The purpose of this chapter is to investigate, for the first time, the variation across metropolitan areas in their changing levels of housing discrimination, with the aim of exploring whether they are “causally” related to corresponding variations in fair housing enforcement activities during the 1990s, controlling for other factors. The specific research questions include: How do changes in the 1989–2000 incidence of racial/ethnic discriminatory behaviors of various sorts correlate with fair housing enforcement activity during the 1990s? Does this answer vary depending on whether one considers the sales or rental housing markets, or discrimination directed against black or Hispanic home seekers?

We now know that, although prohibited by federal statutes since 1968, discrimination by real estate agents and landlords directed against minority home seekers continues to occur throughout America’s metropolitan areas (see chapter 2 by Turner, Richardson, and Ross within). Such discrimination can impose substantial psychological as well as economic costs on those who are directly victimized (Yinger 1995). Moreover, housing discrimination has been shown to perpetuate residential segregation (Galster 1986, 1987a, 1988a, 1988b, 1991; Massey, Eggers, and Denton 1994), which in turn has been linked to a variety of negative social and economic outcomes for minority communities.¹

We now also know that housing discrimination declined substantially in magnitude over the last decade on most measures, with the exception of discrimination against Hispanic renters (Turner et al., 2002; Ross and Turner 2005).² This decline has occurred immediately following a substantial strengthening of the federal fair housing law in 1988. There has also been a
range of changes and improvements in the enforcement capacity at the go-
vernmental and nongovernmental levels during this time period that, arguably,
may have been influential in creating the lowered levels of discrimination
practiced by the real estate industry. This coincidence raises the provocative
issue of how these two occurrences may be causally related.

Our analysis is based on a nationally representative sample of seventeen
metropolitan areas where black-white paired tests were conducted and eleven
areas where Hispanic-white tests were conducted for both the 1989 and 2000
Housing Discrimination Studies (HDS). We measure the 1989–2000 changes
in five different real estate agent behaviors related to differential treatment of
minority and white homebuyers, and four landlord behaviors related to dif-
ferential treatment of rental apartment seekers, plus summary indexes of such
behaviors in each sector. We use damages awarded in racial-ethnic housing-
discrimination lawsuits during the 1990s as a proxy for effectiveness of fair
housing efforts. To our knowledge, our work is the first to explore statistically
the relationships between fair housing enforcement activities and changes in
the incidence of housing discrimination across metropolitan areas.

The remainder of this chapter is organized as follows. In the first section, we
highlight key changes in fair housing law and enforcement capacity com-
mencing at the beginning of our study period that could plausibly suggest that
law enforcement practices contributed to the reduction of housing discrimina-
tion over time. In the next section we review the three earlier studies that have
investigated the sources (besides enforcement) for the cross-metropolitan vari-
ability of housing discrimination. Following that, we discuss the main method-
ological challenge in measuring the causal impact of fair housing enforcement
on behavior: the likelihood that fair housing resources are targeted to where
discrimination is most pervasive. We next present our empirical approach and
discuss the data sources and enforcement measures with which we experi-
mented. We conclude by discussing results that are suggestive of deterrence
effects and draw a limited set of recommendations for needed research and
policy implications about fair housing enforcement efforts.

CHANGES IN FAIR HOUSING LAW AND
ENFORCEMENT SINCE 1987

Fair housing enforcement activities in the United States are carried out by nu-
merous agencies, including private, nonprofit organizations, human rights
commissions at the state and local levels, and the federal Departments of Jus-
tice and Housing and Urban Development. Considerable enhancements to the
enforcement system were put into place at about the time of the 1989 HDS,
and others appeared in the early 1990s. Thus, it is reasonable to expect that if this expanded fair housing enforcement had its intended impact, it would register as a decline in discrimination observed between the 1989 and 2000 HDS.

The Fair Housing Amendments Act of 1988 created a new administrative adjudication process for more timely resolution of housing discrimination complaints and allowed for stiffer civil penalties (see Schill, chapter 7 within; Mathias and Morris 1999). The 1988 act notably expanded the federal government’s role in enforcing fair housing statutes and provided the U.S. Department of Housing and Urban Development (HUD) with the apparatus for a legal, binding resolution of complaints other than conciliation.

The other important legislative initiatives were contained in the 1987 and the 1991 Housing Acts. These acts created the Fair Housing Initiatives and Fair Housing Assistance Programs (FHIP and FHAP) that distribute funds to private fair housing groups and state human rights commissions, respectively, in order to provide education to local communities and conduct investigations of fair housing complaints.

Also noteworthy was the expansion of the U.S. Department of Justice (DOJ) enforcement efforts during the Clinton administration (Galster 1995; Goering and Squires 1999). The Department of Justice (DOJ) manages a civil rights enforcement division and during this period developed its own housing testing division. As a result, the number of DOJ-initiated fair housing cases increased from less than twenty prior to 1988 to well over a hundred during the mid-1990s (Lee 1999).

This listing of enhancements since 1987 does not, of course, suggest that the nation’s efforts to combat discrimination have been efficient or sufficient, as there is no reference to staffing or resource levels. Indeed, HUD’s implementation of the act has been criticized (see Schill, chapter 7 within) and the adequacy of enforcement capacity nationwide questioned by many (Yinger 1995). More structurally, Galster (1990, 1999) has argued that many enforcement efforts of this period were unlikely to create an effective deterrent against housing discrimination so long as the system relies almost exclusively on individual bona fide home seekers recognizing that they have been victimized and then filing suit. Discrimination is often practiced in a subtle fashion so that victims are typically unaware; even those who are suspicious are unlikely to bear the substantial time, monetary, and psychological costs of pursuing a complaint in light of a protracted process with prospects for minimal compensatory awards.

Thus, we think it is appropriate to investigate empirically the degree to which the strengthening of fair housing law and enforcement capacity beginning in 1987 reduced discrimination over the last decade. Of course, discrimination may well have declined since 1989 for reasons having nothing to do
with fair housing activities. We therefore review in the next section studies that have investigated the predictors of housing discrimination in metropolitan areas, so that we may draw upon this work when devising control variables for our own statistical analysis.

**PREVIOUS RESEARCH ON CROSS-METROPOLITAN VARIATIONS IN HOUSING DISCRIMINATION**

What, besides the intensity of fair housing enforcement, might explain why one metropolitan area has a higher level of housing discrimination than another? Only three studies have examined this question; all utilized cross-metropolitan differences in discrimination as revealed by the national paired-testing studies.³ They have estimated multiple regression models of the metropolitan level of discrimination, based on metro-wide economic, social, and demographic characteristics, but few robust conclusions have been produced. Perhaps most centrally for the current work, none have tried to model the impact of fair housing enforcement efforts.

