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Fair Housing Enforcement and Changes in Discrimination between 1989 and 2000: An Exploratory Study

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Abstract

Using paired testing data from the 1989 and 2000 Housing Discrimination Studies (HDS) and data on fair housing enforcement activities during the 1990s in the corresponding metro areas, we investigate whether 1989-2000 changes in the metropolitan incidence of racial/ethnic discrimination correlate with fair housing enforcement activity during the 1990s. We found that higher amounts of state and local enforcement activity supported by HUD through its FHIP and FHAP programs (especially the amount of dollars awarded by the courts) were consistently associated with greater declines in discrimination against black apartment-seekers and home-seekers. The evidence does not support similar conclusions for housing market discrimination against Hispanics where the level of enforcement is much lower.

Journal of Economic Literature Classification: J15, K42, L85, R30

Keywords: Housing Discrimination, Fair Housing Enforcement, and Paired Testing

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I. Introduction

Though 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 (Turner et al., 2002). Such discrimination can impose substantial costs on those who are directly victimized (Yinger, 1995). Moreover, housing discrimination perpetuates 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]

Fortunately, housing discrimination has declined substantially in magnitude over the last decade on most measures (Turner et al., 2002; Ross and Turner, 2005).[2] This decline follows on the heels of a substantial strengthening of federal fair housing law in 1988 and enforcement capacity at the federal, state, and local levels during the Clinton administration. This coincidence raises the provocative issue of how these two occurrences may be related. We therefore investigate variation across metropolitan areas in their changing levels of discrimination, with the aim of exploring whether they are related to corresponding variations in fair housing enforcement activities during the 1990s.

More specifically, our research questions are:

- How do 1989-2000 changes in the metropolitan incidence of racial/ethnic discrimination correlate with fair housing enforcement activity during the 1990s?
- To what extent does the answer vary depending on whether one considers: (1) sales or rental housing markets, (2) discrimination directed against black or Hispanic home seekers, or (3) enforcement activities of different fair housing agencies?

Our analysis is based on a nationally representative sample of 17 metropolitan areas where black-white paired tests were conducted and 11 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 differential treatment of rental apartment seekers, plus summary indices of such behaviors in each sector. We operationalize a variety of fair housing enforcement variables involving legal cases brought by the U.S. Department of Justice (DOJ), private fair housing organizations, and state and local fair housing agencies. To our knowledge, our work is the first to explore statistically the relationships between various kinds of fair housing enforcement activities and changes in the incidence of housing discrimination, which are consistently measured across time and metropolitan areas.

The remainder of the paper is organized as follows. In section II, we highlight key changes in fair housing law and enforcement capacity that ensued at the beginning of our study period. We review in section III. the three prior studies that have

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[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.

investigated the cross-metropolitan variability of housing discrimination. Section IV. describes in more detail the aforementioned Housing Discrimination Studies and discusses the measures of discrimination that we derive from them. The fair housing enforcement variables are operationalized here as well. We discuss the multiple statistical challenges this study faces: small sample sizes, measurement errors, and bias from reverse causality and selection. Descriptive statistics portraying changes in our battery of discrimination measures and then their correlations with fair housing activities are presented in Section V. We conclude by discussing results that are suggestive of deterrence effects and draw policy implications about fair housing enforcement efforts.

II. Key Changes in Fair Housing Law and Enforcement

Several legislative and administrative changes in the late 1980's and early 1990's strengthened federal fair housing law and the capacity of federal, state, and local agencies to enforce it more effectively. Of primary importance, the Fair Housing Amendments Act of 1988 created an administrative adjudication process for more timely resolution of housing discrimination complaints and allowed for stiffer civil penalties (Schill and Friedman 1999; Mathias and Morris 1999). The 1988 act also dramatically 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, which 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. Administratively, the Department of Justice (DOJ) under President Clinton created its own enforcement testing division, and the number of DOJ-initiated fair housing cases annually increased from less than 20 prior to 1988 to well over 100 during the mid-1990's (Lee 1999).

The foregoing should not be interpreted as a suggestion that the nation's efforts to combat discrimination have been efficient or sufficient. On the contrary, HUD's implementation of the act may be criticized (Schill and Friedman 1999) and the adequacy of enforcement capacity nationwide questioned (Yinger, 1995). More fundamentally, Galster (1990, 1999) has argued that these legislative efforts above cannot create an effective deterrent against housing discrimination so long as enforcement relies on *bona fide* home seekers recognizing that they have been victimized and then filing suit. Rather, here we are suggesting that it is plausible that fair housing law and enforcement capacity were indeed strengthened beginning in 1988 and that such reduced discrimination to some degree over the last decade. Of course, the implementation of these changes did not occur consistently across the nation, allowing room for variation that we exploit in our analysis strategy.

III. Previous Research on Cross-Metropolitan Variations in 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

[3] Several theoretical and empirical treatises have probed the motives for housing discrimination: Galster (1987b), Yinger (1995), Ondrich, Ross and Yinger (2000, 2003).

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. Galster and Keeney (1988) investigated the variations in housing discrimination against blacks across 40 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 conduced there. They employed this measure (instrumented) as an endogenous variable in a four-simultaneous-equation 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) and 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 the 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 25 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]

Of course, none of these studies has attempted to relate cross-sectional variations in housing discrimination to variations in fair housing enforcement activities. Nevertheless, the research reported here draws on the above work by employing as controls several variables measuring metropolitan demographic and economic conditions that have proven predictive.

