better position to use legal or market remedies when they encounter it. And discrimination that results in the denial of service to patrons willing and able to pay for it will often decrease sellers’ profits, rather than presumptively increasing profits as is the case in cell 1.

These predictions are borne out by the meager available evidence. In several kinds of everyday transactions—such as eating at restaurants and shopping—the probability of discrimination per unit of exposure (restaurant visit, shopping trip) appears from survey evidence to be roughly 1 to 5 percent, which is substantially lower than in cars, taxicab service, employment, or housing. Despite the relatively low probability of encountering discrimination on any given shopping trip or restaurant visit, the frequency with which any individual experiences discriminatory treatment is relatively high: 10 to 30 percent of blacks report one or more discriminatory experience in a given month. This apparent paradox results from the fact that individuals do a lot of shopping and restaurant dining, so even though discrimination is relatively unlikely on any given trip, it is almost certain to occur if enough trips are taken.

Bargaining for a New Car
I start by reviewing the evidence obtained from an audit study of race and gender discrimination in new car negotiations conducted by Ian Ayres and myself in 1990-91.\textsuperscript{5} The data reveal that dealers quoted significantly lower prices to white males than to black and/or female test buyers, even though the testers closely resembled each other in dress and general demeanor and followed an identical bargaining script.

A possible weakness of testing in this context is that it was impossible to actually purchase the cars for which the testers were negotiating. Hence, some critics have argued that our results do not accurately reflect the amount of discrimination found in bargaining that culminated in genuine purchases. However, subsequent evidence from a study of actual purchases, and from pending litigation, appears to broadly confirm the conclusions we reached.\textsuperscript{6}

Audit Results

The techniques used in auditing car dealerships were described in our earlier articles, and I will not discuss them in detail here. We relied on the standard procedures of selecting testers with similar observable attributes (age, education, “appearance”) and further homogenizing by giving them false biographies and training them to follow a uniform bargaining script. Varying pairs of testers,\textsuperscript{7} one of whom was always a white male, were then sent to negotiate for new cars at randomly selected Chicago-area dealerships. Testers were not told of the true purpose of the study, and did not know that more than one of them would visit each dealership.

Column 1 of table 2 summarizes the study’s key results. In brief, white male testers were able to negotiate an average final markup of roughly $560, while white females were quoted a final price that was roughly $130 higher than this (controlling for unobservable, dealership-specific effects). Although this disparity was not statistically significant, the black testers in our study negotiated final
offers that were much higher than their white male counterparts. Black female testers were asked to pay an additional $400, and black males an additional $1,060 over what white males were quoted for similar cars at the identical dealerships. (These figures represent markups of 1 to 9 percentage points over what white males were asked to pay.) The black male and black female results are economically meaningful and statistically significant, and all the results are robust—alternative specifications and different statistical tests do not alter the basic findings.

[Table 2 about here]

How Realistic Are the Audit Findings?

Evidence From “Nearly Completed” Transactions

Only about 20 percent of the tests ended with a seller attempting to accept a tester’s offer; the remainder concluded when the parties reached a bargaining impasse, and hence may not reflect what occurs in actual sales, which of course are all consummated bargains. For obvious reasons, looking at actual transactions is impossible with audit data in this context. But we can focus on those tests in which the seller tried to accept a tester’s offer, which are as close to completed transactions as it is possible to get with test buyers. Using only these attempted acceptances, we found essentially the same pattern of discriminatory markups as when all tests were used. Moreover, dealers were more likely to attempt to accept offers made by white males than by the other tester types: This means that our estimate of the discriminatory premiums are understated, as sellers might have been willing to make even lower offers to white males.

In sum, no internal evidence suggests that the lack of actual transactions data caused us to overstate the discriminatory premium.

Evidence From Survey Data on Transactions Prices
Additional evidence about the pattern of markups in actual transactions comes from a recent regression study by Goldberg (1996), who used data from the Consumer Expenditure Survey, a nationwide representative sample of households that is used to compute the Consumer Price Index. Although derived from actual transactions, the CES data used by Goldberg are not true “transactions prices,” but are based on consumers’ recollections of what they paid for their car. Thus, although the CES data do offer an important perspective on the operation of the market for new cars, they are subject to several important limitations, discussed at length below.

Even though Goldberg (1996, 624) characterized her results as “quite different from the ones reported by . . . Ayres and Siegelman,” columns 2 and 3 of table 2 demonstrate some striking similarities between the two sets of findings. Goldberg’s estimates of the discriminatory premiums paid by white females and “minority” females are virtually identical to ours. The only differences between columns 1 and 2 is that Goldberg found black males paying a much smaller premium than we did, and none of her results are statistically significant, whereas ours were, at least for the black testers. Because there are at least 6 dimensions on which our audit data allowed for more precise measurement and better controls than the survey Goldberg used, her failure to obtain statistically significant results is not surprising and should not be taken as evidence against the existence of discrimination in new car sales.

Goldberg’s estimated black male premium is at odds with our audit findings, as it is only about one-fourth the size of the discriminatory premium we found (although at $275, her estimate is still economically meaningful). Various technical explanations might reconcile Goldberg’s estimated black male premium with our substantially higher estimate, but none of them seems sufficient to
explain the $800 difference between Goldberg’s estimate and ours, which remains something of a puzzle.

One possible explanation is that even though black customers face higher prices at most dealerships (as demonstrated in the audit results), they do not in the end pay substantially higher prices, as Goldberg’s black male result suggests. This could happen if black male shoppers who face discrimination at some dealerships “solve” this problem by searching longer and harder for those less-discriminatory dealerships that will offer them a better deal. By doing so, they may succeed in offsetting some of the discriminatory premium detected in audit studies, but only by paying higher search costs than white consumers.\(^\text{13}\)

Looking only at prices paid by minorities in actual transactions thus ignores the other margins on which the effects of discrimination can be felt. Goldberg (1996, 643) mentions this point, only to dismiss it because the black testers in our study did not receive better offers at dealerships in black neighborhoods. But just because black testers didn’t receive better offers in black neighborhoods does not imply that black customers would not find it in their interest to search for nondiscriminatory dealers; quite the contrary.\(^\text{14}\)

Finally, regardless of which estimate of the black male premium turns out to be more accurate, it is important to remember that at $275, Goldberg’s estimate is still economically significant. While the two disparate estimates of the black male premium poses an open question for further research, both our audit study and Goldberg’s analysis of survey data show that discrimination in the market for new cars is a reality. In my view, Goldberg’s study should be interpreted as confirmative of the audit results.
Evidence From Litigation

As far as I know, there is only one pending case alleging racial discrimination in new car sales, but it apparently reveals both deliberate discriminatory policies by a dealership and substantial discriminatory premiums in actual transactions. According to testimony by former salespersons at an Atlanta dealership, management put pressure on them to charge blacks higher prices and to offer less-favorable terms on trade-ins and financing. Evidence from sales commission sheets apparently confirms that blacks did indeed pay higher prices than whites for comparable cars. Although evidence from a single dealership is subject to the obvious question of generalizability, it does strengthen the case that discrimination in the market for new cars is an ongoing phenomenon in actual transactions, not just in unconsummated audits.

What Next? The Implications of Car Audits

Discrimination in the Car Market

Additional work on discrimination in the market for new cars could usefully take several forms. It would be interesting to replicate the audit results in another large city. Even better would be to test for discrimination using more reliable measures of transaction prices than the data Goldberg used. One strategy would be to sample dealership records, preferably from a number of dealerships. The buyer’s race could be inferred from his or her residential zip code, or by direct interviews. It seems unlikely that dealerships would willingly turn such information over to researchers, however. An alternative would be to start from tax records, which are publicly available in certain states, and which describe the purchase price and the make and model of car in some detail. Again, the buyer’s race could be inferred from his or her zip code, or preferably through a supplementary interview with the purchaser in which additional information about bargaining and search could also be obtained.
The major obstacle to this kind of research is the problem of how to oversample black new car buyers. In Goldberg’s data, only 67 of 1300 respondents (5 percent) were “minority,” with blacks making up presumably only 60 percent of that group, or less.

Discrimination in Other Markets

There are at least three important differences between automobile dealerships and most of the other public accommodations contexts where audits might be deployed. First, negotiated car prices are both flexible and hidden. Discrimination in this context takes the form not of denial or degradation of service to black customers, but of charging them more than otherwise identical whites would be charged for the same product. Discrimination based on idiosyncratic bargaining presumably increases seller profits and is much easier to conceal than outright denial of service. Other things equal, this leads me to expect more discrimination in car sales than, say, in restaurant meals.

A second important difference between the car market and other kinds of public accommodations is that the problem of incomplete transactions is not really applicable when discrimination takes the form of denials of service; in a sense, it is precisely the incomplete transaction that audits are trying to capture.

Third, issues of tester matching, which are crucial in the context of employment and housing audits, and significant in auditing car dealerships, are likely to be substantially less important in auditing restaurants or shops. Restaurant testers presumably do not need simulated biographies or extensive coaching on presentation of self. Although the testers need to resemble each other in terms of dress and general demeanor, more than that does not seem necessary.16

Other Markets in Which Racial Price Discrimination May be Prevalent
As Graddy points out, “. . . price discrimination based on race in retail markets has been recently almost ignored” (Graddy1997, 391).\textsuperscript{17} Despite the lack of empirical evidence, Table 1 does suggest that there are other markets with the same structural characteristics as the market for new cars. Consider, for example, the markets for home repairs, television repairs, used car purchases, or new appliances. In each of these markets, products or services have an important idiosyncratic dimension. Although a repair shop may post its hourly rate for work on a television set, for example, most individual consumers are in no position to know what was actually wrong with their television, or how long it should take to fix it. Under such circumstances, sellers could easily charge different prices based on the race of the customer. Whether this actually occurs is an open question, which could be addressed relatively easily using paired audit techniques.\textsuperscript{18}

Taxicabs

Prompted by repeated complaints, the Washington Lawyers’ Committee sponsored an audit study of racial discrimination in taxi service in the District of Columbia. The study design involved pairs of testers, one white and one black, who were positioned “about three car lengths apart” at selected Washington locations.\textsuperscript{19} The testers were randomly assigned to be “first” in the pair. Each then attempted to hail a taxi. If a cab stopped, testers were instructed to request rides to selected locations in the city; even when the testers were successful in hailing a cab, they were sometimes refused service to their chosen destination.

