The Dissertation Committee for Caitlin Knowles Myers
certifies that this is the approved version of the following dissertation:

Inequality in Housing and Labor Markets: Three Essays

Committee:

Daniel Hamermesh, Supervisor

Paul W. Wilson, Supervisor

Stephen Trejo

Gerald Oettinger

Kelly Raley

Inequality in Housing and Labor Markets: Three Essays

by

Caitlin Knowles Myers, B.A., M.S.

Dissertation

Presented to the Faculty of the Graduate School of

The University of Texas at Austin

in Partial Fulfillment

of the Requirements

for the Degree of

Doctor of Philosophy

The University of Texas at Austin

May 2005

To my mother,
who taught me how to write, how to think, and how to use a semicolon.

and

To my husband,
who didn't bat an eye when I announced my desire to go to graduate school, and who managed to keep both open when I wanted to talk about what I was doing there.

Acknowledgments

I am grateful to numerous suppliers of helpful suggestions. Seminar participants at the University of Texas at Austin, Middlebury College, The University of South Carolina, Kenyon College, Tulane University, and Georgia State University provided numerous comments on Chapter 2. McKinley Blackburn, Randall Watson, and Thomas Wiseman also provided excellent advice. I received comments on Chapter 3 from seminar participants at the University of Texas, the 11th International Conference on Panel Data, and at Texas Camp Econometrics IX. In addition, I'd like to thank Kelly Raley for serving on my committee and my other committee members, Stephen Trejo and Gerald Oettinger, who have offered insightful comments on numerous drafts of these essays. Finally, but most importantly, I have benefitted greatly from the mentoring of my supervisors, Daniel Hamermesh and Paul Wilson, who have allowed me the freedom to pursue my interests while providing unflagging guidance and support at all stages of my work.

Caitlin Knowles Myers

The University of Texas at Austin
May 2005

Inequality in Housing and Labor Markets:

Three Essays

Publication No. ____________________

Caitlin Knowles Myers, Ph.D.
The University of Texas at Austin, 2005

Supervisors: Daniel Hamermesh and Paul W. Wilson

This dissertation is made up of three essays that examine racial and gender differentials in labor and housing markets.

The first essay uses unique data on local television news to examine how firms may compete via employee differentiation in response to customer prejudice. The results indicate that there is a negative correlation between the racial, gender, and age composition of competing stations. Moreover, the ratings data suggest that the stations with relatively few blacks on-air are catering to the more discriminatory customers. While a similar result is found for age and gender, the reverse holds for other groups, suggesting possible tastes for diversity for Hispanics and Asians. Taken as a whole, the evidence supports a new theoretical model in which firms differentiate

via the characteristics of their employees in response to customer prejudice.

The second essay disentagles the relationship between race, neighborhood characteristics, and housing prices. Because race and neighborhood characteristics are strongly correlated, studies of racial housing price differentials have yielded results that vary widely depending on the types of neighborhood controls used. This paper shows that even with relatively thorough neighborhood controls, there is still evidence that correlation between the error term and regressors is a source of bias. While recent studies have tended to find evidence of a negative premium for blacks, fixed effects estimates in this paper indicate that black owners pay premiums of around 10 percent for housing. Moreover, house values decline in neighborhoods as the percentage of blacks increases, suggesting prejudicial attitudes.

The third essay examines the labor market effects of Proposition 209, which ended state affirmative action programs in California. I use Current Population Survey (CPS) data and triple difference techniques to take advantage of the natural experiment presented by this change in state law to gauge the labor market impacts of ending affirmative action programs. There appears to have been little change in the relative unemployment rates of women and minorities, but labor force participation declined sharply. This decline suggests that either affirmative action programs in California had been inefficient or that they failed to create lasting change in prejudicial attitudes.

Contents

List of Tables

List of Figures

Chapter 1

Introduction

Nearly fifty years after the peak of the Civil Rights movement, inequality persists in American society. On average, women earn twenty-one percent less than men. Blacks are twice as likely to be unemployed as whites and are half as likely to be college graduates. Asians are twenty-five percent less likely than whites to be home owners. Hispanics are three times more likely to be living below the poverty line and three times more likely to lack health insurance than non-Hispanic whites. In the 2000 elections, the voting rate for eligible minorities was seventy-eight percent that of whites. Blacks are five times more likely to be incarcerated than whites. Even in death we do not escape inequality: black infant death rates are more than twice those of whites and black men live an average of six years less than white men.[1]

Observations of racial and gender differentials, however, are not tantamount to evidence of discrimination. If we wish to craft effective policy for closing these gaps, or if we want to figure out whether they should or can be closed at all, we need to understand their source. We need to ask difficult questions about fundamental differences in innate ability, preferences, and opportunity. We need to figure out

[1]Employment, wage, and education statistics were calculated by the author using 2003 Current Population Survey (CPS) microdata. Home ownership statistics are from United States Census Bureau (2004). Voting data are from Jamieson et al. (2002). Incarceration rates are from Harrison and Karberg (2004). Poverty and health insurance information are based on DeNavas-Walt et al. (2004). Mortality figures are from National Center for Health Statistics (2004).

how all three interact. Women, for instance, might earn less than men because of labor market discrimination. However, they also might earn less than men because they are fundamentally less skilled, are less attached to the work force due to child bearing, or because they tend to enter lower-paying occupations. But then, women could enter lower paying occupations as a result of inequality in upbringing or education. In short, we need to untangle the web of preference, opportunity, prejudice, and discrimination to understand how differentials are created. Is it the playing field that is not level, or is it the entrants?

This dissertation is composed of three essays that look at that proverbial playing field by measuring how differentials in labor and housing markets are influenced by prejudice, by direct discriminatory action, and by government policy.

The first chapter uses unique data on local television news to demonstrate that firms strategically alter their racial composition in response to customer preferences. Previously, only the impact that customer preferences might have on labor market outcomes has been considered, without taking into account the potential for product differentiation in response to these preferences. This chapter provides both theory and evidence to show that competing local news stations appear to differentiate through the characteristics of their on-air staff in order to capture certain groups of customers.

The second chapter examines how racial differentials in housing markets are influenced by neighborhood characteristics, demander prejudice, and supplier discrimination. It shows that previous studies have yielded estimates that were biased downward because of incomplete neighborhood controls. Holding neighborhood characteristics fixed, black homeowners tend to pay more for identical housing than white homeowners but, at the same time, house prices fall as the black composition of a neighborhood rises, suggesting that both demander prejudice and supplier discrimination play a role in housing markets.

The final chapter examines the impact of California Proposition 209, which removed state-sponsored affirmative action in 1997. Using triple difference techniques, it provides evidence that female and minority employment fell sharply as these groups left the labor force with the removal of affirmative action. The finding of a decline in participation for affected groups suggests that either government policy was inefficient or that it was ineffective in engendering permanent change in preferences.

Each essay asks an important question about how both prejudicial attitudes and direct discrimination can affect differentials and how to separate the two. After all, if we are to craft policy to try to shrink the gaps in our society, we need to know whether it is sufficient to simply outlaw direct discrimination or whether we need to worry about the source.

Chapter 2

Labor Market Discrimination as a Competitive Device

2.1 Introduction

Local television news does not seem to fit the mold that we economists have cast for customer discrimination. As Becker (1957) first demonstrated, the racial preferences of customers can directly affect the marginal revenue product of labor of different groups and, hence, labor market outcomes. But if, as several recent papers have suggested (e.g., Kanazawa and Funk, 2001; Holzer and Ihlanfeldt, 1998; Burdekin and Idson, 1991; Nardinelli and Simon, 1990; Kahn and Sherer, 1988), consumers prefer not to interact with minority employees, then why do we see so many blacks, Asians, and Hispanics on the local news? In 2002 an average of 21 percent of broadcast news employees at local television stations were minorities versus 12 percent of newspaper journalists and 8 percent of radio broadcast employees (Papper, 2003; American Society of Newspaper Editors, 2003). Given the frequent supposition of prejudice against minorities, it seems strange, at first glance, that minorities have greater representation in the more visible media. Might it be the case that customers actually have a preference for diversity in some circumstances? Or are other factors at play here?

Casual observations of diversity are not the only source of interest in the market for local television news. Identifying the presence and extent of customer discrimination is not an easy task; it requires either directly or indirectly finding a way to measure the preferences of different labor market agents and how these attitudes affect labor market outcomes. As a result, most studies of customer discrimination have focused on professional sports, where worker output and customer demand are easily observable. The evidence from these studies has varied considerably with the particular sport, time period, and type of position examined. Gwartney and Haworth (1974) find that black players increased attendance at baseball games in the 1950s; Sommers and Quinton (1982) find that blacks had an insignificant effect on baseball team revenue in the 1970s; and Nardinelli and Simon (1990) find that baseball cards picturing minority players sell for less than those of white players. Studies of basketball have tended to find evidence of discrimination (e.g., Kahn and Sherer, 1988; Burdekin and Idson, 1991; Kanazawa and Funk, 2001) with the exception of trading cards for players from the 1970s (Stone and Warren, 1999). Looking at football quarterbacks, Arcidiacono et al. (2004) find evidence of customer tastes for diversity. The disparities in the empirical literature could indicate that the degree and magnitude of customer discrimination is affected by the visibility of employees and the racial composition of customers and/or employees. Recent studies of markets in which a large percentage of employees are black tend to find evidence of discrimination, while studies in which blacks are not as prevalent or not as visible are less likely to find evidence of discrimination.

While they have provided a great deal of empirical evidence, sports markets may have more than their share of idiosyncracies. Most markets have no more than one team in any one sport, precluding analysis of the relationship between the characteristics of firms within a market. Moreover, the consumers of sports are predominantly male and may not be representative of consumers in general.[1] Holzer and Ihlanfeldt (1998) avoid this problem by using special survey data for businesses in four US cities to match customer characteristics with labor market outcomes. However, their conclusions are based on estimates made by the owners of firms about the characteristics of their customer base rather than on a direct indicator of customer preferences.

Television news presents another window into customer discrimination, both because employees are visible to customers and because it offers a measure of customer preferences through television ratings. In this paper, I use a combination of Nielsen ratings for November 2003 broadcasts of local television news in 25 U.S. cities and data on the demographic characteristics of on-air personalities. Because there is evidence of sorting among stations within a market, with some having a much larger number of minorities on their newscasts than others, I present a theory demonstrating that customer discrimination can cause intra-market segregation in which firms select their racial compositions to cater to certain groups of customers. Then, turning to the empirical evidence, I examine how the characteristics of station employees are related to the composition of competing stations as well as of the market in which it is located, and whether the relationships vary with changes in laws governing the employment of minorities in broadcast news. Also, using fixed effects estimators, I examine how the racial make-up of a station's on-air staff affects ratings in order to identify and measure customer preferences for different characteristics including not only race and ethnicity, but also sex and age.

The remainder of this chapter is organized as follows. Section 2 describes the data. Section 3 presents a theoretical model that demonstrates how customer preferences can lead to intra-market segregation. Section 4 presents the econometric

[1] ESPN, for instance, reports that while 45 percent of TV viewers are male, 77 percent of its viewers are (2004). Direct comparison to local television news viewers is difficult because these audience profiles are not made readily available. However, a Pew Research Center survey found that 61 percent of female respondents and 56 percent of male respondents report regularly watching local television news, suggesting a more balanced audience (2004) .

model and describes the results. I conclude in Section 5 with a summary of the findings.

2.2 Local television news data

The data used in this study are a combination of local ratings and market demographics furnished by Nielsen Media Research and station and broadcast data compiled from a variety of sources. The data can be broken down into three basic groups: ratings data, biographical data, and station data.

Ratings Data

The ratings data are collected by Nielsen using electronic meters that measure whether a household is tuned in to a particular program minute-by-minute. Nielsen aggregates the data to calculate the average per-minute audience for a show which, in turn, is used to calculate a program's rating and share. The rating measures the ratio of the average number of households viewing a program to the number of households that have television sets (the potential audience). The share measures the ratio of the number of households viewing a program to the number of households viewing television at that time. The ratings data used in this paper were compiled by Nielsen for the 25 largest "Designated Market Areas" (DMAs) in the United States.[2] They measure the average audience over the month of November 2003 for the different daily local television news broadcasts on FOX, CBS, ABC, and NBC affiliates in each of the 25 DMAs. Ratings data are used for original newscasts between 5 a.m. and 12 a.m. local time. Table 2.1 reports average ratings and shares for local newscasts in the sample by time of day and part of week. News broadcasts that begin in the late evening are the most popular, with an average of 8 percent of

[2]The 25 markets in the data, in order of size, are New York, Los Angeles, Chicago, Philadelphia, San Francisco-Oakland-San Jose, Boston, Dallas, Washington, D.C., Atlanta, Detroit, Houston, Seattle-Tacoma, Tampa-St. Petersburg, Minneapolis-St. Paul, Phoenix, Cleveland-Akron, Miami-Ft. Lauderdale, Denver, Sacramento-Stockton-Modesto, Orlando-Daytona Beach-Melbourn, St. Louis, Pittsburgh, Baltimore, Portland, OR, and Indianapolis.

Table 2.1: **Average Ratings and Shares**

	Weekdays		Weekends	
Local Start Time	Rating	Share	Rating	Share
5:00 a.m. –7:59 a.m.	2.9	13.0	3.5	12.7
8:00 a.m. –10:59 a.m.	2.5	8.9	4.3	11.3
11:00 a.m.–4:59 p.m.	4.2	12.7	3.9	9.9
5:00 p.m. –8:59 p.m.	6.4	12.0	5.5	9.8
9:00 p.m. –11:59 p.m.	7.9	13.9	6.7	12.3

T.V. households and 14 percent of households that are watching television watching a particular news program at this time. These averages are for one time of day and one station alone, and most cities have at least 4 stations broadcasting local news in English. Taken together, the ratings suggest that a large portion of the population watches local television news broadcasts, an observation corroborated by a Pew Research Center survey in which 59 percent of respondents reported "regularly" watching and an additional 23 percent reported "sometimes" watching local news (2004).[3]

Biographical Data

To accompany the ratings data, I collected information from the biographies and pictures of news teams that nearly every station makes available online and augmented this with information from newspaper reports, news broadcasts, press releases, and other sources of data on news staff. Using these sources, I made detailed notes on the characteristics of each of the thousands of on-air employees in the sample of stations. On-air jobs were divided into 4 categories: news anchor, sports anchor, weather anchor, and reporter. Each person's occupation was noted and the days and times at which the anchors regularly appear were also collected. Station employees sometimes have more than one of these job titles. Most commonly, a news anchor for one newscast may also be a general assignment reporter for others or

[3]The study showed no large difference in local news viewing habits by income, education levels, or sex. However, older respondents did report watching the news more regularly than younger respondents and blacks reported watching more regularly than other racial groups.

a sports anchor might also be a sports reporter. In these cases, both titles were noted although the more senior position alone is coded for the purposes of summary statistics.

In addition to position, data were also collected on other characteristics of the on-air staff. If available, information was collected on origin (state native or not), first year at the station, first year of paid employment in television news, total number of stations an employee has worked for, approximate age range (20–29, 30–39, etc.), and highest degree obtained. Data were also collected on the sex, race, hair color,[4] and ethnicity of each employee. Measures of race and ethnicity are obviously subjective. In general, an employee was considered to be white and non-Hispanic unless there was direct evidence to the contrary. In the majority of cases it appears that minorities are members of the various large national associations for minority journalists such as the National Association of Black Journalists. Membership in such an organization, mention of the person's race or origin, or clear visual evidence were used to decide if a person is black or Asian rather than white. "Hispanic" is an ethnic identifier rather than a racial characteristic but, in the interest of simplicity and because nearly all of the Hispanic journalists in the sample would have also been categorized as "white," Hispanics were entered as a separate racial category. As a result, each employee was classified as white, black, other, or Hispanic. All other races fall into the category of "other," which is referred to as "Asian" because of the 125 employees in this category, 2 were Native American and the remaining 123 were Asian.

Table 2.2 reports average characteristics of the on-air staff by race. Interestingly, minorities are more likely than whites to be a news anchor, a highly visible position. Also, fewer minorities, especially Hispanics and Asians, are male. Minorities also tend to be younger and more educated than their white counterparts. A final

[4] An indicator of whether an employee was blond was created. This is likely just as subjective a measure as race, but I included it because I was curious to see if there was any evidence of preference for blonds.

Table 2.2: **Average Characteristics of On-Air Staff**

	White	Black	Hispanic	Asian
news anchor	26.0	43.3	34.2	36.8
weather anchor	15.4	5.8	3.1	3.2
sports anchor	7.2	9.2	3.7	3.2
reporter	51.7	41.7	59.0	56.8
male	64.2	50.4	35.4	22.4
age	42.7	41.7	38.3	35.4
state native	25.0	19.7	42.9	21.6
post-college degree	10.8	18.4	13.0	16.0
years at station	9.2	8.7	6.8	4.9
years in local television news	18.9	18.7	15.8	12.7
number stations worked at	3.6	3.6	3.8	3.8
number of observations	1850	381	161	125

interesting pattern is that Hispanics and Asians tend to have less experience and tenure than whites and blacks, but to have worked at a similar number of stations, suggesting that they may change jobs more frequently.

Station Characteristics

In addition to providing ratings data, Nielsen also furnished data on the number of people and households in each DMA as well as breaking this down by age, sex, and black and Hispanic composition. In order to measure the Asian composition of the markets, data from the 2000 U.S. Census for each corresponding metropolitan area were used. Therefore, the average characteristics of each market are known, but ratings and shares were only made available as aggregate numbers and not broken down by these demographic groups. In addition, local television listings were used to add information on the number of stations in each DMA that broadcast local news in English, the number of stations that broadcast local news in Spanish, and a dummy variable indicating that the DMA has a 24-hour local news station.

Furthermore, the biographical information for each station was aggregated to calculate average characteristics of each station's on-air staff such as the percent who are black and the average tenure at the station. After matching anchors to time-slots, indicators of the characteristics of the anchor team that is specific to each broadcast were also included. However, because reporters do not tend to appear at regular times, they were not matched to specific broadcasts.

Information was also collected from each station's regional chapter of the Academy of Television, Arts, and Sciences, which have annual "Emmy Awards" for their respective regional stations. The awards process requires stations to submit news tapes for consideration for each category in which an award is granted and the station would like to be considered. The tapes are sent to another regional chapter (there are twenty in all) for review and winners are selected. There can be one, more than one, or no winner in a particular category. Emmy awards are used as a proxy for station quality. While the number of categories and the level of competition differs by region, fixed effects estimators which compare stations in the same market are used in later analyses, so this issue does not present a problem. For 23 of the 25 DMAs, data from the 2003 local Emmy awards were used. For New York City and Detroit, which did not have 2003 Emmy information available, data from the 2004 Emmy awards were used.

Of the 100 possible stations (4 per each of the 25 DMAs), 12 were dropped because they did not have local news programming or little or no biographical information was available, leaving $2,569$ biographical observations for 88 stations across 25 markets. These data were combined with the Nielsen ratings to leave 762 newscast observations. One observation might be the weekday 6 p.m. news on the NBC affiliate in Atlanta, Georgia, and another might be the weekend 11 p.m. news on another affiliate in the Seattle-Tacoma area. Each of these observations includes the show's average rating and share for November 2003 as well as characteristics of the market, demographic characteristics of the station's on-air employees and the anchors on that particular show, indicators of the amount of competitive programming at that time, controls for experience, and indicators of the quality of the station's local news programming. Table A.1 in the appendix presents a summary of all the basic variables in the data set.

The average station has about 29 on-air employees, with an average of 9 news anchors, 3 weather anchors, 2 sports anchors, and 15 reporters. Twenty-six percent of the on-air employees in this sample are minorities. This is somewhat larger than the 21 percent reported by Papper (2003) in his summary of the RTDNA/Ball State University survey of news directors, but that could be explained by differences in samples. Specifically, the sample used here is only the 25 largest markets in the United States while Papper's results are based on more markets and different methodology.[5] It may simply be the case that stations in larger markets are more diverse than stations in smaller markets. While there is a large number of minorities appearing on-air at local news stations, the match between station composition and market demographics is closer for blacks and Asians than for Hispanics. Local stations have an average of 15 percent black employees while the average market is 12 percent black and an average of 5 percent Asian employees while the average market is 5 percent Asian. However, stations have an average of 6 percent Hispanic employees while the average market is 14 percent Hispanic. The difference could be an artifact of methodology: Hispanics may have been miscategorized more often than the other racial groups.[6]

While minority representation is fairly high, it is frequently suggested that minority journalists are relegated to the lower-rated time slots.[7] For each show, indicators were constructed for the presence of at least one minority (black, Hispanic, or Asian) news anchor, a minority weather anchor, and a minority sports anchor. Table 2.3 summarizes the proportion of shows that have a minority anchor by time (morning/mid-day or evening) and part of week. The sample proportions suggest

[5]The RTNDA survey conducted by Papper asked station managers what percent of their staff are minority.

[6]The basic assumption when identifying either group was that a person was white unless "proven" otherwise. Under this rule, to be classified as Hispanic, a person had to have more than a Spanish surname; he or she had to be a member of an organization of Hispanic journalists, mention heritage in their biography, or have some other piece of clear evidence. In the case of blacks and Asians, visual evidence could frequently also be used, making them less likely to be incorrectly identified as white.

[7]For example, an article in the *St. Petersburg Times* reports that some minority journalists refer to weekends as the "weekend ghetto" (Deggans, 2003).

Table 2.3: **Proportion of Shows with a Minority Anchor**

Time/Week Part	Minority News Anchor	Minority Weather Anchor	Minority Sports Anchor
morning/mid-day	0.61	0.06	0.96
evening	0.51	0.13	0.40
difference*	***0.10***	***-0.06***	***0.56***
weekend	0.47	0.16	0.57
weekday	0.59	0.08	0.71
difference*	***-0.12***	***0.08***	***-0.14***

**All sample proportion differences are significant at the 10% level.*

that morning news shows are 10 percent more likely to have at least one minority anchor than are evening newscasts, but that weekend newscasts are 12 percent less likely than weekday casts to have a minority anchor. This trend is reversed for minority weather anchors; weekend shows are more likely to have minority weather anchors, but so are evening shows. The results for news and weather anchors, therefore, do not offer consistent evidence that minorities are segregated to lower-rated slots. However morning and midday shows are 56 percent more likely and weekend shows are 14 percent less likely to have a minority sports anchor. This trend appears similar to that for news anchors, but, because many local sporting events take place on weekends, it is possible that the weekend sportscasts are actually more popular than weekday sportscasts.[8] If this is the case, then it would appear that minority sports anchors tend to be segregated to lower-rated slots.

While there is no conclusive evidence that minority anchors are segregated to lower-rated time slots, the possibility of such endogeneity in the relationship between race and ratings must be addressed. In order to measure the presence and magnitude of customer racial preferences, it is necessary to see the impact on ratings of an *exogenous* change in race. If the race of anchors is also affected by the rating a time slot can generally be expected to get, then estimates of customer discrimination

[8]The assertion that weekend sportscasts may be more popular than weekday sportscasts is also supported by station scheduling of sports news and commentary. Local stations that have special news sections or news shows devoted to sports almost universally broadcast the additional coverage on weekends rather than weekdays.

against minorities could be biased upward. This possibility will be addressed in the econometric model.