Galster and Keeney (1988) investigated the variations in housing discrimination against blacks across forty metropolitan areas that were sampled as part of the HUD-sponsored Housing Market Practices Survey of 1977, the first national study to employ paired testing (Wienk et al. 1979). They created a composite measure of the incidence of rental and sales discrimination in each of these metropolitan areas, based on the results of the paired tests conducted there. They employed this measure (instrumented) as an endogenous variable in a four-simultaneous-equations model of black-white discrimination, segregation, and disparities in occupations and incomes. They found that discrimination was higher in metropolitan areas where: (1) above-median- and below-median-priced, single-family housing was more dissimilarly distributed across space; (2) housing vacancy rates (both tenures combined) were lower; and (3) interracial income disparities were greater. Whites’ educational levels and the absolute and relative sizes of the black population in the metropolitan area were not related to the incidence of this composite measure of housing discrimination.

Galster (1991) used data from the aforementioned 1977 Housing Market Practices Survey to explore the geographical differences in discrimination on both the rental and sales sectors. He found that only the metropolitan-wide percentage of whites residing in their current home more than five years provided consistent (inverse) explanatory power for both sectors. In the rental sector, discrimination was more prevalent in metropolitan areas with absolutely larger black populations. In the home sales sector, discrimination was
more prevalent in areas with slower-growing black populations and those with lower vacancy rates.

These results could not be replicated by Page (1995) with more recent discrimination data, though it appears likely that her smaller sample of metropolitan areas tested may be the reason. Page could not discern from the 1989 HDS data statistically significant variations in rental discrimination across the twenty-five metropolitan areas sampled. Although such variation was present in the sales sector, her attempts to estimate a multiple regression model of cross-sectional differences in sales discrimination rates revealed no statistically significant predictors.4

The research reported above is valuable for our effort because it draws upon the analytic foundations and control variables measuring metropolitan demographic and economic conditions that have proven predictive of underlying discriminatory behavior. None of this earlier work has attempted, however, to relate cross-sectional or historical variations in housing discrimination to variations in fair housing enforcement activities. Our key contribution in this chapter is to introduce for the first time a new set of predictors related to enforcement agency actions in relationship to patterns of discrimination. As we will discuss below, however, there are a number of daunting methodological challenges in attempting this form of causal specification.

**THE CHALLENGE OF ESTABLISHING CAUSALITY**

The fundamental challenge that any study of the relationship between fair housing enforcement activity and housing market discrimination must face is identifying and measuring unambiguously their causal connections. Ideally, we seek to find out how discrimination will change when enforcement activity changes due to some exogenous force. Under this scenario of causation, a negative correlation between discrimination and enforcement would unambiguously support the notion that the latter deters the former. Unfortunately, causation in the real world cannot be assumed to run in such a simple, unidirectional way. On the contrary, it is likely that metropolitan areas in which discrimination has historically been most severe and resistant to change are precisely where scarce fair housing enforcement resources have been targeted. Thus, we might expect more housing discrimination to cause more enforcement actions, thereby producing a positive correlation between these variables.

We therefore suspect that, in technical terms, enforcement variables are likely to be endogenous, not exogenous. To the degree that they are endogenous there will be a statistical bias toward observing a *positive* correlation between
enforcement and discrimination, thus reducing the prospects for concluding that enforcement reduces discrimination. Put simply: the empirical deck is stacked against finding that enforcement matters in the fight against illegal discrimination; thus any observed negative correlation between these two variables offers strong evidence indeed of the efficacy of enforcement. We shall return to this theme later as we describe the particular measures of enforcement activity that we have available to us.

OUR EMPIRICAL APPROACH: REDUCING OMITTED VARIABLES BIAS BY MODELING CHANGES IN DISCRIMINATION

Though causality is the dominant challenge of our research, omitted explanatory variables is another of note. If we are to get the best possible estimate of the relationship between enforcement activity (E) and discrimination (D), we must control statistically for a wide range of other factors that may influence the incidence of discrimination in a metropolitan area. Prior empirical work cited above suggests some feasible control variables to employ. Unfortunately, many of the other control variables are virtually impossible to measure. For example, there are probably a host of metropolitan area-specific historical particulars that have shaped racial attitudes that motivate discriminatory acts. Moreover, other local idiosyncrasies that characterize the real estate brokerage industry in each metropolitan area remain unknown to us. Thus, we consider the potential problem of omitted variables bias a serious one.

Insofar as many of these factors vary inconsequentially over the span of a decade, however, we can minimize their omission by constructing a model of changes in discrimination (D), instead of its level. Symbolically, the relationships may be specified for a particular metropolitan area j:

\[ D_{89j} = \alpha + [X_{89j}]\beta_i + E_{89j}\Phi_i + [C_{kj}]\gamma_k + \epsilon \]

\[ D_{00j} = \delta + [X_{00j}]\theta_i + E_{00j}\psi_i + [C_{kj}]\lambda_k + \epsilon \]

where: 89, 90, and 00 subscripts indicate years 1989, 1990, and 2000; Greek letters represent parameters to be estimated by multiple regression; \(E_j\) is fair housing enforcement activity; \([X_i]\) is a vector of i measurable metropolitan characteristics that change over time; and \([C_k]\) is a vector of k time-invariant, unmeasurable metropolitan characteristics. Because \([C_k]\) is constant, by taking the difference between equations (2) and (1) we can obtain an estimable equation where only measurable variables appear:
In our model we employ as X the metropolitan statistical area 1990 to 2000 changes in: (1) the proportion of blacks; (2) the proportion of Hispanics; (3) the segregation of blacks; and (4) the ratio of black/white median household incomes. For D we alternately employ the incidence of discrimination against black and Hispanic home seekers, each related to a different sort of discriminatory behavior, to test for the robustness of the patterns we discern. For E we ideally would use changes in enforcement activity by federal, state, and local agencies, but due to data limitations, we were limited to an estimated cumulative value for the period. Details on all these measures follow in the next section.