What Is the Appropriate Measure of Racial Discrimination?

The results of the study are presented in table 3, and summarized in table 4 in a way that makes them more consistent with other studies.\textsuperscript{20} I think the most logical measure of the discrimination black patrons face purely on the basis of their race is simply to compare the probability of
successfully hailing a cab for black and white testers, as shown in table 4. The table demonstrates that whites are about 8 percentage points (11.2 percent) more likely to be able to get a taxi to stop for them than are blacks. Put another way, it took blacks an average of 5.7 minutes to get a taxi to stop for them, while it took whites 4.5 minutes; blacks had to wait, on average, 27 percent longer for a cab to stop.

[Tables 3 and 4 about here]

What are the Costs of Discrimination?

One reading of these data is that discrimination is not such a big deal: Black patrons simply had to wait an extra 72 seconds to hail a taxi. Valuing this time at $12 per hour, roughly the average wage, the “cost” is a modest $0.25. However, this calculation is inappropriate because discrimination almost surely has an important psychological dimension that is not adequately captured by a simple opportunity cost valuation such as this one.

There are alternative methods for assessing the true costs of discrimination, although they are difficult to implement and theoretically controversial. But the taxicab study does raise an issue that will be important in other kinds of public accommodations audits. Simply monetizing the time or inconvenience that result from discrimination runs the risk of incorrectly making it seem like no big deal. Most people would instinctively recognize that discrimination in jobs or housing carries significant costs, but an additional 1.2 minutes spent hailing a taxi may not seem that important to some observers. Audits can tell us a great deal about the frequency of discrimination in public accommodations, but we need to be prepared with surveys or other approaches to measure its costs appropriately.
Are Simultaneous Paired Audits Appropriate in this Context?

Start with the following story about what motivates cab drivers. Drivers believe that blacks are marginally less profitable passengers than whites, and other things equal, will therefore prefer to pick up a white customer. If the difference in profitability between black and white passengers is relatively small, however, drivers will usually be willing to pick up black passengers; bypassing a potential paying customer requires continued search for the next customer, and if customers are scarce, this means the cab remains empty while the driver continues to look for the next fare. At times when customers are plentiful (e.g., at rush hour), drivers may still find it in their interests to pass by blacks in search of more profitable whites, because they expect to find their next (white) customer relatively quickly. But—again assuming a small profitability difference—drivers will usually prefer an available black passenger to the uncertain prospect of trying to find a white alternative.

If this theory accurately describes drivers’ behavior, then the paired audit with simultaneous testers seems to me to be an inappropriate way to measure discrimination, for two reasons. First, the situation being audited is not typical of the setting that black customers typically encounter. When attempting to hail a cab, the testers positioned themselves 30 to 40 feet apart; both were almost certainly visible to a cab driver who was considering which, if either, of them to stop for. In this setting, drivers might always prefer the white customer, even if, in a more realistic context—one in which the alternative to passing a black patron is an indefinite search for the next customer—the black customer might have substantially less trouble hailing a cab.

A second problem with the experimental design is more technical: The outcomes for the two auditors are not statistically independent, which means that the estimated race effect may be biased.
Consider trying to investigate the effect of fertilizer on plant growth. A control group of seeds is planted on plot A and receives no fertilizer. A treatment group is planted on identical plot B, but it does get fertilized. The difference between the average heights of the plants in the two groups is then taken to be a measure of the effect of the fertilizer.

But this is a valid procedure only if the treatment of the plants in plot B has no effect on the height of the plants in plot A. Suppose that adding fertilizer to the treated plants caused them to grow taller and cast shade on the plants in the neighboring control plot, retarding their growth. The observed difference in the average heights of the two groups of plants would then overstate the true effect of the fertilizer, because it would reflect not only the effect of the fertilizer on the treated plants, but also the (negative) effects of shade on the control group. Much the same problem could occur in the taxicab tests if, as seems likely, the presence of the white tester reduces the probability that the black tester will be able to hail a cab.

Given the possible interaction effects caused by the proximity of the two testers, it might make sense to position the testers on opposite sides of the street (instead of using two testers who stand within a few yards of each other), so that they are not in direct competition for the same cabs. Randomizing which tester is assigned to which side of the street would then allow for comparison of each tester’s success rate.

In sum, the taxicab study suggests three lessons for future audits. First, it is useful to measure race discrimination as the difference (or ratio) in probabilities of service, by race. By this measure, white testers were about 11 percent (8 percentage points) more likely to hail a cab than their black counterparts. Second, audit studies by themselves cannot adequately measure the cost of this discrimination. Simply valuing the additional time that blacks spend waiting for a cab at its
appropriate opportunity cost is virtually certain to understate the true cost of discrimination. Public accommodations audits will have to face this problem, because unlike housing or employment, it may not be intuitively obvious to some people that such discrimination is more than an inconvenience in this context. Third, audits should be designed to insure that the treatment of one tester will not influence the outcome experienced by the other.

Other Kinds of Public Accommodations

I propose a broad definition of “Public Accommodations” that encompasses most of the commercial transactions of everyday life, including eating in a restaurant, renting or buying a car, hailing a taxi, or going shopping. It doesn’t much matter how one defines the phrase, however, because regardless of the definition, there is virtually no systematic evidence about the extent of discrimination in public accommodations outside of the few areas discussed earlier. One could almost stop here: The strongest case for conducting audits in this area is simply that we are almost completely ignorant of the very thing audits are designed to measure—how pervasive is discrimination in restaurants, shopping, hotels, car rentals, and so on?

We can gain some useful—albeit tentative—insights about public accommodations discrimination from two flawed sources: (1) survey data measuring self-reports of experiences with discrimination, and (2) individual reports (including journalistic accounts and formal legal opinions) of discriminatory behavior in various kinds of public accommodations. I consider these in turn.

Survey Data

The Limitations of Survey Data

Before I discuss the survey results themselves, it is important to recognize their limitations, even though these problems do not ultimately bear on the case for audits.
Surveys of discrimination are plagued by two obvious problems. First, respondents may not be aware of some instances of discrimination. This is particularly likely when it takes the form of higher prices—as in the case of the discrimination uncovered in the audits of auto dealerships, virtually none of which were recognized by the victims—as opposed to outright refusals of service. In a world where race discrimination is illegal in most contexts and is widely considered to be immoral, discriminators have both a legal and a social incentive to practice deceptive “Have a Nice Day Racism” rather than overt discrimination. When discrimination is difficult to detect, we cannot count on victims to give an accurate estimate of its extent, and surveys are thus potentially flawed as measures of the frequency of discrimination.

But the reverse problem also exists. Respondents may incorrectly classify some instances of simple bad service (a long wait for a table at a restaurant, for example) as race-based discrimination when in fact the bad service is not racially motivated.

It is impossible to know a priori how important either of these effects actually is. But whether victims over- or understate the amount of discrimination they face, there is a clear social benefit to a better, more objective understanding of the prevalence and nature of discrimination.

The Implications of Some Recent Survey Evidence

A 1997 Gallup study provides some of the most recent and authoritative survey evidence of blacks’ experiences with discrimination (Gallup 1997, 30-31).30 Asked about whether they had encountered discrimination (unfair treatment because of their race) in the last 30 days, 45 percent of blacks said that they had had at least one discriminatory experience. Thirty percent said they had experienced discrimination while “Shopping;” the figure for “Dining Out” (including bars, theaters and other entertainment) was 21 percent (Gallup 1997, 30).
The Rate of Discrimination in Public Accommodations

Even assuming that these results are perfectly accurate, it is not obvious how to turn them into the appropriate rates of discriminatory behavior. But a crude calculation is outlined in table 5.

Suppose there are 50 billion meals served in restaurants and school and work cafeterias each year, of which roughly half are in commercial establishments, and roughly 10 percent of these are served to blacks. If the black dining public consists of 25 million persons, then the average black customer eats roughly 8 restaurant meals a month. A group of 100 black respondents should thus eat roughly 800 restaurant meals per month. Twenty percent of respondents claim they experienced discrimination at least once during the past month. Conservatively supposing that each victim experienced only a single incident of discrimination, we have 20 acts of discrimination out of 800 total meals, for a discrimination rate per meal served of about one in forty, or 2.5 percent. This is substantially lower than (net) rates of discrimination found in the Urban Institute’s employment audits, which ranged from 5 to 15 percent.31

[Table 5 about here]

These calculations have two important implications for auditing discrimination in public accommodations. First, even if the rate of discrimination in some activity is relatively low, people who perform that activity frequently will nevertheless have a high probability of experiencing discrimination at some time.32 Consider the rate of discrimination in “Shopping.” It is hard to know what the interviewers or respondents meant by the word “Shopping,” but broadly defined, the average respondent probably went “Shopping” dozens of times in the month before he or she was surveyed. If so, even though 30 percent of respondents experienced discrimination on a shopping
trip, the rate of discriminatory incidents per trip would be extremely low—on the order of one percent—and hence very difficult to detect via random audits.\textsuperscript{33}

Public accommodations audits thus pose a different set of challenges to investigators than housing or employment audits. One problem concerns questions of sample size and statistical power. For example, suppose that, using audits of a random sample of stores, we measure the rate of discrimination in shopping at 1 percent per trip in 1998, but that in 2003 we observe a rate of 0.05 percent, a 50 percent drop. If it were “real,” such a large drop in the rate of discrimination would obviously be extremely important. But it is of course possible that the decrease could be caused simply by sampling variation: It could be that the 2003 sample just happened by chance to include a group of firms that were less likely to practice discrimination, even though the overall (population) rate of discrimination remained unchanged.