IV. Data and Measurement Issues

Housing Discrimination

Features of HDS. Measures of discrimination were obtained 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 HDS were conducted using consistent sampling and paired testing protocols in multiple metropolitan areas for both years: 17 for black-white tests and 11 for Hispanic-Anglo tests. Metropolitan areas were chosen to provide a nationally representative sample of housing markets where black and Hispanic home seekers

[4] See Page (1995) footnote 7 for details.

constituted a substantial fraction; the specific areas sampled and the number of rental and sales tests in each are presented in Table 1.

[insert Table 1 about here]

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 socioeconomic 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 estate agency placing the sampled advertisement and initiated contact by asking to see the advertised home and others similar to it. They often made subsequent 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, etc.

Measuring Differences in Treatment of Testers. From test report forms we constructed various measures of differences in treatment, coded comparably for both 1989 and 2000. A paired test can result in any one of three basic outcomes for each measure of treatment: 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 provides 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.[5] Specifically, adverse treatment may occur during a test not only because of differences in race or ethnicity, but also because 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 apartment 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.[6]

One strategy for estimating systematic discrimination is to remove the cases where non-discriminatory 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

[5] We use the term "systematic discrimination" 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.

[6] See Yinger (1986), Heckman and Siegelman (1993), Fix, Galster and Struyk (1993), Heckman (1998), Foster et al. (2002), and Ross (2002) on the methodological issues related to the use of paired testing to measure discrimination.

these assumptions, the net measure subtracts differences due to random factors from the total incidence of white-favored treatment.[7]

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. As a result, net measures may understate the frequency of systematic discrimination. Nevertheless, we employ the net measures in order to provide a lower-bound on the level of discrimination in the market.[8]

Measuring Different Discriminatory Behaviors. A visit with a rental or sales agent is a complex transaction and may include many forms of favorable or unfavorable treatment. This paper presents results for a series of individual treatment indicators that reflect important aspects of the housing transaction, see Table 2. The first set of indicators capture "housing availability," with the first indicator being recorded as favorable when a tester is told that the unit advertised in the paper is available, the second being recorded as favorable when at least one unit is available with the same number of bedrooms and similar price to the advertised unit, and the last availability indicator based on comparing the number of units that the white and minority testers were told about. The "inspection" indicators follow a similar structure based on each tester being allowed to inspect the advertised unit, inspect similar units, and the number of units inspected by one tester relative to another. The "encouragement" indicators, listed at the bottom of table 2, include whether a tester was contacted either by mail or phone after the visit, whether the agent took the time to make arrangements for future contact, whether the agent made positive comments concerning the tester's qualification to purchase or rent, and for the rental tests whether the tester was invited to fill out a rental application.

Additional indicators are presented that are specific to either the rental or sales market. In the rental market, a series of indicators are developed to describe the terms and conditions for which the advertised unit is offered for rent. These indicators include comparisons of the monthly rent and security deposit quoted to the two testers, whether a tester was offered incentives to rent the apartment, and whether a tester was required to pay an application fee. In the sales market, additional indicators are developed to measure treatment in terms of the geographic steering and financing assistance provided. The geographic steering measures are based on a relative comparison of the average racial or ethnic composition of the units available to or inspected by each tester.[9] The financing assistance indicators include whether the agent explicitly offered a

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[7] It is important to note that even when no statistical pattern of race-based differential treatment is observed, individual cases of discrimination may occur.

[8] 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.

[9] Percent African American and Hispanic in a census tract are used to describe the racial and ethnic composition for Black-White and Hispanic-Anglo tests, respectively. Equal treatment arises when the two testers' units have an average composition within five percentage points of each other.

tester help with the process of obtaining a mortgage, whether the agent provided a tester with a list of recommended lenders, and whether the agent discussed the downpayment necessary to purchase the advertised or similarly priced units.[10]

[Insert Table 2 Here]

This paper combines the treatment indicators within each category to create a *composite or consistency measure*, such as "housing availability" or "terms and conditions," which we employ as dependent variables in our analyses. Specifically, tests are classified as white-favored if the white tester was consistently favored, i.e. 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.[11] 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.

The statistical significance levels of the net measure and changes in all net measures of discriminatory treatment are examined using the standard t-statistic.[12] Due to the complex sample and the historical evidence of heterogeneity across sites, standard errors are generated using a Monte Carlo simulation approach that repeatedly samples from the underlying sample in order to describe the distribution of estimates.[13]

Fair Housing Enforcement Activities

We obtained data from various sources about the fair housing enforcement activities of three types of agencies. Unfortunately, the kinds of information available from the various sources were often inconsistent, so strictly comparable variables could not be constructed. Instead, we employed an opportunistic approach, utilizing whatever information was available to specify appropriate variables.

The first type of activity was the "pattern and practice" enforcement testing undertaken by the U.S. Department of Justice, as part of their enhanced efforts beginning in the early 1990s (Lee, 1999). This activity was often conducted in conjunction with selected local fair housing groups, and targeted large rental complexes for paired testing by the DOJ that had previously been the source of fair housing

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[10] Note that these are not exactly the same treatment measures reported in the 1989 HDS report. Real estate markets changed dramatically between 1989 and 2000, and changes in testing protocols were required to maintain the integrity of the process. Regardless, treatment variables in this paper use a common definition between 1989 and 2000. See Turner et. al. (2002) for details.

[11] 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).

[12] Due to the large sample size, the distribution of the estimates should be approximately normal. In fact, non-parametric statistics, such as McNemar's test, yield nearly identical results.