**DATA SOURCES AND MEASUREMENT ISSUES**

**Measuring Housing Discrimination Consistently Over Time: Key Feature of HDS**

We obtained measures of discrimination from the two national Housing Discrimination Studies (HDS), conducted in 1989 and 2000 by the Urban Institute under contract to HUD (Struyk, Turner, and Yinger 1991; Turner et al. 2002). These two HDSs were conducted using consistent sampling and paired-testing protocols in multiple metropolitan areas for both years. Metropolitan areas were chosen to provide a nationally representative sample of housing markets where black and Hispanic home seekers constituted a substantial fraction; the specific areas sampled and the number of rental and sales tests in each are presented in the appendix to this chapter. Because testing was conducted and measures of discrimination were coded in a comparable manner both years, the two HDSs offer a unique opportunity to measure discrimination over time.

A visit with a rental or sales agent is a complex transaction and may include many forms of favorable or unfavorable treatment. We present results for a series of individual treatment indicators that reflect important aspects of the housing transaction. These indicators are identical to those employed by Turner et al. (see chapter 2) and therefore we will not describe them again in detail; they are listed in table 8.1.

We also combine the treatment indicators within each category to create a composite or consistency measure, such as “housing availability” or “terms and conditions.” Specifically, tests are classified as “white-favored” if the

\[
(3) \quad D_{t0} - D_{s0} = [\delta - \alpha] + [X_{t0}] - [X_{s0}] + [\theta - \beta] + (E_{t0} - E_{s0}) \cdot [\Phi - \psi] + \epsilon
\]
white tester was consistently favored, that is, received favorable treatment on one or more individual constituent items, while his or her minority partner received no favorable treatment relative to the white tester on any items. Tests are classified as “neutral” if one tester was favored on some individual treatment items and his or her partner was also favored on at least one item. One advantage of this consistency composite is that it identifies tests where one partner was unambiguously favored over the other. Finally, we specify an “overall” composite measure, based on the same principles as above, except that consistent favoritism must be demonstrated on one or more of the four (in rental) or five (in sales) behavioral categories, with no countervailing favoritism on any category.

Changes in discriminatory treatment, as measured by the foregoing indicators, has previously been analyzed in detail by Turner et al. (chapter 2), so we will provide only an overview. In the rental market, discrimination persists in 2000 against apartment seekers in both groups in the areas of availability and inspection even though the incidence of such discrimination has declined considerably for blacks since 1989, but not for Hispanics; see table 8.2. Discrimination in the area of encouragement has declined for both groups. However, in fewer types of treatments did discrimination against Hispanic renters decline compared to black renters.
In the sales market, discrimination against black home buyers persists in 2000 in all areas of treatment, but only against Hispanics in financing; see table 8.3. In general, the levels observed in 2000 are substantially lower than they were in 1989, however. The two key exceptions are higher incidences of racial steering of both groups in 2000 and higher incidences of discrimination against Hispanics in the area of financial assistance. For further analysis of steering, see Galster and Godfrey (2005).

Table 8.2. Incidence of Net Adverse Treatment in the Rental Market

<table>
<thead>
<tr>
<th>Treatments</th>
<th>Net 1989</th>
<th>Net 2000</th>
<th>Change</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Black-White Tests</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Availability</td>
<td>0.144 (0.035)*</td>
<td>0.046 (0.021)*</td>
<td>−0.097 (0.038)*</td>
</tr>
<tr>
<td>Inspection</td>
<td>0.148 (0.030)*</td>
<td>0.069 (0.019)*</td>
<td>−0.078 (0.037)*</td>
</tr>
<tr>
<td>Terms</td>
<td>0.045 (0.026)*</td>
<td>−0.004 (0.016)</td>
<td>−0.050 (0.030)</td>
</tr>
<tr>
<td>Encouragement</td>
<td>0.099 (0.036)*</td>
<td>0.016 (0.022)</td>
<td>−0.083 (0.044)*</td>
</tr>
<tr>
<td>Overall</td>
<td>0.098 (0.028)*</td>
<td>0.026 (0.020)</td>
<td>−0.071 (0.034)*</td>
</tr>
<tr>
<td><strong>Hispanic-Anglo Tests</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Availability</td>
<td>0.106 (0.038)*</td>
<td>0.111 (0.026)*</td>
<td>0.005 (0.047)</td>
</tr>
<tr>
<td>Inspection</td>
<td>0.083 (0.029)*</td>
<td>0.064 (0.022)*</td>
<td>−0.018 (0.038)</td>
</tr>
<tr>
<td>Terms</td>
<td>0.014 (0.032)</td>
<td>0.012 (0.023)</td>
<td>−0.001 (0.042)</td>
</tr>
<tr>
<td>Encouragement</td>
<td>0.147 (0.037)*</td>
<td>0.035 (0.027)</td>
<td>−0.111 (0.049)*</td>
</tr>
<tr>
<td>Overall</td>
<td>0.146 (0.028)*</td>
<td>0.061 (0.025)*</td>
<td>−0.085 (0.039)*</td>
</tr>
</tbody>
</table>

Note: Standard errors are shown parenthetically; * p<.05; # p<.10

In the sales market, discrimination against black home buyers persists in 2000 in all areas of treatment, but only against Hispanics in financing; see table 8.3. In general, the levels observed in 2000 are substantially lower than they were in 1989, however. The two key exceptions are higher incidences of racial steering of both groups in 2000 and higher incidences of discrimination against Hispanics in the area of financial assistance. For further analysis of steering, see Galster and Godfrey (2005).