Our ability to distinguish between a real (population-level) change in the rate of discrimination and an artifact of sampling variation depends on the precision of the two estimates. This in turn depends on the size of the two samples. It is useful to ask: How big a sample size would we need to be able to reject the null hypothesis of no real change at the 5 percent significance level, given that the rate was 1 percent in 1998 and appeared to fall by 50 percent in 2003? The answer is about 4600 observations (2300 in each year).\textsuperscript{34} This is dramatically larger than the biggest testing study ever conducted, and probably not feasible. If we observed a decline of less than 50 percent (which seems more plausible), we would need an even larger sample size to distinguish sampling error from a real decrease. Statistical power considerations thus make it extremely difficult to assess changes in discrimination rates over time when discrimination rates are already relatively low, as they appear to be in some public accommodations activities such as shopping.
Heterogeneity

The other important fact that emerges from the Gallup survey is that the incidence of perceived discrimination apparently varies a great deal within the black population. As table 6 indicates, there appear to be significant effects of age and gender, with young black males reporting substantially more discrimination than any of the other age/gender groups. It is impossible to know whether these results reflect differences in perceptive acuity, different definitions of what constitutes discrimination, or actual differences in treatment; but it is interesting that at least the gender pattern observed here is consistent with the results of the Ayres/Siegelman car audits, in which black male testers were quoted dramatically higher prices than any of the other three groups. In any case, the data suggest that future audit studies need to pay attention to age and gender as well as race. This further complicates the audit process—requiring larger sample sizes, for example—but the heterogeneity evidenced in table 6 suggests that “one size fits all” audits may not give a true picture of the extent of discrimination.

[Table 6 about here]

Evidence From Litigation and Journalistic Accounts

A second source of information on discrimination in public accommodations is individual narratives, ranging from newspaper accounts to judicial opinions. But generalizing from such accounts—and especially from judicial opinions—to the social world from which they originate is extremely hazardous. Incidents that lead to litigation or generate substantial publicity are a tiny and nonrandom fraction of what actually goes on; tried cases are a small and unrepresentative proportion of filed cases; and tried cases are not randomly selected for opinion-writing or publication. Thus, although judicial opinions in public accommodations cases (and journalistic accounts of
discriminatory incidents) are an important source of what we think we know about public accommodations discrimination, their message is not always what it seems.

Table 7 provides what I believe to be a reasonably complete listing of all the judicial opinions in (federal and state) public accommodations cases since 1990, plus some of the additional incidents that have received substantial press coverage. I want to consider three aspects of this table: the small number of cases, the possible importance of “Race-Plus” discrimination, and the identity of defendants.

Why so Few Cases?

Perhaps the most striking fact about Table 7 is its length. Although there have been tens of thousands of federal employment discrimination cases filed since 1990, and several thousand opinions written, my search turned up a mere 23 opinions in public accommodations cases in both state and federal courts. The contrast is striking. It is even more striking if we combine it with the estimates in table 5, which suggest that blacks experience something on the order of (0.025 × 2.5 billion = ) 60 million discriminatory incidents in restaurants alone each year.

What is going on? I think the most likely possibility is that potential plaintiffs realize that a large fraction of the perceived incidents of discrimination they experience are in some sense not worth the costs of taking to court. This is not to suggest that such incidents are psychologically unimportant, but only that plaintiffs (or their lawyers) who perform the simplest cost/benefit calculation probably conclude that the expected monetary gains from litigation are unlikely to be greater than the costs.

If true, this scenario makes it pretty clear that we cannot rely on private citizens to enforce the civil rights laws prohibiting discrimination in public accommodations. The case for audits as an
enforcement tool requires more than this, however. We still need to know whether market forces can effectively provide a check on discriminatory behavior, and whether audits can be designed so as to detect the discrimination that does occur. Sorting all this out would be a major contribution that audits could make to our understanding of discrimination and to the design of an effective enforcement effort.

“Race-Plus” and the Problem of Auditing Special Circumstances

While table 7 contains many instances in which plaintiffs allege that they were simply refused service or mistreated because of their race, many of the incidents being complained about involved a normal transaction that somehow went awry. In one case, black customers got into a dispute with a cashier at McDonald’s about what they had ordered; the patrons were then forcibly ejected from the restaurant, even though they were apparently eating peacefully at the time they were thrown out. In another example, a dispute over whether the room rate included breakfast led to a confrontation between plaintiffs and the hotel staff. It is impossible to be precise, but these kinds of “race-plus” incidents, in which race combines with some other factor to generate disparate treatment, seem to account for one-fourth to one-third of the cases in Table 7.

Let me be clear that these “race-plus” cases do not imply the absence of discrimination. Rather, they suggest that discriminatory treatment is often conditional on something else in addition to race. For example, suppose that black hotel guests receive the same treatment as white guests as long as there are no complaints about the service (which is equally bad for both races). If they do complain, however, black customers are then subject to hostile or disrespectful treatment that complaining whites do not receive. This clearly constitutes discrimination, but it is in some crucial respects different from the stark denial of service to black customers.
The existence of “race-plus” discrimination poses a problem for the design of audits because tests that look only at the treatment of “exemplary” customers of both races will understate the true level of discrimination when it takes the form that “non exemplary” blacks are treated worse than non exemplary whites. Of course, it is virtually impossible to know what proportion of all perceived instances of discrimination is accounted for by this kind of “race-plus” discrimination; in the end, it may not prove to be a major part of the problem.\textsuperscript{45} A well-designed survey could illuminate the importance of this kind of “race-plus” effect, and would be an extremely useful precursor to designing public accommodations audits.\textsuperscript{46}

Who Is Being Sued?

Another surprising fact about Table 7 is the identity of defendants listed there. Sixteen of the twenty-three defendants are large, nationwide, publicly traded corporations such as Sears, Holiday Inn, and Burger King. Moreover, all of the recent, well-publicized incidents of public accommodations discrimination seem to have involved large, nationwide firms: Shoney’s, Denny’s, Avis Rent-a-Car, Dillard’s, and Eddie Bauer.

In spite of the pattern that seems to emerge from Table 7, I would expect that discrimination is more prevalent at smaller, single-location shops and restaurants. To be sure, large national chains are capable of discriminatory conduct, either as a matter of top-down policy\textsuperscript{47} or as an unauthorized exercise of low-level managerial discretion.\textsuperscript{48} But economic theory strongly suggests that nationwide chains should be substantially less likely to discriminate overtly than single-outlet shops or restaurants. The reason is simply that McDonald’s, Sears, and Holiday Inns risk losing black customers at all their outlets, nationwide, if they are perceived to be discriminating against blacks at any individual outlet.\textsuperscript{49} A locally owned diner that caters to highway travelers can afford to serve bad
food (or to discriminate against blacks), knowing that most of its customers are “one-shotters” who will never patronize the restaurant again regardless of the quality of food or service it provides. But a bad meal or a discriminatory experience at a restaurant that relies on repeat customers for a substantial share of its business is much more costly to the restaurant. Even though any individual customer may not patronize the same McDonald’s more than once, a bad experience at a McDonald’s in Dubuque could lead customers to shun their local McDonald’s in D.C., and this possibility provides McDonald’s with a strong incentive not to provide a substandard dining experience for any of its customers.\textsuperscript{50}

Neither the cases in Table 7 nor the widely-covered incidents at Denny’s, Shoney’s, etc. constitute random samples of real-world behavior. Discrimination at large, nationwide entities is newsworthy in a way that discrimination at the corner gas station is not, so press reports will therefore be much more likely to ignore the latter and concentrate on the former. Larger defendants are more attractive to potential plaintiffs for a variety of reasons.\textsuperscript{51} Even though the evidence seems to suggest the contrary, I believe that public accommodations audits are much more likely to uncover discrimination at single-outlet entities than at national chains, which typically have too much to lose by encouraging or permitting discriminatory practices.

Depending on one’s goals, this analysis suggests either a testable hypothesis or an enforcement strategy. A sensible first step in either case would be to audit both large, nationwide chains and small, single-outlet entities. Enforcement efforts could then be concentrated on the latter if, as I expect, the probability of encountering discrimination is found to be higher there.\textsuperscript{52} Conclusions
Discrimination has often been found in places where one might think a priori that it was impossible or unlikely. The important question about discrimination in public accommodations is not whether it occurs at all, but how often, in what circumstances, and what can be done about it.

Audits are a necessary, but not sufficient, technique for answering these questions. They are necessary because they provide virtually the only objective measure of discriminatory treatment in many contexts, especially where discrimination takes the form of refusal or degradation of service rather than higher prices. There are simply no alternative transaction-based measures of how often race is a factor in getting service at a restaurant or while shopping.

In one sense, public accommodations audits will be significantly easier to perform than housing or employment tests because the problems of matching testers (including the creation of false biographies) are much less challenging in this context than in earlier studies. On the other hand, public accommodations audits pose some technical challenges that have either not been considered before or have received insufficient attention.