Turning to the market as a whole, the data indicate that there is quite a bit of competition in the local news market relative, at least, to sports markets where there is frequently only one home team or only one game being televised on broadcast television. The average market has 6 stations that broadcast local news in English and 1 station broadcasting local news in Spanish, and about a third of the cities have a 24-hour local news station. Additionally, at the time of any given news broadcast there are an average of 3 broadcasts in English on the air (not including any broadcasts on 24-hour channels) and 0.3 broadcasts in Spanish. The presence of competition presents an opportunity to examine the strategic response of firms to the actions of their rivals.

So far I have shown that stations tend to have a quite a few minorities on-air and that there is not strong evidence that these minorities are relegated to less visible positions or time-slots. However, these aggregate statistics mask important intra-market differences. Table 2.4 reports the average differences between the largest and smallest station composition variables in a DMA and tests the hypothesis that this difference is equal to 0. For each of the four measures, the null can strongly be rejected.[9] On average, in each DMA the station with the highest number of minorities has 4 more minorities on staff than the station with the lowest. Similarly, the station with the highest minority composition (*pctminority*) has 12 percentage points more minorities than the station with the lowest minority composition. This indicates that the stations within a market area do not look alike: one station has

[9] This test of the difference in sample proportions relies on a normal approximation of the binomial distribution. While it presents a simple way to illustrate the variation within markets, the actual sample sizes here are small enough that the law of large numbers on which the test relies does not hold. As an alternative, I assumed that the probability of hiring a black (Hispanic) at each station was equal to the percent of blacks (Hispanics) in the market. For each market, I then calculated the probability of seeing both a station with the minimum observed composition and a station with the maximum observed composition. Averaging the results across markets, this probability was 5.5 percent for blacks and 6.7 percent for Hispanics. So the results still indicate a significant difference between station composition.

Table 2.4: **Tests of Within-DMA Station Differences**

H_0 :	$\overline{D}$	s_d	t-stat	$P > t$
$max(no\ minorities) - min(no\ minorities) = 0$	3.80	2.48	7.65	< 0.001
$max(no\ black) - min(no\ black) = 0$	2.32	1.41	8.25	< 0.001
$max(no\ hisp) - min(no\ hisp) = 0$	1.64	1.66	4.95	< 0.001
$max(pctminority) - min(pctminority) = 0$	11.98	7.46	8.03	< 0.001

significantly more minorities than another. Stations may be differentiating along racial lines, but there is nothing in the existing customer discrimination theory that predicts or explains why this might happen.

2.3 Theoretical model of racial differentiation in response to customer discrimination

Basic model

Becker's (1957) well-known model of customer discrimination assumes that customers will interact with one employee when they purchase the output of a firm and that they act as though the price is marked up if that employee is black. He shows that, in equilibrium, firms will pay a lower wage to black employees and charge a lower nominal price for their output than for that of white employees. There is no implication for overall firm composition. In equilibrium, employers are indifferent between black and white employees; black employees have a lower marginal revenue product, but also have a lower marginal cost.

However, the assumptions that support this prediction are not always realistic. In many markets, local news being one, customers cannot interact with only a black or only a white employee. Rather, a customer will come into contact with many of a firm's employees. Incorporating this possibility into Becker's theoretical model, and using some of the tools of models of product differentiation, we will see that firms may, in fact, have a preference for the racial composition of their workforces.

Suppose that there are two possible groups from which a firm can hire employees:

minorities and non-minorities. Let $m \in [0, 1]$ be the percentage of a firm's workforce that is composed of minorities. There are N consumers who each purchase one unit of output and have preferences over the racial composition of a firm so that consumer i behaves as if he paying $P + d_i m$ for that unit when he buys it for price P from a firm with racial composition m. The discrimination coefficient, d_i, is distributed uniformly over [L, H]. Note in particular that if $L < 0$ some consumers have a preference for interacting with minorities.

Assume that there are two firms in this market and that each can choose the racial composition of its workforce and the price that it will charge for its output. This set-up is quite similar to that of a Hotelling-type model of product differentiation and will be solved as a two-stage game in which the firms first choose racial composition simultaneously and then, given the composition choices, choose prices.[10] To solve for the Nash equilibrium, we work in reverse: we first see what the equilibrium prices are for fixed composition and then solve for the optimal composition given these prices.

Assume, then, that Firm 1 and Firm 2 have fixed racial compositions $\overline{m}_1$ and $\overline{m}_2$, respectively, and, without loss of generality, that $\overline{m}_1 < \overline{m}_2$. Then consumer i will buy from Firm 1 if $p_1 + d_i\overline{m}_1 < p_2 + d_i\overline{m}_2$. Relying on the uniform distribution of d_i, we can derive the demand for output from each firm:

$$D_1(p_1, p_2) = \left(\frac{N}{H - L}\right)\left(H - \frac{p_2 - p_1}{\overline{m}_1 - \overline{m}_2}\right) \tag{2.1}$$

and

[10] There is, however, a key difference between the model presented here and a standard model of product differentiation. In the standard set-up, the distribution of customer tastes lies in the same space on which the firms locate. However, rather than assuming that each consumer has an ideal employee racial composition and that there is a uniform distribution of this ideal, I assume that consumers are either prejudiced ($d_i > 0$) or not prejudiced ($d_i < 0$) and that it is the intensity of this preference that varies. I find this specification to be more plausible as well as in keeping with previous work on discrimination. Moreover, this model's prediction of differentiation holds for a wide variety of functional forms for the consumer's price function. A similar prediction is found with a standard differentiation model with quadratic transportation costs but, as has been demonstrated, the results are sensitive to the functional form of costs.

$$D_2(p_1, p_2) = \left(\frac{N}{H-L}\right)\left(\frac{p_2 - p_1}{\overline{m}_1 - \overline{m}_2} - L\right). \tag{2.2}$$

Each firm faces the same constant cost c per unit of output. Then Firm i chooses its price to maximize total profits given by

$$\Pi_i = (p_i - c)D_i(p_i, p_{-i}). \tag{2.3}$$

Maximizing this function for each firm and solving for the Nash equilibrium prices, we obtain

$$p_1^*(\overline{m}_1, \overline{m}_2) = c + \frac{1}{3}(L - 2H)(\overline{m}_1 - \overline{m}_2) \tag{2.4}$$

and

$$p_2^*(\overline{m}_1, \overline{m}_2) = c + \frac{1}{3}(2L - H)(\overline{m}_1 - \overline{m}_2). \tag{2.5}$$

Moving to the choice of composition given the optimal prices, each firm will choose m to maximize the reduced-form profit function

$$\Pi^i(m_1, m_2) = [p_i^*(m_1, m_2) - c]D_i[m_1, m_2, p_1^*(m_1, m_2), p_2^*(m_1, m_2)]. \tag{2.6}$$

Noting that in the Stage 1 maximization problem each firm sets $D_i + (p_i - c)\partial D_i/\partial p_i = 0$ and using the envelope theorem,

$$\frac{d\Pi_1}{dm_1} = (p_1^* - c)\left(\frac{\partial D_1}{\partial m_1} + \frac{\partial D_1}{\partial p_2}\frac{\partial p_2}{\partial m_1}\right) \tag{2.7}$$

and

$$\frac{d\Pi_2}{dm_2} = (p_2^* - c)\left(\frac{\partial D_2}{\partial m_2} + \frac{\partial D_2}{\partial p_1}\frac{\partial p_1}{\partial m_2}\right). \tag{2.8}$$

Solving the two maximization problems,

$$\frac{d\Pi_1}{dm_1} = \frac{N}{9}\left[\frac{(L-2H)^2}{L-H}\right] < 0 \tag{2.9}$$

and

$$\frac{d\Pi_2}{dm_2} = \frac{N}{9}\left[\frac{(2L-H)^2}{H-L}\right] > 0. \tag{2.10}$$

So, in equilibrium, $m_1^* = 0$ and $m_2^* = 1$; that is, Firm 1 will always choose to have a workforce that is all non-minority while Firm 2 will be made up solely of minorities. The equilibrium price differential will depend on the distribution of tastes. If $L > 0$, then Firm 2, which is all minority, will charge a lower price than Firm 1, which is all white. However, if L and H are sufficiently low ($H < -L$) so that there is a relatively large number of consumers with preferences for minorities relative to those who are prejudiced against them, Firm 2 will charge a higher price than Firm 1.

This model of racial differentiation has implications for what we might see empirically. Firms will want to differentiate by racial composition in order to gain control of certain segments of the market. The equilibrium relationship between racial composition and demand depends on the distribution of consumer preferences. In the case where $H < -L$, that is in which there are more consumers who prefer minorities than do not, firms with low minority composition will charge less and have lower demand than their rivals. But if $H > -L$, that is if more consumers are prejudiced against minorities than prefer them, then firms with low minority composition will cater to the most prejudiced consumers and will charge more and have greater demand than their competitors. Moreover, the response of demand to changes in firm composition would be different for the different firm types. For instance, in the case where all consumers are prejudiced, we could see a more sharp decline in demand in response to an increase in minorities for low-minority firms than for high minority firms which cater to the less-prejudiced customers.

Incorporating costs of altering racial composition

One potential shortcoming of this theoretical model is that the prediction of maximal product differentiation seems extreme. Given the presence of equal employment opportunity (EEO) laws and racial differentials in labor supply, it may be quite expensive for firms to differentiate completely. To incorporate this possibility, suppose that Firm i incurs an additional cost of altering its racial composition from arbitrary level R which might be, for instance, the racial composition of the market as a whole. This cost could be the result of increasing the risk of lawsuits under EEO laws or of increasing search costs as a firm attempts to find minorities to fill all of its slots. Given quadratic costs of adjusting racial composition, Firm i's profit function is

$$\Pi_i = (p_i - c)D_i(p_i, p_{-i}) - (m_i - R)^2. \tag{2.11}$$

This cost of altering racial composition does not affect the demand functions or firm i's best response price function, but does alter its location decision. Now the Nash equilibrium compositions are:

$$m_1^* = \max\left[0, R - \frac{1}{18}\left(\frac{N}{H-L}\right)(2H-L)^2\right] \in [0,1] \tag{2.12}$$

and

$$m_2^* = \min\left[R + \frac{1}{18}\left(\frac{N}{H-L}\right)(2L-H)^2, 1\right] \in [0,1]. \tag{2.13}$$

So, if there is a cost of adjusting composition, the firms will differentiate, but not necessarily completely. The equilibrium distance between them depends on the size of the population and the tastes of consumers. Note that

$$\frac{dm_1*}{dN} = -\frac{1}{18}\frac{(2H-L)^2}{H-L} \leq 0 \tag{2.14}$$

and

$$\frac{dm_2*}{dN} = \frac{1}{18}\frac{(2L-H)^2}{H-L} \geq 0 \tag{2.15}$$

So, as the population size, N, grows, the degree of separation increases. However, the comparative statics for consumer preferences are not straightforward. The effect of changes in L and H on product differentiation depends on the size and sign of L and H.

2.4 Analysis of Station Composition and Customer Preferences

I will first look at firm racial composition to see if the characteristics of competitors are negatively related, and I will then look at the effect of these characteristics on demand to see if it is consistent with the model of product differentiation.

Racial Composition of Employees

The theoretical model presented in the preceding section suggests that a station's racial composition is negatively correlated with the composition of the other stations in the market. But it can also depend on employer discrimination and the demographic characteristics of the market. The characteristics of the market are not so important as a measure of the potential labor pool, but rather because of how they interact with EEO laws.[11] While the rules and enforcement of EEO laws in the local television industry have changed over previous decades, since their enactment in 1967, station racial composition has generally been compared to the composition of the surrounding market.

In order to analyze the determinants of employee composition, I use the station-level data to estimate random effects regressions of racial and gender composition

[11] Journalists appear to move easily and frequently between markets. In this sample, the average journalist has worked at 4 stations and at the average station only a quarter of employees are natives of that state.

($pctblack$, $pcthispanic$, $pctasian$, $pctfemale$) and age composition (age, the average age of on-air employees at a station) that allow for possible correlation of the error terms within markets. Explanatory variables include station and market characteristics.[12]

A potential concern is that the right-hand-side variable measuring the average composition of competing stations may be correlated with unobserved market characteristics. Consider, for instance, the regression for station black composition:

$$\textit{pctblack} = \boldsymbol{\alpha_1} + \textit{dmapctblack}\boldsymbol{\alpha_2} + \textit{(other stat pctblack)}\boldsymbol{\alpha_3} + \mathbf{X}\boldsymbol{\alpha_4} + \boldsymbol{\epsilon} \tag{2.16}$$

where $\mathbf{X}$ contains additional station and market characteristics listed in Table 2.5. The theoretical model predicts that α_3, the coefficient on the average composition of competing stations, should be negative. However, any unobserved market characteristics that influence the composition of other stations should also influence the composition of the observed station. For instance, suppose that stations in more educated markets are more likely to hire minorities. Because education is not observed, α_3 will be biased upward. In fact, any unobserved market characteristic that influences station composition should have a similar effect for all stations. Therefore, to the extent that negative correlation is observed in the presence of unobserved market characteristics, this only bolsters the results.

However, an endogeneity problem remains due to the fact that racial composition regressions represent a system for each market in which the composition of station i is determined by the average composition of the remaining stations in the market. The appendix presents a brief derivation of the statistical properties of the estimator

[12]A negative binomial count model for the actual number of employees from any one group was also estimated as was a truncated dependent variable model. Both yielded similar results. Another possibility is that there is substitution between worker types. That would suggest a system of equations with endogenous dependent variables, but no instrument suggests itself for identifying these variables. However, in that case, omitting measures of the station composition from the right hand side as done here could lead to bias. If, for instance, black and Hispanic workers are substitutes, then anything that tends to increase demand for one group will decrease the demand for the other group. This possibility should be kept in mind when interpreting the results.

of α_3 in such a system. In the case of positive correlation between the error terms (as seems likely), the probability of rejection for a negative coefficient estimate is not overstated. However, it is possible that if the relationship between station compositions is positive and very large, the expected value of the estimate could be negative. For this reason, I proceed with the racial composition regressions but note that, while a significant negative estimate is likely to indicate a true negative correlation between composition, the magnitude of the effect will be biased.

Table 2.5 reports the results of the five regressions. There is not much evidence of discrimination by employers. An indicator that a station has a minority station manager and/or news director, *minmanager*, was included in order to see if the race of managers has any relationship with the race of employees. For instance, white managers may discriminate against black employees while black managers may not.[13] However, the coefficient is positive and significant only in the sex regression, indicating that minority managers are positively correlated with female employees. An indicator that a station has a female manager was also included, but the coefficient is not significant in any of the regressions.[14] It is possible that station managers and news directors do not bear all of the burden of hiring decisions. But in this case, one would expect that there would be a positive correlation between the characteristics of managers and on-air staff because *both* are employees subject to the same employer discrimination. That there is not strong evidence of this only further supports the conclusion that employer discrimination does not play a role

[13]Antonovics and Knight (2004), for example, use evidence from traffic stops in Boston to show that police officers are more likely to conduct a search if the driver is of another race, suggesting that racial preferences vary by race.

[14]Because it was especially difficult to collect information on the characteristics of station managers, the race of the station manager or news director is only known for 49 of the 88 stations. Hence, the coefficient on *minmanager* is actually comparing the impact of a minority manager relative to a white manager *or* to a manager of unknown race. This could be the source of the lack of significance in the remaining 3 regressions. However, the results are similar if a dummy indicating that manager race was not observed is included. Because manager names were more readily available than biographies and pictures, the sex of managers was identified at 79 of the 88 stations.

Table 2.5: **Random Effects Racial Composition Regressions**

dep. variables: station percent black, Hispanic, Asian and female and mean age

	pctblack		**pcthispanic**		**pctasian**		**pctfemale**		**age**	
	coef	se	coef	se	coef	se	coef	se	coef	se
constant	11.30	118.07	-108.32	72.26	47.79	83.21	**-412.69**	144.99	46.41	67.65
Market Chars.										
dma pctblack	**1.07**	0.25	<0.01	0.15	0.09	0.18	**-0.54**	0.31	0.01	0.14
dma pcthispanic	-0.18	0.23	**0.65**	0.14	-0.01	0.16	**1.01**	0.28	**-0.36**	0.13
dmapctasian	0.19	0.30	-0.28	0.18	**0.68**	0.21	0.43	0.37	-0.22	0.17
dma pctfemale	-1.03	1.90	1.80	1.16	-1.60	1.33	**8.05**	2.33	-0.68	1.09
dma pctyoung	0.41	0.56	-0.25	0.34	-0.25	0.39	**-1.79**	0.68	0.45	0.32
dma pctprime	0.08	0.57	**0.93**	0.35	-0.22	0.40	1.01	0.70	0.13	0.33
log(persons)	-0.35	2.34	**-2.84**	1.43	**3.16**	1.65	-0.32	2.88	0.59	1.34
south	0.88	2.77	1.08	1.70	1.46	1.95	**6.05**	3.40	-0.32	1.59
west	-4.50	3.73	-3.63	2.28	3.03	2.63	3.03	4.58	**3.32**	2.14
midwest	**4.79**	2.29	**2.70**	1.40	1.43	1.61	**4.82**	2.81	-1.39	1.31
english stations	0.34	1.15	**2.21**	0.70	0.15	0.81	-1.67	1.41	0.74	0.66
spanish stations	0.89	2.00	**3.50**	1.22	-1.50	1.41	**-10.51**	2.46	**3.03**	1.15
24hr stations	-1.44	1.75	-1.64	1.07	0.02	1.23	1.48	2.15	1.31	1.00
NBC	1.83	1.51	0.35	0.93	1.17	1.07	2.08	1.85	1.39	0.87
CBS	2.14	1.55	0.60	0.95	0.66	1.09	**3.16**	1.90	**1.85**	0.89
ABC	2.41	1.58	0.38	0.97	-0.06	1.11	2.10	1.94	**1.80**	0.90
Station Chars.										
minority manager	2.65	1.78	0.13	1.09	-0.94	1.25	**6.64**	2.18	-0.68	1.02
female manager	1.48	1.26	0.41	0.77	-0.82	0.89	2.14	1.54	-0.16	0.72
other stat pctblack	**-0.52**	0.23	**-0.25**	0.14	-0.16	0.16	0.26	0.28	**0.24**	0.13
other stat pcthispanic	-0.38	0.39	**-1.14**	0.24	0.03	0.28	0.34	0.48	0.08	0.22
other stat pctasian	-0.09	0.29	0.11	0.18	-0.14	0.20	**0.64**	0.35	-0.01	0.16
other stat pctfemale	0.12	0.16	0.07	0.10	0.08	0.12	**-0.89**	0.20	**0.27**	0.09
other stat age	**0.92**	0.35	0.21	0.21	0.04	0.25	**1.62**	0.43	**-0.33**	0.20
no. observations	88		88		88		88		88	

**Bold coefficients are significant at 10% level.*

in this market.[15]

As suggested by the theoretical model, there is a significant negative relationship between the racial compositions of stations within a market. A 1 percentage point increase in the average percent black at other stations in a market is correlated with a 0.5 percentage point decrease in a station's own percent black. And a 1 percentage point increase in the average percent Hispanic at other stations is correlated with a decrease of 1.1 percentage points in own percent Hispanic. Similar trends hold for sex and age. A 1 percentage point increase in the average percent female at competing stations is correlated with a 0.9 percentage point decrease in percent female. And a one year increase in the average age of employees at competing stations is correlated with an own average age decrease of four months. While there is no evidence of this sorting for Asian employees, this is also the only one of the categories in which the market composition is proxied with Census data on the metropolitan area because Nielsen did not provide information on Asian market composition.

The negative correlation between the representation of a group at a station and at its competitors suggests that there is some intra-market sorting going on among competing stations. In some markets, this might be the result of a small pool of potential employees, so that if a firm hires a black employee, the ability of its competitors to hire one is reduced. However, as mentioned before, television journalists move frequently between stations and regions, suggesting that even if a particular market does not have many minorities, that does not mean that there are no minorities available to hire. Furthermore, with its reputation as a tough field to crack, there appears to be no shortage of potential journalists of any race. Hence, it seems likely that results are indicative of racial differentiation by firms.

While the evidence supports sorting among stations, there is also a significant positive relationship between the racial composition of a market and the composition

[15] Another way to look for employer discrimination is to control for the corporate owner of a station. There are 23 corporate owners (such as News Corp., Gannett, Disney, etc.) in this sample. However, coefficients on owner indicators were neither individually nor jointly significant.

of all stations within it. A 1 percentage point increase in the percent of a market's population that is black is correlated with a 1.1 percentage point increase in the percent black at a station. Similarly, a 1 percentage point increase in the percent of a market's population that is Hispanic is correlated with a 0.7 percentage point increase in the percent Hispanic at a station. There do not, however, appear to be cross-racial effects for markets and stations; the percent black in a market is not significantly related to the percent Hispanic at a station and vice versa. Looking at females, there is a large effect of the gender composition of a city on the composition of a station, an especially surprising result given the low variance in $dmapctfemale$, which only ranges from 50 to 53 percent in this sample. With the exception of age, therefore, it seems that the characteristics of television journalists are related to the corresponding characteristics of the market.

As previously mentioned, it is not clear to what extent this finding might represent the effects of labor supply, station response to audience demand, and the impact of EEO laws. In particular, because of their enforcement by the Federal Communications Commission (FCC), EEO laws may play an especially large role in the hiring decisions of local stations. In order to examine this possibility, I take advantage of several shifts in the provisions and enforcement of these laws by the FCC.

EEO laws actually encompass a range of congressional acts pertaining to job discrimination. The first and most well known is Title VII of the Civil Rights Act of 1964, which forbids employers from discriminating based on a variety of characteristics. In response to this act, the FCC, which oversees the federal licensing of local stations, began requiring stations to submit written reports summarizing the composition of their employees in 1970. Stations that had 50 or more employees or that did not have sufficient minorities on staff relative to the composition of their market were reviewed by the FCC at licensing renewal time. In 1987 the FCC modified its policy to place greater weight on recruitment efforts and, as a

Table 2.6: **EEO Enforcement Regimes**

Regime	Time Period	Enforcement
Regime 1	1970-1986	Beginning of FCC regulation: Stations submit written EEO reports to FCC. Stations that did not employ sufficient numbers of minorities relative to their market were examined closely at the time of license renewal.
Regime 2	1987-1997	Stricter regulations: FCC shifts emphasis to minority outreach and recruitment. Stations must submit additional forms detailing these efforts and results continue to be tied to license renewal
Regime 3	1998-1999	No regulations: FCC practices are ruled unconstitutional in 1998 and are suspended.
Regime 4	2000-2003	Fluctuating regulations: FCC tries two sets of rules that tie EEO compliance to licensing.

result, the amount of required paperwork and monitoring increased. However, in 1998 the tying of federal licensing to these requirements was ruled unconstitutional and the FCC suspended its EEO program, leading to a two year period in which no FCC enforcement was in place. In 2000 the FCC implemented a new standard but this was also quickly ruled unconstitutional, leading to another period during which the FCC did not attempt to mandate or enforce adherence to EEO guidelines. In 2003 the FCC again promulgated regulations that required stations to submit information on their minority recruitment efforts and, again, compliance was tied to license renewal. These changes present several natural "regimes" of EEO laws, summarized in Table 2.6.