[13] This simulation approach is needed because the number of tests in each site in any year is unlikely to match that site's importance in the estimate, and the level of adverse treatment in a site is likely to persist between the two decades, creating a correlation between the 1989 and 2000 samples.

complaints. The goal was to conduct multiple tests of a single, significant apartment supplier so that a potential pattern and practice of discrimination might be observed. Upon evidence of same, DOJ filed suit and in the overwhelming number of cases the court mandated a well-publicized consent decree involving substantial civil and other penalties.[14] We tabulated unpublished information on the number of race-based, DOJ pattern and practice suits and court-awarded penalties associated with such for the 1992-1999 period in all our HDS sites.[15] Unfortunately for the purposes of statistical analysis, these DOJ data are not ideal because of their lumpy, skewed distribution. Under the race-based component of this initiative, the DOJ filed suits involving only black renter plaintiffs and in only four of our metropolitan areas; in New Orleans and Chicago it filed one case, in Los Angeles-Long Beach it filed four cases, and in Detroit it filed eight cases. Given this distribution, we were able to specify only a dummy variable indicating whether a metropolitan area had a DOJ suit filed during the 1990s or not. Descriptive statistics for this variable are presented in Table 3.

The second source of fair housing enforcement data was provided through the National Fair Housing Alliance (NFHA), an association of over 100 private, non-profit fair housing agencies.[16] They have recorded complaints by *bona fide* home seekers that have been filed with the assistance of their affiliates during the 1990-1999 period, taken to court based on probable cause, and the court-ordered awards and actual recoveries to the plaintiffs (if any) resulting from such suits.[17] We tabulated all cases of racial/ethnic discrimination that found in favor of the plaintiff (or settled with monetary recovery for plaintiff) and the dollars recovered by the plaintiff.[18] Unlike the DOJ cases, cases filed with assistance from NFHA affiliates cover a much broader swath of our sample metropolitan areas (14 of the 21 sites listed in Table 1), and are more numerous (mean of 17 for sites with cases during the 1990s). We specified a dummy variable indicating whether either a NFHA-affiliated or a DOJ case had been filed in the metropolitan area during the 1990s, which is virtually equivalent to flagging whether a private, non-profit fair housing organization was active there; see Table 3 for descriptive statistics.[19]

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[14] In only three of the 75 DOJ pattern and practice cases nationwide during the period was there no finding of defendant liability.

[15] We thank Fred Freiberg, the former Supervisory Equal Opportunity Specialist for the DOJ during the 1990s, for supplying this information. The suits analyzed involve those brought by the Housing and Civil Rights Enforcement Section of the DOJ's Civil Rights Division. The data do not consider cases that may have had testing evidence generated exclusively from private fair housing organizations that may have been brought by DOJ.

[16] Not all affiliates actively participated in litigation and reported the same to NFHA; the active reporters grew from 51 during the 1990-1994 period to 89 in 2002 (National Fair Housing Alliance, 2003).

[17] Cliff Schrupp, Executive Director of the Fair Housing Center of Metropolitan Detroit, generously shared these accumulated records. Often the NFHA affiliate investigated the allegation with testers that mimicked the situation forming the basis of complaint. It has been estimated that approximately half of these cases were brought with support of the FHIP (National Fair Housing Alliance, 2003). The report excludes lawsuits that may have been investigated by an affiliate but were then referred to government agencies by those agencies, unless the complainant was represented by private counsel who represented the separate interests of the private complainant.

[18] These dollars recovered represent an underestimate because several cases contained nondisclosure clauses for the terms of the settlement.

[19] The only metro area where a DOJ case was filed without a NFHA-affiliated case filed was New Orleans. Experiments using the number of NFHA-affiliated cases during the 1990s proved fruitless. This is likely due to the lumpy character of the distribution of cases. Five observations constituted the vast bulk of all cases filed: Chicago (69), Detroit (59), New York (40), Los Angeles

Though more frequent, the awards typically were smaller in successful NFHA-affiliate-assisted cases ($105,542 average recovery) than in DOJ-filed cases ($200,399 average), which is to be expected given the pattern-and-practice nature of the latter.[20]

The third source of fair housing activity information came from HUD's fair housing complaint database. This the most comprehensive dataset available, for it records all fair housing cases filed directly with HUD and with private, local public, and state agencies that were funded through HUD's FHIP and FHAP initiatives.[21] For each of the 21 sites shown in Table 1, we extracted from the HUD dataset all cases filed during the 1990s that involved allegations by black or Hispanic home seekers of discrimination based on race, ethnicity, or color, and the disposition of the case in terms of findings and awards. Details of the resulting data are presented in Table 3. 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 was 1,920. Of these totals, 121 and 163 (7 percent and 8 percent, respectively, of the number filed) yielded findings of discrimination. Given that both total cases filed and those judged discriminatory represented reasonably continuous, well-behaved distributions in our sample we specified their numbers as explanatory variables. The awards associated with such findings averaged much less than in DOJ or NFHA-assisted cases, however: only $813 and $695 for HDS black-white test sites and Hispanic-Anglo test sites, respectively.[22] Because the HUD database contained information on the racial/ethnic designation of the complainant, we were also able to experiment with variables comparable to those above but differentiated by complainant type; see Table 3.

[Insert Table 3 about here]

The challenge in utilizing the aforementioned fair housing case data sources is developing meaningful indicators of *enforcement effectiveness*. 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 deter illegal behaviors that otherwise would have occurred. Unfortunately, the extant information on complaint filings is not an unsullied measure of effectiveness. It is certainly the case that, for any *given* ambient level of discrimination in the market, a larger number of complaints would be indicative of greater enforcement efforts from the various fair housing agencies and/or awareness on the part of home seekers of their legal rights and remedies to suspected discriminatory treatment. In this case, higher case filings (and/or higher damage wards) would indeed be a reasonable indictor of greater deterrence as likely experienced by prospective discriminators. In these circumstances we would expect a negative coefficient for our enforcement variable: metropolitan areas creating a more efficacious deterrent would evince larger declines (or smaller increases) in discrimination during the 1990s. However, the practical difficulty here is in controlling for the ambient level of discrimination. Without such, a higher

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(25) and Miami (16). The remaining nine sites had only 33 cases total, ranging from one to seven.