First, if my analysis of the survey data is correct, the “low-incidence/high-frequency problem” could make discrimination in shopping or restaurants difficult to detect without either a priori targeting of suspected discriminators or substantially larger sample sizes than have been used in previous testing.

Second, if “race-plus” discrimination is significant, conventional audits based on “exemplary behavior” could miss an important aspect of discriminatory behavior in public accommodations. Designing audits that can capture the effects of nonexemplary behavior may prove to be impossible for reasons discussed earlier. In that case, it is important to stress that the results of testing should be
interpreted as *conditional* on exemplary behavior, and as understatements of the true amount of discrimination that minorities actually face.

A third technical issue in interpreting the results of public accommodations audits might be termed the General Equilibrium (or “Other Margins, Other Rooms”) Problem. As I noted in discussing the car studies, if people who might experience discrimination take avoidance measures (for example, by refusing to patronize suburban malls where they might be accused of shoplifting), then audits that randomly sample all stores could well produce higher estimates of the extent of discriminatory behavior than survey data based on actual experience would. This may not be evidence against the reliability of audits, but could instead simply indicate that some of the true costs of discrimination are experienced along some margin other than the one on which it nominally occurs—as higher search costs or diminished shopping opportunities, for example.\(^{53}\)

Finally, tests for racial discrimination in public accommodations also need to be sensitive to differences of gender and age, since the survey data show such dramatic differences in (perceived) discriminatory treatment by age and gender. Social class and region could be significant as well, although the survey data do not allow for breakdowns on these dimensions. The survey results could reflect differences in treatment, or in perceptions. It is precisely because we need to sort out which explanation is correct that audits should be designed to shed light on these issues.

Audit studies, especially if they are used for enforcement, should therefore be complemented by well-designed surveys that can help reveal those areas where discrimination is most likely and help uncover the true costs of discrimination, which may involve avoidance, higher search costs, and other alternatives that are not revealed by audits.\(^{54}\)
Surveys should be designed to measure rates of discrimination accurately. Rather than asking respondents if they have experienced discrimination during the last month, they should ask in detail about how many times the respondent could have been exposed to discrimination (e.g., how many restaurant visits) and on how many of those times, if any, the respondent actually experienced discrimination. Surveys should try to measure the nature of discrimination: Was it outright refusal of service? Was it delay in getting a table? They should also ask in detail about the respondent’s subsequent behavior: Did he or she leave, complain, file suit? Why? Finally, it is important to know about avoidance measures that respondents may have taken: Are there stores, restaurants, or malls that respondents will not patronize in an effort to avoid discriminatory treatment?

Survey research techniques might usefully be complemented by experimental evidence. Social psychologists have developed many clever experimental techniques for uncovering the importance of race in explaining whites’ “helping behavior,” aggression, and nonverbal communications. Many of these studies could usefully be replicated over time, supplementing survey research questions as an indicator of the evolution of white attitudes towards blacks. Although behavior in experimental settings is not direct evidence of discrimination in the real world, carefully designed experimental studies can play an important role in assessing the background level of prejudice that motivates certain kinds of discrimination.

A final word about enforcement. In an era when some elected officials have suggested abolishing the Internal Revenue Service because its audits are too intrusive, the idea of covert discrimination audits at local restaurants, movie theaters, hotels, and department stores is unlikely to be greeted with much enthusiasm. I think the political realities thus argue strongly for a two-stage process. Stage one would involve purely descriptive/analytical social science research; enforcement
audits would be used only if/when this research uncovers a serious problem. There is a practical reason for a two-stage approach, as well: if I am correct that shopping and restaurant discrimination is relatively uncommon, enforcement audits might need to be targeted towards those firms, industries, or regions where discrimination is most likely.
Table 1. Classification of Markets by Type and Visibility of Discrimination

<table>
<thead>
<tr>
<th>Discrimination Takes the Form of Higher Prices</th>
<th>Discrimination is Hidden</th>
<th>Discrimination is at Least Partially Overt</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>Discrimination Takes the Form of Denial or Degradation of Service or Opportunity</td>
<td>3</td>
<td>4</td>
</tr>
</tbody>
</table>

1. A. New Car Sales; B. TV Repair?; Appliance Sales?; Home Repair?; Auto Repair?

2. None?

3. Housing; Employment

4. Restaurants; Car Rentals; “Shopping;” Taxicabs?
Table 2. Estimated Price Premium over White Males in Two Studies of Markups on New Cars, by Demographic Group

<table>
<thead>
<tr>
<th>Demographic Group</th>
<th>Audit Data (Ayres/Siegelman)</th>
<th>Survey Data (Goldberg)</th>
<th>Standardized Difference$^a$</th>
</tr>
</thead>
<tbody>
<tr>
<td>White Females</td>
<td>129 (123) [53]</td>
<td>129 (117) [259?]</td>
<td>0.0</td>
</tr>
<tr>
<td>“Minority” Females</td>
<td>405$^b$ (116) [60]</td>
<td>426 (525) [14?]</td>
<td>0.04</td>
</tr>
<tr>
<td>“Minority” Males</td>
<td>$1060^b$ (142) [40]</td>
<td>$274 (263) [52?]</td>
<td>2.6$^b$</td>
</tr>
</tbody>
</table>

Notes and Sources:
Column 1, Ayres and Siegelman (1995, table 2, col. 4).
Column 2, Goldberg (1996, table 2, col. 1). Goldberg does not give separate Ns for race/gender subgroups; these were inferred from the numbers of men, women, minorities, and whites given in table 4. Goldberg’s “Minority” categories include all nonwhites, while Ayres/Siegelman used blacks only. Ayres/Siegelman controlled for dealership-specific errors with a fixed effects specification, and for other sources of heterogeneity through the audit design. Goldberg controlled for a large variety of model and buyer characteristics.

Standard errors in parentheses. Number of observations in brackets.
$^a$ Difference in markups divided by its standard error, $\sqrt{\sigma_a^2 + \sigma_g^2}$, where $\sigma_i$ is the standard error from study $i$.
$^b$ = significantly different from zero at the 5% level.
Table 3. Outcomes of Taxicab Tests, by Race and Tester-Order

<table>
<thead>
<tr>
<th>Tester 1, Tester 2</th>
<th>Accept, Pass (a)</th>
<th>Accept, Attempt (b)</th>
<th>Pass, Pass (c)</th>
<th>Pass, Accept (d)</th>
<th>Pass, Refuse (e)</th>
<th>Refuse, Pass (f)</th>
<th>Refuse, Accept (g)</th>
<th>Refuse, Refuse (h)</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>T1 Black, T2 White</td>
<td>45</td>
<td>44</td>
<td>3</td>
<td>14</td>
<td>12</td>
<td>5</td>
<td>12</td>
<td>17</td>
<td>151</td>
</tr>
<tr>
<td>T1 White, T2 Black</td>
<td>34</td>
<td>54</td>
<td>1</td>
<td>1</td>
<td>0</td>
<td>16</td>
<td>13</td>
<td>23</td>
<td>141</td>
</tr>
</tbody>
</table>

Source: Calculated from Ridley, et al (1989), table 1. Rows may not sum to totals because of rounding from percentages.
Table 4: The Effect of Race on the Probability of Getting a Taxicab to Stop

|   | PR(Stop|Black) | PR(Stop|White) | Difference (2-1) | Percent Gain from Being White (3/1) |
|---|----------|------------|-----------------|------------------|
| 1. | 0.728    | 0.809      | 0.081           | 0.112            |

Source: Calculated from table 2, above.

Notes:

a Treats refusals to transport passenger to his or her chosen destination and attempts to pick up both testers as “acceptances.”

b PR(Stop|Black) is the probability that a cab will stop for a black tester, regardless of position and regardless of whether the cab subsequently refuses to take the fare. It is calculated from Table 2 as [(Row 1: cols a, b, f, g, and h) + (Row 2: cols b, d, e, g, and h)]/(151+141).

c PR(Stop|White) is the probability that a cab will stop for a white tester, regardless of position and regardless of whether the cab subsequently refuses to take the fare.
Table 5. A Rough Estimate of the Prevalence of Race Discrimination in Restaurant Dining

<table>
<thead>
<tr>
<th></th>
<th>Description</th>
<th>Estimate</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.</td>
<td>Total Number of Meals Served in Restaurants and School and Work Cafeterias, per year&lt;sup&gt;a&lt;/sup&gt;</td>
<td>50 Billion</td>
</tr>
<tr>
<td>2.</td>
<td>Of which, Commercial Meals&lt;sup&gt;b&lt;/sup&gt;</td>
<td>25 Billion</td>
</tr>
<tr>
<td>3.</td>
<td>Of which, Served to Blacks&lt;sup&gt;b&lt;/sup&gt;</td>
<td>2.5 Billion</td>
</tr>
<tr>
<td>4.</td>
<td>Number of Black Restaurant Customers&lt;sup&gt;b&lt;/sup&gt;</td>
<td>25 Million</td>
</tr>
<tr>
<td>5.</td>
<td>Restaurant Meals per Black Customer, per Month&lt;sup&gt;c&lt;/sup&gt;</td>
<td>8.3</td>
</tr>
<tr>
<td>6.</td>
<td>Percent of Blacks Who Say They Have Experienced Discrimination While Dining Out During the Past Month&lt;sup&gt;c&lt;/sup&gt;</td>
<td>20 percent</td>
</tr>
<tr>
<td>7.</td>
<td>Percent of all Meals Served to Blacks that Result in Perceived Discrimination&lt;sup&gt;d&lt;/sup&gt;</td>
<td>2.5 percent</td>
</tr>
</tbody>
</table>

Sources and Notes:


<sup>b</sup> Rough Estimate.