While I do not have data on station racial composition over time, I do know when most current employees were hired. In order to get an idea of the importance of FCC enforcement of EEO laws, I estimate a set of regressions of the percent of employees who are black and hired during a particular regime ($pct(black * regime_{j=i})$) on market racial composition. The regression is of the form

$$pct(black * regime_{j=i}) = dmapctblack\boldsymbol{\beta_1} + pct(black * regime_{j\neq i})\boldsymbol{\beta_2} + \boldsymbol{\epsilon}. \quad (2.17)$$

β_1 measures the relationship between the market composition in 2003 and station composition during each regime. β_2 is included to attempt to minimize the impact of these regressions being based only on employees who were both hired during a particular regime and are presently at the station. For instance, the percent of employees who are black and who were hired before 1987 could be low because of attrition. In that event, there would be more blacks hired during later regimes to replace those lost previously, which would be picked up by the second coefficient, preventing the impact of the city racial composition from being biased downward.

Table 2.7 reports the estimates for β_1 for regressions where the dependent variable is percent black during each of the 4 regimes, as well as separate regressions for Hispanics and Asians.[16] Looking at the results for blacks, the impact of the racial composition of a market on the hiring practices of a station increases between regimes 1 and 2 as FCC enforcement becomes stricter, decreases with Regime 3 and the period of lax enforcement, and increases again with Regime 4 and the implementation of new rules. The R^2 for each of the regressions for blacks also increases with stricter regimes. A similar trend is evident for Hispanics and Asians, suggesting that market composition has more power in explaining station composition during periods of stricter EEO regulations and enforcement. This evidence, therefore, does indicate that FCC enforcement of EEO regulations in the local television market has had an impact on station hiring practices. However, even during recent periods of confusing or absent enforcement, there is still a significant and positive relationship between the racial composition of a city and minority hiring at stations, indicating that while it does place some constraint on stations, EEO enforcement is not the only driving force behind minority hiring.

Customer Preferences

Ratings regressions measure the effect of employee characteristics on customer de-

[16] Only 2 blacks and no Hispanics or Asians were hired by their station before 1970, so the analysis does not include the period before 1970

Table 2.7: **Impact of DMA Composition on Minority Hiring over Time**

	Blacks		**Hispanics**		**Asians**	
dependent variable:	dma pctblack	R^2	dma pcthispanic	R^2	dma pctasian	R^2
$pct(regime_1 * race)$	**0.26**	0.14	0.03	0.09	0.05	0.06
$pct(regime_2 * race)$	**0.43**	0.32	**0.20**	0.45	**0.31**	0.24
$pct(regime_3 * race)$	**0.21**	0.20	**0.12**	0.31	**0.20**	0.15
$pct(regime_4 * race)$	**0.68**	0.40	**0.25**	0.50	**0.43**	0.23

**bold coefficients are significant at 10% level*

mand.[17] Regressions are of the form

$$rating_{ijt} = \alpha + \boldsymbol{S}_{ij}\boldsymbol{\beta}_1 + \boldsymbol{N}_{ijt}\boldsymbol{\beta}_2 + \upsilon_{ijt}, \tag{2.18}$$

where $rating_{ijt}$ is the rating of show i in market j at time-slot t, $\boldsymbol{S}_{ij}$ is a vector of containing the characteristics of the station, and $\boldsymbol{N}_{ijt}$ is a vector containing the characteristics of that particular newscast. It is plausible and indeed, Hausman specification tests indicate that there are certain unobserved effects that are particular to a time slot in a given market and are correlated with the regressors. For instance, if minorities are indeed assigned to slots that are expected to receive low ratings, then the estimated effect of racial composition may be downward biased. In an attempt to account for this possibility, the specification incorporates market and time fixed effects. In other words, the estimates are based on differences in outcomes for newscasts in the same market at the same time of day (e.g., early morning weekdays, etc.).

Table 2.8 reports the results for the above model using a matching specification to measure station composition. The racial and gender composition of a station is measured relative to the same characteristics of the market.[18] These weighted variables

[17]Another option would be to use *share* as the dependent variable rather than *rating*. Because *share* measures viewership only as a fraction of those watching TV, this would essentially measure preferences conditional on turning the television on at all, while *rating* is unconditional on this. If, for instance, customers are prejudiced against blacks and tend not to watch television when blacks are on, the effect of race may be underestimated. In this instance, the results of regressions using *share* are only a little smaller than the results from the ratings regressions.

[18]*Age* is not weighted because Nielsen did not provide detailed age characteristics of markets

can be thought of as matching measures; the variables *blackmatch*, *hispanicmatch*, *asianmatch* and *femalematch* are the ratios of station composition to market composition multiplied by 100. So, if *blackmatch* is equal to 100, the station exactly matches the black composition of the city while if *blackmatch* is less than 100 the station under-represents blacks and if *blackmatch* is greater than 100 the station over-represents blacks. Using the matching specification allows for the possibility that the effect of adding blacks is different in markets with different racial compositions. In addition to these variables measuring the overall composition of journalists at a station, there are also indicators for the sex and minority status of the news, weather, and sports anchors on the particular newscast being observed.[19]

Turning to the results, only 2 of the 6 anchor variables, which are matched to each newscast, are significant. The significant estimates suggest that if a male news anchor is replaced with a female one, ratings decline by 0.61 percentage points and if a white weather anchor is replaced with a minority one, ratings decline by 0.51 percentage points. The insignificance of the remaining anchor variables could indicate that the effect of the specific anchors on any one show is not large; or, it could be a product of the small sample size.

The variables measuring overall station composition, however, do suggest that customers have preferences for employee characteristics over all of the dimensions examined. The coefficient on *pctblond* suggests that a 1 percentage point increase in the percent of employees who are blond yields ratings that are 0.1 percentage points higher.[20] Because specification tests indicate that cubic functions are appropriate for the remaining composition variables, the matching variables for average station characteristics were entered along with their squares and cubes. Figure 2.1 shows the

[19]More detailed indicators for anchor team composition were also considered so that,for instance, a solo white male anchor could be compared to a black male/white female duo and so on. However, these more detailed indicators were jointly insignificant.

[20]Interacting hair color with sex did not indicate that there was a significant difference in how customers respond to blond women relative to blond men. However, the sample of blond men was much smaller.

Table 2.8: **Fixed Effects Ratings Regressions**

dep. variable: average Nielsen rating for November 2003

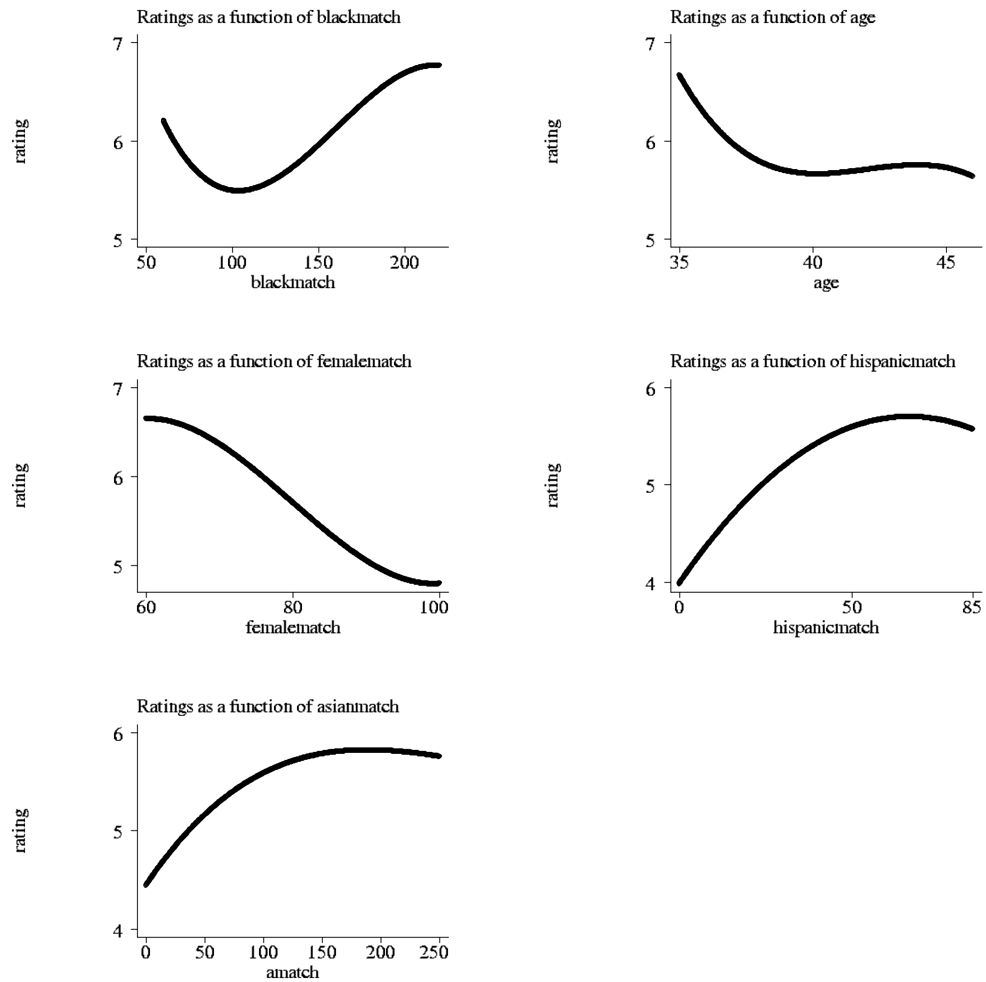
	coef	se		coef	se
Employee Characteristics			*Anchor Characteristics*		
pctnative	**-0.021**	0.009	sport anch	**1.164**	0.287
pcthighed	-0.019	0.013	duo	-0.203	0.237
tenure	**0.369**	0.062	trio	$1.1 \text{x} 10^{-4}$	0.504
experience	-0.015	0.029	min. anchor	0.178	0.156
no. stations	**0.633**	0.301	female anchor	**-0.610**	0.356
pctblond	**0.122**	0.019	min. weather	**-0.507**	0.247
age	**-18.380**	9.245	female weather	-0.236	0.173
age^2	**0.434**	0.224	min. sports	0.229	0.236
age^3	**-0.003**	0.002	female sports	-0.111	0.527
female match	**1.149**	0.295	*Station Characteristics*		
female match^2	**-0.015**	0.004	NBC	**2.305**	0.256
female match^3	$\mathbf{6.4x10^{-5}}$	$1.8\text{X}10^{-5}$	CBS	**1.468**	0.287
black match	**-0.118**	0.018	ABC	**2.279**	0.280
black match^2	**0.001**	$1.2\text{X}10^{-4}$	length	**0.254**	0.054
black match^3	$\mathbf{-1.74x10^{-6}}$	$2.48\text{X}10^{-7}$	pct emmys	**0.024**	0.004
hispanic match	**0.052**	0.015	post network	**-0.823**	0.211
hispanic match^2	$\mathbf{-3.9x10^{-4}}$	$2.2\text{x}10^{-4}$	pre network	0.305	0.188
hispanic match^3	$-2.3\text{x}10^{-8}$	$8.0\text{x}10^{-7}$	no. persons	0.011	0.021
asian match	**0.018**	0.005	min. manager	**-0.544**	0.317
asian match^2	$\mathbf{7.1x10^{-5}}$	$2.6\text{x}10^{-5}$	female manager	**0.469**	0.207
asian match^3	$\mathbf{8.5x10^{-8}}$	$3.3\text{x}10^{-8}$	constant	**228.363**	125.662

**bold coefficients are significant at 10% level* estimated mean of a show's rating conditional on the five composition variables.[21] Looking at the graph for the effect of the match between station percent black and market percent black, ratings initially decline as black representation at a station increases but, as station composition begins to exceed market composition, ratings develop a positive relationship with black composition. A similar result holds for age, although it appears that the negative effect of age diminishes as average age increases but does not exhibit a strong positive trend. For gender composition, there is a negative but decreasing effect of adding women. The shapes of these functions are not consistent with Becker's model of customer discrimination in which diminishing marginal utility would cause the negative impact of race to increase with minority representation. They are also not explained by a taste for diversity, since this would suggest that the functions would be concave. However, the decline in the negative response as composition increases is consistent with the theory of station differentiation in which stations sort in response to customer prejudices against blacks, women, and older workers, yielding $p_1 < p_2$. The results for blacks, for example, suggest that those stations with a relatively low composition of blacks are catering to consumers with a high discrimination coefficient, while those stations with more blacks are catering to consumers who are without such strong prejudice against blacks or even have preferences for them. However, if the black composition variable becomes high enough, ratings will begin to decline again, suggesting that at very high levels of representation (when there are more than 2 and a half times as many blacks on the station than in the market, which is only observed for a few outlying stations and hence not shown on the domain of the graph), even the less prejudiced consumers begin to exhibit a strong response.

Turning to the remaining two graphs in Figure 2.1, the results indicate that at least some consumers have a preference for Hispanics and Asians. Looking at Hispanics, ratings increase over low levels of composition but decline with the addition

[21] The functions are evaluated over the 10^{th} to 90^{th} percentiles of the independent variables that are observed in the data.

Figure 2.1: **Effects of Employee Compositions on Ratings**



of more Hispanic employees when *hispanicmatch* exceeds 60. Similarly for Asians, ratings increase with Asian representation among employees until that representation exceeds Asian composition in the market. The concavity of these functions is not consistent with the model of differentiation, but instead could suggest a taste for diversity in which consumers prefer to see about half as many Hispanics and about the same number of Asians on television as are in the market. However, as mentioned earlier, Hispanics may have been categorized incorrectly more often than other groups. If, for instance, Hispanics employees in markets with fewer Hispanics were less likely to acknowledge or point out their ethnicity than were employees in more Hispanic markets, this could bias the estimates. Finally, stations that broadcast in Spanish or other languages are not included in this sample. It could simply be the case that these stations cater to customers with the greatest taste for seeing Hispanic or Asian minority groups.

In addition to measures of the characteristics of the on-air staff, indicators for a female and minority manager were also included in the ratings regressions as a sort of check on the results. Assuming that managers are much less visible to customers than the on-air journalists, their race or sex should not have an effect on ratings. However, the estimates suggest a positive and significant effect for female managers and a negative and significant effect for minority managers. If these managers were visible, the results could indicate some preference of customers. An alternative explanation might be that there is some fundamental difference in the productivity of the two groups in these positions. Or perhaps these two groups are assigned to different types of stations, so there is endogeneity here. In either event, it is possible that the results for the on-air cast members could also be picking up some effect other than strict racial preferences.

2.5 Conclusion

Understanding the role and nature of customer discrimination is an important component of understanding differential outcomes in labor markets because, to the extent that customer preferences drive outcomes, competition will not necessarily eliminate discrimination. However, empirical studies have tended to focus on a single market–that for sports– and, even within this somewhat limited arena, have provided a variety of findings.

This chapter presents a look at another labor market and a different group of employees in order to infer the preferences of customers there. While the initial motivation was driven by the observation of a high number of minorities on local television news and by an attempt to see if customers might have preferences for some level of diversity, a closer examination of the local news market provided evidence of a previously unconsidered phenomenon on the employer side: local stations appear to be sorting along racial lines. While very few television stations are all white (most likely as a result of FCC enforcement of EEO laws), there is significant variation in racial composition within a market that is masked by averages. The theoretical model presented in Section 3 demonstrates that if customers have a distribution of preferences for the composition of firms, firms will compete through racial differentiation.

The empirical findings support this model and suggest another layer of complexity to consider with the basic Becker model of customer discrimination. Local news stations appear to respond to the racial composition of their competitors and to try to differentiate themselves by race, age, and sex of their on-air employees. For three of the five groups examined, the ratings regressions indicate that the response of consumers varies with the racial composition of the firms in a manner consistent with the predictions of this model of racial differentiation. Viewers of the more "white" stations have a stronger negative reaction to an increase in blacks than do viewers of the "black" stations, suggesting that stations with few blacks cater to consumers with a high discrimination coefficient against blacks, while stations with more blacks cater to customers who are less prejudiced or who prefer blacks. Similarly, viewers of stations with more females and older employees have a smaller negative response to these groups than viewers of stations with lower concentrations. While the results for Hispanics and Asians suggest a customer preference for diversity rather than differentiation or strict racial preferences as originally modeled in the literature, it seems likely that the exclusion of foreign language local news has biased the estimates. Taken as a whole, the results here suggest that customer discrimination may be a more complex phenomenon than we have previously considered.

Chapter 3

Understanding Racial Differentials in U.S. Housing Prices

3.1 Introduction

Research on racial housing price differentials has yielded vastly different results ranging from indications in the early literature that black households pay premiums for housing to estimates of significant discounts in the more recent literature. This decline and reversal of the differential might be due to a reduction in discriminatory practices over the past forty years. However, differences in estimation techniques and data sets may also explain some, perhaps all, of the perceived decline.

The key to identifying the results of discrimination is to ask whether blacks pay different amounts than whites for *identical* housing. This requires controlling not only for characteristics of the house itself, but also for characteristics of the surrounding neighborhood. Controlling for neighborhood effects is important for two reasons. First, as described below, economic theory predicts that discrimination can produce price differentials within a neighborhood, while prejudice and segregation produce price differentials between neighborhoods. Thus, if neighborhood characteristics are not controlled for, these forces will be confused and it will be impossible to separate the causes of an observed racial price differential. Second, evidence suggests that black neighborhoods tend to have relatively higher crime rates, lower wealth, poorer provision of public goods, and other negative characteristics.[1] Since being black is correlated with living in a black neighborhood, a researcher who does not control for neighborhood characteristics may find that blacks tend to pay less for housing than do whites. Such a result would be biased by neighborhood quality and would not reliably indicate the presence or absence of discrimination.[2]

In relatively recent studies such as Chambers (1992) and Kiel and Zabel (1996), researchers have typically used large national data sets and, if they controlled for neighborhood characteristics at all, have used census tracts or larger areas as neighborhood proxies. Census tracts, the smallest areas that have been used, have between 1,500 and 8,000 inhabitants, with an optimum given by the census bureau of 4,000 inhabitants (United States Census Bureau, 2000). Given that the Census Bureau (2001) reports that the average number of people per household in the U.S. was 2.62 in 2000, a census tract with 4,000 inhabitants would represent about 1,527 houses or about 100 city blocks of 15 houses each. Although the Census Bureau intends them to be proxies for neighborhoods, it seems likely that there is still substantial variation within tracts with such a large number of houses. The results of studies using these proxies indicate that blacks receive price discounts relative to whites. However, since neighborhood racial composition and other amenities may have been insufficiently controlled for, this negative finding could be due to the tendency of blacks to live in lower-priced black neighborhoods rather than due to the absence of discriminatory behavior of suppliers.

This chapter attempts to remedy this problem by controlling for neighborhoods

[1] See, for example, Harris (1999).

[2] It can certainly be argued that the poor quality of some black neighborhoods is a result of racism in the housing market. However, in order to clearly identify how different factors affect price differentials it is necessary to separate these effects from those of pure discrimination in which blacks pay different prices for identical housing.

at a much smaller level than that of census tracts. The 1985, 1989, and 1993 national American Housing Surveys (AHS) contain a special "neighbors sample" that is composed of sub-sampled "kernel" housing units and observations about the ten nearest neighbors of each. Using these data, I control for the racial composition, educational attainment, income levels and other characteristics of neighborhoods defined by relatively small areas that should be more homogenous than census tracts. By using information about the composition of the neighbor group surrounding each household, I hope to more thoroughly control for neighborhood effects, and thus be able to separate any racial price differential into portions that are (i) due to neighborhood effects and (ii) due to supplier discrimination. I also deviate from previous studies, which have used only cross-sectional techniques, and take advantage of the time series characteristics of the AHS. Once neighborhood and other unobserved effects are controlled for, a finding that blacks pay less than whites for identical housing would corroborate other recent studies in indicating that supplier price discrimination against blacks is not present. On the other hand, a finding that blacks pay more than whites for housing would indicate that bias due to unobserved effects has tainted other recent results and that price discrimination continues.

The remainder of this chapter is organized as follows. Section 2 reviews economic theory on racial housing price differentials. Section 3 describes how previous empirical studies have attempted to separate the types of differentials predicted by this theory. I proceed with a description of the data set used in this study, an outline of the econometric model, and a description of the results. Section 7 concludes with a summary of findings.

3.2 Theoretical Models of Racial Housing Price Differentials

Before outlining the three major theoretical sources of racial housing price differentials, it is important to distinguish between *discrimination*, *prejudice*, and *seg-*

regation. These terms not only mean different things, but their presence also has different implications for housing market outcomes. Prejudice is defined as an attitude about a group of people; it does not necessarily result in direct adverse action against that group. Discrimination, on the other hand, is some action or treatment of a group that differs from that received by others. Finally, segregation is the physical separation of two groups. Economic theory suggests three major sources of racial price differentials: supplier price discrimination, consumer prejudice, and segregation. The impact of these factors on prices varies in direction and in whether the differential would be observed within or between neighborhoods. I describe each factor in turn.

Supplier Price Discrimination

First, price differentials could result from supplier price discrimination. Applying Becker's (1957) approach to discrimination, if suppliers such as landlords, real estate agents, and owners do not like to deal with blacks and will do so only if they receive a premium, then one would expect this discriminatory behavior to result in blacks paying more for a comparable unit of housing than whites. This is an instance of price discrimination by suppliers and its effects can be captured by comparing the prices that blacks and whites pay for identical housing. However, if blacks and whites do not tend to live in the same neighborhoods, they may not consume identical housing since neighborhood is a key component of housing services. So, in order to attempt to find evidence of supplier price discrimination, it is necessary to control for neighborhood characteristics and effects. Once this is done, the effects of supplier price discrimination can be captured by including dummy variables for the race of the household head. The coefficient on a dummy variable indicating that the household head is black, for instance, would measure any difference in the price paid by a black household relative to that paid by a white household for otherwise identical housing.

Prejudice

Two well-known models demonstrate how private preferences for racial composition can create inter-neighborhood price differentials. Bailey's "border model" assumes that blacks and whites are segregated into black central city neighborhoods and white suburban neighborhoods (Bailey, 1966). It also assumes that both blacks and whites prefer living in white neighborhoods. Competition ensures that the prices that blacks and whites pay for housing in the border areas of their neighborhoods will be equal. Furthermore, because whites prefer to live far from blacks, whites will pay more for housing in the white interior than in the border areas. And because blacks prefer to live close to whites blacks will pay less for housing in the black interior than in the border area. Combining these results, in the absence of discrimination, the model predicts that prices in the interior of black neighborhoods will be lower than prices in the interior of white neighborhoods and that prices in border areas will be intermediate.

Courant and Yinger (1977) provide an overview and criticism of border models. They describe two major flaws. First, to ensure that whites and blacks remain segregated in equilibrium, restrictive assumptions must be added to the model. Either blacks must be indifferent to racial composition or all whites must be willing to pay more than any black to live in a white neighborhood.[3] Otherwise, there is nothing to keep blacks from simply moving into the white neighborhoods that they prefer, which would contradict the assumptions of the model. Another problem is that this model does not have an equilibrium solution if black incomes can vary. It is possible for a wealthy black demander to outbid a poor white demander for housing in the white interior.

[3]The assumption that blacks are indifferent to racial composition seems especially strong given the evidence against it. Researchers have used survey data on neighborhood preferences and have found that whites tend to prefer living in predominantly white neighborhoods and that blacks tend to prefer integrated areas. See for instance, Farley et al. (1997). On the other hand, it is unclear to what extent respondents incorporate neighborhood characteristics that are correlated with race. For example, a white respondent who expresses discomfort with the idea of living in a predominantly black neighborhood may do so not because he dislikes the idea of living near blacks but because he considers that there are likely to be fewer and poorer quality services provided in such a neighborhood.