[20] The figures are not strictly comparable, because the DOJ cases involved only civil and other penalties whereas the NFHA cases tallied the totals recovered by plaintiffs, including attorney's fees.

[21] 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.

[22] Eight sites have zero dollars, nine have between $1-9 thousand, two have between $10 and 16 thousand, one has $37 thousand, and one has $60 thousand for average awards (though the last is associated with only one case).

observed number of case filings may merely be indicative of a higher incidence of discriminatory actions in that metropolitan area, a certain percentage of which are likely to generate complaints whatever the state of fair housing enforcement.[23] In these circumstances we would have no clear expectation of coefficient sign for our enforcement variable, unless *changes* in discrimination during the 1990s were systematically related to the average *level* of discrimination during the period.[24]

In principle, our dummy variables denoting whether a metropolitan area had a DOJ case filed and whether there was a NFHA-affiliated private fair housing group present might avoid this problem of muddled causality. Of course, such would still require the unrealistic assumption that all DOJ cases created the same deterrent effects and that all NFHA-affiliated groups were equally efficacious in building deterrence. In practice, even with such strong assumptions the problem remains because of selection biases. The DOJ did not conduct their pattern and practice investigations randomly, but rather concentrated their efforts in metropolitan areas that reputedly had the worst problems of discrimination. Similarly, NFHA-affiliated groups are not randomly located, but arguably have been developed in areas where their need has been most acute. To the extent that either of these selections is present, we might expect the coefficient signs of our dummy variables denoting DOJ and/or NFHA-affiliated cases filed to be positively biased. That is, metropolitan areas with such cases may be those with the most virulent, implacable discrimination problems, whereupon they would evince the least declines (or greatest increases) in discrimination during the decade, despite enforcement efforts to the contrary.

Analogous arguments can be made in the case of our enforcement variables measuring award amounts. On the one hand, higher awards might signal to prospective discriminators a lower expected net monetary benefit from discriminating. On the other hand, areas with the most egregious forms of discrimination may elicit larger damage awards from the courts. Thus, as before, results for our enforcement variables cannot be interpreted unambiguously because of unclear directions of causation.

We make two observations regarding this concern over possible bias arising from reverse causality and selection. First, by modeling *changes* in the level of discrimination from 1989 to 2000 we reduce the import of this concern compared to if we had modeled the level of discrimination at some point. In a change model, bias only arises if there is a correlation between the persistence of discrimination (after controlling for its level) and enforcement activities. For example, were discrimination to prove more persistent in places with higher observed levels of discrimination in 1989, and those persistently high levels were to lead to more more fair housing agency efforts, fair housing complaints, and findings of guilt once cases were adjudicated, we would observe a negative correlation between declines in discrimination 1989-2000 and our measure of enforcement efforts during the period. The second observation is that these potential

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[23] One might question why we do not use our 1989 HDS data as a measure of the ambient level of discrimination and construct an enforcement effectiveness variable consisting of the number of cases standardized for this level. Unfortunately, this would introduce on the right-hand side of our regression equation the same term as appears on the left-hand side. As a result, any statistical significance of enforcement effectiveness, our key explanatory variable of interest, might merely be the result of tautological statistical artifact. Unfortunately, we have been unable to devise any instrumental variable that would help us overcome this problem.

[24] This raises an additional challenge here: timing. The DOJ cases occur rarely and irregularly; the NFHA database does not contain information on dates of cases. No fair housing databases contain information prior to 1989. As a result, we cannot explore issues related to timing of enforcement efforts and resultant (lagged) changes in discrimination or measure changes in enforcement activity during the 1990s.

biases will work against observing an enforcement effect. Thus, any result that is consistent with an enforcement effect (i.e., negative correlations between the enforcement variables and changes in discrimination) should be treated with confidence.

A final limitation of our enforcement measures must be noted. Virtually all of the cases tallied in all three fair housing databases alleged violations in the rental, not sales, market. Thus, when we examine changes in sales market discrimination we are essentially exploring whether there were any substantial deterrent effects for real estate sales agents that may have emanated from suits primarily involving the rental sector.

Other Data

Finally, as controls in our regression analyses we employed a series of changes in demographic and economic characteristics of each metropolitan area. These data were acquired from the 1990 and the 2000 *Censuses of Population and Housing* (U.S. Department of Commerce, 1993, 2002). In particular, controls include changes in black and Hispanic metropolitan population shares and instrumental variables for changes in minority-white income gaps and changes in black or Hispanic residential segregation.

V. Empirical Results

Changes in Discrimination against Black and Hispanic Home Seekers

Tables 4 and 5 present nationally weighted estimates and standard errors (in parentheses) of the incidence of net adverse treatment (our lower-bound measure of discrimination) for 1989, 2000, and the change between those two HDS measurements. The figures give the difference in the proportion of cases where the white tester was favored and the proportion of cases where the minority tester was favored.

In the rental market, discrimination persists in 2000 against apartment seekers in both groups in the areas of availability and inspection; see Table 4. This incidence has declined considerably for blacks since 1989, but not for Hispanics. Discrimination in the area of encouragement has declined for both groups. For further analysis of these changes, see Ross and Turner (2005).