<sup>c</sup> Source: Gallup (1997, 30).

<sup>d</sup> Assuming that the 20 percent who reported discrimination had one discriminatory and 7.3 non-discriminatory meals, while the remaining 80 percent of respondents had 8.3 non-discriminatory meals, the share of non-discriminatory meals among all meals is \((0.8 \times 8.3 + 0.2 \times 7.3)/8.3 = 0.975\). See text for caveats.
Table 6. Percent of Black Respondents Who Report Having Experienced Discrimination Within the Last 30 Days, by Activity, Age, and Gender

<table>
<thead>
<tr>
<th>Activity</th>
<th>Men Ages 18-24</th>
<th>Women Ages 18-24</th>
<th>Men Over 35</th>
<th>Women Over 35</th>
</tr>
</thead>
<tbody>
<tr>
<td>Shopping</td>
<td>45</td>
<td>28</td>
<td>25</td>
<td>26</td>
</tr>
<tr>
<td>Dining Out</td>
<td>32</td>
<td>24</td>
<td>19</td>
<td>15</td>
</tr>
</tbody>
</table>

Source: Gallup (1997, 31) based on a survey conducted in Jan./Feb. 1997, with a total of 1269 black respondents. (N for this table not available).
Table 7. State and Federal Court Decisions and Other Incidents Involving Race Discrimination in Public Accommodations Between January 1990 and Jan 1998^a

<table>
<thead>
<tr>
<th>Name</th>
<th>Description</th>
<th>Outcome</th>
</tr>
</thead>
<tbody>
<tr>
<td>Federal Cases</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Morris v Office Max</strong>, 89 F.3d 411 (7th Cir. 1996)</td>
<td>Black male customers incorrectly suspected of shoplifting and questioned by security guards</td>
<td>Plaintiffs lost on summary judgment &amp; on appeal</td>
</tr>
<tr>
<td><strong>Alexis v McDonald’s Restaurants of Mass., Inc.</strong>, 67 F.3d 341 (1st Cir. 1995)</td>
<td>Dispute between black customer and Hispanic clerk leading to ejection of patrons</td>
<td>Plaintiffs lost on summary judgment &amp; on appeal</td>
</tr>
<tr>
<td><strong>Efstathiou v Romeo Carryouts &amp; Liquors, Inc.</strong>, 1997 U.S. Dist Lexis 14810 (N.D.Ill.)</td>
<td>White males accompanied by black females denied service at diner</td>
<td>Unclear, but Plaintiffs survived motion for summary judgment</td>
</tr>
<tr>
<td><strong>Haywood v Sears, Roebuck &amp; Co.</strong>, 1996 U.S. Dist. Lexis 11954 (E.D.N.C.)</td>
<td>Black customers incorrectly suspected of shoplifting, interrogated/harassed by security guards</td>
<td>Unclear, but Plaintiffs survived motion for summary judgment on some claims, though <em>not</em> on those relating to discrimination in public accommodations</td>
</tr>
<tr>
<td><strong>Perkins v Marriott</strong>, 945 F.Supp. 282 (D.D.C. 1996)</td>
<td>Dispute over whether room rate included breakfast led to confrontation between black couple and hotel staff</td>
<td>Plaintiffs lost on summary judgment</td>
</tr>
<tr>
<td><strong>Lewis v J.C. Penny Co.</strong>, 948 F.Supp 367 (D.Del. 1996)</td>
<td>Black customer accused of shoplifting, allegedly treated differently than white friend shopping with her</td>
<td>Plaintiff lost on summary judgment</td>
</tr>
<tr>
<td><strong>Perry v Burger King Corp.</strong>, 924 F.Supp. 548 (S.D.N.Y. 1996)</td>
<td>Black customer allegedly denied use of bathroom because of his race</td>
<td>Unclear</td>
</tr>
<tr>
<td><strong>Jackson v Motel 6, Inc.</strong>, 931 F.Supp. 825 (M.D.Fla. 1996)</td>
<td>Black police officers told that motel was full; white officers subsequently obtained a room</td>
<td>Unclear, but Plaintiffs survived motion for summary judgment</td>
</tr>
</tbody>
</table>

^aThe Federal cases were found in Westlaw’s ALLFEDS database using the key numbers covering violations of civil rights relating to public accommodations in general; in inns, restaurants, bars and taverns; in theaters; in public conveyances; and in places of business or public resorts. I excluded discrimination in private clubs. The exact search was:

(78K119 78K120 78K121 78K122 78K123) and DA(AFT 1/1/1990) and RACE.

This was supplemented with a Lexis search in the Courts library using

42 USC 2000a and date aft(1/1/90).

The State cases were located using the identical searches in the ALLSTATES database. The searches produced 59 Federal and 37 state cases, of which only those included here were relevant.
<table>
<thead>
<tr>
<th>Name</th>
<th>Description</th>
<th>Outcome</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>White v Denny’s Inc., 918 F.Supp. 1418 (D.Colo. 1996)</strong></td>
<td>Restaurant allegedly seated white customers before black Plaintiffs, then sided against Plaintiffs in dispute w/white fellow patrons</td>
<td>Plaintiff lost on summary judgment on all federal claims, unclear on state law claims</td>
</tr>
<tr>
<td><strong>Jackson v Tyler’s Dad’s Place, Inc., 850 F.Supp. 53 (D.D.C. 1994)</strong></td>
<td>Black customers allegedly denied seating in restaurant because of their race</td>
<td>Plaintiffs lost on summary judgment</td>
</tr>
<tr>
<td><strong>Robertson v Burger King, 848 F.Supp. 78 (E.D. La. 1994)</strong></td>
<td>Black patron alleges whites who arrived after him were served first</td>
<td>Plaintiff lost for failure to state a claim.</td>
</tr>
<tr>
<td><strong>Harvey v NYRAC, Inc., 813 F.Supp. 206 (E.D.N.Y. 1993)</strong></td>
<td>Black Plaintiff alleges she was denied a car rental because of her race</td>
<td>Unclear, but Plaintiff survived motion for summary judgment</td>
</tr>
<tr>
<td><strong>Estiverne v Saks Fifth Ave., 1992 U.S.Dist. Lexis 18089 (E.D. La.)</strong></td>
<td>Black customer whose check was not approved alleges race discrimination</td>
<td>Plaintiff lost on summary judgment. and was subject to Rule 11 sanctions.</td>
</tr>
<tr>
<td><strong>Bermudez Zenon v Restaurant Compostela, Inc., 790 F.Supp. 41 (D. Puerto Rico 1992)</strong></td>
<td>Group allegedly denied seating at restaurant because some were black</td>
<td>Unclear, but Plaintiffs survived motion for summary judgment</td>
</tr>
<tr>
<td><strong>Stearns v Baur’s Opera House, Inc., 788 F.Supp. 375 (C.D. Ill. 1992)</strong></td>
<td>Black patron alleges club deliberately selected music blacks wouldn’t enjoy in order to keep them out</td>
<td>Plaintiff lost on summary judgment</td>
</tr>
<tr>
<td><strong>Franceschi v Hyatt Corp., 782 F.Supp. 712 (D.Puerto Rico 1992)</strong></td>
<td>Hotel refused to allow son of black patrons to visit them on the premises</td>
<td>Plaintiff survived summary judgment</td>
</tr>
<tr>
<td><strong>Bray v RHT, Inc., 748 F.Supp. 3 (D.D.C. 1990)</strong></td>
<td>Black patron alleges he was asked to leave restaurant because of his race</td>
<td>Plaintiff lost on summary judgment</td>
</tr>
<tr>
<td><strong>Roberts v Walmart Stores, Inc., 736 F.Supp. 1527 (E.D.Mo. 1990)</strong></td>
<td>Black patrons object to their race being recorded on check they used to purchase items from store</td>
<td>Unclear, but Plaintiff survived motion for summary judgment on some claims.</td>
</tr>
<tr>
<td><strong>Jones v City of Boston, 738 F.Supp. 604 (D.Mass. 1990)</strong></td>
<td>Black patron alleges he was subject of abusive remarks by bartender at hotel</td>
<td>Unclear, but Plaintiff lost on summary judgment on most claims.</td>
</tr>
</tbody>
</table>