Yinger (1976) revised Bailey's border model. In border models whites and blacks are completely segregated and whites derive disutility from living near the black/white border. In the Yinger model whites and blacks derive disutility or utility from living with and near the other race. This key difference in how prejudice enters the utility function allows Yinger to abandon the restrictive assumption of border models that a city starts off completely segregated and instead to model whites and blacks as caring about the racial composition of the area in which they live. Yinger shows that the coefficient on racial composition in a regression on value or rent will measure the effects of prejudice in this model. If the coefficient on a variable measuring the percent of neighborhood occupants who are black is negative, then there is evidence that prejudice against blacks lowers prices in neighborhoods as the proportion of black inhabitants increases. Since the degree of prejudice and, hence, the magnitude of such a coefficient could differ across different neighborhood types, Yinger suggests that racial composition should be interacted with dummy variables measuring whether a neighborhood is predominantly white, black, or integrated.

What Yinger does not discuss is that if one includes both observations for white and black households, then the coefficient on racial composition will measure a combination of white and black racial preferences. In this case, the coefficient on racial composition tells us what the effect of prejudice is, but does not divide the price differential into portions that are a result of white and black attitudes. Hence it is preferable to also interact racial composition with the race of the household head to fully separate the effects of black and white prejudice. However, due to data limitations, this will not be done in this chapter, leaving the source of any measured prejudice unclear.

Segregation

Segregation describes the physical separation of two groups. It may be the result of supplier price discrimination limiting the housing choices of blacks or of prejudicial attitudes among consumers that lead to an equilibrium separation. If either of these is the case, segregation would be the result of the two sources of price differentials already described. However, there are other possible sources of segregation. It could also result from non-price discrimination such as the steering of black home-seekers to certain neighborhoods, red-lining mortgage applications, government confinement of a group to ghettos, or other methods of threat or coercion that limit the neighborhood choices of a group. Also, the exclusion may not be based strictly on race. For example, it could also be the case that "poor" housing is restricted to a small area and that blacks tend to live in poor areas.

Whatever the source of the segregation, economic theory predicts that the exclusion itself could generate inter-neighborhood housing price differentials.[4] Suppose that blacks are trapped in a central ghetto. If the boundaries of black neighborhoods are relatively fixed, increasing population pressures within them would result in higher prices in black neighborhoods than in white ones. A finding that *ceteris paribus* black neighborhoods tend to have more expensive housing would provide evidence that a combination of exclusion and population pressures are pushing prices upward in black neighborhoods.

This simple prediction about the direct effect of exclusion, however, is complicated by consideration of its source. If segregation is the result of supplier price discrimination, one would expect to find that blacks pay more than whites for housing in white neighborhoods, but that housing prices in black neighborhoods are higher than those in white neighborhoods. In other words, one would find evidence of intra-neighborhood differentials and inter-neighborhood differentials. In this case, of course, the source of both differentials would be supplier discrimination, but the effects of such discrimination would have been separately identified. Similarly, if segregation is the result of prejudice, one would expect to find that housing values fall with increases in the composition of blacks in a neighborhood, but that black neighborhoods tend to cost more than white neighborhoods. Cutler et al. (1999), Ihlanfeldt and Scafidi (2002), and Ross (2002) have focused on explaining the sources

[4]See Yinger (1978) and King and Mieszkowski (1973).

of segregation. This chapter follows a different tack by attempting to identify the direct effects of supplier price discrimination and demander prejudice and also including the effect of segregation only in so far as it can create intra-neighborhood population pressures. The source of segregation is not identified. Note that these three explanations (price discrimination, prejudice, and segregation) for racial price differentials have different implications. Price discrimination is the only theory that predicts intra-neighborhood racial price differentials. Exclusion and prejudice predict inter-neighborhood price differentials between neighborhoods of differing racial composition. Hence, if one wishes to examine racial differentials in housing prices it is necessary to control for the racial composition of the neighborhood within which a house is located. Only then is it possible to separate inter-neighborhood and intra-neighborhood outcomes and obtain evidence about the presence and nature of discriminatory behavior, exclusion, and prejudice.

3.3 Previous Empirical Literature

The first empirical studies of racial differentials in housing prices tend to use small data sets with information on the racial composition of the immediate block surrounding a household. King and Mieszkowski (1973) employ special survey data from 1968-1969 for approximately 220 rental units in New Haven, Connecticut that allow them to control for a renter's race as well as for the racial composition of a neighborhood and whether the neighborhood is in what they term the black ghetto, white interior, or boundary areas. The authors find that blacks pay about 7 percent more than whites in the boundary areas, evidence of supplier discrimination. Yinger (1978) finds similar results using 1967 data on St. Louis. He estimates that blacks pay approximately 14 percent more than whites for housing in any given neighborhood. He also finds that black neighborhoods tend to be more expensive and that increasing the percentage of black residents in any given neighborhood lowers housing prices there. This indicates both that exclusion may play a role in increasing prices in black neighborhoods and that prejudice decreases prices in any given neighborhood as more blacks move in.

Studies such as King and Mieszkowski's and Yinger's provide careful controls for neighborhood composition but are limited by small data sets for specific cities and the results are now dated. Follain and Malpezzi (1981) attempt to address the former problem by using metropolitan data for 39 SMSA's from the Annual Housing Survey (AHS). Running separate regressions for owners and renters, the authors find that black owners pay an average of 15 percent less and black renters pay 6 percent less than whites, a result that is dramatically different from previously estimated premiums. However, the only neighborhood variable that may control for racial composition is a central city dummy. While it is possible that the premiums estimated in earlier work were reversed in later years, it seems more likely that the omission of controls for neighborhood racial composition have biased the results.

Other authors have attempted to combine the large and comprehensive data provided by the AHS with more stringent neighborhood controls. Chambers (1992) uses special AHS data for Chicago that includes information on the characteristics of 24 zones that were made up of groups of census tracts. Kiel and Zabel (1996) use AHS data for Chicago, Denver, and Philadelphia to isolate individual census tracts and control for racial composition. Both studies demonstrate that the price discounts estimated for blacks are greatly overstated when neighborhood racial composition is not a control. Once a variable measuring the racial composition of neighborhoods is introduced, Chambers estimates that the discount for renters disappears and the discount for owners declines to 12 percent while Kiel and Zabel estimate that the discount for owners decreases to 5 percent. However, Kiel and Zabel point out that their census tract proxy is likely too broad to fully control for neighborhood characteristics and suggest that more exact controls for a smaller area could possibly eliminate the perceived discount altogether.

In fact, all of the recent literature on racial housing price differentials either has no neighborhood controls or controls that researchers suspect are too broad. The smallest geographic area that has been used in recent research has been the census tract and, as the authors themselves suggest, this is likely too large an area to be a good proxy for neighborhood (Kiel and Zabel, 1996).

Thus far the literature discussed has focused on separating racial price differentials into portions caused by discrimination, prejudice, and segregation. More recent work has examined the sources of segregation, including discrimination and prejudice. Cutler et al. (1999), use census data to show that segregation in the United States declined between 1970 and 1990. The authors then compare how the black/white differential in average prices for housing varies across cities as the level of segregation varies. They find that earlier in the 20th century, blacks tended to pay more in cities with higher levels of segregation. This indicated that segregation was the result of discrimination or a preference on the part of blacks for segregation from whites. However, by 1970 the differential had decreased and by 1990 it had reversed: blacks tend to pay less than whites for housing in more segregated cities. This provides evidence that white prejudice (but not direct discriminatory action) has replaced discrimination and/or black preferences as the driving force behind segregation. This finding is supported by Ihlanfeldt and Scafidi (2002) who use special survey data for Atlanta, Detroit, and Los Angeles to show that black preferences for segregation do not explain a large portion of observed segregation. However, it is important to note that Cutler et al. are using broad city-level measures of prices and segregation. In the event that the level of segregation in a city is correlated with unobserved quality differences, the results will be biased. This is troublesome because previous literature has already suggested that predominantly black neighborhoods tend to have poor amenities relative to white neighborhoods. So increases in segregation might be associated with decreased in housing quality for blacks relative to whites.

Ross (2002) uses 1985 AHS data for the city of Philadelphia to examine choice of community. He finds that differences in socio-economic characteristics and preferences for neighborhood amenities explain some of the racial differences in community choice and resulting segregation. However, a large portion of segregation is explained by differences in how neighborhood racial composition affects the community choices of whites and blacks. However, as Ross points out, it is unclear whether this is due to racial differences in preferences for composition or due to discrimination that limits available options.

In summary, the evidence that the discrimination premium paid by blacks has declined and reversed may be tainted by omitted variable bias. However, evidence on the causes of segregation does point to white prejudice, so it is not clear that discrimination continues to play a role in housing markets. A better understanding of the components of racial price differentials will help to identify and separate the effects of discrimination, prejudice, and segregation.

3.4 Data

The American Housing Survey (AHS) has been conducted by the United States Census Bureau for the Department of Housing and Urban Development (HUD) since 1973.[5] The data are collected by interviewers who travel to each of approximately 55,000 housing units to ask questions about the characteristics of the housing unit and occupants. The same address is visited each year until a new sample is drawn. Hence the panel consists of observations of a housing unit over time, but the occupants may change. Due to confidentiality restrictions, the public files of the AHS do not reveal the geographic location of a unit below the MSA level. Therefore it usually is not possible to generate information about the racial composition of the neighborhood surrounding a household or other characteristics of the neighborhood that are not included in the data.

The special neighbors samples taken in the 1985, 1989, and 1993 national AHS provide an alternate proxy for neighborhood. In 1985 approximately 680 units were sub sampled from the full AHS sample. These form the "kernel" units of

[5] Prior to 1984 the survey was known as the Annual Housing Survey.

the neighbors sample. The 10 nearest neighbors of each kernel unit were then also interviewed, and the kernel unit and its neighbors form a "cluster." These neighboring units had to be within a mile of the kernel unit and could not be separated from it by a discontinuity such as a bridge, river, railroad tracks, interstate highway, etc.[6] Hence, in the case of a relatively densely populated area, the ten nearest houses are observed but, if a kernel unit is more remote from neighboring houses, there may be fewer than ten neighboring units. In 1989 and 1993 most of the clusters from 1985 were re-interviewed and new clusters also were added in each year. In addition, if new housing was added through construction or some other means to the geographic area that had comprised the original cluster, the new housing was added to the interview. Hence, in 1989 and 1993 some of the clusters contain more than 11 housing units.

In the data used in this study, there are 648 to 936 clusters in each year.[7] The clusters, with an average of 9.6 houses and 26 inhabitants, are considerably smaller both in area and population than census tracts or groups of census tracts. Every household within a neighborhood cluster is considered part of the sample and I use the average characteristics of the cluster to which each household belongs to proxy for neighborhood characteristics.[8] The total sample consists of 21,712 observations of housing units of which approximately 13,000 are owner-occupied and 8,500 are rented.

The racial characteristics used here are what the interviewee ("reference person") reports for him or herself. The reference person could classify himself as white, black,

[6]Thanks to Barbara T. Williams of the U.S. Census Bureau for information on the neighbors sample.

[7]Any observations that both were not owned and reported no cash rent were dropped. Observations that did not report the race of the inhabitants, were non-interviews because the unit was vacant or for other reasons, or were part of a cluster with less than five houses also were dropped.

[8]The kernel unit is usually roughly at the center of the cluster. Therefore, the characteristics of the cluster may be a better proxy for the surroundings of the kernel unit than for the surroundings of other units in the cluster. For this reason, it would be ideal to only include observations on kernel units in the analysis, but, because the resulting sample size would be very small, all units are in fact included in the sample. However, because the cluster is a small area, it seems likely that the characteristics of the cluster are acceptable proxies for the neighborhood surrounding all units in the cluster.

or other, with other including American Indian, Chinese, Korean, etc.. Note that these classifications are strictly based on the respondent's perception of his own race, and not on ethnicity. Therefore, black Hispanics should fall into the category "black," white Hispanics should fall into the category "white," and Hispanic of native American origins should fall into the category "other." Although it is possible to identify Hispanics in the data, the distinction is not used here. Race and ethnicity are certainly nebulous and changing concepts and there is room for debate and interpretation of to what group a person belongs. However, for the purposes of this paper, we are concerned strictly with race and do not add the complication of trying to separate ethnicity. There is also the implicit assumption that it is to what group the respondent believes he or she belongs that determines his race.

Neighborhood Characteristics and Definitions

The AHS includes several questions about neighborhood quality. Respondents are asked if crime is a problem and if there is something bothersome about the neighborhood. The enumerator also records observations about the surrounding of a unit including nearby abandoned structures and bars on the windows of nearby houses. The averages of observations of these characteristics are constructed for each neighborhood as measures of amenity levels. Because there are many missing observations for some of the neighborhood quality variables, only indicators of neighborhood problems, crime, bars on windows, and abandoned houses are used in later regressions. Variables that control for neighborhood quality are also constructed using the characteristics of houses within the cluster. The median household income, median number of years of education of the reference people in a cluster, and the percents of black, white, and other inhabitants in a cluster are calculated.

Once respondents are classified according to race and the racial composition of each neighborhood is measured, each neighborhood is further classified by submarket as a "white neighborhood", "black neighborhood", or an "integrated neighborhood." A complication in this classification is how to treat the racial group "other," which makes up just under 4 percent of the total sample. This chapter is primarily concerned with the outcomes of blacks relative to non-blacks. Therefore, others and whites are combined and white neighborhoods are composed of at least 85 percent white and other households (15 percent or less black), black neighborhoods are composed of at least 30 percent black households, and integrated neighborhoods are composed of between 15 and 30 percent black households.[9, 10] These neighborhood definitions were selected to be similar to the definitions used in the previous literature, which tend to define black neighborhoods at a relatively lower black concentration than the white concentration used to delimit a white neighborhood. In addition, when the fully specified hedonic regressions used in the econometric analysis were run with all possible combinations of cutoff levels for neighborhood types (in increments of 5 percentage points), the levels that minimized the sum of squared errors were near the levels used here. However, the cutoff levels that resulted in the lowest sum of squared errors defined integrated neighborhoods as having only 5 to 10 percent black, which was too narrow a band to be useful for analysis. The sum of squared errors of the specification used here is close to that minimum and the definitions in use allow a broader range of racial composition within integrated neighborhoods.

While census tracts and larger areas are likely too-broad proxies for neighborhood, there is some danger that using clusters as neighborhood proxies goes too far in the other direction. There is a particularly large chance of misclassifying an integrated neighborhood because the boundaries of such a neighborhood are narrow. There is also more error for neighborhoods that are near the boundaries for their classification, such as a white neighborhood with 14 percent black inhabitants. This problem could introduce error to the coefficients on neighborhood type which help to measure the effects of exclusion. However, this problem is likely less severe than that introduced by using proxies that are too large and hence not homogenous

[9] The outcomes reported are not greatly different if blacks and others are combined instead.

[10] Note that the race of the reference person is used to measure the race of the household

because the these can introduce bias to all of the racial coefficients of interest.

Rent and Value

Measurements of rent and value also merit special attention. Rent is the respondent's report of monthly contract rent. It is a direct measure of what renters pay for housing. Value, on the other hand, is the owner's estimate of the fair market value of his or her home. It is possible that people incorrectly estimate the values of their homes. But, Kiel and Zabel (1999) have shown that while people do tend to overestimate the value of their homes, there is no evidence that one group tends to have any greater bias in their estimates than another. The only exception to this is that new home owners tend to overstate the value more than those who have owned the home for a while. Therefore, once length of tenure is controlled for, overestimates of value should not bias the results since respondents tend to uniformly overestimate fair market value.

Sample Characteristics by Race

Table 3.1 reports average characteristics of the housing, neighborhoods, and households of black, white, and other respondents. Note that since the neighborhood characteristics are constructed by averaging the characteristics of houses and households within the neighborhood, these figures are means of means and medians. The differences in neighborhood characteristics across races are striking. Relative to whites, black respondents tend to live in neighborhoods with more abandoned houses, more crime, and more respondents reporting that something is bothersome about the neighborhood. Although 13 percent of the sample is composed of black reference people, blacks also tend to live in neighborhoods that are 74 percent black and whites tend to live in neighborhoods that are only 4 percent black. Not surprisingly, black respondents people tend to live in black neighborhoods. 84 percent of black respondents live in black neighborhoods while only 8 percent live in white neighborhoods. This evidence that blacks tend to live near other blacks and that the neighborhoods that they live in have different characteristics than the neighbor-

Table 3.1: **House, Neighborhood, and Household Characteristics by Race of Respondent**

	Black	White	Other
Housing			
Median house value	60,000	90,000	190,000
Median Rent	349	451	523
Mean number rooms	5.15	5.56	5.20
Mean unit square feet	1372	1637	1515
Neighborhood			
Percent living in black neighborhoods	84.07	3.42	9.66
Percent living in white neighborhoods	7.95	92.25	83.09
Percent living in integrated neighborhoods	7.98	4.33	7.25
Mean percent black in neighborhood**	74.15	3.78	8.12
Median income in neighborhood	23,848	36,648	36,681
Median years of education in neighborhood	11.87	13.05	12.72
Mean pct of houses in neighborhood with reports of:			
bad crime	30.75	17.89	25.31
nearby abandoned structures	17.31	3.21	4.78
something bothersome about neighborhood	47.66	40.60	46.03
Household			
Percent married	35.66	54.79	58.21
Percent male	46.10	66.21	67.15
Mean age	47.54	49.55	43.92
Median household income	19,000	30,814	31,448
Mean number of occupants	2.73	2.52	3.46
Number observations	2894	77,990	828

*All monetary values are 1993 dollars.

**13 percent of sample is black.

hoods that whites live in reinforces the idea that it is necessary to control carefully for neighborhood when estimating racial price differentials.

3.5 Econometric Model

A hedonic housing price regression is used to examine how various factors affect equilibrium prices. Because demand and supply are not separately identified, it is only economic theory that leads to conclusions about the sources of different types of differentials. The reduced form regression for owners, including all racial variables and neighborhood characteristics, is

$$\begin{aligned}\ln(value_{it}) = \delta + \mathbf{X}_{it}\boldsymbol{\beta} + \alpha_b black_{it} + \alpha_o other_{it} + \delta_{ih} inthood_{it} + \delta_{bh} bhood_{it} \\ +\gamma_{pw} pctb\_whood_{it} + \gamma_{pi} pctb\_inthood_{it} + \gamma_{pb} pctb\_bhood_{it} \\ +\gamma_o pctother_{it} + \upsilon_{it}\end{aligned} \tag{3.1}$$

where $\mathbf{X}_{it}$ is a vector of characteristics of the house, neighborhood, and reference person, excluding the racial variables that are of particular interest in this case. $\mathbf{X}_{it}$ includes such variables as number of rooms and age of house and the marital status of the reference person. The remaining variables are the racial variables that identify the effects of supplier discrimination, prejudice, and exclusion. A description of all the variables included in the hedonic regressions appears in the appendix; a more detailed summary of the racial and neighborhood variables that are of special interest appears in Table 3.2. The regression for renters uses the natural logarithm of rent as a dependent variable and a few additional variables that account for rent control status and other factors.

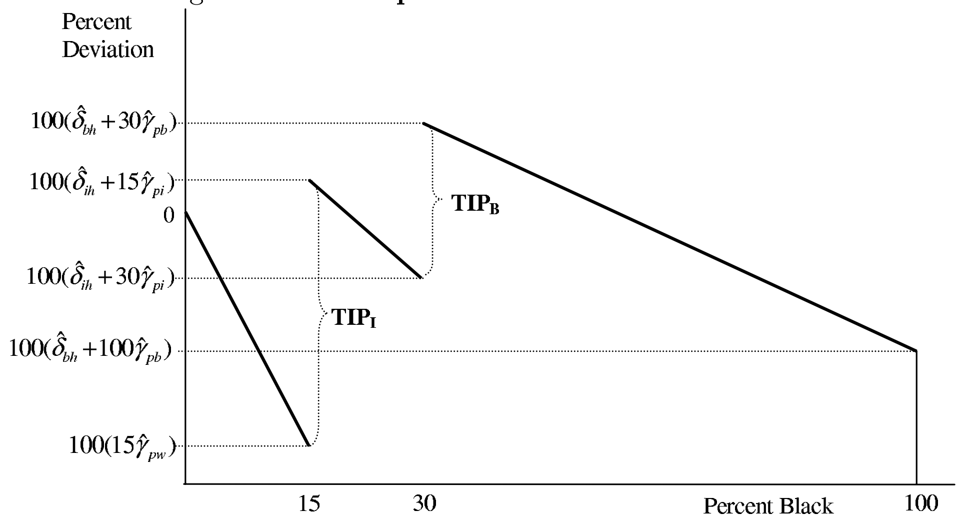
The Racial Variables

The racial variables can be thought of as terms in a racial price gradient which

Table 3.2: **Racial and Neighborhood Variables**

Variable	Description	Range
black	$I(race = black)$	{0,1}
other	$I(race = other)$	$\{0, 1\}$
pctblack	% of houses in neighborhood with black reference person	[0,100]
whood	$I(pctblack \leq 15)$	$\{0, 1\}$
bhood	$I(pctblack \geq 30)$	$\{0, 1\}$
inthood	$I(15 < pblack < 30)$	$\{0, 1\}$
pctb_whood	$pctblack * whood$	$\{0, 1\}$
pctb_bhood	$pctblack * bhood$	$\{0, 1\}$
pctb_inthood	$pctblack * inthood$	$\{0, 1\}$
mbcrime	% of respondents in neighborhood reporting bad crime in neighborhood	$[0, 100]$
maban	% of houses in neighborhood with abandoned houses nearby	$[0, 100]$
mproblems	% of respondents in neighborhood reporting something about neighborhood is bothersome	$[0, 100]$
mbar	% of houses in neighborhood with bars on nearby buildings	$[0, 100]$
lmedzinc	natural logarithm of median neighborhood income	$[7.3, 11.8]$
mededuc	median years of education in neighborhood	$\{1, 2, ..., 18\}$

Figure 3.1: **Example of Racial Price Gradient**



measures how prices change as racial composition changes. Because the regression is a log-linear model, the coefficients on the racial terms measure relative changes in prices from what would be observed in a neighborhood with no black inhabitants. Figure 3.1 represents a racial price-gradient as a function of the coefficients of interest. The y-axis measures the percent deviation in house values or rents from those values in a neighborhood with no blacks. Within white neighborhoods, where the percentage of blacks inhabitants is less than or equal to 15 percent, a one percentage point increase in the percentage of black inhabitants will change prices by $100\gamma_{pw}$ percent. Similarly, γ_{pi} and γ_{pb} measure how prices change in integrated and black neighborhoods as the percentage of black inhabitants changes within in each neighborhood. If prejudice is a factor in the market, then one expects these coefficients to be negative since increasing numbers of black inhabitants in a neighborhood would decrease housing prices there.