[insert table 4 about here]

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 5. 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).

[insert table 5 about here]

Relationship with Enforcement Variables

Tables 6 through 9 present the estimated coefficients for the enforcement variables (shown in columns) produced by a multiple regression explaining cross-metropolitan variations in 1989-2000 changes in each of the discriminatory treatment indices (shown in rows), controlling for demographic and economic features noted above. These coefficients should be interpreted a partial correlations conditional on other factors, because the implicit causation is open to interpretation. Moreover, it should be recalled that these coefficients are based on a small sample of observations: 17 black-white and 11 Hispanic-Anglo HDS sites. Accordingly, the majority of the

estimates are not statistically significant, even though many are sizeable in magnitude. Therefore, in addition to the standard t-statistics (presented parenthetically below the estimated coefficient) an overall test is conducted for all estimates in a sample for a given set of enforcement variables to ascertain whether the coefficients as a group systematically take on a positive or negative sign. This small sample test is conducted using a sign test; the row labeled such contains the differences between the number of positive and negative estimated coefficients, and the number in brackets contains the statistical significance level. The test can be interpreted as whether there is a consistent patterns of positive or negative coefficient signs across the alternative discrimination measures.

Table 6 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 of DOJ case is 0.141 and for the log of dollars awarded from both DOJ and NFHA-affiliated cases the coefficient is 0.016; see top panel of Table 6. The significance levels of these two estimates are 10 percent and one percent, respectively (two-tailed tests). These findings can be interpreted as follows. Metropolitan areas with a DOJ case in the 1990s evinced a 14 percentage-point greater increase (or smaller decrease) in rental availability discrimination against blacks during the decade, compared to areas with no such cases. A metropolitan area with a ten percent higher level of dollar awards from fair housing cases evinced a 16 percentage-point greater increase (or smaller decrease) in rental availability discrimination against blacks during the decade, compared to an otherwise-comparable area. In addition, the sign test indicates a consistent pattern of positive coefficients across the three DOJ-NFHA enforcement variables and five treatment indicators. All these results are consistent with the aforementioned arguments regarding selection of where DOJ investigations were initiated, NFHA affiliates developed, and the most egregious cases severely penalized.

[insert table 6 about here]

The results for partial correlations between changes in rental discrimination against blacks and enforcement by HUD-supported agencies evince a different pattern, however; see the upper right-hand section of Table 6. The sign test shows a consistent pattern of negative coefficients. The coefficients of log of total award dollars for rental availability and encouragement are -0.016 and -0.030, respectively, and are statistically significant at the ten and five percent levels, respectively. Metropolitan areas with a ten percent higher amount of awards over the decade have, all else equal, a 16 percentage-point and a 30 percentage-point greater decline (or smaller increase) in rental availability and encouragement discrimination against blacks during the period. A consistent portrait is painted when we consider HUD-supported agency enforcement variables measured specifically for black plaintiffs; see the left panel of Table 7. The strong, inverse relationships between award dollars and changes in rental availability and encouragement persist, the sign test grows stronger, and now the same relationship also emerges with the overall indicator of consistent discrimination against black renters. These results are consistent with the hypothesis that metropolitan areas whose HUD-supported FHIP and FHAP agencies are more successful in winning larger cumulative monetary awards from their fair-housing suits create a stronger enforcement environment that deters more prospective discriminators against blacks in the rental market.

[insert table 7 about here]

The lower panel of Table 6 contains rental discrimination results for Hispanic-Anglo HDS sites. Only one of the 30 estimated coefficients is statistically significant (positive correlation between DOJ-NFHA awards and change in rental terms discrimination), and the sign test is insignificant for both the DOJ-NFHA and the HUD-

supported agency sets of enforcement variables. The right-hand panel of Table 7 analyzes the relationship estimated using HUD data for the few observations involving Hispanic plaintiffs. No statistically significant coefficients for individual HUD-related enforcement variables emerge, although the sign test indicates a consistent pattern of positive coefficient signs.

These differences between results in Table 7 for black and Hispanic rental discrimination might be explained as follows. Cases alleging discrimination against Hispanics were few compared to those alleging discrimination against blacks. With a greater baseline volume of cases, an increase in enforcement may increase the deterrent effect relative to sites with the average level of enforcement, but an increase relative to a low baseline may only serve to signal that discrimination is a problem in a site because enforcement is too limited to have any deterrence effect.

Tables 8 and 9 provide comparable estimates for discrimination changes in the sales market. The results generally mimic those described in the rental market above. Again in the black-white tests the relationship between the change in discrimination and DOJ and NFHA enforcement measures is generally positive and that for the HUD-supported agency cases is generally negative. In the case of Hispanic-Anglo tests these relationships are typically not significant for DOJ and NFHA enforcement but more strongly positive for HUD-supported agency cases than in the rental market.

[insert tables 8, 9 about here]

Given that there is the suggestion of reverse causality and/or selection bias in several sets of results, we explored this issue further by computing correlations between the level of discrimination in 1989 and the enforcement effort observed from 1989-2000. As explained above, if our hypothesis of bias was valid, we would expect to observe a positive correlation between the 1989 level of discrimination and enforcement efforts (as well as between 1989 level and 1989-2000 changes in discrimination). In fact, we typically observed a negative correlation in exactly the sets of results where we suspected selection or reverse causality bias; no statistically significant positive correlations were observed.