**State Cases**

<table>
<thead>
<tr>
<th>Name</th>
<th>Description</th>
<th>Outcome</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Crook v American Inn., Inc., 680 So.2d 361 (Ala.Civ.App. 1996)</strong></td>
<td>Black couple and 6 children denied room at hotel, despite reservation. Hotel claims “2-to-a-Room” policy</td>
<td>Plaintiff lost at trial, and on appeal</td>
</tr>
<tr>
<td><strong>Jackson v Superior Court, 30 Cal.App.4th 936 (Cal.App. 1 Dist. 1994)</strong></td>
<td>Black investment advisor accused by bank employee of attempting to defraud his clients</td>
<td>Unclear, but Plaintiff’s loss on summary judgment overturned on appeal</td>
</tr>
<tr>
<td><strong>Clarke v K Mart Corp., 495 N.W.2d 820 (Mich.App. 1992)</strong></td>
<td>Black shopper detained/harassed by store in dispute over behavior of clerk</td>
<td>Unclear, but plaintiff’s loss on summary judgment overturned, in part, on appeal</td>
</tr>
<tr>
<td>Incident</td>
<td>Description</td>
<td>Outcomes</td>
</tr>
<tr>
<td>----------</td>
<td>-------------</td>
<td>----------</td>
</tr>
<tr>
<td>Denny’s Restaurants (1993-1998)</td>
<td>Several incidents at restaurants across the country involving disparate treatment of black customers (refusals of service, prepayment of bill, etc.)</td>
<td>Cases settled after suits filed. Settlement involved structural changes to corporation, expanded minority recruiting, monitoring, and $46 MM in payments to injured claimants.</td>
</tr>
<tr>
<td>Shoney’s Restaurants (?-1992)</td>
<td>Widespread corporate policy of discrimination against customers and employees.</td>
<td>Shoney’s agreed to $100 million settlement of some 10,000 employment discrimination claims;</td>
</tr>
<tr>
<td>Dillard’s Department Stores (1997)</td>
<td>Black shopper wrongly accused of shoplifting, allegedly because of her race. Security guards apparently had explicitly race-conscious policy.</td>
<td>Jury awarded plaintiff more than $1 million.</td>
</tr>
<tr>
<td>Avis Car Rental (?-1997)</td>
<td>Franchisee explicitly trained staff to avoid renting to black customers; no evidence of pattern and practice of discrimination elsewhere, although corporate HQ may have ignored complaints about this franchise.</td>
<td>Avis and franchisee agree to $3.3 million settlement of class action lawsuit; individual claims continue.</td>
</tr>
<tr>
<td>Eddie Bauer Clothing Store (1997)</td>
<td>Store security personnel wrongly detained black youth suspected of shoplifting; there was apparently no race-conscious policy at issue.</td>
<td>Jury awarded plaintiff more than $1 Million,</td>
</tr>
</tbody>
</table>

Michael Fix and Raymond J. Struyk, eds., *Clear and Convincing Evidence: Measurement of Discrimination in America*.


Christopher Flinn and James J. Heckman, *Are Unemployment and Out of the Labor Force Behaviorally Distinct States?* 1 *J. Lab. Econ.* 65 (1983);


Peter A. Diamond and Jerry A. Hausman, Contingent Valuation: Is Some Number Better Than No Number, 8 J. Econ. Persp. 45 (1994).


William J. Wilson, The Declining Significance of Race (1980)


Abigail and Stephan Thernstrom, America in Black and White (1997)

Orlando Patterson, The Ordeal of Integration.


Orlando Patterson, *The Ordeal of Integration: Progress and Resentment in America’s “Racial” Crisis* (1997)


1. The audit studies in employment and housing are summarized and critiqued in Fix and Struyk (1993). For a summary of research in employment discrimination using conventional regression methods, see Cain (1986).

2. John Yinger (1998) surveys some of the same landscape, and reaches roughly similar overall conclusions.

3. Kathryn Graddy (1997) concludes that posted prices at fast food chains are higher in minority neighborhoods, after controlling for a large number of supply-side variables such as crime rates and labor costs. But Graddy’s results do not contradict the emptiness of cell 2: Fast food chains appear to be price discriminating on the basis of neighborhood characteristics, but do not charge different prices because of the race of an individual customer.

4. It will often be obvious to the potential patron that he or she has been bypassed by a vacant taxi, and in this sense, such discrimination is overt. On the other hand, it is not simply the ability to detect discrimination, but the ability to punish it, that discourages discriminatory behavior. And even though taxicab discrimination is observable, it is virtually impossible for patrons to take their business elsewhere or invoke legal sanctions against an individual cab driver. Hence, taxicabs could be classified in cell 3 instead of cell 4.

5. The original idea was Ayres’, and his analysis of a smaller-scale study is contained in Ayres (1991). The results of the larger study are analyzed in Ayres and Siegelman (1995).

6. See, Goldberg (1996). Note that Goldberg’s own interpretation of her results is that they are at odds with ours; I argue below that this is incorrect.

The issue is important for audits in other areas as well: our confidence in audits is strengthened if the results are confirmed using other techniques. As discussed below, however, even if audits seem to reveal more discrimination than occurs in actual transactions, this does not imply that the audits are inaccurate. It could simply mean that the discrimination uncovered in an audit study operates on some margin other than the price paid by black consumers—for instance, blacks could end up paying the same price as whites, but only by being forced to search more diligently or bargain harder.

7. Selling a car is a discrete transaction that requires less knowledge about the purchaser than hiring an employee does about the applicant. Hence, issues of tester matching are less significant in this context than in tests for discrimination in hiring. We were therefore able to “mix” testers, so that A would sometimes be matched with B, sometimes with C, and so on. The importance of matching is discussed further below.

8. See, for example, Epstein (1994, 34) (“A technique of testing that leaves so many incomplete transactions cannot be an accurate replica of a functioning market.”) and Goldberg (1996, 623) (“The reported markups [in the Ayres and Siegelman study] may . . . be different from the ones realized in actual purchases of new cars.”).
9. One possible criticism of the audit findings is that we might expect completed bargains to equalize prices paid: The higher the offer at the time negotiations ended, the larger should be the subsequent concession that the dealer would be willing to make. While seemingly plausible, there is no evidence of such behavior in our data. To the contrary, black males testers started the bargaining process by receiving the highest initial offers, and dealers conceded *less* to them than to any other tester group.

10. Column 3 of the table is the standardized difference between Goldberg’s estimates and those of Ayres/Siegelman. The numbers in column 3 are test statistics for the null hypothesis of no difference between the two estimates of the minority premiums: Under the null, the difference between the two estimates has a standard normal distribution. For white females and “minority” females, one can not reject the null hypothesis that the two estimates are identical at any conventional significance level. The black male estimates are significantly different, however.

11. How should one interpret the fact that Goldberg’s estimates of the discriminatory premiums were not statistically significant while ours were? Goldberg suggests that these differences in statistical significance make her results different from ours. She is unable to reject the hypothesis that her estimates were generated purely by sampling variation from a population where the true average discriminatory premium was zero. She seems to suggest, therefore, that the true discrimination premium actually *is* zero, not the much larger parameter values we both estimated.

A more convincing interpretation is simply that the differences in the statistical significance of our results arise because we are both measuring the same underlying parameter(s), but that she is doing so with data that are substantially noisier than ours. Among the factors that make it harder for Goldberg precisely to measure the discriminatory premiums are (1) she did not distinguish between various minority groups, while we employed only black testers; (2) her data are for the race and gender of the household head, not the actual purchaser of the car; (3) her data on the options purchased with each car, and even the model purchased, are less detailed than ours; (4) roughly half of her transactions involved trade-ins, whose value can be assessed only on average (using the wholesale blue-book value for a given make, model and year); (5) Goldberg’s data do not include the household’s state of residence, and do include sales taxes, which could only be factored-out probabilistically; and (6) the survey data she used were collected as long as three months after the actual transaction took place and are subject to errors of recall or memory that add noise to the price and other variables of interest.

Further evidence of the noisiness of Goldberg’s data is that her *R*²’s were about 0.18, while ours ranged from 0.22 to 0.44.

12. One possible explanation for the discrepancy is that the race variable in Goldberg’s study is measured with error, which would impart a downward bias to her estimated race effects. More plausibly, if there are variables omitted from Goldberg’s survey data that are positively correlated with race and negatively correlated with price, their omission would result in biased (understated) estimates of the race premium.

13. The importance of these kind of general equilibrium concerns has long been recognized in the labor market context, and is discussed at greater length below. See, for example, Flinn and Heckman (1983) or Duleep and Zalokar (1991). Yinger (1997) formalizes the intuition that discrimination by sellers reduces the benefits of additional search by buyers, causing them to
accept higher prices or lower quality than they otherwise would. Yinger applies this methodology to the housing market, and finds that the costs of discrimination are roughly $4000 per minority household per search.

14. Goldberg (1996, 643-644) also tests for whether minorities respond to discrimination by switching to the second-hand market or deciding not to buy a car at all, and concludes that there is no evidence for either of these effects in her data. However, there is independent evidence that, controlling for income, education, region, and age, blacks are less likely to own a car, and more likely to own an older or used car, than are whites. See Mannering and Winston (1991, Table A-1).


16. On the importance of matching in the context of employment audits, as well as other methodological issues, see Heckman and Siegelman (1993).

17. Graddy (1997, 391). In other work, Graddy has found race to have a significant effect on prices at the wholesale level in the highly competitive Fulton Fish Market in New York, where buyers of Asian ethnicity paid roughly 5 percent less for identical fish. See Graddy (1995). Wholesale fish are relatively standardized and homogenous in quality. But apparently, buyers are nevertheless unable to compare prices charged by the same seller to different customers. If race-based price discrimination is possible in a highly centralized market with sophisticated repeat buyers, it should be all the more likely in markets that lack such characteristics (such as television repair).

18. Many years ago, the Federal Trade Commission conducted a study in two states that found a high level of consumer fraud in the television repair industry. Phelan (1974). Although not the same as race-based price discrimination, this fraud does suggest that such discrimination is possible in settings where consumers are unable to compare the price they pay with that paid by others.

19. More detailed results and descriptions of the methods, as well as raw materials such as testing procedures and other protocols, can be found in Ridley (1989).

20. In analyzing race discrimination, the taxicab study decomposed the possible audit outcomes into several categories. A “passby” occurred when the taxi refused to stop for a tester; an “acceptance” meant that the taxi stopped and agreed to take the tester to his or her chosen destination; a “refusal” occurred when the taxi stopped for the tester but would not agree to go to his or her destination; and finally, an “attempt” occurred when a taxi picked up the first tester, and then stopped for the second tester as well. (D.C. cabs are allowed to pick up multiple passengers, but testers were told not to accept rides from a cab that had already stopped for their partner.)