The dummy variables for neighborhood type, δ_{ih} and δ_{bh}, allow prices to jump between neighborhoods as exclusionary theories suggest might happen. For instance, as the percentage of blacks in a neighborhood increases to just over 15, the neighborhood will change from being defined as a white neighborhood to being defined as an integrated neighborhood. In a white neighborhood with 15 percent black inhabitants, the percent deviation in value from that if there were no blacks is $100(15\gamma_{pw})$. As the percentage black in an integrated neighborhood approaches 15 from above, the limiting percent deviation in value from that in a white neighborhood with no blacks is $100(\delta_{ih} + 15\gamma_{pi})$. The difference in these values, $TIP_I = 100[\delta_{ih} + 15(\gamma_{pi} - \gamma_{pw})]$, measures the change in the percent deviation in prices as a neighborhood changes from the lower limit of an integrated neighborhood to the upper limit of a white neighborhood. If segregation prevents blacks from moving into white areas and population pressures exert upward pressure on prices in black neighborhoods, this value will be positive. Similarly, $TIP_B = 100[(\delta_{bh} - \delta_{ih}) + 30(\gamma_{pb} - \gamma_{pi})]$ measures how the percent deviation in prices changes from the lower limit of a black neighborhood to the upper limit of an integrated neighborhood. These variables are denoted "TIP" in keeping with Kiel and Zabel (1996) and the literature that suggests that at a certain threshold point of the proportion of blacks in a neighborhood, the neighborhood type and, possibly, house prices suddenly shift. In this case the threshold points, 15 and 30 percent, shift the neighborhood types from white to integrated and integrated to black, respectively.

Finally, the coefficients on *black* and *other* are parameters that allow the racial price gradient to shift within any neighborhood according to the race of the owner or renter. The coefficient on black should capture supplier price discrimination because it measures how prices vary for blacks relative to whites for the same housing services.

Random vs. Fixed Effects

Although previous authors have used data from the American Housing Survey, they have not taken advantage of the time series characteristics of the data. Because the data form an unbalanced panel, it is possible to test for correlation between the error term and dependent variables, a concern that, as has been discussed, is of particular importance in the case of racial price differentials.

Table 3.3: **Within Changes in Racial Variables**

Variable	No. of observations that change for owners	No. of observations that change for renters
black	84	279
other	64	216
whood	202	392
bhood	84	222
inthood	224	380
pctb_whood	653	706
pctb_bhood	346	335
pctb_inthood	48	58

The consistency of both OLS and Random Effects methods depends crucially on υ_{it} being uncorrelated with the regressors. In the case that the error term is uncorrelated with the regressors, both OLS and Random Effects models will yield consistent results, but random effects will be more efficient. However, *a priori* it seems unlikely that this assumption will hold. Even with some neighborhood controls and controls for many of the characteristics of housing, there are certainly unobserved factors such as the proximity of a neighborhood to commercial centers and the design of a house that are omitted in this and other analyses. And since one suspects that many neighborhood characteristics are correlated with race, there is no reason to think that the neighborhood characteristics that are still omitted do not suffer from this problem. It is also plausible that unobserved characteristics of the house are correlated with race. For instance, perhaps blacks not only tend to live in neighborhoods with lower amenity levels, but they also tend to live in houses that are not built or designed well. So, while random effects models will be estimated and are of particular importance for comparison to the OLS models that previous authors have used, the results are suspect even before a Hausman specification test is performed.

Fixed effects models, however, offer the well-known trade off of being consistent in the presence of correlation between the error term and regressors but of relying on differences in the regressors across time for each observation. Table 3.3 summarizes the number of changes observed for certain racial and neighborhood characteristics.

The neighborhood variables tend to have several hundred observations that change, increasing the likelihood that the coefficients on these variables can be estimated with some precision. However, there are only 84 observations of addresses with a change in the race of the owners from non-black to black or from black to non-black. This lack of variation suggests that the estimated coefficients are likely to have large standard errors and that it may not be possible to precisely estimate the coefficients that indicate supplier discrimination. On the other hand, the observations that we do have offer what may be the best way to capture supplier discrimination—by comparing how house values and rents change when the race of the inhabitants of a particular dwelling changes.

3.6 Estimates and Results

Various random effects specifications were considered in order to examine how the inclusion of neighborhood controls affects estimates of racial price differentials. Table 3.4 presents a comparison of racial and neighborhood coefficients in random effects models as differing levels of neighborhood controls are added.[11] Specification 1, similar to that used by Follain and Malpezzi (1981), includes no neighborhood controls. The highly significant estimates of the coefficients on *black* indicate that black owners live in homes that have 23 percent lower values than those of whites and that black renters pay 8.3 percent less rent. These results are similar to those obtained by other authors when no neighborhood controls are in place and are not surprising given the correlation between blacks living in neighborhoods with lower amenity levels. Specification 2 demonstrates that when controls for the racial composition and neighborhood category are added, the coefficients on race dummies become insignificant and the estimates indicate that an increase in the black population in any given neighborhood results in a decrease in house values. However,

[11] Complete regression results, including coefficients on the variables included in the vector **X**, which are listed in the appendix, are available upon request. Only the neighborhood and racial variables of interested are presented here.

when controls are added for non-racial neighborhood characteristics such as crime, the coefficients on neighborhood racial characteristics decrease in magnitude, as shown in Specification 3, which is considered the "full" specification.

The results in Table 3.4 demonstrate that once neighborhood characteristics are controlled for as completely as possible, the estimated racial price differentials (the coefficients on black and other), which were large and negative in specifications without neighborhood controls, become insignificant for both owners and renters.[12] This finding supports the assertion of Kiel and Zabel (1996) that smaller geographic proxies for neighborhood may increase the estimated differential from the negative estimates that have been found in recent literature.

The results of the full random effects specification indicate that blacks do not pay different prices from non-blacks for either housing type. They do, however, support the hypothesis that prejudice causes both values and rents to decline in some neighborhoods as the percent of blacks increases. They also indicate that exclusion may drive up house values in black neighborhoods. These results are comparable to earlier research that has used OLS to estimate similar hedonic regressions. However, the use of panel data in this chapter offers the added ability to use a Hausman specification test to test the null hypothesis that the error component is not correlated with the regressors. Not surprisingly, the test statistic for the first specification, which has no neighborhood controls, is large and the null hypothesis can be rejected. However, even when neighborhood characteristics are controlled for as thoroughly as possible, the Hausman test statistic remains large and the null can be readily rejected. This result not only indicates that the coefficients in this random effects specification are biased and inconsistent, but that the coefficients in the OLS specifications used by other authors, even those that control for neighborhood characteristics, are likely to be biased and inconsistent as well.

[12] Note that in the specifications discussed, the effect of the reference person's race is not interacted with neighborhood type. A more flexible model interacts race with neighborhood type so that the effects of supplier discrimination would be allowed to vary across neighborhoods. However, in all of the specifications discussed, when race was interacted with neighborhood type, the null hypothesis that the coefficients were equal could never be rejected.

Table 3.4: **Select Random Effects Estimates**

	Owners (1)	Owners (2)	Owners (3)	Renters (1)	Renters (2)	Renters (3)
Coefficients						
mbcrime	-	-	-0.0003	-	-	**0.0008**
			(0.0003)			(0.0003)
maban	-	-	**-0.0013**	-	-	**-0.0004**
			(0.0003)			(0.0002)
mproblems	-	-	**-0.0008**	-	-	**0.0006**
			(0.0002)			(0.0002)
mbar	-	-	**0.0007**	-	-	**0.0005**
			(0.0002)			(0.0002)
lmedzinc	-	-	**0.3292**	-	-	**0.2410**
			(0.0142)			(0.0118)
mededuc	-	-	**0.0288**	-	-	**0.0180**
			(0.0034)			(0.0029)
black	**-0.2276**	0.0276	0.0235	**-0.0831**	0.0129	0.0017
	(0.0212)	(0.0341)	(0.0325)	(0.0150)	(0.0220)	(0.0212)
other	**0.2205**	0.0549	0.0401	**0.0894**	0.0290	0.0192
	(0.0351)	(0.0394)	(0.0378)	(0.0225)	(0.0262)	(0.0254)
bhood	-	-0.0276	-0.0004	-	**0.0706**	0.0534
		(0.0605)	(0.0590)		(0.0366)	(0.0354)
inthood	-	**0.2888**	**0.2391**	-	0.0485	-0.0262
		(0.1358)	(0.1328)		(0.1078)	(0.1060)
pctb_bhood	-	**-0.0037**	**-0.0031**	-	**-0.0026**	**-0.0016**
		(0.0008)	(0.0008)		(0.0005)	(0.0005)
pctb_inthood	-	**-0.0193**	**-0.0158**	-	-0.0032	0.0005
		(0.0062)	(0.0061)		(0.0049)	(0.0048)
pctb_whood	-	**-0.0092**	**-0.0009**	-	-0.0023	-0.0018
		(0.0018)	(0.0017)		(0.0014)	(0.0014)
pctother	-	**0.0073**	**0.0007**	-	**0.0025**	**-0.0024**
		(0.0007)	(0.0007)		(0.0005)	(0.0005)
Estimates						
TIP_i	-	**0.1372**	**0.1380**	-	0.0349	0.0082
		(0.0529)	(0.0515)		(0.0414)	(0.0408)
TIP_b	-	**0.1530**	**0.1480**	-	0.0409	0.0165
		(0.0664)	(0.0655)		(0.0488)	(0.0476)
Hausman test statistic	742.4	1087.2	623.2	566.6	554.6	425.2

*Dep. var.: $ln(value)$ for owners and $ln(rent)$ for renters. Coefficients in bold are significant at the 10% level.

As discussed previously, an address-specific fixed effects estimator will be unbiased and consistent in the presence of such correlation, although low variation in some variables may lead to imprecise estimates.[13] The fixed effects estimates for Specifications 3, the "full" specification, appear in Table 3.5. The fixed effects standard errors are robust to possible spatial correlation within a neighborhood in a given year. Despite concerns about low variation of many of these variables, the estimates indicate significant racial differentials for owners (but not for renters). When the full controls for neighborhood characteristics are included, the fixed effects estimate for owners of the coefficient on black is both positive and significant at the ten percent level. It indicates that blacks pay approximately 10 percent more than whites for identical housing in identical neighborhoods, providing evidence of supplier discrimination. Although this coefficient becomes significant only at the 10 percent level, it is measuring how values changed for a given house when the race of the occupants changed, which is exactly what we wish to capture. The positive value of this coefficient in the fixed effects model indicates that correlation between the black dummy and error term continued to exert downward bias in the random effects model even with neighborhood controls in place. This offers evidence that supplier price discrimination may still be a force in the ownership market but that it hasn't been reliably captured in previous studies because of bias caused by omitted neighborhood effects.

The estimates also provide evidence that prejudice causes house prices to fall as the percent of blacks in a neighborhood increases. The coefficient on racial composition in integrated neighborhoods is negative, but with a p-value of .107 is insignificant. However, the estimates indicate that a 10 percentage point increase

[13] Another possibility would be to incorporate neighborhood fixed effects rather than address fixed effects. However, Hausman-type specification tests of neighborhood fixed effects versus address fixed effects indicated that correlation was still a problem for all of the models estimated when just using neighborhood-level fixed effects. Another model that would not suffer from problems induced by correlation is a Hausman-Taylor IV random effects model. However, this requires assumptions about which variables are correlated with the error term and requires a certain number of regressors that are not correlated with the error term. There are not many likely regressor candidates that are uncorrelated and when the author tried such a model, specification tests continued to indicate that correlation was a problem.

Table 3.5: **Select Fixed Effects Estimates**

	Owners			Renters		
	(3)	(4)	(5)	(3)	(4)	(5)
Coefficients						
mbcrime	-0.0007	-0.0008	-0.0008	0.0008	0.0007	0.0007
	(0.0006)	(0.0006)	(0.0006)	(0.0007)	(0.0007)	(0.0007)
maban	-0.0005	-0.0005	-0.0005	-0.0004	-0.0005	-0.0005
	(0.0007)	(0.0007)	(0.0007)	(0.0006)	(0.0006)	(0.0006)
mproblems	-0.0003	-0.0003	-0.0003	-0.0006	-0.0006	-0.0006
	(0.0005)	(0.0005)	(0.0005)	(0.0007)	(0.0007)	(0.0007)
mbar	0.0009	0.0009	0.0009	0.0016	0.0002	0.0002
	(0.0006)	(0.0006)	(0.0006)	(0.0004)	(0.0004)	(0.0004)
lmedzinc	**0.0603**	**0.0615**	**0.0616**	**0.0909**	**0.0843**	**0.0888**
	(0.0328)	(0.0327)	(0.0326)	(0.0324)	(0.0327)	(0.0323)
mededuc	-0.0042	-0.0049	-0.0048	0.0014	0.0021	0.0026
	(0.0090)	(0.0090)	(0.0090)	(0.0071)	(0.0072)	(0.0071)
black	**0.1038**	**0.1028**	**0.1023**	0.0241	0.0225	0.0229
	(0.0589)	(0.0592)	(0.0592)	(0.0373)	(0.0370)	(0.0371)
other	0.0225	0.0229	0.0235	0.0410	0.0404	0.0396
	(0.0853)	(0.0852)	(0.0853)	(0.0302)	(0.0302)	(0.0302)
bhood	0.1006	-	-	-0.1579	-	-
	(0.1616)	-	-	(0.1013)	-	-
inthood	0.3591	-	-	-0.0165	-	-
	(0.2422)	-	-	(0.1885)	-	-
pctb_bhood	**-0.0048**	-	-	0.0022	-	-
	(0.0028)	-	-	(0.0022)	-	-
pctb_inthood	-0.0169	-	-	-0.0034	-	-
	(0.0105)	-	-	(0.0084)	-	-
pctb_whood	**-0.0071**	-	-	-0.0014	-	-
	(0.0032)	-	-	(0.0027)	-	-
pctblack	-	**-0.0033**	-0.0031	-	-0.006	**-0.0039**
		(0.0017)	(0.0029)		(0.0010)	(0.0018)
$pctblack^2$	-	-	0.0000	-	-	0.0001
			(0.0000)			(0.0000)
pctother	0.0029	0.0030	0.0025	-0.0002	-0.0001	0.0006
	(0.0023)	(0.0023)	(0.0034)	(0.0011)	(0.0011)	(0.0019)
$pctother^2$	-	-	0.0000	-	-	0.0000
			(0.0000)			(0.0000)
Estimates of TIP						
TIP_i	**0.2128**	-	-	-0.0470	-	-
	(0.1035)			(0.0722)		
TIP_b	0.1045	-	-	0.0263	-	-
	(0.0980)			(0.0822)		

*Dep. var.: $ln(value)$ for owners and $ln(rent)$ for renters. Coefficients in bold are significant at the 10% level.

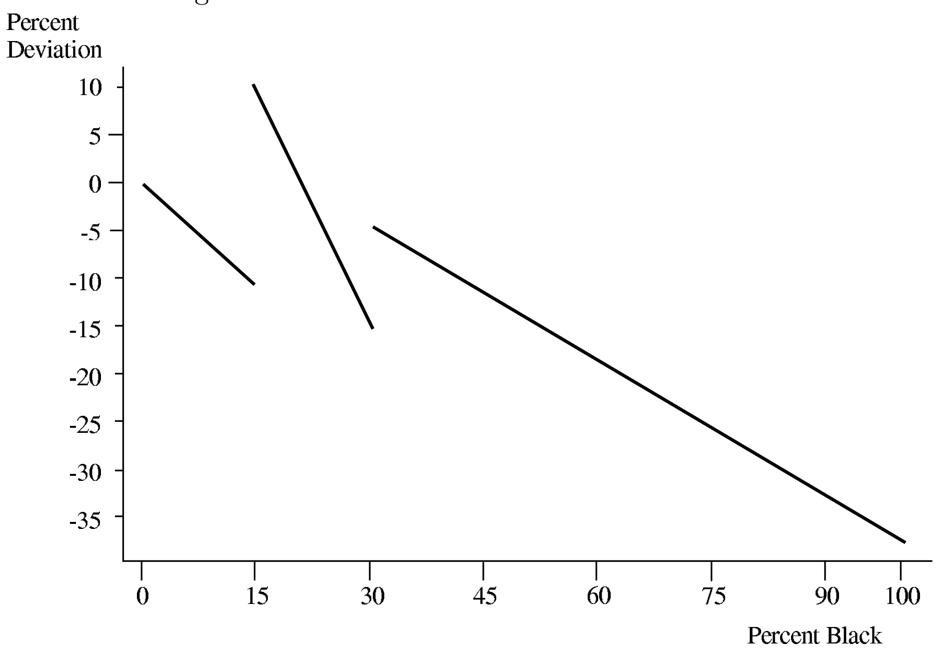
in *pctblack* lowers house values in black neighborhoods by an average of 4.8 percent and lowers values in white neighborhoods by an average of 7.1 percent. The null hypothesis that the two coefficients are equal can be readily rejected with a p-value of 0.006. The results indicate that the impact of racial composition is larger in white neighborhoods than in black neighborhoods. However, without interacting the effect of racial composition it is not possible to say whether this is because of differences in black and white preferences for racial composition or due to differences in the preferences of inhabitants of the two neighborhood types in general.

The estimates of the tipping points are also of interest. Although $\widehat{TIP}_B$ is not significant, $\widehat{TIP}_I = .2128$ and is significant with a p-value of .040. This indicates that as the percentage of blacks moves just above 30, causing a neighborhood to "tip" from integrated to black, there is not a jump in prices. However, as the percentage of blacks moves just above 15, causing a neighborhood to "tip" from white to integrated, the estimated increase in value is 21.3 percent with a 95 percent confidence interval of $[.99031, 41.6]$ percent. Although the estimate of the change in value between white and integrated neighborhoods is imprecise, perhaps due to higher error in categorizing integrated neighborhoods with current sample sizes, it does provide evidence that exclusion from white neighborhoods may keep prices high in neighborhoods with blacks. However, the source of the exclusion is not identified in this model.

Figure 3.2 represents the fixed effects estimates of the percent deviation in house values from those in an all white neighborhood as a function of racial composition. The negative slopes for all neighborhood types reflects declining prices as blacks move in. The point estimates indicate that the effect of an increase in blacks on neighborhood prices is lower in black neighborhoods than in white ones. Again, the jump in the gradient between white and integrated neighborhoods is evidence that exclusion keeps prices lower in non-black neighborhoods. The entire gradient is shifted upward by 10 percentage points for black buyers.

The results indicate that supplier discrimination causes blacks to pay more for

Figure 3.2: **Estimated Racial Value Gradient**



housing than whites but that prejudice causes housing prices to fall as the percent of blacks in a neighborhood rises. To judge whether blacks pay more or less than whites overall for housing, consider that the average white in the sample lives in a neighborhood that is 3.78 percent black and 2.62 percent other and the average black lives in a neighborhood that is 74.15 percent black and 2.32 percent other. Evaluating the explanatory variables $\mathbf{X}$ at a set level, Equation 2 shows the difference in predicted average prices paid for housing by whites in their typical neighborhoods ($\widehat{value}_w$) and blacks in their typical neighborhoods($\widehat{value}_b$)holding all non-racial variables constant:

$$\ln(\widehat{value}_w) - \ln(\widehat{value}_b) =$$
$$[3.78\hat{\gamma}_{pw} + 2.62\hat{\gamma}_o] - [\hat{\alpha}_b + \hat{\delta}_{bh} + 74.15\hat{\gamma}_{pb} + 2.32\hat{\gamma}_o]. \tag{3.2}$$

Plugging in the coefficient estimates, the results indicate that the value of an average home occupied by a white in his typical neighborhood is 13.3 percent higher than the value of an average home occupied by a black in his typical neighborhood. If discrimination were not present (that is, if $\hat{\alpha}_b$ equalled zero), then whites would pay 26 percent more on average for housing than blacks because houses in their predominantly white neighborhoods have a higher value.

The fixed effects results for renters do not provide similar evidence of discrimination or prejudice. In fact, all of the coefficients on the racial terms are insignificant in the fully specified fixed effects model. This may be because racial differentials are not present in the rental market or it may be due to correlation between race and rent assistance programs. Although variables such as rent control, public housing, and rent assistance are included, it is possible that error in reporting these or error in calculating rent (which should not include any assistance) lead to poor estimates.

Another source of concern lies in the choosing of neighborhood boundaries. Dividing the neighborhoods into three categories allows for an attempt to identify the effects of segregation. It also allows the price gradient to be estimated as a spline function so that the effect of racial composition can be different within the three categories. However, as discussed in the data section, the small neighborhood proxies may result in large error in assigning neighborhood types, especially for the relatively narrow integrated neighborhood category. Specification 4 removes the neighborhood type variables and their interactions, leaving only a variable measuring the race of the reference person and the racial composition of the neighborhood. The results for owners indicate that a 10 percent increase in the percent or neighborhood inhabitants who are black is associated with an average decrease in house values of 3 percent. Specification 5 adds a quadratic term for racial composition to provide more flexibility. Unfortunately, combining a quadratic term with a fixed effects estimate may require more variation across time than is observed in the data. The coefficients on the racial composition terms for owners are insignificant. Interestingly, the results for renters are significant. The estimates indicate that as the percentage of blacks in a neighborhood increases, rents decline at a decreasing rate. The coefficient on the racial dummy does not change much with the different specifications.

3.7 Conclusion

Although the estimates indicate that prejudice, exclusion, and supplier price discrimination all play roles in the housing market for owners, several improvements would make the results more robust. First of all, the neighborhood proxies used here may fall at the other end of the spectrum from those used in previous research: rather than being too broad, they may be too narrow. Neighborhood proxies at a level such as a census block or block group (which are smaller than census tracts and larger than the ten nearest neighbors) might both be more uniform in characteristics and have large enough sample size to decrease the error in categorizing sub-markets. This, in turn, would make the estimates of the change in prices between neighborhood less subject to error. Secondly, a larger sample would increase the precision of the fixed effects estimator because one would expect to see more change in the race of households.

However, despite these limitations, the data used in this research have yielded several key findings. First, they provide more evidence supporting the important role of neighborhood characteristics in housing services. As has been observed in previous studies, the estimated difference in housing prices paid by blacks decreases in magnitude as neighborhood characteristics are added to the random effects hedonic regression. Moreover, by using a smaller geographic proxy for neighborhood than previous researchers, such differentials become insignificant once neighborhood effects are incorporated. This suggests that previous negative differentials may have been biased in part due to neighborhood controls that were too broad. The use of the smaller geographic proxy for neighborhood also allows for better estimates of how non-racial neighborhood characteristics affect housing prices.

Secondly, although the random effects results offer interesting comparisons to previous studies, Hausman specification tests indicate that even with controls for neighborhood amenities such as racial composition, income levels, education, crime, and abandoned houses, there is still correlation between the regressors and the unobserved address-specific error term. This suggests that not only might these results be biased, but that the OLS results in previous studies, which tended to use broader neighborhood controls, are also likely to suffer from bias.

Finally, fixed effects estimates provide evidence of racial price differentials in the ownership market. House values fall as the percent of blacks in a neighborhood rises, indicating that high concentrations of blacks may be perceived as a neighborhood disamenity by some consumers. Furthermore, although there is no evidence of supplier price discrimination in the random effects estimates, the fixed effects estimates, which should be unbiased and consistent, provide evidence that black homeowners in any neighborhood pay 10 percent more for their homes than white homeowners. This suggests that previous biased results may have falsely indicated no supplier discrimination against blacks. That this finding is so different from the findings in other research only further emphasizes the importance of the relationship between neighborhood effects and race.