What might be going on to explain this? We offer spurious correlation due to institutional performance persistence as a potential explanation. Imagine that the enforcement efforts of non-profit and local governmental fair housing agencies are relatively constant over time inasmuch as the institutional capacities of these organizations are persistent. This means that the enforcement efforts measured pre- and post-1989 will be positively correlated. Next, assume that the (unobserved) enforcement efforts of agencies during the period preceding 1989 are negatively correlated with the level of discrimination observed in 1989 (due to deterrence). These two assumptions imply that observed discrimination levels in 1989 will be negatively (but spuriously) correlated with the observed enforcement efforts post-1989 (as indeed we often observed). More intuitively, active enforcement in a metropolitan area led to lower levels of discrimination during 1989 allowing little room for reductions in discrimination during the 1990s as racial attitudes improved and reforms occurred in the real estate industry. If this scenario were indeed the case it would suggest further support for the deterrence hypothesis, albeit for a period for which we have no direct observations.

VI. Discussion and Conclusion

In this study we have attempted for the first time to ascertain if there are relationships between direct measures of fair housing enforcement effectiveness and corresponding reductions in 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. This effort proved 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, there is the problem of bias due to reverse causality and selection. If the persistence of discrimination in an area is correlated with its prior level, then it is likely that higher fair housing complaint volumes and damage awards will be observed where discrimination declines the least. There are strong *a priori* reasons to believe that private fair housing organizations have more likely been developed in areas where historically the levels of discrimination have been the most virulent and persistent. Moreover, DOJ enforcement testing has been selectively targeted to a few metropolitan areas with intransigent discrimination reputations. These biases mitigate against observing a statistically significant inverse relationship between enforcement efforts and corresponding declines in discrimination.

Despite these challenges, some remarkable results emerged. We found that higher amounts of state and local enforcement activity by agencies supported by HUD through its FHIP and FHAP programs (especially the amount of dollars awarded by the courts in fair housing cases) were consistently associated with greater declines in discrimination against black apartment-seekers and home-seekers. This evidence is consistent with the hypothesis that if these agencies are more successful in exacting 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 spill over to create deterrence in the home sales sector as well, although this result has only tentative reliability given the insignificance of sign tests.

We also found, however, several situations involving particular sorts of enforcement measures and housing sectors where the relationships between apparent enforcement activity and discrimination change during the 1990s were positive. Yet, further analysis revealed that biases from reverse causality or selection seemed implausible explanations for these results. We forwarded an alternative explanation, based on spurious correlation generated by an institutional persistence in the level of enforcement efforts, coupled with deterrence effect in a prior time period. Though hardly conclusive, this set of results is at least consistent with the aforementioned evidence of the deterrence effect of fair housing efforts.

In sum, it appears that more effective enforcement of fair housing laws does have a measurable impact. Indeed, we therefore conclude that at least part of the observed general reduction in housing market discrimination against blacks 1989-2000 may be attributed to such enhancements. Given the reduction of DOJ paired testing enforcement initiatives and 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 21st century.

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Table 1: HDS Sample Sizes by Metropolitan Area, Year and Minority Group

	HDS 2000[1]		HDS 1989	
	Black-White Tests	**Hispanic-Anglo Tests**	**Black-White Tests**	**Hispanic-Anglo Tests**
Black-White/Hispanic-Anglo Sites				
Los Angeles	69/68	75/69	75/104	81/120
New York	75/68	66/70	54/87	64/118
Chicago	65/63	65/68	66/103	81/122
Houston	70/78	68/75	42/43	51/53
Miami	74/71	73/70	32/39	58/60
Denver	72/71	73/78	44/51	65/73
Austin	69/75	70/72	32/43	55/63
Black-White Only Sites				
Atlanta	81/78	—	66/94	—
Philadelphia	73/70	—	30/44	—
Detroit	66/71	—	33/48	—
Washington, DC	74/69	—	32/43	—
New Orleans	68/76	—	33/44	—
Pittsburgh	79/75	—	38/46	—
Dayton-Springfield	70/70	—	33/47	—
Orlando	72/76	—	32/43	—
Macon/Warner/Robins	69/73	—	33/45	—
Birmingham	77/66	—	34/48	—
Hispanic-Anglo Only Sites				
San Antonio	—	74/74	—	67/116
Pueblo	—	74/76	—	50/68
San Diego	—	69/74	—	61/76
Tucson	—	75/75	—	59/71
All Sites	1223/1218	782/801	709/972	692/940

1. Each entry contains two numbers. The first is the number of rental tests and the second is the number of sales tests.

Table 2: Discriminatory Treatment Measures by Category of Agent Behavior

Rental Treatments	Sales Treatments
Unit Availability	Unit Availability
Advertised Unit Available	Advertised Unit Available
Similar Unit Available	Similar Unit Available
Number of Units Available	Number of Units Available
Unit Inspection	Unit Inspection
Advertised Unit Inspection	Advertised Unit Inspection
Similar Unit Inspection	Similar Unit Inspection
Number of Units Inspection	Number of Units Inspection
Terms and Conditions	Geographic Steering
Rent for Advertised Unit	Steering on Recommendations
Rental Incentives Offered	Steering on Inspections
Amount of Security Deposit	Financing Assistance
Application Fee Required	Help with Financing Offered
Encouragement	Lender Recommendations
Follow-up Contact	Discuss Downpayment
Asked to Apply	Encouragement
Arrangements for Contact	Follow-up Contact
Told Qualified to Rent	Arrangements for Contact
	Told Qualified to Purchase