Table 8.3. Incidence of Net Adverse Treatment in the Sales Market

<table>
<thead>
<tr>
<th>Treatments</th>
<th>Net 1989</th>
<th>Net 2000</th>
<th>Change</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Black-White Tests</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Availability</td>
<td>0.172 (0.026)*</td>
<td>0.049 (0.022)*</td>
<td>−0.122 (0.034)*</td>
</tr>
<tr>
<td>Inspection</td>
<td>0.113 (0.021)*</td>
<td>0.069 (0.025)*</td>
<td>−0.044 (0.033)</td>
</tr>
<tr>
<td>Steering</td>
<td>−0.058 (0.016)*</td>
<td>0.049 (0.017)*</td>
<td>0.107 (0.024)*</td>
</tr>
<tr>
<td>Financing</td>
<td>0.121 (0.025)*</td>
<td>0.047 (0.023)*</td>
<td>−0.074 (0.034)*</td>
</tr>
<tr>
<td>Encouragement</td>
<td>0.130 (0.026)*</td>
<td>0.052 (0.023)*</td>
<td>−0.078 (0.037)*</td>
</tr>
<tr>
<td>Overall</td>
<td>0.147 (0.023)*</td>
<td>0.037 (0.018)*</td>
<td>−0.109 (0.030)*</td>
</tr>
<tr>
<td><strong>Hispanic-Anglo Tests</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Availability</td>
<td>0.148 (0.027)*</td>
<td>0.028 (0.040)</td>
<td>−0.120 (0.054)*</td>
</tr>
<tr>
<td>Inspection</td>
<td>0.127 (0.025)*</td>
<td>−0.029 (0.034)</td>
<td>−0.156 (0.044)*</td>
</tr>
<tr>
<td>Steering</td>
<td>0.038 (0.019)</td>
<td>0.034 (0.029)</td>
<td>−0.004 (0.037)</td>
</tr>
<tr>
<td>Financing</td>
<td>0.022 (0.025)</td>
<td>0.135 (0.031)*</td>
<td>0.112 (0.043)*</td>
</tr>
<tr>
<td>Encouragement</td>
<td>0.164 (0.025)*</td>
<td>0.042 (0.030)</td>
<td>−0.122 (0.044)*</td>
</tr>
<tr>
<td>Overall</td>
<td>0.121 (0.023)*</td>
<td>0.067 (0.023)*</td>
<td>−0.054 (0.037)*</td>
</tr>
</tbody>
</table>

Note: Standard errors are shown parenthetically; * p<.05; # p<.10
Although the HDS data provide the only reliable measures of housing discrimination over time, we should not fail to note that they have two important limitations for use in estimating statistical models such as equation (3). First, they are measured with error. As shown in table 8.4, the estimated incidence of discrimination in each metropolitan area is based on a finite sample of paired tests and thus has an associated statistical confidence interval. Second, the HDS samples of metropolitan areas are extremely small: seventeen for black-white discrimination and eleven for Hispanic-Anglo discrimination.

### Table 8.4. HDS Sample Sizes by Metropolitan Area, Year and Minority Group

<table>
<thead>
<tr>
<th></th>
<th>HDS 20001</th>
<th></th>
<th>HDS 1989</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Black-White Tests</td>
<td>Hispanic-Anglo Tests</td>
<td>Black-White Tests</td>
<td>Hispanic-Anglo Tests</td>
</tr>
<tr>
<td><strong>Black-White/Hispanic-Anglo Sites</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Los Angeles</td>
<td>69/68</td>
<td>75/69</td>
<td>75/104</td>
<td>81/120</td>
</tr>
<tr>
<td>New York</td>
<td>75/68</td>
<td>66/70</td>
<td>54/87</td>
<td>64/118</td>
</tr>
<tr>
<td>Chicago</td>
<td>65/63</td>
<td>65/68</td>
<td>66/103</td>
<td>81/122</td>
</tr>
<tr>
<td>Houston</td>
<td>70/78</td>
<td>68/75</td>
<td>42/43</td>
<td>51/53</td>
</tr>
<tr>
<td>Miami</td>
<td>74/71</td>
<td>73/70</td>
<td>32/39</td>
<td>58/60</td>
</tr>
<tr>
<td>Denver</td>
<td>72/71</td>
<td>73/78</td>
<td>44/51</td>
<td>65/73</td>
</tr>
<tr>
<td>Austin</td>
<td>69/75</td>
<td>70/72</td>
<td>32/43</td>
<td>55/63</td>
</tr>
<tr>
<td><strong>Black-White Only Sites</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta</td>
<td>81/78</td>
<td>—</td>
<td>66/94</td>
<td>—</td>
</tr>
<tr>
<td>Philadelphia</td>
<td>73/70</td>
<td>—</td>
<td>30/44</td>
<td>—</td>
</tr>
<tr>
<td>Detroit</td>
<td>66/71</td>
<td>—</td>
<td>33/48</td>
<td>—</td>
</tr>
<tr>
<td>Washington, DC</td>
<td>74/69</td>
<td>—</td>
<td>32/43</td>
<td>—</td>
</tr>
<tr>
<td>New Orleans</td>
<td>68/76</td>
<td>—</td>
<td>33/44</td>
<td>—</td>
</tr>
<tr>
<td>Pittsburgh</td>
<td>79/75</td>
<td>—</td>
<td>38/46</td>
<td>—</td>
</tr>
<tr>
<td>Dayton-Springfield</td>
<td>70/70</td>
<td>—</td>
<td>33/47</td>
<td>—</td>
</tr>
<tr>
<td>Orlando</td>
<td>72/76</td>
<td>—</td>
<td>32/43</td>
<td>—</td>
</tr>
<tr>
<td>Macon/Warner/Robins</td>
<td>69/73</td>
<td>—</td>
<td>33/45</td>
<td>—</td>
</tr>
<tr>
<td>Birmingham</td>
<td>77/66</td>
<td>—</td>
<td>34/48</td>
<td>—</td>
</tr>
<tr>
<td><strong>Hispanic-Anglo Only Sites</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>San Antonio</td>
<td>—</td>
<td>74/74</td>
<td>—</td>
<td>67/116</td>
</tr>
<tr>
<td>Pueblo</td>
<td>—</td>
<td>74/76</td>
<td>—</td>
<td>50/68</td>
</tr>
<tr>
<td>San Diego</td>
<td>—</td>
<td>69/74</td>
<td>—</td>
<td>61/76</td>
</tr>
<tr>
<td>Tucson</td>
<td>—</td>
<td>75/75</td>
<td>—</td>
<td>59/71</td>
</tr>
<tr>
<td><strong>All Sites</strong></td>
<td>1223/1218</td>
<td>782/801</td>
<td>709/972</td>
<td>692/940</td>
</tr>
</tbody>
</table>

Each entry contains two numbers. The first is the number of rental tests and the second is the number of sales tests.
This means that multivariate models like (3) must be extremely parsimonious in the variables they employ for controls because of the limited degrees of freedom. In combination, both limitations make it less likely that estimated regression parameters of (3) will pass conventional tests of statistical significance.

Measuring Fair Housing Enforcement Activities

We obtained data from HUD’s fair housing complaint database. This is the most comprehensive dataset available, as it records all fair housing cases filed directly with HUD as well as with private, local public, and state agencies that were funded through either HUD’s FHIP or FHAP initiatives. For each of the twenty-one sites shown in table 8.4, we extracted from the HUD database all cases filed during the 1990s that involved allegations by black or Hispanic home seekers of discrimination based on race, ethnicity, or color. We also recorded the disposition of the case in terms of findings and awards.