The authors chose to compare the rate of “passbys” among “those passby outcomes where race might be a factor” with the rate of “acceptances.” But a case can be made for including both “refusals” and “attempts” along with “acceptances” for these purposes. If the taxi was willing to
stop for the tester, but chose to refuse him or her only when told of the tester’s destination, this might better be thought of as discrimination on the basis of destination rather than race.

Given patterns of residential segregation, discrimination on the basis of the customer’s race and on the basis of his or her destination are likely to be highly correlated. But there is a difference: One can think of a taxi that refuses to stop for a black tester as committing a kind of disparate treatment discrimination; a taxi that refuses to go to a (mostly black) neighborhood is committing a kind of disparate impact discrimination. Although this distinction is in one sense academic, I think it is one that is worth preserving because it could have important policy implications.

21. The probability of hailing a cab includes all of the tests in which a black tester was able to hail the cab, including those in which his or her white partner had already hailed it and those in which the cab refused to take the tester where he or she wanted to go. By focusing only on columns d, e, and f, the taxicab study authors ignore the fact that nearly 40 percent of the time, a taxi that picked up the white tester first attempted to pick up the black tester as well.

22. The difference is statistically significant at the 0.02 level using a $\chi^2$ test with 1 d.f.

23. Note that there are two kinds of costs of discrimination that need to be measured. The first are the opportunity costs imposed on victims who have to bear increased search costs or who substitute away from one activity to what would otherwise be a less-preferred alternative in order to avoid discrimination—for example, taking a bus instead of a taxicab. These kinds of costs are discussed by the authors cited in note 13.

But there are almost certainly additional costs that are purely psychological, and are therefore more difficult to measure. One possible measurement strategy would be to use a “willingness to accept” approach. Suppose everyone were guaranteed non-discriminatory taxi service. Now imagine (as only an economist could) asking black customers what’s the least amount of money it would take to get them to give up their right to non-discriminatory service, if this meant they would have to wait an additional 72 seconds, on average, to hail a cab? The answer to this question is one measure of the true cost of discrimination. My intuition is that it would be dramatically larger than the 25-cent figure derived above.

So-called “contingent valuation” methods, based on surveys of this sort, have been widely used in attempting to value non-market goods such as environmental quality. They are controversial within the economics profession, and I don’t want to suggest that they are the definitive answer to the problems of assigning a cost to discrimination. For a positive assessment of the methodology, see Hanemann (1994); a critical view is offered by Diamond and Hausman (1994).

24. There could be many reasons for such a belief. Drivers might think that they will have a harder time finding a return fare from predominantly black neighborhoods that are the likely destinations of black passengers; or that black passengers are more likely to rob them; or that blacks leave lower tips than whites. Some evidence for this last possibility comes from an unpublished study by Ian Ayres and Suzanne Perry of Yale Law School, which finds that black taxi patrons left significantly lower tips than whites, controlling for destination and length of trip.
25. Suppose that drivers’ motivations were based purely on animus towards blacks, rather than on profitability concerns. The results should be no different. Following Becker (1971), drivers’ animus can be modeled as an implicit psychological tax that prejudiced drivers incur when they pick up black passengers. If the tax is low relative to the costs of searching for another customer, drivers will prefer to pick up the black patron and pay the “tax,” rather than continuing to search for a preferred white passenger. If an alternative white customer is immediately available, however, the driver will have no reason to incur the psychological costs that result from serving a black customer, and will choose to pick up the white patron instead.

26. In this context, statistical independence means that \( \text{PR(Taxi Stops|Customer is Black)} \) is the same as \( \text{PR(Taxi Stops|Black Customer and Alternative White Customer 40 feet away)} \). If the two conditional probabilities are not the same, the presence of the second tester influences the probability of the first tester’s successfully hailing a cab. Contrast this with testing in the context of housing, employment, or car purchases. The large numbers of applicants or buyers should mean that the presence of one tester has no effect on the outcomes of the others.

Note that nonindependence also implies that the standard errors used to construct test statistics are also incorrect.

27. Note that non-independence is much less likely to be a problem with the housing or employment audits discussed in Fix and Struyk (1993), or new car audits conducted by Ayres and Siegelman (1995). In the housing and employment context, testers were instructed to turn down job offers or apartment rentals in order to prevent a successful outcome from influencing the success or failure of their audit-mate. In the new car study, one tester was unlikely to influence the price quoted to his or her partner because the number of prospective buyers who visit a showroom without buying is so large.

28. Of course, enforcement of laws prohibiting discrimination does require a precise definition, and courts in the heyday of the Civil Rights era did have to struggle with these kinds of definitional issues, especially in interpreting Title II of the 1964 Civil Rights Act, 42 U.S.C. § 2000(a)-(a)6, which prohibits discrimination in various public accommodations. But most of these issues have been settled for decades; the last public accommodations case heard by the Supreme Court was apparently in 1973 (\text{Tillman v Wheaton-Haven Recreation Assn., Inc.} 410 U.S. 431).

Essentially, Title II forbids racial discrimination in hotels, motels, inns, restaurants, gas stations, movie theaters, and other places of entertainment. Section 1981 of the 1866 Civil Rights Act, 42 U.S.C. § 1981, prohibits (intentional) race discrimination in the making and enforcement of contracts; it presumably applies to most instances of discrimination in shopping.

29. This is not to deny the existence of discriminatory conduct, of which the record contains numerous examples—Shoney’s, Denny’s, and so on. But the plural of anecdote is not data.

30. The poll was based on telephone interviews with a representative sample of 1,269 blacks and 1,680 whites who were interviewed in early 1997. The margin of error for a percentage estimate for blacks is approximately \( \pm 5 \) percentage points. Gallup (1997, 5-6).

There appears to be very little social science literature that speaks directly to the question of how frequently blacks encounter discrimination in public accommodations. Wilson (1980)
asserted that poverty, rather than discrimination, is the real obstacle blocking the advancement of the black urban underclass. Wilson’s book does not squarely address the basic question of how much discrimination is actually out there, however. Feagin (1991) asserts that discrimination is still important, but his data are useless for estimating how frequently it occurs. More recently, Thernstrom and Thernstrom (1997) skirt this question by focusing not on the current prevalence of discrimination but on its rate of change over time—allegedly negative.

31. This calculation is not sensitive to the assumption about how restaurant meals are distributed among the population: It wouldn’t matter if 20 percent of the respondents did all of the restaurant dining and the remaining 80 percent did none. But the calculation is sensitive to the total number of meals consumed, and to the assumption that those reporting discrimination experienced only a single incident. Fewer total visits or more incidents per respondent would obviously raise the rate of discrimination per meal; the relationship is linear, so doubling the number of meals or discriminatory incidents per person doubles the estimated rate of discrimination per meal.

32. In fact, under the above assumptions, the probability that a black person will experience discrimination in a restaurant at least once during the course of a year is \((1 - (1 - .025)^{100})\), or 92 percent.

33. If we assume one shopping trip per respondent per day, then by the same logic as in Table 5, the probability of discrimination per shopping trip is roughly 1 percent.

34. The test statistic is \((O - E)/SE\), where \(O\) is the observed difference in discrimination between the two years, \(E\) is the expected difference if the null hypothesis (no change in the rate of discrimination) is true, and \(SE\) is the standard error of this difference. The test statistic has a standard normal distribution. Assume that the observed rate of discrimination in year 1 is 1 percent, and that it is half as big (0.5 percent) in year 2. Hence, we have \(O = 0.01 - 0.005 = 0.005\). By hypothesis, \(E = 0\).

The standard error of the difference is \(SE = \sqrt{\sigma_1^2 + \sigma_2^2}\), where the subscripts denote the first and second years being compared. To calculate \(\sigma_1\), take \(\sqrt{p_1(1-p_1)/N_1}\), where \(p\) is the probability of encountering discrimination. Thus, \(\sigma_1 = \sqrt{0.01\times0.99}/N_1\); \(\sigma_2 = \sqrt{0.005\times0.995}/N_2\). To simplify, assume \(N_1 = N_2\).

The critical value of the test statistic at the 5 percent significance level is 1.96. Hence, we are looking for values of \(N\) such that \((O - E)/SE \geq 1.96\). Given that \(O - E = 0.005\), we need \(SE\) less than 0.00254, which in turn requires \(N \geq 2285\). Notice that the identical problem occurs if we try to compare discrimination rates across regions of the country, instead of across points in time.

35. Another important question that might arise from Table 7 is why plaintiffs seem to be winning so small a fraction of the cases. It is hard to be precise about what constitutes a win, and much relevant information is missing, but plaintiffs clearly failed to prevail at a very early stage of adjudication in more than half (13 of 23) of the cases listed. Does this imply that meritorious claims of public accommodations discrimination are rare, and hence that discrimination is not really a serious problem in this context? The answer is clearly “No.” The reason is that the cases that are adjudicated to the point at which an opinion is written are not a random sample of all filed cases, let alone all potential suit-generating incidents. Without knowing how many cases settled, and on what terms, we should not infer anything from the results of adjudicated cases.
36. If public accommodations cases generate opinions at the same rate as employment discrimination cases, then these 23 opinions should represent something like 160 filed cases. But unlike Title VII (before the 1991 Civil Rights Act), some of the public accommodations statutes allow jury trials, which rarely generate published opinions. Settlement rates may also be higher in public accommodations than in employment discrimination cases. Both factors mean that there were probably more than 160 cases filed, but it is hard to say more than this.

37. Incidents of discrimination generally have low rates of legal claiming compared with other kinds of grievances, in large part because discriminatory intent is often difficult to prove. See Miller and Sarat (1980); Curran (1973). But see, Kritzer, et al, (1991) (discrimination grievances have a higher rate of claiming than traditionally thought). Moreover, litigation is a costly process, and people do not usually sue over a single rude remark or a few minutes delay in being seated at a restaurant. Even though such incidents can be psychologically very wounding, the costs of litigation make it an impractical response to most perceived instances of discrimination.