Table 3: Means and Standard Errors of Fair Housing Enforcement Activity Variables			
Enforcement Variables	Variable Definitions	Black Sites	Hispanic Sites
DOJ Case	Variable is one if site had a DOJ case	0.235 (0.437)	0.182 (0.405)
DOJ or NFHA Case	Variable is one if site had a case filed with NFHA affiliate and/or a DOJ case	0.765 (0.437)	0.636 (0.505)
Compensation	Log of compensation paid in DOJ + NFHA cases during 1990s	5.118 (6.404)	3.465 (5.961)
HUD Cases	Number of HUD (FHIP/FHAP agency) cases in site during 1990s, in thousands	1.655 (1.326)	1.920 (1.528)
HUD Findings	Number of HUD (FHIP/FHAP) cases with discrimination finding, in hundreds	1.211 (1.858)	1.634 (2.217)
Compensation	Logarithm of total dollar compensation	6.701 (4.697)	6.544 (5.369)
HUD Cases (Black)	Number of HUD cases involving a black plaintiff	0.648 (0.476)	0.642 (0.571)
HUD Findings (Black)	Number of HUD cases with discrimination finding involving a black plaintiff	0.514 (0.880)	0.664 (1.071)
Compensation (Black)	Logarithm of total dollar compensation for cases involving black plaintiff	2.965 (4.882)	2.159 (4.830)
HUD Cases (Hispanic)	Number of HUD cases involving an Hispanic plaintiff	0.124 (0.122)	0.185 (0.112)
HUD Findings (Hispanic)	Number of HUD cases with discrimination finding involving an Hispanic plaintiff	0.094 (0.131)	0.136 (0.137)
Compensation (Hispanic)	Logarithm of total dollar compensation for HUD cases involving Hispanic plaintiff	0.000 (0.000)	0.000 (0.000)

Table 4: Incidence of Net Adverse Treatment in the Rental Market			
Treatments	Net 1989	Net 2000	Change
Black-White Tests			
Availability	0.144 (0.035)*	0.046 (0.021)*	-0.097 (0.038)*
Inspection	0.148 (0.030)*	0.069 (0.019)*	-0.078 (0.037)*
Terms	0.045 (0.026)#	-0.004 (0.016)	-0.050 (0.030)
Encouragement	0.099 (0.036)*	0.016 (0.022)	-0.083 (0.044)#
Overall	0.098 (0.028)*	0.026 (0.020)	-0.071 (0.034)*
Hispanic-Anglo Tests			
Availability	0.106 (0.038)*	0.111 (0.026)*	0.005 (0.047)
Inspection	0.083 (0.029)*	0.064 (0.022)*	-0.018 (0.038)
Terms	0.014 (0.032)	0.012 (0.023)	-0.001 (0.042)
Encouragement	0.147 (0.037)*	0.035 (0.027)	-0.111 (0.049)*
Overall	0.146 (0.028)*	0.061 (0.025)*	-0.085 (0.039)*

Table 5: Incidence of Net Adverse Treatment in the Sales Market			
Treatments	Net 1989	Net 2000	Change
Black-White Tests			
Availability	0.172 (0.026)*	0.049 (0.022)*	-0.122 (0.034)*
Inspection	0.113 (0.021)*	0.069 (0.025)*	-0.044 (0.033)
Steering	-0.058 (0.016)*	0.049 (0.017)*	0.107 (0.024)*
Financing	0.121 (0.025)*	0.047 (0.023)*	-0.074 (0.034)*
Encouragement	0.130 (0.026)*	0.052 (0.023)*	-0.078 (0.037)*
Overall	0.147 (0.023)*	0.037 (0.018)*	-0.109 (0.030)*
Hispanic-Anglo Tests			
Availability	0.148 (0.027)*	0.028 (0.040)	-0.120 (0.054)*
Inspection	0.127 (0.025)*	-0.029 (0.034)	-0.156 (0.044)*
Steering	0.038 (0.019)	0.034 (0.029)	-0.004 (0.037)
Financing	0.022 (0.025)	0.135 (0.031)*	0.112 (0.043)*
Encouragement	0.164 (0.025)*	0.042 (0.030)	-0.122 (0.044)*
Overall	0.121 (0.023)*	0.067 (0.023)*	-0.054 (0.037)

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

Table 6: Correlation of Discrimination Change with Enforcement Actions (Rental)						
	DOJ and NFHA Cases			FHIP/FHAP Agencies		
	DOJ Case	DOJ or NFHA Case	Award Dollars	Number of Cases	Number Findings	Award Dollars
Black-White Tests						
Availability	0.141 (1.86)	0.114 (0.98)	0.016 (2.39)	0.027 (0.53)	-0.009 (0.31)	-0.016 (1.93)
Inspection	-0.044 (0.32)	0.248 (1.38)	-0.006 (0.48)	-0.038 (0.46)	-0.039 (0.90)	-0.006 (0.38)
Terms	0.039 (0.42)	0.094 (0.74)	-0.002 (0.24)	0.044 (0.86)	-0.020 (0.70)	0.011 (1.18)
Encouragement	0.031 (0.23)	0.045 (0.24)	0.004 (0.29)	-0.027 (0.34)	-0.028 (0.66)	-0.030 (2.38)
Overall	0.043 (0.41)	0.211 (1.54)	0.001 (0.11)	-0.039 (0.63)	-0.040 (1.25)	-0.011 (0.90)
Sign Test	9 [0.035]			-9 [0.035]		
Hispanic-Anglo Tests						
Availability	-0.012 (0.06)	-0.088 (0.86)	-0.003 (0.12)	0.101 (0.99)	0.042 (1.39)	0.015 (1.38)
Inspection	0.076 (0.66)	-0.072 (1.28)	-0.004 (0.25)	0.030 (0.47)	-0.005 (0.27)	-0.002 (0.30)
Terms	0.188 (1.32)	0.069 (0.84)	0.034 (2.50)	0.030 (0.35)	0.024 (0.97)	-0.003 (0.37)
Encouragement	0.346 (1.20)	0.021 (0.12)	0.046 (1.37)	0.215 (1.45)	0.035 (0.67)	-0.014 (0.75)
Overall	0.129 (0.77)	0.037 (0.40)	0.028 (1.62)	0.151 (2.12)	0.020 (0.69)	-0.003 (0.34)
Sign Test	5 [0.302]			5 [0.302]		