On average in the metropolitan areas where HDS conducted black-white tests, 1,655 race/ethnicity-based complaints were filed with HUD or its affiliated agencies during the decade; the comparable figure for the areas where Hispanic-Anglo tests were conducted (which often were black-white test sites as well) was 1,920. Of these totals, 121 and 163 (or 7 percent and 8 percent, respectively, of the number filed) yielded findings of discrimination. The awards associated with such findings averaged $5,732 and $8,910 for the HDS black-white test sites and Hispanic-Anglo test sites, respectively, though there were wide variations across metropolitan areas and the two sets have seven sites in common. As for the remainder of the cases, HUD and its affiliated agencies closed the bulk of cases with no finding of discrimination (see Schill, chapter 7, for more detail).

As explained above, the central challenge in devising and interpreting a fair housing enforcement variable is endogeneity. We would argue that, given available HUD data, the most likely specification to minimize this problem is a variable that measures the (natural logarithm of) dollar amount of court-ordered awards in each metropolitan area’s fair housing cases involving black or Hispanic plaintiffs. First, the dollar amount of awards is influenced by the effectiveness of the local fair housing agency in not only winning cases but doing so in a convincing fashion, which is not likely driven by the overall incidence of discrimination in the area. Second, awards will be exogenously influenced by the practices, precedents, and political leanings of the relevant state and federal court districts. We also would suggest that dollar amount of awards is, in principle, a better measure of deterrence than either numbers of cases filed or won because it comes closer to representing an expected value
of loss from discriminating. Moreover, large awards are more likely to get coverage in the local news media than a discrimination finding in a minor case, thus generating a more visible deterrence effect.

In this chapter we therefore employ as our prime explanatory variable the (natural logarithm of) cumulative dollar amount of court-ordered awards in all fair housing cases alleging discrimination on the basis of race, ethnicity, or color in each HDS metropolitan area during the 1990s. We also experiment with an analogous variable specified only for cases in which HUD data allow us to identify the race or ethnicity of the complainant and that complainant is black or Hispanic.9 Surprisingly, we were able to operationalize this experiment only for cases involving black complainants because the HUD database showed no cases in our HDS sites involving an identified Hispanic complainant where an award was made by the court. The mean award in cases where the complainant was black was $6,448 across our seventeen black-white test sites.

Clearly, the most desirable variable from a conceptual standpoint as a predictor of changes in housing discrimination (our dependent variable) would be one that measures the effectiveness of efforts to avoid illegal behaviors that otherwise would have occurred. We recognize that our measure fall short of this ideal in several ways. First, the range of fair housing activities we can measure are circumscribed; we have no information about other activities that may not have yielded cases, such as fair housing education training activities for housing providers. Second, we have no measure of the full set of resources invested in fair housing activities aimed at racial-ethnic discrimination. Even if we were to undertake successfully the daunting task of acquiring budgets of all the relevant agencies, we would still be pressed to measure the budgetary share directed at eradicating discrimination against blacks and Hispanics. Moreover, the budgets would miss the significant in-kind and pro bono resources that they often utilize. Third, this database cannot be used to precisely measure changes in the intensity or effectiveness of enforcement activities during the decade, as indicated in equation (3), inasmuch as the data show case numbers varying markedly from year to year. At best it provides indicators of cross-metropolitan variations in the levels of awards against housing discriminators during the period. Fourth, virtually all of the cases tallied alleged violations in the rental, not sales, market. Thus, we are essentially testing whether there were any substantial deterrent effects for real estate sales agents that may have emanated from suits primarily involving the rental sector. All these concerns militate against our observing a statistically significant negative correlation between our enforcement measure and changes in discrimination.

Before leaving the topic of enforcement activity, we should note that we also obtained data from unpublished sources about the fair housing enforce-
Fair Housing Enforcement and Changes in Discrimination

Measuring Control Variables

Finally, as controls in our regression model of changes in discrimination we employed several changes in demographic and economic characteristics of each metropolitan area that theory and prior research suggest are predictive. Specifically, we included changes in: black and Hispanic metropolitan population shares, income gaps between whites and minorities (black or Hispanic, as appropriate), and black or Hispanic (as appropriate) residential segregation.

These control variables are likely to be related to the intensity of white prejudices that may motivate discriminatory actions in the housing market (Yinger, 1995). The “out-group hostility” theory of prejudice stresses that white prejudice is an antipathy toward members of a minority group, rooted in negative affect and stereotypes. The “group position” theory involves a commitment to relative group position, conditioned by the difference between in-group (white) and out-group (minority) positions that in-group members have socially learned to expect (Blumer 1958; Bobo and Zubrinsky 1996). Following the out-group hostility vein, white prejudice may be abetted in areas where there is a stronger correlation between race and socioeconomic status, as a result of the synergism between race and class prejudices (Bobo, Kruegel, and Smith 1996). Following the group position vein, white prejudice should be intensified in areas where blacks are a more significant share of the population and therefore constitute more of a perceived threat to white neighborhoods, economic status, and political power (Bradburn et al. 1970; Marshall and Jiobu 1975; Giles 1977). This perceived threat to group position...
may also be greater in areas where the interracial gap in socioeconomic status is less. Residential segregation may also proxy for white’s desires to self-segregate and, by implication, for real estate agents to cater to this desire by discriminating.\footnote{11}

We acquired data for measuring the population and economic variables from the 1990 and the 2000 Census of Population and Housing (U.S. Department of Commerce 1993, 2002). We also obtained segregation index data from the Census Bureau (Iceland, Weinberg, and Steinmetz 2002).

Summary Remarks about the Challenges of Measuring the Effect of Fair Housing Enforcement Activity on Discrimination

The effort to both measure the appropriate concepts and then ferret out their causal connections is extremely challenging on several fronts. First is measurement error: both the dependent variable and the key explanatory variable are unavoidably measured with error. Second is small sample sizes: only a few metropolitan areas had their rates of discrimination comparably measured in the 1989 and 2000 Housing Discrimination Studies. The effect of these two challenges is that reaching conventional standards of statistical significance is difficult. Third is omitted variables: precise measures of the motives of housing discriminators, housing consumer preferences and prejudices, and market and institutional context are not available. Our specification of a change model helps in minimizing this potential bias but does not guarantee its elimination. Fourth—and most importantly—is endogenity: many potential measures of fair housing efforts may be influenced by the historical levels and intensity of discrimination, creating a reverse causation that obscures the measurement of the desired causal connection. We employ a measure of fair housing effectiveness (dollars awarded by the courts) that tries to avoid this difficulty to the extent possible, but our observed correlations between changes in discrimination and this variable may still be biased in a positive direction.