Another important consideration is the damages a potential plaintiff could expect to receive if he or she prevailed. Although “courts are in general agreement that punitive damages may be awarded in appropriate circumstances in actions to recover for violations of civil rights statutes,” Annotation, Punitive Damages in Actions for Violations of Federal Civil Rights Acts, 14 A.L.R. Fed. 608, § 2a (1998), it seems unlikely that they would routinely be granted for an isolated incident. Actual damages might include some payment for emotional distress or other psychological harm, but it would presumably also be small in most cases.

In sum, potential plaintiffs simply may not have an incentive to enforce their legally protected rights, given the high costs of doing so and the meager returns they could expect to receive. Class action litigation offers a partial solution to some of the structural problems posed by large numbers of small monetary injuries, but class action public accommodations suits apparently are relatively rare.

38. For compelling accounts of the psychological impact of discrimination while shopping, see Buchanan (1997). The existence of the phrase “shopping while black”—with its allusions to “driving while intoxicated” or other criminal offenses—suggests that the problem of discrimination while shopping is frequent enough to merit its own shorthand description in the black community.

39. This is not to suggest that the laws are unimportant. It is obvious that legal penalties against discrimination played a major role in abolishing segregated eating and recreational facilities in the decade after 1964. See, for example, U.S. v Boyd, 327 F.Supp. 998 (S.D.Ga. 1971) (supplemental decree) (requiring detailed changes in operation of Vandy’s Bar-B-Q in Statesboro, Ga., in order to eliminate segregation); Katzenbach v McClung, 379 U.S. 294, 296 (1964) (applying Title II of the 1964 Civil Rights Act to Ollie’s Barbecue, “a family-owned restaurant in Birmingham, Alabama, specializing in barbecued meats and homemade pies, . . . [that] has refused to serve Negroes in its dining accommodations since its original opening in 1927.”)

40. It is easy to overlook the role of market forces in recent celebrated cases of public accommodations discrimination such as those involving Denny’s or Shoney’s. Litigation or the threat of litigation undoubtedly played a major role in the extensive reform efforts that both these
corporations undertook; but adverse publicity, the potential loss of business, and even pressure from the capital markets also apparently played an important role, albeit not until the discriminatory practices had been exposed. On Shoney’s, for example, see Kerr (1993) and Chicago Tribune (1992) (replacement of CEO and possible return to discriminatory practices caused stock price to fall).

41. See, for example, Jackson v Motel 6, Inc., 931 F.Supp. 825, (M.D.Fl. 1996) (black police officers told that motel was full; white officers subsequently obtained a room); Harvey v NYRAC, Inc., 813 F.Supp. 206, (E.D.N.Y. 1993) (black plaintiff allegedly denied a car rental because of her race).

42. Alexis v McDonald’s Restaurants of Mass., Inc., 67 F.Supp. 825, (M.D.Fl. 1996) (black police officers told that motel was full; white officers subsequently obtained a room); Harvey v NYRAC, Inc., 813 F.Supp. 206, (E.D.N.Y. 1993) (black plaintiff allegedly denied a car rental because of her race).


44. The analogy is to so-called “Sex-Plus” discrimination in employment discrimination law, under which an employment policy that does not discriminate solely on the basis of sex, but on sex and some other neutral classification, may nevertheless be held to violate Title VII. See, for example, Phillips v Martin Marietta Corp., 400 U.S. 542 (1971) (employer who refused to hire women, but not men, with pre-school-age children is potentially liable under Title VII); Willingham v Macon Telegraph Publ. Co., 507 F.2d. 1084 (5th Cir. 1975) (employer who refused to hire men, but not women, with long hair not liable under Title VII because the “plus” characteristic did not involve a fundamental right).

The difference between “race-plus” and “sex-plus” discrimination is that the latter involves an official policy that openly treats men and women differently (women, but not men, can have long hair). In “race-plus” discrimination, there can be no overt policy to treat the two groups differently, since a policy that whites, but not blacks, can protest about how long they’ve had to wait for a table would clearly be illegal. “Race-plus” discrimination could result from covert policies, or emerge out of some combination of fear, prejudice, or stereotypes.

45. It may be relevant that John Donohue and I noticed a similar pattern in our study of employment discrimination cases, in which frequently “a worker is fired . . . because of some alleged individual misconduct such as tardiness. The worker then alleges that those of the opposite race or gender were either less productive or even more guilty of the alleged offense but were not fired.” Donohue and Siegelman (1991, 1012) (extensive citations omitted).

Zwerling and Silver (1992) reached a similar conclusion after an extensive review of the careers of 2100 newly hired workers in one post office district. Even though they concluded that most of the fired black workers in their study probably deserved to be fired, they found a significant disparity in black/white firing rates, which they attributed to the fact that many whites who deserved to be fired were not. Thus, the discrimination they observed was based not on race alone, but on race plus some other factor (e.g., unexcused absence from work).

46. “Race-plus” audits pose some significant challenges. Ordinary audits are taxing enough, but asking the auditors deliberately to start a confrontation with shopkeepers or hotel clerks in order to engineer a “Race-plus” situation seems both difficult and dangerous. Experiments involving confrontation are possible in very controlled circumstances, as evidenced by the work of Nisbett
and Cohen (1996), which compared reactions of Northern and Southern students when a confederate of the experimenter bumped into the subject in a school hallway and called the unsuspecting subject an "asshole." But they seem too dangerous to attempt in a field setting. (On the other hand, there may be special circumstances that could generate “Race-plus” discrimination without confrontation: for example, testers could attempt to return goods without a store receipt, or could show up one minute after closing time and ask to be admitted to a store.)

Moreover, even a survey could run into the general equilibrium problem discussed earlier: Respondents may not report experiencing many instances of “race-plus” discrimination precisely because they believe (whether correctly or not) that it is a serious problem and therefore always feel constrained to act on their best behavior in any public accommodations setting. Note that the reverse could also be true: suppose black customers draw the reasonable, but sometimes incorrect, inference that when they receive bad service, it is because of their race. This reaction, especially when it is incorrect, could provoke precisely the hostile or surly counter-reaction that constitutes “race-plus” discrimination.

47. The Shoney’s case appears to be a textbook example of racist management that discriminated against blacks, both as employees and as customers, as a matter of corporate policy.

48. A North Carolina Avis franchise strongly discouraged its personnel from renting cars to blacks; but as far as I know there is no evidence to suggest that this was corporate policy, or that other Avis franchises also practiced this kind of discrimination. In fact, subsequent testing at other Avis outlets apparently revealed no irregularities.

49. Moreover, an explicit national policy of discrimination is more difficult to conceal because it requires more participants, at least one of whom is likely to find it objectionable.

50. In fact, McDonald’s (and presumably many other large corporations) already has its own internal auditors who pose as customers, order meals, and note any shortfalls from company standards, including incorrect orders, slow service and so on. Hostile treatment of black customers would presumably be detected by such internal audits and met with the appropriate sanctions. This analysis follows closely on Nelson’s underappreciated article (Nelson, 1976).

Of course, profit-maximizing managers might conclude that discriminating against blacks encourages more white customers than the black business it forecloses, as Shoney’s apparently believed. But the careful survey evidence in Sniderman and Piazza (1993) suggests that there are unlikely to be enough white racists to make this kind of discriminatory strategy profitable. This is especially true when the possible legal penalties are factored in.

51. A short list might include: deeper pockets, greater stakes in appearing not to be discriminating (higher willingness to pay), more personnel (and therefore a higher chance of finding a disgruntled insider who can testify to discriminatory practices), more customers (so that a pattern or practice of discrimination is easier to detect), and so on.

52. Of course, enforcement efforts have to take account of the benefits, as well as the costs, of enforcement. A lawsuit or settlement involving Joe’s Diner might affect the treatment of 1,500 black customers each year, whereas getting a Denny’s to change its practices could easily result in improved service for 100 times that number of patrons.
53. The flip side of this problem is part of what Orlando Patterson has called “The Ordeal of Integration”: If minorities now feel freer to go places and do things they formerly avoided, they may be more likely to run into a racist even as the percentage of discriminators declines. In this sense, incidents of discrimination can be increasing even as the number of discriminators is decreasing. See Patterson (1997, 52-63). John Donohue and I similarly argued that increasing workforce integration makes it easier to detect discrimination, since it gives black workers a more accessible white co-worker against whom they can measure their treatment; hence, integration can lead to more employment discrimination suits at the same time that the amount of discrimination is falling. See Donohue and Siegelman (1991, 1011-14).

54. Surveys can also help explicate the psychological costs of discrimination, which, I suspect, do not necessarily track the dollar amounts at stake. In part because it is covert, discrimination in new car sales probably has a much smaller psychological impact on black buyers than does discrimination in restaurants or shopping, even though the dollar amounts at stake are much larger.

It is also important to acknowledge the social costs of discrimination, which, again, audits are not well suited to uncover. Perceived discrimination presumably results in alienation, loss of faith, and disaffection that undermine the social fabric in subtle but potentially important ways.

55. For an excellent review of such studies, now unfortunately rather dated, see Crosby, et al (1980).

56. In fact, generalized random auditing of public accommodations could easily give rise to charges of “Discrimination Nazis run amok,” and so on. Rather than an Occupational Safety and Health Administration model of general inspection, a more sensible enforcement strategy would rely on audits only when a complaint of discrimination has already been initiated from some other source.