Chapter 4

A Cure for Discrimination? Affirmative Action and the Case of Proposition 209

4.1 Introduction

Introducing and removing affirmative action are not opposite sides of the same coin. Proponents of affirmative action maintain that it will provide a long-term cure for discrimination by allowing victims to demonstrate their skill and worth, thus changing prejudicial attitudes. Under this scenario, if affirmative action "works," then when it is time to get rid of the program there will be no deleterious effects for minorities. Opponents of these controversial programs, however, argue that it does not address the root source of inequality and, moreover, that it may create labor market inefficiencies and result in reverse discrimination against white males. Both sides, therefore, suggest that an effective affirmative action program would cause minority employment to rise, but they disagree on whether this increase is efficient and whether it would be sustainable if formal affirmative action were ended.

To date, there has been little opportunity to measure empirically the impact of removing affirmative action programs. While federal support for enforcement has ebbed and flowed and Supreme Court rulings in the past decade have chipped away at affirmative action, it is difficult to say whether concurrent changes in minority outcomes were due to affirmative action policy or other trends in inequality. A similar problem plagued attempts to measure the impact of instituting affirmative action in earlier years. While minorities and women made gains in the labor market in the seventies and eighties, it is not clear what portion of this was due to affirmative action and what was the result of other influences. Empirical studies of the impact of affirmative action on labor markets have relied on differences in outcomes for government contractors, who are subject to the program, and non-contractors, who are not. While these studies have provided evidence of minority gains among contracting firms, the results could be biased because contractor status is not exogenous: firms with the lowest cost of meeting affirmative actions requirements may be more likely to be contractors. Hence, we are left with an incomplete picture of both the impact of a controversial program and the potential consequences of its removal. What is needed, essentially, is a control group to which we can compare changes in outcomes for those affected by affirmative action.

The enactment of California Proposition 209 provides just such an opportunity. The measure, passed in the 1996 state elections and made effective in November of 1997, essentially outlawed existing local and state affirmative action programs in education, public hiring, and contracting, unless superseded by federal law. This change in state policy presents a natural experiment for measuring the labor market impact of removing of affirmative action programs. I use Current Population Survey (CPS) data to compare outcomes for minorities in California before and after affirmative action was removed to those same outcomes for white males. Then, to control for national trends in minority differentials, I compare this difference to the difference for a control group: states not undergoing similar changes in the law. The use of this triple difference technique to analyze the impact of removing affirmative action on employment, unemployment, labor force participation, and wages will provide evidence on the long-term effects of affirmative action.

The remainder of this chapter is organized as follows: In Section 2, I provide an overview of affirmative action policy and its impacts and a description of the gaps that this analysis can help to fill. In Section 3, I describe and summarize the data. Section 4 presents the econometric model and Section 5 describes the results. Section 6 provides a summary of findings and conclusions.

4.2 The history and consequences of affirmative action policy

National legislation and impacts

Whereas equal employment opportunity (EEO) laws such as Title VII of the Civil Rights Act prohibit discrimination, affirmative action legislation goes further by requiring that proactive steps be undertaken to remedy inequalities produced by past discrimination. In 1965, President Johnson issued Executive Order 11246, the primary regulation governing affirmative action, which requires that federal contractors "take *affirmative action* to ensure that applicants are employed, and that employees are treated during employment, without regard to their race, color, religion, sex, or national origin." Under its provisions, federal contractors must provide written affirmative action plans, progress reports, and submit to government compliance reviews. While EO 11246 only directly affected federal contractors, many state and local agencies and non-contractor private businesses voluntarily adopted similar programs in an attempt to address discrimination and avoid litigation under equal employment laws (Thomas and Garrett, 1999).

Early studies tend to indicate that affirmative action had a positive impact on the employment and occupational advancement of racial minorities.[1] Because of the inherent difficulty in separating gains from affirmative action from general trends in

[1] For a survey of the literature on affirmative action, see Holzer and Neumark (2000a).

racial inequality, these studies rely on data from the Equal Employment Opportunity Commission (EEOC) to compare outcomes for firms that are federal contractors, and hence subject to federal affirmative action programs, with firms that are not. Ashenfelter and Heckman (1976) find that the demand for black males increased 3.3 percent more among contractors than non-contractors between 1966 and 1970. While they find similar employment gains for black males during the early seventies, Heckman and Wolpin (1976) and Goldstein and Smith (1976) find no improvement or even declines in employment for females at contractor establishments. However, as Leonard (1989) points out, affirmative action for women did not become stringently enforced until after the Equal Employment Act of 1972.

Studies of affirmative action in the late seventies and beyond tended again to find positive employment gains for racial minorities and additional, although smaller, gains for white females. Leonard (1984c) finds that between 1974 and 1980, contractor demand for black males grew 3.8 percent faster, demand for other minority males grew 7.9 percent faster, and the demand for white females grew 2.8 percent faster than that of non-contractors. Leonard (1984b) also finds that affirmative action appeared to have a relatively greater impact on minorities in skilled occupational groups, although Smith and Welch (1984) suggest that observed gains in occupational status may be due to contractors re-classifying jobs rather than to any real upward mobility. Rodgers and Spriggs (1996) find that the positive impact on employment continued through to 1992 for all groups except Hispanics, for whom they find a negative impact. Holzer and Neumark (2000b) have one of the few empirical studies with wide scope that does not depend on EEOC data. Using information from a survey of employers in four U.S. cities, they find that firms that use affirmative action do tend to recruit and hire more minorities and women. In fact, contrary to most earlier results, the use of affirmative action in hiring seems to have the largest effect for white women. For firms that report using affirmative action in hiring, the last employee hired is 8 percent more likely to be a white woman and 3 percent more likely to be a black man.

Benefits through employment gains and occupational advance, however, may mask underlying losses in efficiency. While the effects of affirmative action on market efficiency are not fully understood,[2] what evidence is present does not seem to suggest large declines in productivity. Leonard (1984a) combines EEOC data with industry level data and finds no evidence of lower productivity among federal contractors. In their study, Holzer and Neumark (2000b) find that while minorities and women hired under affirmative action appear to have lower readily observable qualifications, their employers do not report significantly lower performance for these groups than for white males. The authors suggest that this is the result of more intensive screening and training programs.

California legislation and impacts

While empirical studies have tended to focus on national legislation, state governments have also instituted equal employment laws and affirmative action programs. In 1959, five years before the passage of the federal Civil Rights Act, California passed the Fair Employment Practices Act, which outlawed discrimination in that state and created the Fair Employment Practices Commission (FEPC) (later given responsibility for housing as well and re-named the Fair Employment and Housing Commission) to enforce the act. The FEPC was also granted the power to "engage in affirmative action with owners" in order to remedy discrimination (State of California). In practice, the FEPC has been responsible for oversight of affirmative action plans for state contracts over $200,000. In addition, in 1974 California began requiring all public agencies to submit affirmative action reports to the State Personnel Board (SPB), which was responsible for the oversight and development of public affirmative action programs(Thomas and Garrett, 1999). In 1989 California established contracting set asides for minority and women-owned business, requiring that at least 15 percent of the total value of state contracts go to minority-owned businesses and 5 percent to women- owned businesses. So, prior to 1997, not only were federal employers and contractors in California subject to mandated affirmative

[2]Holzer and Neumark (2000a) suggest that this is an important area for future research

action programs, but so were all public employees and state contractors.

However, attacks on these state programs in the mid 1990's have resulted in their formal dismantling. In 1995, then-governor Pete Wilson signed Executive Order 124-95, which directed state agencies to eliminate preferential treatments that exceed federal or state statutory requirements. Legally this could only apply to pre-standing executive orders and thus should not have affected state affirmative action laws, but it is not clear, in practice, what effect it would have (Thomas and Garrett, 1999). A year later, California voters passed Proposition 209 outlawing all state affirmative action programs and hence releasing public employers as well as state contractors from affirmative action requirements. After lengthy court challenges, the new law went into effect in November, 1997.

While there has been a flurry of research on the impacts of Propositon 209 on higher education in California, economists have neglected to pay attention to the corresponding impacts on labor markets. Yet, given that 8 percent of California's work force is in the non-federal public sector[3] and nearly 15 percent of California small businesses claim California state and local governments as clients (Williams, 1999), we might expect Proposition 209 to affect more than educational institutions. On the other hand, Holzer and Neumark (1999) suggest that approximately 60 percent of firms are federal contractors and subject of federal affirmative action policy. So, while Proposition 209 is likely to have had an effect on public employers in California, it may have been considerably less binding on private firms that are still subject to federal law.

The Proposition 209 experiment

Not only does it seem reasonable to expect that this change in policy would have an impact on California labor markets, but it also provides an opportunity to address two shortcomings of the empirical evidence to date.

First, previous work has had to rely on the comparison of firms that participate in affirmative action to those that do not. Researchers have either used EEOC data

[3]This average is from the employment data used in this chapter.

to compare federal contractors to non-contractors or firm-level data to compare firms that report using affirmative action to those that do not. Yet, because firms self-select into using affirmative action (by choosing to be federal contractors or by voluntarily implementing programs of their own), estimates of the impact of affirmative action may be biased downward. Federal contractors and voluntary participants may self-select precisely because it is relatively cheap to implement affirmative action. Moreover, the results of these studies have only provided an indication of the firm or sector-level impact of affirmative action, not of its economy-wide impacts. For instance, it is known that minority employment was rising at both contracting and non-contracting firms that file EEO-1 reports (albeit more rapidly at the contracting firms), but what was happening at firms that do not have to provide data on their composition? Did this rise in employment mask a re-shuffling of minorities between sectors?

Second, there has been no previous opportunity to gauge the impact of removing affirmative action— only of implementing it. While we do not suffer from a shortage of theoretical models of affirmative action, there is comparatively scant evidence on its long-term consequences. Theoretically, any model of a binding and effective affirmative action program will predict that minority employment should rise while the policy is in place, leaving only the need to see empirically whether existing programs appear to be effective and what the extent of their impact is.

Depending on the assumptions made about the source of pre-existing inequality, affirmative action may or may not engender a long-term change in labor market differentials that would remain even if the program were removed. If labor market discrimination did not exist in the first place or if, as some models (e.g., Johnson and Welch, 1976) suggest, affirmative action is not an efficient policy, then removing affirmative action may cause the labor market to revert to its competitive equilibrium. On the other hand, certain models of discrimination do suggest a long-term impact for affirmative action. If, for example, labor market inequalities are the result of classic employer discrimination, then it is possible that by being forced to interact with minority groups, employer prejudices will diminish so that once affirmative action is removed there is no longer inequality. Alternatively, Coate and Loury (1993) consider a form of statistical discrimination in which employers are less likely to place minority workers in high skilled jobs because of negative stereotypes. As a result, minorities have less incentive to invest in human capital, leading to a self-fulfilling prophecy. Assuming that minority workers have the same fundamental ability, affirmative action could break this cycle and potentially create permanent change in negative stereotypes. A third theoretical alternative for predicting the continued effectiveness of affirmative action after its removal is that presented by Athey et al. (2000). In their model, entry level employees receive more mentoring from senior employees with similar characteristics. As a result, there is bias towards one type of employee in promotion that can be permanently broken by a temporary affirmative action program that introduces diversity.

The passage of Proposition 209 provides a natural experiment that can be used to address both shortcomings of previous studies. First, it provides a (presumably) exogenous shock to affirmative action policy that affects only workers in California, leaving workers in the rest of the country as a control group. Second, this is the first legislation that has attempted to dismantle affirmative action.[4] By comparing the relative change in labor market outcomes in California to the rest of the country, we can see what impact removing state-sponsored affirmative action had on women and minorities in California. If there was no impact, it could be the case that affirmative action was either ineffective in the first place in California or that it was effective in engendering long-term changes that remained even after its removal. If there was a negative impact on the employment of minorities, this suggests that either the prejudicial attitudes of employers were not changed under California's affirmative action program or that the program itself had engendered inefficiencies and reverse discrimination against whites.

[4]Other states and political entities followed suit after the proposal of Proposition 209. Washington state passed its own repeal of affirmative action in 1998 although similar proposals have failed elsewhere.

4.3 Data

I employ data from the outgoing rotation groups in the monthly Current Populaton Survey (CPS) from 1994-2001, placing emphasis on 1995, the year before the proposal of Proposition 209, and 1999, two years after the new law had gone into effect. Observations are dropped if an individual is employed but reports no hours or pay, reports unknown sector of employment, or is self-employed.[5] Observations from Washington state were also dropped because that state passed legislation similar to Proposition 209 in 1998.

The triple difference estimates in this analysis will rely on three divisions of the data. First, the observations are categorized as before or after the enactment of proposition 209 (e.g. 1995 or 1999, 1995 or 2000, and so on depending on the years being used). Second, individuals are divided into eight mutually exclusive and collectively exhaustive categories: white males, white females, black males, black females, other males, other females, Hispanic males, and Hispanic females. And third, the country is divided into two groups: an experimental state (California) and and the remaining control states or "nation."

Table 4.1 reports sample sizes for each cell. Because of its population, the sample sizes within California for even this detailed breakdown of minority groups are still fairly large. However, California is not necessarily representative of the country as a whole. It is more minority heavy than the rest of the country and has slightly lower rates of employment, but a similar distribution of employment across sectors and industry. The fact that California is more diverse than the country as a whole means that extrapolations from its experience with affirmative action to general predictions should be made cautiously.

Labor Force Status Averages

Turning to the effects of affirmative action, Table 4.2 explores the change in non-

[5]Because men are more likely than women to be self-employed, omitting this group tends to increase the number of women in the sample relative to the number of men

Table 4.1: **Sample Sizes**

	1995		1999	
	Nation	California	Nation	California
white male	94,335	5,113	84,120	5,191
white female	112,672	6,045	98,649	5,927
black male	11,582	595	10,238	597
black female	16,689	773	14,409	812
hispanic male	7,301	2,805	8,315	2,992
hispanic female	8,533	3,144	8,991	3,168
other male	4,857	1,310	4,431	1,246
other female	5,758	1,500	5,198	1,486
total	261,727	21,285	234,351	21,419

participation in the labor force for white females after Proposition 209 was enacted. In 1995, 46.0 percent of California women over age 16 were not in the labor force while 32.1 percent of males were not participating. In 1999, after Proposition 209 had gone into effect, the percentage of white females who were not in the labor force had fallen to 44.5 percent, but the participation of men showed a similar change. Overall, there was no significant change in the participation of white women relative to that of white men in California. As a control, I look at the same outcomes for the rest of the nation. Over the same period nationwide, the non-participation of white women had fallen by 1.3 percentage points relative to white men. Differencing these effects, relative to the rest of the country, non-participation among white females in California rose by 1.6 more percentage points than that for white males. However, this estimate is not significant.

In addition to women, racial minority groups in California may have also been affected by Proposition 209. Table 4.3 presents the triple difference average changes in hourly wages as well as in the three labor force categories into which each individual falls: employed, unemployed, and not in the labor force. For each group, the triple difference is calculated as in the preceding example. Note in particular that because employment, unemployment, and non-participation are mutually exclusive and collectively exhaustive, the relative changes for each group across these categories sum to 0. The averages indicate that, relative to white males and the rest of

Table 4.2: **Change in Non-Participation: White Males and Females**

	1995	1999	Time Difference for Group
California			
white females	0.460	0.445	**-0.015**
	(0.006)	*(0.006)*	*(0.009)*
white males	0.321	0.302	**-0.018**
	(0.007)	*(0.006)*	*(0.009)*
Group difference for a given year	**0.140**	**0.143**	
	(0.009)	*(0.009)*	
Double Difference	0.003		
	(.013)		
Nation			
white females	0.432	0.421	**-0.012**
	(0.001)	*(0.002)*	*(0.002)*
white males	0.284	0.285	0.001
	(0.001)	*(0.002)*	*(0.002)*
Group difference for a given year	**0.148**	**0.135**	
	(0.002)	*(0.002)*	
Double Difference	**-0.013**		
	(0.003)		
Triple Difference	0.016		
	(0.013)		

*Standard errors are in parentheses below estimates. All differences in bold are significant at the 10% level. Rounding is done after calculations.

Table 4.3: **Triple Differences Summary**

	Employed	Unemployed	Not in Labor Force	Hourly Wage
white males	-	-	-	-
white females	-0.021	0.005	0.016	0.761
black males	**-0.054**	0.008	0.047	-0.240
black females	-0.034	-0.019	**0.053**	0.256
hispanic males	-0.002	-0.010	0.012	0.185
hispanic females	**-0.029**	-0.004	0.033	0.504
other males	**-0.060**	0.003	**0.057**	0.028
other females	-0.016	0.002	0.014	0.765

*Values in bold are significant at the 10% level. All monetary values are in 1995 dollars.

the nation, the proportion of black males, Hispanic females, and other males who were employed fell in California. There was no significant change in unemployment, but non-participation rose for black females and other males. Among five of the seven groups, the observed drop in employment is mirrored by a corresponding rise in non-participation. However, for black females and Hispanic males, a relatively larger portion appear to leave employment and move to unemployment. While the estimates find no significant change in wages, the point estimates are positive for six of the seven groups suggesting, for example, that the relative wage of white females rose by 76 cents. However, there is no clear prediction about the wage changes that might accompany a policy that can directly affect both wages and participation. It may be the case that the employees who are left are relatively more skilled, so average wages might rise. Alternatively, it might be the case that affirmative action also served to augment wage equality and so its removal might create a drop in wages. Given the possibility of opposing effects, if is not surprising to find no significant impact on wages.

Individual-specific differences

Although it is primarily viewed as a cross-sectional data set, the CPS can also be used as a panel in which each individual in the outgoing rotation group is observed twice. The CPS is constructed by interviewing the residents of a particular address for four months, dropping them from the sample, and then interviewing them again for four months one calendar year later. Hence, individual i first appears in the outgoing rotation group during his fourth month in the CPS sample. He then leaves the sample and re-enters one year later where he again appears in the outgoing rotation group in his eighth month in the sample. However, if an individual moves, it is the address that is followed by the Bureau of Labor Statistics and not the individual. To construct a panel, I match the respondents at a particular address across year and then assume that the respondent is the same person if sex and race have not changed and if age has increased by 0 to 2 years. This allows approximately two thirds of the individuals in the outgoing rotation group in any given year to be matched to the previous year. However, because of a change in CPS methodology, matching is not possible for June-December of 1994 and 1995 and January-August of 1995 and 1996.

If Proposition 209 had an effect on the labor force status of women and minorities, then one would expect to find differences in the status of individuals across years. Moreover, by examining the change in outcome for the same individual, individual specific fixed effects (such as ability or skill) are eliminated. I examine the probability that an individual left the labor force between $t = 1$ and $t = 2$ given an observed change in labor force status. Thus, the first difference in non-participation for any individual is 1 if he left the labor force and 0 if he entered the labor force. Conditioning on a change in labor force status reduces the sample size, but creates a binary variable for the first difference, assisting with inference for the double and triple differences.[6]

Table 4.4 reports the triple difference estimates of the relative probability that members of each minority group left the labor force given a change in participa-

[6] Consider, for instance, the difference for an individual in non-participation. It could be 1 (left labor force), 0 (no change), or -1 (entered labor force) and so is not binomially or normally distributed. Because of small sample sizes, it does not seem reasonable to invoke the Central Limit Theorem and non-parametric tests of differences for matched pairs are not appropriate for double or triple differences. By looking at whether an individual entered or left the labor force conditional on a change in participation, I create a binomial random variable and avoid these issues.

Table 4.4: **Triple differences for proportion of each group that left the labor force conditional on a change in participation**

	93-94	94-95	95-96	96-97	97-98	98-99
white males	-	-	-	-	-	-
white females	0.015	**0.167**	0.155	0.017	-0.007	0.038
black males	-0.039	**0.477**	0.251	0.010	0.019	-0.013
black females	-0.156	**0.289**	**0.318**	-0.061	0.052	0.075
hispanic males	-0.017	0.177	-0.072	-0.018	-0.041	-0.033
hispanic females	-0.036	**0.186**	**0.235**	-0.009	-0.010	-0.040
other males	0.015	0.068	0.100	0.046	-0.127	0.006
other females	-0.054	0.155	**-0.276**	0.048	-0.022	-0.032
all minorities	-0.038	**0.157**	0.116	-0.008	-0.034	-0.022

*Values in bold are significant at the 10% level.

tion. The first difference is the proportion of each group that left the labor force conditional on a change in participation. The second difference gives the proportion of each minority group that left the labor force relative to the proportion of white males. The third difference compares this change in California to the change in the rest of the nation. The estimates indicate that significant changes took place between 1994 and 1995 and 1995 and 1996.[7] Between 1994 and 1995 in California, white women were 16.7 percent more likely to have left the labor force given a change in participation than were white men relative to the nation as a whole. Black males, black females, and Hispanic females were also more likely to leave the labor force than to enter to it. Between 1995 and 1996, black women and Hispanic women were again more likely to leave the labor force than to enter it although the reverse is true for other females. As a whole, the estimates suggest a significant climb in the proportion of minorities who were leaving the labor force relative to entering it in the mid-nineties.

Note that the years for which these changes are observed are directly preceding or during the period when Proposition 209 was debated and passed. This could indicate an anticipation of the change in affirmative action policy, which seems plausible given the political environment in California at the time. While these

[7]These are the same years for which limited matching was possible due to change in CPS methodology. The significant estimates are noteworthy given the small sample sizes

results are indicative of significant change, they cannot be compared directly to estimates in upcoming sections which are based on cross sectional cuts of the data. In particular, the outcome variable in the regressions will not be conditioned on a change in participation and the span of time examined is longer than one year. However, these findings do bolster later results suggesting a significant change in participation.

4.4 Econometric model

To control further for the characteristics of the potential or actual labor force in estimating the impacts of Proposition 209, I turn to a triple difference regression framework. Suppose that there are only two racial/ethnic/gender groups: white males (wm) and white females (wf). In the case of general outcome y, consider the equation

$$\begin{aligned} y_{ijt} = \mathbf{x}_{ijt}\boldsymbol{\gamma} + \beta_1 year_t + \beta_2 cali_j + \beta_3 wf_i + \beta_4 (year_t * cali_j) + \\ \beta_5 (year_t * wf_i) + \beta_6 (cali_j * wf_i) + \beta_7 (year_t * cali_j * wf_i) + \epsilon_{ijt} \end{aligned} \tag{4.1}$$

where $\mathbf{x}_{ijt}$ is a vector containing a constant and explanatory variables other than those that are part of the differencing, i indexes an individual, j indexes location, and t indexes time. In this case, $year$ is a dummy for the latter year in the regression (e.g. 1999 if we are comparing 1999 to 1994), $cali$ is a dummy indicating that the individual resides in California, and wf, again, indicates that the individual is a white woman. The coefficient β_7 represents the triple difference estimate of the impact of Proposition 209 on outcome y for white women. This framework is used to estimate the impact of Proposition 209 on employment, unemployment, labor force participation, and ln(wages).

The first three outcomes are binary variables and are commonly estimated with probit or logit models. However, as Ai and Norton (2003) point out, the marginal effect of the interacted variables in a probit or logit model is not the same thing as the marginal effect of the interaction term. In previous studies, the triple difference marginal effect in a probit model has been estimated via some variant of the following calculation:

$$\Delta^3 = \Phi(\bar{\mathbf{x}}\hat{\boldsymbol{\gamma}} + \hat{\beta}_7) - \Phi(\bar{\mathbf{x}}\hat{\boldsymbol{\gamma}}) \tag{4.2}$$

.

However, this estimation ignores the fact that one cannot simultaneously "turn off" β_7 without affecting the other related interaction variables. In short, there is little intuitive explanation for what this particular calculation might mean.