EMPIRICAL RESULTS

Does More Effective Enforcement Reduce Discrimination?

Our primary finding is that for metropolitan areas whose HUD offices and HUD-supported FHIP and FHAP agencies are more successful in winning larger cumulative monetary awards from their fair housing suits there has been a greater decrease in all sorts of discriminatory behaviors against black apartment and home seekers during the 1990s. The evidence points in this di-
rection in both sales and rental sectors, though is stronger in the rental market. This is consistent with the claim that in such places there was a stronger enforcement environment that deterred more prospective discriminators against blacks. The explanatory power of monetary awards is considerably weaker in the case of discrimination against Hispanics, which perhaps is explicable due to the smaller sample size and comparatively limited amount of award-generating enforcement efforts directed toward them.

Tables 8.5 and 8.6 present the estimated coefficients for the monetary awards enforcement variable (shown in columns) produced by a multiple regression explaining cross-metropolitan variations in 1989–2000 changes in each of the aforementioned discriminatory treatment indexes (shown in rows). Each regression controls for demographic and economic features noted above; regressions are estimated separately for seventeen black-white sites (columns 1 and 2) and eleven Hispanic-Anglo HDS sites (column 3). Estimates in column 2 differ from others inasmuch as they employ as the enforcement variable the logarithm of the cumulative dollar amount of awards in cases involving complainants who were identified as black.

In interpreting these estimates we compute the standard t-statistics, which are shown parenthetically in tables 8.5 and 8.6. As we expected due to the myriad reasons presented earlier, the majority of the estimated coefficients are not statistically significant, even though many are sizeable in magnitude. Therefore, we also conduct an overall test for estimated coefficients for all discriminatory behaviors in both rental and sales for a given black or Hispanic sample. This small sample test is conducted using a sign test. The test can be interpreted as whether there is a consistent pattern of

<table>
<thead>
<tr>
<th>Behavior</th>
<th>Black-White Tests ($ from race-ethnic cases)</th>
<th>Black-White Tests ($ from black cases)</th>
<th>Hispanic-Anglo Tests ($ from race-ethnic cases)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Availability</td>
<td>-0.016</td>
<td>-0.010</td>
<td>0.015</td>
</tr>
<tr>
<td></td>
<td>(1.93)*</td>
<td>(1.26)</td>
<td>(1.38)</td>
</tr>
<tr>
<td>Inspection</td>
<td>-0.006</td>
<td>-0.006</td>
<td>-0.002</td>
</tr>
<tr>
<td></td>
<td>(0.38)</td>
<td>(0.45)</td>
<td>(0.30)</td>
</tr>
<tr>
<td>Terms</td>
<td>0.011</td>
<td>-0.004</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(1.18)</td>
<td>(0.45)</td>
<td>(0.37)</td>
</tr>
<tr>
<td>Encouragement</td>
<td>-0.030</td>
<td>-0.029</td>
<td>-0.014</td>
</tr>
<tr>
<td></td>
<td>(2.38)*</td>
<td>(2.75)*</td>
<td>(0.75)</td>
</tr>
<tr>
<td>Overall</td>
<td>-0.011</td>
<td>-0.019</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(.090)</td>
<td>(2.07)*</td>
<td>(0.34)</td>
</tr>
</tbody>
</table>

Note: t-statistics shown in parentheses; * = p<.05; # = p<.10 (two-tailed tests)
positive or negative coefficient signs across the alternative discrimination measures.

Table 8.5 presents results for the rental market. For the apartment availability regression in the sample of sites where black-white HDS tests were conducted, the coefficient for the log of dollars awarded for all race/ethnic/color discrimination cases is -0.016, which is statistically significant at the 10 percent level (using a conservative, two-tailed test). In the case of the encouragement discriminatory behavior, the enforcement variable coefficient is -0.03, which is twice as large and more statistically significant.

When considering column 2 of table 8.5 showing results for when only awards in cases involving black complainants are measured, we see that the sizes and magnitudes of the coefficients closely track those in column 1, except that availability becomes insignificant and the overall measure becomes significant (with a magnitude of -0.019). No coefficients of the enforcement variable were statistically significant in the case of discrimination against Hispanic renters (see column 3, table 8.5).

To provide a sense of the magnitude of the implied causal relationships here, we compare the differences in changes in discrimination our estimates would predict between two metropolitan areas that were otherwise identical but one had during the 1990s the mean level of monetary awards and the other had one standard deviation above the mean value of awards. Compared to the metropolitan areas with mean level of awards, the one with higher awards would be expected to evince greater declines in discrimination against black
renters by: .075 (availability); .141 (encouragement); and .093 (overall). These are sizable predicted amounts indeed, given that the mean actual declines in these three indicators in the HDS black-white sites were .097, .083 and .071, respectively (cf. table 8.2).

Consider next the results for discrimination in the home sales sector, as shown in table 8.6. In the case of blacks, the only statistically significant coefficient for the log of awards occurs for the encouragement behavior. The magnitude and statistical significance of this result is not affected by whether all awards or just awards in cases involving black complainants are considered (cf. columns 1 and 2 in table 8.6). If we conduct the same thought experiment as above, the size of the coefficient here implies that the metropolitan area with one standard deviation higher level of awards would be expected to evince greater declines in encouragement discrimination against black home buyers by 0.061, compared to the metropolitan area with mean level of awards. The mean actual decline in this treatment variable was 0.078. In the case of Hispanics, all coefficients of the awards variable prove positive, one even statistically significantly so; we explore these Hispanic results more below.

Sign tests provide additional insights. When considering the awards enforcement variable coefficients across all eleven discriminatory behaviors against blacks measured in both rental and sales sectors (column 1 in tables 8.5 and 8.6), eight are negative, a result that barely misses statistical significance at the 10 percent level. The comparable test using only awards in cases with black complainants (column 2 in tables 8.5 and 8.6) reveals negative coefficients in all eleven measures, which is significant at the .0005 level. In the case of discrimination against Hispanics, only four of eleven measures evince negative coefficients for the enforcement variable, which is not different from what would be expected by chance.