Note: t-statistics shown in parentheses

Table 7: Correlation of Discrimination Change with FHIP/FHAP Agency Enforcement Actions, by Race or Ethnicity (Rental)						
	Black-White Tests			Hispanic-Anglo Tests		
	Number of Cases	Number Findings	Award Dollars	Number of Cases	Number Findings	Award Dollars
Availability	0.077 (0.065)	-0.134 (0.24)	-0.010 (1.26)	0.883 (1.26)	0.849 (1.64)	NA
Inspection	-0.079 (0.41)	-0.069 (0.80)	-0.006 (0.45)	0.438 (1.04)	0.003 (0.01)	NA
Terms	0.131 (1.09)	-0.031 (0.55)	-0.004 (0.45)	-0.182 (0.30)	0.488 (1.10)	NA
Encouragement	-0.062 (0.33)	-0.055 (0.65)	-0.029 (2.75)	0.545 (0.45)	0.516 (0.54)	NA
Overall	-0.046 (0.31)	-0.065 (1.00)	-0.019 (2.07)	0.650 (1.04)	0.369 (0.72)	NA
Sign Test	-11 [0.007]			8 [0.022]		

Note: t-statistics shown in parentheses

Table 8: Correlation of Discrimination Change with Enforcement Actions (Sales)						
	DOJ and NFHA Cases			FHIP/FHAP Agencies		
	DOJ Case	DOJ or NFHA Case	Award Dollars	Number of Cases	Findings	Award Dollars
Black-White Tests						
Availability	-0.112 (1.02)	0.272 (1.96)	-0.009 (0.85)	-0.081 (1.27)	-0.080 (2.83)	0.001 (0.05)
Inspection	-0.008 (0.05)	0.210 (0.92)	-0.009 (0.060)	-0.020 (0.20)	-0.027 (0.51)	0.014 (0.75)
Steering	0.156 (2.87)	0.056 (0.056)	0.007 (1.14)	0.047 (1.16)	0.017 (0.74)	-0.009 (1.16)
Financing	0.037 (0.30)	-0.036 (0.22)	-0.008 (0.71)	0.025 (0.34)	0.020 (0.49)	-0.010 (0.73)
Encouragement	0.239 (2.09)	-0.176 (0.97)	0.020 (1.81)	-0.004 (0.06)	0.007 (0.17)	-0.027 (2.09)
Overall	0.084 (0.79)	0.076 (0.50)	-0.001 (0.05)	-0.009 (0.14)	-0.005 (0.15)	-0.012 (1.01)
Sign Test	2 [0.815]			-4 [0.481]		
Hispanic-Anglo Tests						
Availability	0.120 (0.54)	-0.052 (0.44)	0.026 (1.05)	0.022 (0.18)	0.061 (2.08)	0.005 (0.37)
Inspection	0.060 (0.23)	-0.102 (0.75)	0.003 (0.10)	-0.088 (0.62)	0.071 (2.04)	0.014 (0.93)
Steering	-0.041 (0.42)	-0.050 (1.05)	-0.020 (2.37)	-0.062 (1.36)	-0.013 (0.81)	0.001 (0.24)
Financing	0.695 (2.98)	0.064 (0.33)	0.079 (2.67)	0.338 (2.40)	0.093 (1.90)	0.010 (0.47)
Encouragement	0.117 (0.40)	0.033 (0.21)	0.021 (0.60)	0.170 (1.17)	0.053 (1.17)	0.031 (2.48)
Overall	0.064 (0.31)	-0.112 (1.09)	0.009 (0.38)	0.050 (0.45)	0.049 (1.66)	0.007 (0.56)
Sign Test	6 [0.238]			12 [0.008]		

Note: t-statistics shown in parentheses

Table 9: Correlation of Discrimination Change with FHIP/FHAP Agency Enforcement Actions, by Race or Ethnicity (Sales)						
	Black-White Tests			Hispanic-Anglo Tests		
	Number of Cases	Number Findings	Award Dollars	Number of Cases	Number Findings	Award Dollars
Availability	-0.149 (0.97)	-0.142 (2.39)	-0.008 (0.73)	0.177 (0.20)	1.437 (3.91)	NA
Inspection	-0.06 (0.02)	-0.057 (0.55)	-0.001 (0.04)	-0.405 (0.39)	1.50 (2.73)	NA
Steering	0.156 (1.74)	0.041 (0.93)	-0.004 (0.60)	-0.282 (0.78)	-0.269 (0.96)	NA
Financing	0.087 (0.51)	0.049 (0.63)	-0.006 (0.52)	0.882 (0.64)	1.618 (1.79)	NA
Encouragement	-0.022 (0.12)	0.026 (0.31)	-0.025 (2.13)	1.030 (0.96)	1.140 (1.45)	NA
Overall	0.037 (0.24)	-0.005 (0.07)	-0.013 (1.26)	0.625 (0.81)	1.203 (2.88)	NA
Sign Test	-6 [0.238]			6 [0.146]		

Note: t-statistics shown in parentheses