In this case, to calculate the triple difference marginal effect one would need to calculate the double differences as follows:

$$\begin{aligned}\Delta^2_{cali} = \\ &[\Phi(\bar{\mathbf{x}}\hat{\boldsymbol{\gamma}} + \beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_5 + \beta_6 + \beta_7) - \Phi(\bar{\mathbf{x}}\hat{\boldsymbol{\gamma}} + \beta_2 + \beta_3 + \beta_6)] - \\ &[\Phi(\bar{\mathbf{x}}\hat{\boldsymbol{\gamma}} + \beta_1 + \beta_2 + \beta_4) - \Phi(\bar{\mathbf{x}}\hat{\boldsymbol{\gamma}} + \beta_2)]\end{aligned} \tag{4.3}$$

$$\begin{aligned}\Delta^2_{nation} = \\ &[\Phi(\bar{\mathbf{x}}\hat{\boldsymbol{\gamma}} + \beta_1 + \beta_3 + \beta_5) - \Phi(\bar{\mathbf{x}}\hat{\boldsymbol{\gamma}} + \beta_3)] - [\Phi(\bar{\mathbf{x}}\hat{\boldsymbol{\gamma}} + \beta_1) - \Phi(\bar{\mathbf{x}}\hat{\boldsymbol{\gamma}})].\end{aligned} \tag{4.4}$$

and then calculate the triple difference by

$$\Delta^3 = \Delta^2_{cali} - \Delta^2_{nation}. \tag{4.5}$$

In addition to needing to calculate the marginal effects in this way, the standard errors of these estimates are difficult to obtain. Ai and Norton (2003) point out that simply because the estimated probit coefficient on the triple difference term is significant does not mean that the marginal effect is.

To avoid these serious complications in estimating, interpreting, and conducting inference for the triple difference marginal effects, I turn instead to a linear probability model. The advantages of using such a model are that the triple difference effects can be easily estimated (it is, as in the original simple exposition, β_7, the coefficient on the triple interaction term) and standard errors can be calculated. Linear probability models are oft-criticized on three basic grounds: non-normality of the disturbances, heteroskedastic variance of the disturbances, and the fact that the predicted probabilities are not restricted to the interval $[0, 1]$. However, in this case, the large sample size negates the importance of the first consideration, it is possible to correct for the heteroskedasticity, and, only a small portion of the predicted values lie outside of the [0,1] range.[8] Therefore, I use a linear probability model with robust standard errors. However, as noted in the following section, the point estimates of the marginal effects, when calculated as in Equation (8), are very similar to those obtained with a Probit model.

Turning to the final labor market outcome of interest, I estimate a log wage regression to gauge the impact of removing affirmative action on hourly wages. Because no likely instrument is present for estimating a two-stage Heckman-type procedure, this is simply a wage regression *conditional* on employment. The possible biases that this may present are discussed along with the results in the following section.

[8] For most specifications, fewer than 5 percent of predicted probabilities were outside of the $[0, 1]$ range. However, these particular predicted values do create a problem for the FGLS procedure since the weight used is the estimated variance of the error term ($\hat{p}(1 - \hat{p})$) which will often be negative if predicted probabilities are less than 0 or greater than 1. To avoid this issue, standard errors were corrected with the Huber-White method. The robust results are very similar to those obtained with FGLS after censoring the predicted values.

4.5 Empirical analysis of Proposition 209

Labor force status

Table 4.5 reports the triple difference coefficients for linear probability models of employment, unemployment, and non-participation. The changes in employment, unemployment, and non-participation for any one group must sum to 0, but all three results are presented for ease of analysis.[9] In an attempt to identify possible short and longer term effects of the legislation, four pairs of years are examined: 1995 and 1998, 1995 and 1999, 1995 and 2000, and 1995 and 2001.[10] Moreover, the results are presented for each of the seven "minority" categories as well as for all of the minorities together to give an average effect.

Between 1995 and 1998, the relative employment of white females declined by 2.0 percentage points and that for other males declined by 5.7 percentage points. Most of the decline appears to be accounted for by a similar rise in non-participation. Hispanic males, on the other hand, seem to have moved from being less likely to be employed to more likely to be unemployed. In later years, however, the trend for all minority groups appears to have been a move out of employment and into non-participation. Between 1995 and 1999, the relative employment of minorities fell by 2.1 percentage points while non-participation rose by 2.3 percentage points. Between 1995 and 2000, relative employment fell by 1.4 percentage points (but the change is not significant) and non-participation rose by 1.8 percentage points. Breaking this down by group, between 1995 and 2000, relative non-participation rose by 1.8 percentage points for white females, 8.4 percentage points for black females, 2.5 percentage points for Hispanic males, 3.1 percentage points for Hispanic females, and

[9] In addition to race and sex, age, marital status, interview month, education, region, urban status, citizenship, and nativity were also controlled for in all regressions. Wage regressions also included indicators of sector of employment (public, private, or federal), occupation, and industry. The appendix contains a complete list of the differencing coefficients for the 1995-1999 regressions.

[10] In all cases, 1995 is used as the base year to which post-legislation years are compared. The results are similar if 1993, 1994, or 1996 is used as the base instead. In addition, 2002-2003 were also examined as post-legislation years. The triple difference coefficients are similar in 2002 but become insignificant in 2003. However, it is not clear how to interpret this since extending the time frame also increases the chance of unobserved events biasing the results.

Table 4.5: **Triple Difference Coefficients for Employment, Unemployment, and Non-Participation LPMs**

	1995-1998		**1995-1999**		**1995-2000**		**1995-2001**	
	coef	*se*	*coef*	*se*	*coef*	*se*	*coef*	*se*
Employment								
white females	**-0.020**	0.011	**-0.031**	0.011	-0.018	0.011	**-0.024**	0.011
black males	0.017	0.026	-0.041	0.026	-0.018	0.026	0.024	0.026
black females	-0.020	0.024	-0.030	0.024	**-0.058**	0.024	**-0.056**	0.024
hispanic males	-0.013	0.015	-0.001	0.014	-0.023	0.014	**-0.025**	0.015
hispanic females	-0.008	0.016	**-0.034**	0.016	**-0.035**	0.015	-0.021	0.015
other males	**-0.057**	0.020	**-0.065**	0.020	**-0.047**	0.019	**-0.070**	0.019
other females	-0.005	0.020	-0.020	0.020	-0.022	0.020	-0.027	0.020
all minorities	-0.010	0.009	**-0.021**	0.009	-0.014	0.009	**-0.016**	0.009
Unemployment								
white females	0.004	0.005	0.006	0.005	0.000	0.005	0.005	0.005
black males	-0.009	0.015	0.009	0.015	0.013	0.016	0.000	0.015
black females	-0.005	0.013	-0.018	0.012	**-0.026**	0.012	-0.000	0.013
hispanic males	**0.015**	0.009	-0.009	0.008	-0.002	0.008	-0.001	0.008
hispanic females	0.011	0.008	-0.004	0.007	0.004	0.007	-0.004	0.007
other males	0.013	0.010	0.005	0.010	-0.011	0.009	0.008	0.010
other females	-0.003	0.008	0.002	0.008	0.002	0.008	0.009	0.008
all minorities	0.005	0.005	-0.002	0.005	-0.005	0.005	0.000	0.005
Non-participation								
white females	0.016	0.011	**0.026**	0.011	**0.018**	0.011	**0.020**	0.011
black males	-0.009	0.024	0.032	0.025	0.005	0.025	-0.025	0.024
black females	0.025	0.023	**0.048**	0.024	**0.084**	0.024	**0.057**	0.024
hispanic males	-0.002	0.013	0.010	0.013	**0.025**	0.013	**0.026**	0.013
hispanic females	-0.003	0.015	**0.038**	0.015	**0.031**	0.015	**0.025**	0.015
other males	**0.044**	0.019	**0.060**	0.019	**0.058**	0.019	**0.061**	0.018
other females	0.009	0.020	0.018	0.020	0.020	0.019	0.018	0.019
all minorities	0.005	0.009	**0.023**	0.009	**0.018**	0.009	**0.016**	0.009

*Standard errors are robust. Values in bold are significant at the 10% level.

Table 4.6: **Triple Difference Coefficients for Log(Wage) Regressions**

	1995-1998		**1995-1999**		**1995-2000**		**1995-2001**	
	coef	*se*	*coef*	*se*	*coef*	*se*	*coef*	*se*
white females	0.017	0.018	0.001	0.018	-0.014	0.019	-0.003	0.020
black males	-0.017	0.040	-0.039	0.039	**-0.092**	0.040	-0.005	0.040
black females	-0.017	0.037	-0.029	0.040	-0.029	0.037	**-0.100**	0.041
hispanic males	0.012	0.021	0.007	0.021	0.005	0.021	-0.019	0.021
hispanic females	0.026	0.024	**0.052**	0.024	**0.044**	0.023	0.037	0.025
other males	-0.028	0.032	-0.016	0.033	0.044	0.031	0.009	0.033
other females	0.000	0.031	0.041	0.030	**0.087**	0.029	0.019	0.032
all minorities	0.007	0.015	0.010	0.015	0.008	0.015	-0.002	0.016

*Standard errors are robust. Values in bold are significant at the 10% level. All monetary values are 1995 dollars.

5.8 percentage points for other males. This increase in non-participation accounts for nearly all of the decline in employment for all groups except black females, who also saw a drop in unemployment as they moved out of the labor force. Only black males and other females do not exhibit significant changes in labor force status. This trend continues through to 2001, when the relative employment of minorities fell by 1.6 percentage points with an equal rise in non-participation.

As a whole, the results suggest that the impact of Proposition 209 was to move females and minorities from employment to out of the labor force. If, as the results indicate, the removal of affirmative action made it more difficult for women and minorities to find work, then this exit from the labor force is not surprising. Previous work has tended to indicate that women have more elastic labor supplies than men and they tend to be more responsive along the extensive participation margin (Blau and Kahn, 2005). In addition, when looking at the impact of minimum wage legislation, Mincer (1976) finds that affected groups tend to leave the labor force and, moreover, that females and minorities have relatively high participation elasticities. It seems that, as in the case of other changes in the costs of working, women and minorities responded to the removal of affirmative action by leaving the labor force.

Wages

As discussed previously, there is no clear prediction of the impact of removing affirmative action on wages. Relative wage changes will depend on the nature of pre-existing discrimination, the effectiveness of affirmative action, and the relative skill levels of the groups affected by its removal. It is thus not surprising that the results in Table 4.6 do not show such clear patterns as the labor force status regressions. No significant changes are observed between 1995 and 1998 but in 1999 the relative wages of employed Hispanic females have risen by 5 percent. This trend continues into 2000 and we see an additional rise for other females, but the wages for black males have fallen by 9.2 percent. Between 1995 and 2001, only black females show a significant change in relative wages. As a whole, the results do not show a consistent effect for any of the groups with the possible of exception of a rise in wages for Hispanic females. This could indicate that affirmative action had little effect on wages. Affirmative action laws, after all, did not directly address wage equality, which was covered by equal employment law. It could also be the result of skill bias among those leaving employment. Since the wage regressions are conditional on employment, the wages of those who remain employed could rise because they are relatively more skilled or fall because they are relatively less skilled than those who left.

Participation effects by age and education level

The wage findings do not provide consistent evidence of skill bias in those who remain unemployed, but they do not prove the contrary either. Previous studies have suggested that affirmative action helps to advance minorities into more skilled occupations (e.g., Goldstein and Smith, 1976; Leonard, 1984b), so it might be that it is high-skilled minorities who leave with the removal of affirmative action. In an attempt to gauge which groups of workers are more affected by the legislation, the non-participation regressions were estimated for separate segments of the sample.

Table 4.7 reports these results. In columns (1)-(3), non-participation regression coefficients are reported for three education levels: less than high school, a high school diploma, and education beyond high school. Interestingly, it is neither the

Table 4.7: **Non-Participation DDD Coefficients by Education and Age (1995-1999)**

	Education			Age		
	< High School	High School	> High School	<30	30-50	> 50
white females	0.034	**0.042**	0.019	-0.012	**0.041**	**0.034**
black males	-0.004	0.050	0.049	-0.071	**0.102**	-0.015
black females	-0.002	**0.097**	0.036	-0.023	**0.067**	**0.070**
hispanic males	-0.001	**0.040**	-0.019	-0.006	**0.045**	-0.041
hispanic females	0.015	**0.075**	**0.046**	0.009	**0.078**	-0.009
other males	-0.043	0.041	**0.110**	**0.092**	**0.079**	-0.009
other females	-0.053	**0.072**	0.032	-0.007	0.007	0.048
all minorities	-0.006	**0.048**	**0.028**	-0.002	**0.043**	0.017

*Standard errors are robust. Values in bold are significant at the 10% level.

lowest nor the highest education levels that are affected: it is the middle. High school graduates who have not completed education beyond that level show the largest and most significant decline in labor force participation. Turning to age, columns (4)-(6) report results for three age brackets: 30 years old or younger, 30 to 50 years old, and older than 50. Again, it is the group in the middle that has both the largest and the significant coefficients. This is especially surprising given the expectation that very young and very old workers will be less attached to the labor force than middle aged workers. One story that is consistent with these findings is that, as suggested by previous work, affirmative action had little impact on unskilled labor so that its removal had little effect for young or uneducated workers. However, it did help to advance employment for workers with intermediate skill and tenure levels. On the other hand, it could also be the case that education and age are poor proxies for skill.

Table 4.8: **DDD Coefficients for Sector of Employment (1995-1999)**

	P(Private|Emp)		P(Public |Emp)		P(Federal|Emp)	
	coef	*se*	*coef*	*se*	*coef*	*se*
white females	-0.007	0.011	0.013	0.011	-0.006	0.005
black males	-0.006	0.030	0.018	0.029	-0.012	0.020
black females	**-0.064**	0.027	**0.076**	0.028	-0.012	0.016
hispanic males	-0.016	0.010	**0.025**	0.010	-0.009	0.006
hispanic females	-0.018	0.014	0.013	0.014	0.005	0.006
other males	0.024	0.017	-0.013	0.016	-0.011	0.012
other females	-0.015	0.019	0.012	0.009	0.003	0.011
all minorities	-0.008	0.008	**0.015**	0.008	-0.006	0.004

*Standard errors are robust. Values in bold are significant at the 10% level.
All monetary values are 1995 dollars.

Sector of employment

Previously I suggested that, because Proposition 209 does not supersede federal affirmative action laws, workers in California's public sector, who were covered by California but not federal policy, might see the largest effects from the measure. However, it is difficult to use CPS data to compare inter-sector differences. The results for the economy as a whole suggest that Proposition 209 did not affect the unemployment rate but did decrease participation. But, if an individual is not in the labor force, then we cannot identify what sector they may have worked in previously. I attempt to circumvent this problem by using a linear probability model to estimate the impact of Proposition 209 on the probability that an individual works in the private, public (state or local), or federal sector given that he is employed.

Table 4.8 presents these results. While few of the point estimates are significant, they tend to be negative for the private and federal sectors and positive for the public (state or local) sector. The results indicate that minorities were relatively more likely to work in the public sector and less likely to work in the private or federal sector after the passage of Proposition 209. This is counter to the expectation that the negative effects of Proposition 209 would be strongest for state and local workers in California. It may be the case that private employers did respond to the removal of state-sponsored affirmative action in California and to the general anti-affirmative action climate of the period. While many private-sector firms in

California were likely to be federal contractors, federal affirmative action policy had also been under intense legal scrutiny during the nineties and private employers in California may have felt more bound by state policy than federal.

It is difficult, however, to draw any strong conclusions based on the evidence about changes in employment across sector. The results presented earlier suggest that minorities were leaving the labor force as a result of Proposition 209 and it is not possible to identify what sector an individual may have worked in previously if they are currently not in the labor force. Moreover, by only looking at changes in aggregate employment probabilities, we could miss changes in skill composition across sector.

4.6 Conclusion

The enactment of Proposition 209 in California created a unique opportunity to study the labor market effects of the removal of affirmative action programs. Changes in minority outcomes in California relative to those of white males are compared to the same differences for the rest of the nation in order to separate the effects of Proposition 209 from general trends in inequality.

The results suggest that there was a sharp drop in employment after the passage of Proposition 209, which resulted in minorities leaving the labor force. Between 1995 and 2000, relative participation rates in California fell by 1.8 percentage points for white females, 8.4 percentage points for black females, 3.1 percentage points for Hispanic females, 2.5 percentage points for Hispanic males, and 5.8 percentage points for other males. There appears to have been little corresponding change in wages rates. While it is possible that this lack of observed change in pay is the result of selection bias, with, for instance, unconditional wages falling but more skilled workers remaining employed, estimates of the participation decision across age and education groups do not indicate that either low or high skilled workers were disparately affected by the removal of affirmative action.

The decline in minority participation raises doubts about affirmative action programs. It is consistent with one of two hypotheses: that affirmative action is inefficient and creates reverse discrimination or that affirmative action is ineffective at engendering permanent change in prejudices that create labor market inequality. A final possibility is that California's affirmative action programs had not been in place long enough to engender permanent alteration in inequality. However, given that California had pursued affirmative action for over twenty-five years, this may be equally discouraging.

Chapter 5

Conclusion

The three essays that make up this dissertation have each taken a detailed look at how preferences, prejudice, and discrimination affect inequality in labor and housing markets. They have provided both theoretical predictions and empirical tests and have demonstrated that prejudicial attitudes can create differentials as readily as direct discrimination.

The first essay, inspired by the observation that local television news personalities are a diverse group, used a combination of Nielsen ratings and author-collected data on the characteristics of television markets, stations, and anchor teams to examine the determinants of the racial make-up of stations and how the characteristics of on-air staff affect ratings. I presented a theory that combines elements of Becker's model of customer discrimination with a Hotelling-type model to show that, under certain circumstances, firms will wish to differentiate along racial lines to attract different segments of the market. The evidence from local news is consistent with this theory. Stations respond negatively to increases in the minority composition of their rivals. Furthermore, the viewers of stations with few blacks react negatively to an increase in the percentage of blacks while viewers of stations with relatively more blacks respond positively, suggesting that these stations are, in fact, differentiating in order to cater to certain segments of the market.

The second essay addressed a long-standing problem in empirical studies of racial differentials in house prices. Because race and neighborhood characteristics are strongly correlated, studies of racial housing price differentials have yielded results that vary widely depending on the types of neighborhood controls used. This paper incorporated two innovations to address this problem. First, I introduced data that provide smaller and more homogenous neighborhood proxies to control for neighborhood characteristics. Secondly, I took advantage of the time series characteristics of the data to test for and deal with correlation between unobserved neighborhood and house effects and race. I show that even with relatively thorough neighborhood controls, there is still evidence that correlation between the error term and regressors is a source of bias. While recent studies have tended to find evidence of a negative premium for blacks, fixed effects estimates in this paper indicate that black owners pay premiums of around 10 percent for housing. Moreover, house values decline in neighborhoods as the percentage of blacks increases, suggesting that prejudicial attitudes among consumers affect house prices within neighborhoods.

The third essay took advantage of a natural experiment created by California Proposition 209, which eliminated state affirmative action programs. By using triple difference techniques to control for state and group trends, I isolated the effect of removing affirmative action on women and minorities. The estimates indicate that minority labor market participation fell approximately two percentage points with the removal of this program, suggesting that either affirmative action was not efficient in the first place or that it had been an efficient solution to discrimination but had not engendered long-run changes in attitudes.

As a whole, these essays highlight the importance of taking into account not only direct discriminatory behavior, but also the more subtle forces of preference and prejudice. In the local news market, although customers themselves are not forcing firms to segregate, it is their preferences that drive that outcome. In housing markets it is the prejudices of neighbors that cause house prices to fall with an influx of blacks although these neighbors are not themselves taking money out of anybody's hand. These essays also sound a cautionary note for government. Even if we could effectively eliminate supplier discrimination in housing or employer discrimination in labor markets, the preferences of demanders could continue to drive inequality. Moreover, as the final chapter on affirmative action suggests, prejudice may not be an easy thing to change.

That is not, however, meant to suggest that we should not try. All three essays open avenues for future work to help us better understand the nature of inequality. The first essay provided evidence of a previously unconsidered phenomenon in which firms differentiate in response to customer discrimination. This leaves open the question of what this means for employment and wage differentials and whether or not such differentiation is efficient. It also does not answer the question of who the prejudiced customers are. The second essay addressed the shortcomings of recent empirical estimates of housing discrimination. It presented strong evidence that housing discrimination and demander prejudice both continue to play a significant role in housing markets. However, the data used are already a decade old. We need more recent data to see if, as I expect, the trend continues. Moreover, because of limited sample sizes, I assumed that the effect of changing racial composition was symmetric so that if house prices fall as blacks move into a neighborhood, then they rise by the same amount as blacks move out. Yet what we know about the dynamics of neighborhoods suggests that the process of neighborhood tipping and gentrification may be more complicated that this. Finally, the third essay leaves open the question of what the removal of affirmative action looked liked on a more micro level. How were firms themselves responding? What happened to profits?

While questions remain, we have come a long way in our understanding of the issue of inequality. We know, first of all, that it exists. And the sum of our work on the causes suggests that, in part at least, this is due to the proverbial playing field not being level. However, inequality in opportunity is not only the result of direct discriminatory action. My work suggests that, even without getting anywhere near the field, the preferences and prejudices of all members of society affect outcomes.

Just by flipping channels during the evening news, viewers affect the labor market for local news personalities. Just by not being happy that a black family moved in down the street, the preferences of neighbors affect housing markets. If we wish to combat inequality, then, we need to attempt to craft policy that not only affects direct discrimination but that also works to change the more subtle prejudices that we all harbor.