Taken holistically in the context of the previous described econometric challenges, the evidence in tables 8.5 and 8.6 therefore strongly indicates that metropolitan areas with fair housing efforts yielding larger cumulative monetary damage awards from the court were associated with substantially greater declines in a wide variety of discriminatory behaviors against black renters and, to a somewhat less degree, black home buyers. There were no comparable relationships revealed in the case of discrimination against Hispanics. These intergroup differences in results might be explained as follows. Recall that cases alleging discrimination against Hispanics were few (and monetary awards nonexistent) compared to those alleging discrimination against blacks. With a greater baseline volume of cases, an increase in monetary awards may substantially increase the deterrent effect, but such an increase given a low baseline may have little deterrence effect because it does not change the expected cost of discrimination much.
What Other Factors May Have Influenced the Change in Discrimination?

As it is not the focus of this chapter, we note only briefly the salient findings regarding relationships between control variables and changes in discrimination in our multivariate models. Generalizations are difficult, inasmuch as results often varied depending on which behavior, tenure, and racial-ethnic group was under consideration. We can say with some confidence that metro areas experiencing larger increases in their minority populations evinced smaller declines in discrimination against black renters. Specifically, growth of the Hispanic population’s share was associated with smaller declines in discrimination on rental availability and terms, and growth of the black population’s share was associated with smaller declines on rental encouragement indicators. Similarly, for discrimination against blacks in the home sales sector, growth in the share of Hispanic population was associated with smaller declines in discrimination on availability, inspection, and encouragement. We could identify no systematically robust relationships for the control variables in equations modeling changes in discrimination against Hispanics in either rental or sales sectors.

DISCUSSION, CONCLUSION, AND DIRECTIONS FOR FURTHER RESEARCH

In this chapter we have attempted for the first time to ascertain if there are statistically detectable relationships between a direct measure of fair housing enforcement effectiveness and corresponding reductions in several directly measured incidences of racial-ethnic discrimination in a metropolitan area’s housing market. We used paired-testing data from the 1989 and 2000 national Housing Discrimination Studies and archival data from HUD and HUD-supported fair housing enforcement agencies to operationalize our measures.

This effort proved challenging on several fronts. First is measurement error: both the dependent variables and the key explanatory variable are unavoidably measured with error. Second is small sample sizes: only a few metropolitan areas (seventeen for black-white tests; eleven for Hispanic-Anglo sites) had their rates of discrimination comparably measured in the 1989 and 2000 Housing Discrimination Studies. The effect of these two challenges is that reaching conventional standards of statistical significance is difficult. Third, there is the problem of bias due to reverse causality. If the persistence of discrimination in an area is correlated with its prior level, then it is likely
that higher fair housing efforts will be observed where discrimination declines the least. Fourth, variables likely needed as controls in our regressions cannot be measured, creating omitted variables bias. These latter two biases militate against observing a statistically significant inverse relationship between enforcement efforts and corresponding declines in discrimination. There was no feasible way to avoid the first two shortcomings. We tried to avoid the latter two biases, however, by specifying a change in discrimination model that employs monetary awards in discrimination cases as the enforcement variable.

We have found that higher amounts of monetary awards secured during the 1990s in cases brought with the help of HUD and state and local enforcement agencies supported by HUD through its FHIP and FHAP programs are consistently associated with greater declines in discrimination against black apartment seekers and home seekers. The magnitude of our estimates suggests that enforcement effectiveness contributed substantially to the decline in discrimination against black apartment and home seekers observed during the decade. This evidence is consistent with the hypothesis that if these agencies are more successful in raising the expected financial penalties from discriminators in a metropolitan area, the industry will respond by lowering the incidence of such acts. It appears that enforcement in the rental sector may also spill over to create deterrence in the home sales sector, even though few legal cases are brought against sales agents.

We recognize that we cannot be absolutely sure that enhanced deterrence is at work here. The evidence also supports the hypothesis that state and local fair housing agencies that are more effective in securing large monetary awards from the court may also be more effective in, for example, fair housing training of industry operatives and public fair housing education and outreach efforts.

Of course, the general effectiveness of fair housing agencies (for enforcement, education, or agent training) will be strengthened if they receive a more stable base of funding, such as through the FHIP-FHAP federal mechanism. We therefore believe that it is important to note that the federal support for state and local enforcement activities may be an important factor in decreasing discrimination, even if our results are driven by local agency effectiveness in education and agent training, instead of deterrence. While federal dollars tend to be dedicated primarily to support investigation of specific complaints, the reliable funding stream created by the FHIP and FHAP programs may contribute substantially to overall agency effectiveness by increasing the size, professionalism, and stability of agency staff.

We were unable to find any statistical evidence that would support a similar set of conclusions in the case of discrimination against Hispanics. Part of this
may be due to the smaller sample of metropolitan areas investigated. But part may also be due to the fact that there appear to be comparatively modest efforts to bring fair housing cases involving Hispanic complainants. In our seven sites where both black-white and Hispanic-Anglo tests were conducted because of their substantial black and Hispanic populations, there were 6,198 cases brought by black complainants and only 1,653 brought by Hispanic complainants, a ratio of nearly 4:1. This fact should be coupled with the observation that there were fewer indicators of discriminatory treatment showing declines from 1989 to 2000 for Hispanic renters than for black renters. These facts suggest that a lack of deterrence and/or fair housing agency efforts aimed at educating Hispanics of their fair housing rights may have been important here.

Future research in this field should address itself to the aforementioned empirical challenges. If one were to continue in the vein of multivariate statistical modeling as here, expanding the sample of metropolitan areas where changes in discrimination are consistently measured should be a high priority. If one concedes that the likelihood of an expanded (or even any) future HDS seems small, efforts should be directed into alternative means of assessing the intensity and prevalence of discrimination across many metropolitan areas. One notion potentially worth reconsidering was introduced a quarter-century ago by Yinger (1979) and Galster (1977, 1981): discrimination should affect the relative prices and rents minorities pay relative to whites in similar neighborhoods. Other investigative approaches that may have merit may involve surveys of white households and housing agents regarding fair housing issues and their racial-ethnic preferences and prejudices.

In closing, we would conclude that more effective enforcement of fair housing laws does have a measurable impact. Indeed, we therefore conclude that a nontrivial part of the observed general reduction in housing market discrimination against blacks 1989–2000 may be attributed to such enhancements. Given the reduction of HUD monetary support for state and local fair housing agencies since the advent of the Bush administration, one is left to wonder whether these favorable trends are continuing into the twenty-first century.

TECHNICAL APPENDIX

The HDS Paired-testing Sample Design

The sample for HDS paired tests consisted of housing units advertised in major metropolitan newspapers, selected randomly each weekend. The study design assured that both tester teammates were equally qualified for the advertised housing unit. Teammates were matched according to gender and age and
were assigned similar incomes, occupations, family profiles, and other socio-
omic characteristics. Teammates were trained to behave similarly during
the test; neither expressed preferences for certain types of neighborhood. The
teammates were sent, in random order over a short period, to visit the real es-
tate agency placing the sampled advertisement and initiated contact by ask-
ing to see the advertised home and others similar to it. They often made sub-
sequent phone and in-person contacts with agents, including going on home
inspections. After each contact the testers independently filled out common
report forms, which recorded the treatment afforded them, locations of the
houses discussed or visited, information provided.

**Measuring Differences in the Treatment of Testers**

From test report forms we constructed various measures of differences in
treatment, established to ensure comparability for both 1989 and 2000. A paired
test can result in any one of three basic outcomes for each measure of treat-
ment: (1) the white tester is favored over the minority; (2) the minority tester
is favored over the white; or (3) both testers receive the same treatment. The
simplest measure of adverse treatment is the share of all tests in which the
white tester is favored over the minority. This *gross incidence* approach pro-
vides very simple and understandable indicators of how often white testers
are treated more favorably than their equally qualified minority teammates.

Although *gross measures* of white-favored treatment are straightforward and
easy to understand, they may overstate the frequency of what ideally we wish
to measure: systematic discrimination. We use the term “systematic discrimi-
nation” to mean differences in treatment that are attributable to a customer’s
race or ethnicity, rather than to any other differences in tester characteristics or
test circumstances. This term is not the same as “intentional” discrimination,
nor is it intended to mean that these differences would necessarily be ruled as
violations of federal fair housing law. Specifically, adverse treatment may oc-
cur during a test not only because of differences in race or ethnicity, but also be-
cause of random differences between the circumstances of their visits to the real
estate agency. For example, in the time between the two testers’ visits, an apart-
ment might have been rented, the agent may have been distracted by personal
matters and forgotten about an available unit, or one member of a tester pair
might meet with an agent who is unaware of some available units. See Yinger
(1986), Heckman and Siegelman (1993), Fix, Galster, and Struyk (1993), Heck-
man (1998), Foster et al. (2002), and Ross (2002) on the methodological issues
related to the use of paired testing to measure discrimination.

One strategy for estimating systematic discrimination is to remove the
cases where nondiscriminatory random events are responsible for differences
in treatment by subtracting the incidence of minority-favored treatment from the incidence of white-favored treatment (gross measure) to produce a net measure. This approach essentially assumes that all cases of minority-favored treatment are attributable to random factors—that systematic discrimination never favors minorities—and that random white-favored treatment occurs just as frequently as random minority-favored treatment. Based on these assumptions, the net measure subtracts differences due to random factors from the total incidence of white-favored treatment. It is important to note that even when no statistical pattern of race-based differential treatment is observed, individual cases of discrimination may occur.

However, it seems unlikely that all minority-favored treatment is the result of random factors. For example, a minority landlord might prefer to rent to families of his or her own race or a real estate agent might think that minority customers need extra assistance. Other instances of minority-favored treatment might reflect a form of race-based steering, in which white customers are discouraged from considering units in predominantly minority neighborhoods or apartment complexes.

While net measures used in HDS may understate the frequency of systematic discrimination, we have nevertheless employed this measure in order to provide a lower-bound on the level of discrimination. Turner et al. (2002) found that the net and gross measures are fairly robust to controlling for differences in the circumstances faced by testers, differences between the white and minority testers’ real-life characteristics, and situations where real estate agents might systematically favor minorities. Ondrich, Ross, and Yinger (2000) use a similar approach to estimate the upper and lower bounds for discrimination and find that the net and gross are typically close to those bounds.

NOTES

The ideas in this chapter do not necessarily represent the views of the U.S. Department of Housing and Urban Development, or any other agency of the federal government. We wish to express gratitude to Fred Freiberg, Todd Richardson, and Cliff Schrupp for their invaluable assistance in obtaining fair housing enforcement data from various sources. Sarah Pratt provided helpful technical assistance regarding the HUD TEAPOTS database. Jackie Cutsinger and Phyllis Seals at Wayne State University and Jason Cutsinger at Compuware Inc. supplied able research and technical assistance.

1. For a wide variety of consequences, as measured through multivariate modeling, see: Galster 1987a, 1991; Galster and Keeney 1988; Massey, Condran, and Denton 1987; Massey and Eggers 1990; Massey and Gross 1991; Cutler and Glaeser 1997; Ellen 2000).
2. The key exceptions to this general decline are discrimination in access to rental housing against Hispanics, racial steering of African Americans, and less assistance in obtaining financing provided to Hispanics.


5. This consistency composite may incorrectly classify tests as neutral, however, when one tester received favorable treatment on more indicators than his or her partner, or when one tester was favored on the most important indicator. Hierarchical composites were constructed by ranking the relative importance of individual treatment measures. The qualitative results are the same using either composite index (Turner et al. 2002).

6. The HUD database, known by its acronym TEAPOTS, records most of the cases noted in the NFHA database. Because of often substantial lags between case filings and ultimate disposition, we included cases that may have been filed as early as 1987 and finally settled as late as 2003.

7. Black complainants filed roughly four times as many cases as Hispanics, though their likelihood of receiving a finding of discrimination was only slightly higher (one to two percentage points).

8. Data were not available about what sorts of dollars might have been involved as part of conciliation agreements.

9. Unfortunately, most of the cases in the HUD fair housing database do not provide this information. We also experimented with a variable based on the number of fair housing cases found in favor of the plaintiff but found this to be a much weaker predictor than awards.

10. We thank Fred Freiberg, the former supervisory equal opportunity specialist for the DOJ during the 1990s, for supplying this information. Cliff Schrupp, executive director of the Fair Housing Center of Metropolitan Detroit, generously shared the accumulated NFHA records. Note that most of the NFHA-affiliated activity is tabulated in the HUD fair housing database, though not the DOJ activity.

11. We recognize that segregation, interracial economic disparities, and housing discrimination may themselves be causally related. To remove these intercorrelations we employ instrumental variables in place of segregation and interracial economic disparities. The identifying exogenous predictors for the former are: the percentages of black and Hispanic populations and the white-black differences in the percentages of respective populations aged forty to fifty-nine years, and for the latter are: white-black differences in the percentage of those aged twenty-five and older who are high school graduates and those who are college graduates.

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