Appendix A

A.1 Variables Used in Chapter 2

Table A.1: **Variables Used in Chapter 2**

Name	Description	min	max	mean
Show Characteristics				
rating	Nielsen rating for November 2003	0.5	17.4	5.1
share	Nielsen share for November 2003	1.0	33.0	12.4
length	number of quarter hours that broadcast lasts	1	12	2.9
post network	indicator that broadcast immediately follows national network news	0	1	0.19
pre network	indicator that broadcast immediately precedes national network news	0	1	0.25
weekday	indicator that broadcast is on a weekday	0	1	0.76
weekend	indicator that broadcast is on a weekend	0	1	0.24
early morn	indicator that broadcast begins between 5 a.m. and 7:59 a.m.	0	1	0.33
mid morn	indicator that broadcast begins between 8 a.m. and 10:59 a.m.	0	1	0.03
midday	indicator that broadcast begins between 11 a.m. and 4:59 p.m.	0	1	0.12
early eve	indicator that broadcast begins between 5 p.m. and 5:59 p.m.	0	1	0.29
late eve	indicator that broadcast begins between 9 p.m. and 11:59 p.m.	0	1	0.22
no. english casts	number of local newscasts in English broadcast during show's time slot	1	6	3.1

Name	Description	min	max	mean
no. spanish casts	number of local newscasts in Spanish broadcast during show's time slot	0	3	0.3
sport anch	indicator that broadcast has a sports anchor	0	1	0.43
minority anchor	indicator that broadcast has a minority news anchor	0	1	0.56
female anchor	indicator that broadcast has a female news anchor	0	1	0.95
minority weather	indicator that broadcast has a minority weather anchor	0	1	0.10
female weather	indicator that broadcast has a female weather anchor	0	1	0.24
minority sports	indicator that broadcast has a minority sports anchor	0	1	0.68
female sports	indicator that broadcast has a female sports anchor	0	1	0.13
Market Characteristics				
dma rank	market rank by size, 1 being largest	1	25	-
north	indicator that dma is in the north	0	1	0.21
south	indicator that dma is in the south	0	1	0.35
west	indicator that dma is in the west	0	1	0.27
midwest	indicator that dma is in the midwest	0	1	0.17
dma pctblack	percent of persons in market who are black	2.2	25.7	12.4
dma pcthispanic	percent of persons in market who are hispanic	0.8	42.4	13.8
dma pctasian	percent of persons in MSA who are asian	1.1	19.4	4.8
dma pctfemale	percent of persons in market who are female	50.2	53.2	51.9
dma pctyoung	percent of persons in market aged 2-17	19.4	26.1	23.1
dma pctprime	percent of persons in market aged 18-49	40.7	50.8	47.1
english stations	number of stations that broadcast local news in English	4	9	6.2
spanish stations	number of stations that broadcast local news in Spanish	0	5	1.0
24hr stations	indicator that there is a 24-hour local news channel in market	0	1	0.3
Station Characteristics				
NBC	indicator that station is NBC affiliate	0	1	0.28

Name	Description	min	max	mean
ABC	indicator that station is ABC affiliate	0	1	0.24
CBS	indicator that station is CBS affiliate	0	1	0.26
FOX	indicator that station is FOX affiliate	0	1	0.22
no. persons	number of on-air employees	11	45	29.2
age	average age of on-air staff	33.4	48.8	41.5
pctnative	percent of on-air staff that is area native	0	56.6	25.0
pcthighed	percent of on-air staff with education beyond a bachelor's degree	0	39.1	11.7
tenure	average staff tenure at station, in years	2	15.4	8.3
experience	average staff experience in local television news, in years	3.0	27.0	17.6
no. stations	average number of stations on-air staff have worked at	2.6	4.7	3.6
pct emmys	percent of regional emmys for local news awarded to station	0	84.6	26.3
pctwhite	percent of on-air staff that is white	54.3	100.0	74.2
pctblack	percent of on-air staff that is black	0.0	38.1	15.0
pcthispanic	percent of on-air staff that is Hispanic	0.0	28.6	5.9
pctasian	percent of on-air staff that is asian or other(2 of the 125 observations are "other")	0.0	20.0	4.8
pctfemale	percent of on-air staff that is female	20.0	57.1	41.8
pctblond	percent of on-air staff that is blond	0.0	36.8	15.2
black match	$pctblack/dmapctblack$	0	322.3	132.0
hispanic match	$pcthispanic/dmapcthispanic$	0	193.0	37.3
asian match	$pctasian/dmapctasian$	0.0	584.1	113.2
female match	$pctfemale/dmapctfemale$	39.3	110.0	80.5
min manager	indicator that station manager and/or news director is a minority	0.0	1	0.22
female manager	indicator that station manager and/or news director is female	0.0	1	0.38
other stat pctblack	average *pctblack* for other stations in DMA	3.1	31.2	15.0
other stat pcthispanic	average *pcthispanic* for other stations in DMA	0.0	24.2	5.9

Name	Description	min	max	mean
otherstat pctasian	average *pctasian* for other stations in DMA	0.0	17.7	4.8
other stat pctfemale	average *pctfemale*for other station in DMA	30.4	51.5	41.8
other stat age	average *age*for other station in DMA	34.8	46.5	41.5

**Note that the means for the station characteristics weight each station evenly and the means for the market characteristics weight each market evenly.*

A.2 Properties of the Racial Composition Estimator in Chapter 2

Consider the case in which each market has two competing stations, indexed 1 and 2. The racial composition of each, denoted y, can be modeled as a system of linear equations:

$$y_1 = y_2\alpha + \epsilon_1 \tag{A.1}$$

$$y_2 = y_1\alpha + \epsilon_2. \tag{A.2}$$

In this case the reduced form equations are:

$$y_1 = \frac{\alpha}{(1-\alpha^2)}\epsilon_2 + \frac{1}{(1-\alpha^2)}\epsilon_1 \tag{A.3}$$

$$y_2 = \frac{\alpha}{(1-\alpha^2)}\epsilon_1 + \frac{1}{(1-\alpha^2)}\epsilon_2. \tag{A.4}$$

In terms of station 1, the estimator $\hat{\alpha}$ is:

$$\hat{\alpha} = \alpha + \frac{cov(y_2, \epsilon_1)}{E(y_2^2)}. \tag{A.5}$$

Assume symmetry so that $Var(\epsilon_1) = Var(\epsilon_2) = \sigma^2$ and let $\rho = \frac{cov(\epsilon_1,\epsilon_2)}{var(\epsilon_1)}$. Then it can be shown that

$$\hat{\alpha} = \alpha + \frac{(1-\alpha^2)(\alpha+\rho)}{1+\alpha^2+2\alpha\rho} = \frac{2\alpha+\rho+\rho\alpha^2}{1+\alpha^2+2\alpha\rho}. \tag{A.6}$$

Any unobserved market factors that influence station composition should influence both stations similarly. So, ρ is expected to be positive. Given $\rho \geq 0$, it is clear that, if $\alpha \geq 0$, then $\hat{\alpha} \geq 0$. In other words, although the estimate of α will be biased and inconsistent, the estimate is not expected to be negative unless the true value is negative. Moreover, under the null hypothesis that $\alpha = 0$, $plim(\hat{\alpha}) = \rho$. Under the null, the critical value for a test of the alternative that $\alpha < 0$ will be lower than necessary. So, the probability for rejection of a negative coefficient is not overstated.

This result, however, does not extend to the case of more than two firms. If there are three firms in a market, then the racial composition of each can be modeled as:

$$y_1 = \bar{y}_{23}\alpha + \epsilon_1 \tag{A.7}$$

$$y_2 = \bar{y}_{13}\alpha + \epsilon_2 \tag{A.8}$$

$$y_3 = \bar{y}_{12}\alpha + \epsilon_3 \tag{A.9}$$

where the racial composition of each station depends on the average composition of competing stations. In this case, the reduced form equation for station i is:

$$y_i = \frac{1}{4 - 3\alpha^2 - \alpha^3}[(4 - \alpha^2)\epsilon_i + \alpha(2 + \alpha)(\epsilon_j + \epsilon_k)]. \tag{A.10}$$

Again assume symmetry so that $Var(\epsilon_1) = Var(\epsilon_2) = Var(\epsilon_3) = \sigma^2$ and $Cov(\epsilon_1, \epsilon_2) = Cov(\epsilon_2, \epsilon_3) = Cov(\epsilon_1, \epsilon_3)$ and let ρ be the correlation coefficient for the error terms across stations within the same market. Then

$$\hat{\alpha} = \alpha + \frac{cov(\bar{y}_{jk}, \epsilon_i)}{E(\bar{y}_{jk}^2)} = \alpha + \frac{(4 - 3\alpha^2 - \alpha^3)(\alpha + 2\rho)}{(2 + \alpha)[(2 + \alpha^2) + 2\rho(1 + 2\alpha)]}. \tag{A.11}$$

Under the null that $\alpha = 0$, $E[\hat{\alpha}] = \frac{2\rho}{1+\rho}$ is positive as in the case of two firms. If random effects estimates eliminate the correlation, then the estimator is consistent under the null. Also, if $\rho = 0$,

$$\hat{\alpha} = \alpha\frac{(4 - \alpha)}{2 + \alpha^2}. \tag{A.12}$$

So if the correlation can be eliminated, the estimator is consistent under the null and the estimated coefficient is expected to be of the correct sign as long as $|\alpha| \leq 4$.

A.3 Variables Used in Chapter 3

Table A.2: **Variables Used in Chapter 3**

Name	Description
Dependent Variables	
value	owner-estimated fair market value of property
rent	monthly contract rent
Characteristics of Unit	
rooms	number of rooms in unit
roomssq	squared number of rooms in unit
baths	number of baths in unit
half baths	number of half bathrooms in unit
porch	indicator of presence of porch or deck
garage	indicator of presence of garage or covered parking
ac	indicator that unit has central air or window units
central heat	indicator that unit has central heat
built 970-	indicator that unit was built in 1970 or later
built 1940-1969	indicator that unit was built between 1940 and 1969
built -1939	indicator that unit was built before 1940
Characteristics of Neighborhood	
mbcrime	percent of units in neighborhood reporting presence of bothersome crime
maban	percent of houses in neighborhood for which enumerator observed one or more abandoned buildings nearby
mproblems	percent of houses in neighborhood reporting that something about neighborhood is bothersome
mbar	percent of houses in neighborhood for which enumerator observed bars on the windows of nearby buildings
lmedzinc	natural logarithm of median income in neighborhood
mededuc	median number of years of education in neighborhood
Location and Year Dummies	
central city	indicator that unit is in the central city of an MSA
north	indicator that unit is in north
midwes	indicator that unit is in south
west	indicator that unit is in west
south	indicator that unit is in south
y85	indicator that observation made in 1985
y89	indicator that observation made in 1989
y93	indicator that observation made in 1993
Characteristics of Household	
age	age of reference person
persons	number of people living in unit
married	indicator that reference person is married
female	indicator that reference person is female
educ1	indicator that highest grade attained by reference person is 0-10

Name	Description
educ2	indicator that highest grade attained by reference person is 11 or 12
educ3	indicator that highest grade attained by reference person is college
educ4	indicator that reference person has schooling beyond college
ln(income)	natural logarithm of total income of family members living in unit
Racial Variables	
white	indicator that reference person is white
black	indicator that reference person is black
other	indicator that reference person is of other race
whood	indicator for white neighborhood ($\geq$85 percent white or other)
bhood	indicator for black neighborhood ($\geq$30 percent black)
inthood	indicator for integrated neighborhood (between 15 and 30 percent black)
pctwhite	percent of units in neighborhood with white reference person
pctblack	percent of units in neighborhood with black reference person
pctother	percent of units in neighborhood with other reference person
pctb_whood	pctblack∗whood
pctb_bhood	pctblack∗bhood
pctb_inthood	pctblack∗inthood
Variables that appear only in regressions on value	
house	indicator that unit is not a mobile home
new owner	indicator that owner bought home in last 5 years
Variables that appear only in regressions on rent	
one unit	indicator that building has 1 unit
2-3 units	indicator that building has2-3 units
4- units	indicator that building has more than 4 units
moved1	indicator that renter moved within last 12 months
moved12	indicator that renter moved 1-2 years ago
public housing	indicator that rental unit is public housing
subsidized housing	indicator that renter receives federal, state, or local rent subsidy
rent control	indicator that unit is subject to rent control
rent adjusted	indicator that rent is adjusted because renter works for or is related to owner

A.4 1995-1999 Difference Coefficients From Chapter 4

Table A.3: **1995-1999 Difference Coefficients From Chapter 4**

	Employed		Unemployed		Participation	
	coef	*se*	*coef*	*se*	*coef*	*se*
cali	**-0.060**	0.006	**0.006**	0.003	**0.054**	0.006
y99	**0.004**	0.002	**-0.007**	0.001	**0.003**	0.002
wf	**-0.106**	0.002	**-0.008**	0.001	**0.114**	0.002
bm	**-0.085**	0.004	**0.027**	0.002	**0.058**	0.004
bf	**-0.152**	0.004	**0.015**	0.002	**0.136**	0.004
hm	**0.029**	0.005	**0.020**	0.003	**-0.049**	0.005
hf	**-0.196**	0.005	0.003	0.002	**0.193**	0.005
om	**-0.079**	0.006	**0.015**	0.003	**0.064**	0.006
of	**-0.182**	0.006	**-0.005**	0.003	**0.187**	0.006
y99*cali	**0.027**	0.008	**-0.008**	0.004	**-0.019**	0.007
wf*cali	**0.022**	0.008	-0.005	0.004	**-0.017**	0.007
bm*cali	-0.011	0.019	-0.004	0.012	0.015	0.017
bf*cali	0.008	0.018	0.003	0.010	-0.011	0.017
hm*cali	**0.061**	0.011	-0.003	0.007	**-0.058**	0.010
hf*cali	**0.043**	0.011	-0.003	0.006	**-0.040**	0.011
om*cali	**0.054**	0.014	-0.012	0.008	**-0.041**	0.013
of*cali	0.022	0.014	**-0.014**	0.006	-0.008	0.014
wf*y99	**0.006**	0.003	**0.003**	0.001	**-0.009**	0.002
bm*y99	0.006	0.006	**-0.006**	0.003	0.001	0.006
bf*y99	**0.044**	0.005	-0.003	0.003	**-0.041**	0.005
hm*y99	**0.024**	0.007	**-0.012**	0.004	**-0.012**	0.006
hf*y99	**0.038**	0.007	0.004	0.003	**-0.042**	0.007
om*y99	**0.018**	0.009	-0.005	0.005	-0.014	0.008
of*y99	0.014	0.009	0.002	0.003	**-0.016**	0.009
y99*cali*wf	**-0.031**	0.011	0.006	0.005	**0.026**	0.011
y99*cali*bm	-0.041	0.026	0.009	0.015	0.032	0.025
y99*cali*bf	-0.030	0.024	-0.018	0.012	**0.048**	0.024
y99*cali*hm	-0.001	0.014	-0.009	0.008	0.010	0.013
y99*cali*hf	**-0.034**	0.016	-0.004	0.007	**0.038**	0.015
y99*cali*om	**-0.065**	0.020	0.005	0.010	**0.060**	0.019
y99*cali*of	-0.020	0.020	0.002	0.008	0.018	0.020

*Standard errors are robust. Values in bold are significant at 10% level.
**The regressions also included the following explanatory variables: *age*, age^2, education dummies, interview month dummies, region dummies, *urbanarea*, and dummies for citizenship and native status.

Bibliography

Chunrong Ai and Edward C. Norton. Interaction terms in logit and probit models. *Economics Letters*, 80, 2003.

American Society of Newspaper Editors. Tables from the 2002 Newsroom Employment Census, 2003. URL `www.asne.org`.

Kate L. Antonovics and Brian G. Knight. A new look at racial profiling: Evidence from the Boston police department. NBER working paper 10634, 2004.

Peter Arcidiacono, Jacob Vigdor, and Eric Aldrich. Do people value racial diversity? Evidence from Nielsen ratings. Working Paper, 2004.

Orley Ashenfelter and James Heckman. Measuring the effect of an antidiscrimination program. In Orley Ashenfelter and James Blum, editors, *Evaluating the Labor-Market Effects of Social Programs*. Industrial Relations Section, Princeton University, 1976.

Susan Athey, Christopher Avery, and Peter Zemsky. Mentoring and diversity. *American Economic Review*, 90, 2000.

Martin J. Bailey. Effects of race and of other demographic factors on the values of single-family homes. *Land Economics*, 42, 1966.

Gary S. Becker. *The Economics of Discrimination*. University of Chicago Press, 1957.

Francine D. Blau and Lawrence M. Kahn. Changes in the labor supply behavior of married women: 1989-2000. NBER working paper 11230, 2005.

Richard C. Burdekin and Todd L. Idson. Customer preferences, attendence and the racial structure of professional basketball teams. *Applied Economics*, 23:179–186, 1991.

Daniel N. Chambers. The racial housing price differential and racially transitional neighborhoods. *Journal of Urban Economics*, 32, 1992.

Stephen Coate and Glenn C. Loury. Will affirmative action policies eliminate negative stereotypes? *American Economic Review*, 83, 1993.

Paul N. Courant and John Yinger. On models of racial prejudice and urban residential segregation. *Journal of Urban Economics*, 32, 1977.

David M. Cutler, Edward L. Glaeser, and Jacob L. Vigdor. The rise and decline of the american ghetto. *Journal of Political Economy*, 107, 1999.

Eric Deggans. Minorities fill 2 WFLA weekend anchor jobs. *St. Petersburg Times*, May 8, 2003.

Carmen DeNavas-Walt, Bernadette D. Proctor, and Robert J. Mills. Income, poverty, and health insurance coverage in the united states: 2003. Current Population Reports P60-226, U.S. Census Bureau, 2004.

Reynolds Farley, Elaine L. Fielding, and Maria Krysan. The residential preferences of blacks and whites: A four metropolis analysis. *Housing Policy Debate*, 8, 1997.

James R. Follain, Jr. and Stephen Malpezzi. Another look at racial differences in housing prices. *Urban Studies*, 18, 1981.

Morris Goldstein and Robert S. Smith. The estimated impacts of the antidiscrimination program aimed at federal contractors. *Industrial and Labor Relations Review*, 29, 1976.

James Gwartney and Charles Haworth. Employer costs and discrimination: the case of baseball. *Journal of Political Economy*, 82(2):873–881, 1974.

David R. Harris. "property values drop when blacks move in, because....": Racial and socioeconomic determinants of neighborhood desirability. *American Sociological Review*, 64, 1999.

Paige M. Harrison and Jennifer C. Karberg. Prison and jail inmates at 2003. Technical Report NCJ 203947, Bureau of Justice Statistics Bulletin, 2004.

James J. Heckman and Kenneth I. Wolpin. Does the contract compliance program work? An analysis of Chicago data. *Industrial and Labor Relations Review*, 29, 1976.

Harry Holzer and David Neumark. Are affirmative action hires less qualified? evidence from employer-employee data on new hires. *Journal of Labor Economics*, 17, 1999.

Harry Holzer and David Neumark. Assessing affirmative action. *Journal of Economic Literature*, 38, 2000a.

Harry Holzer and David Neumark. What does affirmative action do? *Industrial and Labor Relations Review*, 53, 2000b.

Harry J. Holzer and Keith R. Ihlanfeldt. Customer discrimination and employment outcomes for minority workers. *The Quarterly Journal of Economics*, 113(3): 835–867, August 1998.

Keith R. Ihlanfeldt and Benjamin Scafidi. Black self-segregation as a cause of housing segregation: Evidence from the Multi-city Study of Urban Inequality. *Journal of Urban Economics*, 51, 2002.

Amie Jamieson, Hyon B. Shin, , and Jennifer Day. Voting and registration in the election of 2000: Population characteristics. Current Population Reports P20-542, U.S. Census Bureau, 2002.

George E. Johnson and Finis Welch. The labor market implications of an economywide affirmative action program. *Industrial and Labor Relations Review*, 29, 1976.

Lawrence M. Kahn and Peter D. Sherer. Racial differences in professional basketball players' compensation. *Journal of Labor Economics*, 6(1), 1988.

Mark T. Kanazawa and Jonas P. Funk. Racial discrimination in professional basketball: Evidence from Nielsen ratings. *Economic Inquiry*, 39(4):599–608, 2001.

Katherine A. Kiel and Jeffrey E. Zabel. House price differentials in U.S. cities: Household and neighborhood racial effects. *Journal of Housing Economics*, 5, 1996.

Katherine A. Kiel and Jeffrey E. Zabel. The accuracy of owner-provided values: The 1978-1991 American Housing Survey. *Real Estate Economics*, 27, 1999.

A. Thomas King and Peter Mieszkowski. Racial discrimination, segregation, and the price of housing. *Journal of Political Economy*, 81, 1973.

Jonathan S. Leonard. Antidiscrimination or reverse discrimination: The impact of changing demographics, Title VII, and affirmative action on productivity. *The Journal of Human Resources*, 19, 1984a.

Jonathan S. Leonard. Employment and occupational advance under affirmative action. *Review of Economics and Statistics*, 66, 1984b.

Jonathan S. Leonard. The impact of affirmative action on employment. *Journal of Labor Economics*, 2, 1984c.

Jonathan S. Leonard. Women and affirmative action. *Journal of Economic Perspectives*, 3, 1989.

Jacob Mincer. Unemployment effects of minimum wages. *Journal of Political Economy*, 84, 1976.

Clark Nardinelli and Curtis Simon. Customer racial discrimination in the market for memorabilia: The case of baseball. *The Quarterly Journal of Economics*, 105 (3):575–595, August 1990.

National Center for Health Statistics. Health, united states, 2004. Technical report, National Center for Health Statistics, 2004.

State of California. Fair employment and housing act. Title 2 Division 3 Part 2.8 of California government code, section 12988 (1959-2005).

Executive Order no. 11426 3 C.F.R. 339 (1964-1965).

Bob Papper. Tables from the RTDNA/Ball State University Survey on Women and Minorities, 2003. URL `www.rtnda.org`.

ESPN Research. 2003 audience profile, 2004. URL `www.espn.com`.

Pew Research Center. 2004 media consumption and reliability survey, 2004. URL `www.people-press.org`.

William M. Rodgers, III and William E. Spriggs. The effect of federal contractor status on racial differentials in establishment-level employment shares: 1979-1992. *The American Economic Review*, 86, 1996.

Stephen L. Ross. Segregation and racial preferences: New theoretical and empirical approaches. unpublished manuscript, University of Connecticut, 2002.

James P. Smith and Finis Welch. Affirmative action and labor markets. *Journal of Labor Economics*, 2(2), 1984.

Paul M. Sommers and Noel Quinton. Pay and performance in major league baseball: The case of the first family of free agents. *Journal of Human Resources*, 17(3): 426–436, 1982.

Eric W. Stone and Ronald S. Warren, Jr. Customer discrimination in professional basketball: evidence from the trading-card market. *Applied Economics*, 31:679–685, 1999.

Ward Thomas and Mark Garrett. U.S. and California affirmative action policies, laws, and programs. In Paul Ong, editor, *Impacts of Affirmative Action: Policies and Consequences in California*. Alta Mira Press, 1999.

United States Census Bureau. Appendix A: Census 2000 geographic terms and concepts, 2000. Online document available at www.census.gov.

United States Census Bureau. The population profile of the united states: 2000, 2001. Internet Release.

United States Census Bureau. Housing vacancies and homeownership annual statistics: 2004, 2004. Online document available at www.census.gov.

Darrell L. Williams. The California civil rights initiative: Which firms stand to lose and how much? In Paul Ong, editor, *Impacts of Affirmative Action: Policies and Consequences in California*. Alta Mira Press, 1999.

John Yinger. Racial prejudice and racial residential segregation in an urban model. *Journal of Urban Economics*, 3, 1976.

John Yinger. The black-white price differential in housing: Some further evidence. *Land Economics*, 54, 1978.

Vita

Caitlin Meredith Knowles Myers was born November 12, 1977, the daughter of Allen Lee Knowles, III and Iris Lawrence Knowles. After completing her work at LaGrance High School, LaGrange, Georgia in 1995, she entered Tulane University in New Orleans, Louisiana on a Dean's Honor Scholarship. She attended the Universidad Iberamericana in Mexico City during the summer of 1997 and Universidad de la Habana during the summer of 1998. After graduating from Tulane University in 1999, she worked for a year as a project administrator for URS corporation. In 2000 she married Adam Troy Myers. That same year, she entered graduate school at the University of Texas at Austin where she earned a Master of Science in 2002. While at the University of Texas, she has served as a teaching assistant for graduate and undergraduate courses and as an instructor for Principles of Microeconomics.

Permanent Address: 206 Avondale Dr., LaGrange, GA 30241

This dissertation was typeset with LaTeX 2_ε[1] by the author.

[1] LaTeX 2_ε is an extension of LaTeX. LaTeX is a collection of macros for TeX. TeX is a trademark of the American Mathematical Society. The macros used in formatting this dissertation were written by Dinesh Das, Department of Computer Sciences, The University of Texas at Austin, and extended by Bert Kay, James A. Bednar, and Ayman El-Khashab. The author updated the macros to comply with current formatting requirements.